GORDON H. BOWER

An Oral History
conducted by Daniel Hartwig

STANFORD HISTORICAL SOCIETY ORAL HISTORY PROGRAM

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Introduction

This oral history was conducted by the Stanford Historical Society Oral History Program, in collaboration with the Stanford University Archives. The program is under the direction of the Oral History Committee of the Stanford Historical Society.

The Stanford Historical Society Oral History Program furthers the Society’s mission “to foster and support the documentation, study, publication, dissemination, and preservation of the history of the Leland Stanford Junior University.” The program explores the institutional history of the University, with an emphasis on the transformative post-WWII period, through interviews with leading faculty, staff, alumni, trustees, and others. The interview recordings and transcripts provide valuable additions to the existing collection of written and photographic materials in the Stanford University Archives.

Oral history is not a final, verified, or complete narrative of events. It is a unique, reflective, spoken account, offered by the interviewee in response to questioning, and as such it may be deeply personal. Each oral history is a reflection of the past as the interviewee remembers and recounts it. But memory and meaning vary from person to person; others may recall events differently. Used as primary source material, any one oral history will be compared with and evaluated in light of other evidence, such as contemporary texts and other oral histories, in arriving at an interpretation of the past. Although the interviewees have a past or current connection with Stanford University, they are not speaking as representatives of the University.

Each transcript is edited by program staff and by the interviewee for grammar, syntax, and occasional inaccuracies and to aid in overall clarity and readability, while maintaining the substantive content of the interview as well as the interviewee’s voice. As a result of this editing process, the transcript does not match the recording verbatim. In the rare case that a substantive deletion has been made, this is indicated at the relevant place on the transcript. Any substantive additions are noted in brackets or by footnote.
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This oral history should be cited as “Gordon H. Bower, Stanford Historical Society Oral History Program Interviews (SC0932). Department of Special Collections & University Archives, Stanford University Libraries, Stanford, Calif.”
Abstract

Gordon H. Bower, the Albert Ray Lang Professor of Psychology, Emeritus, is a well-known cognitive psychologist. In his six-part oral history Gordon Bower traces the evolution of his career from his childhood, baseball playing, and education in Ohio to his retirement and current life at Stanford. Bower devotes the bulk of the interview to elaborating on his research program, beginning at Yale as a graduate student and continuing through his time at Stanford. He describes his work in learning and memory, including the study of human memory, mnemonic devices, retrieval strategies, recording strategies, and category learning. Bower also discusses his research on cognitive processes, emotion, imagery, language and reading comprehension as they relate to memory. In addition to his own research, Bower examines the work of colleagues and others who influenced him, including developments both within and outside of psychology. Bower recounts his service as associate dean and member of the Appointments and Promotions Committee at Stanford, president of the American Psychological Society, chief science advisor to the director of the National Institutes of Mental Health, and editor of the annual book series The Psychology of Learning and Motivation.
Gordon H. Bower

Biography

Gordon H. Bower, the Albert Ray Lang Professor of Psychology, Emeritus, broadened our understanding of how we learn, remember, and forget, and brought to light the role that emotions play in this process during his long career in Stanford University’s Department of Psychology. He has written over 200 scientific papers and several books and edited twenty-five volumes of The Psychology of Learning and Motivation series. In 2005, Bower was awarded the National Medal of Science.

Bower was born in 1932 in Bowerston, Ohio. In high school he showed great promise both academically and on the baseball field. He entered Western Reserve University (now Case Western Reserve University) with the intention of becoming a psychiatrist or possibly playing baseball professionally, but in the end he was drawn to the quantitative aspects of psychology and chose to pursue experimental and theoretical research. After graduating in 1954, he pursued advance study at Yale University, earning his master’s degree in 1957 and his PhD in 1959 in Neal Miller’s lab. At Yale, Bower researched pain and pleasure stimulation in the brains of rats while also exploring mathematical learning models.

In 1959 Patrick Suppes recruited Bower to the newly formed math and social science program in the Psychology Department at Stanford. He continued his work with rats but was drawn to modeling human learning, specifically short-term memory storage. In an effort to understand the underlying principles of memory, he employed mnemonics and imaging techniques in his experiments, and along with his graduate student, John Anderson, developed the human associative memory model. Bower’s interest in state-related memory led him to discover that memory and emotional states were linked and that recall was better if a person was in the same emotional state as when they learned a particular fact.

In 1966 Bower became a full professor, and in 1973 he became the youngest psychologist to be elected to the National Academy of Sciences. He served as chairman of his department from 1978 to 1982 and associate dean of Humanities and Sciences from 1983 to 1986. Bower has held the position of president in numerous professional groups including the
Psychonomic Society (1967), Division 3 of the American Psychological Association (1975), the Cognitive Science Society (1987), the Western Psychological Association (1990 and again 2004) and the American Psychological Society (1991-1993). He contributed to national science policy when, as senior science advisor to the director of the National Institute of Mental Health (NIHM) from 1992 to 1993, he conducted a review of the effects of basic behavioral research on mental health care, and advocated for increased funding from Congress in the final report. By the time he retired in 2008, he had maintained forty-eight continuous years of funding from the NIHM, trained fifty-four psychology PhDs, and won numerous awards including the Howard Crosby Warren Medal (1986), the Wilbur Cross Medal for Distinguished Scientific Contributions and Career (1995), and the Western Psychological Association’s Lifetime Contribution Award (2006).

A person of broad interests, Bower enjoys auditing classes in history, literature, and art at Stanford University.
Interview Session One
August 1, 2014

Hartwig: Hello. This is Daniel Hartwig on behalf of the Stanford University Archives and the Stanford Historical Society. Today is August 1, 2014. I’m here with professor of psychology, emeritus, Gordon Bower. Mr. Bower, welcome.

Bower: Thank you.

Hartwig: This oral history is part of the Historical Society’s faculty and staff interviews. So, Professor Bower, let’s go back to the beginning, so to speak, and talk about your ancestors, where they came from and where they settled.

Bower: As far as we know, my ancestors on my father’s side came from Germany. In 1751, a man named Conrad Bauer, B-a-u-e-r, it’s spelled, came in through Philadelphia. This is before the Revolutionary War; he must have been in his twenties then, so he was probably too old to fight by the time the Revolutionary War came along, but, nonetheless, he immigrated here.

My nephew has followed the family down through various generations. The family originally settled in Maryland as farmers, and then about four generations ago, a few of them moved to Conotton Creek in eastern Ohio and set up a flour mill that ground wheat for flour. It was called Bower’s Mill.

The surrounding population eventually grew up a bit and it became named Bowersville, and then for some reason the name got changed to Bowerstown, and then Bowerston, so Bowerston is the name that persists today. That’s where my father was born, where he had many relatives—cousins and ancestors.
My grandfather, Dad’s father, was born in Bowerston to the Bower family. He was twelfth out of fifteen kids. He lived around Bowerston most of his young life, and as a young adult he moved to this little town of Scio, about six miles away. There he bought a grocery store that had an attached house, so one could live in the house and walk through a door right into the grocery store. Grandpa also bought a farm on the outskirts of town. I remember my grandpa--this is on my dad’s side--although he died when I was about five or six years old, but I remember him a little bit.

Hartwig: Why did he move to Scio?

Bower: [00:03:20] I don’t know. Maybe there were too many Bowers in Bowerston. Also, in Bowerston he married a woman named Nora Gordon. The Gordons were part of a Scottish clan, and there were many of them living in and around Bowerston. In my youth I thought Bowerston must be made up two-thirds of Bowers and Gordons together. Appropriately, when I was born, I was named Gordon Bower, after my grandma and my grandpa. [laughs]

Hartwig: Do you have any fond memories of your grandparents?

Bower: [00:04:00] Not too many. I remember my grandma was sick with a heart ailment and spent most of the time in bed. In those days when you had a weak heart, they put you to bed. She had a very round face. I remember thinking of her as a moon face. [laughs] Strange. My grandpa was thin and kind of a hard brittle man. He was a horse trader and ran this general store in Scio, where he sold livestock feed and farming equipment, as well as groceries and meat and clothing and shoes and kerosene and gasoline and all of those things that one found in a general store in those days.
Hartwig: Was it a successful operation?

Bower: [00:05:05] I assume so. He survived. Grandpa had a heart attack or stroke around the nineteen mid-twenties sometime, and became relatively incapacitated. My dad had just graduated from Ohio State University. But in those days the eldest son was expected to come back and take care of the property when the father got sick, and that’s what he did. My dad got called back to take over grandpa’s grocery store.

Hartwig: Was he just about to leave for Europe or he was planning to go to Europe?

Bower: [00:05:53] He was indeed planning to go to Europe and/or South America as a salesman, I think for the Swift Company, which was a meatpacking company in Cleveland, Ohio.

Hartwig: I’m familiar with one in Iowa, actually. I don’t know if it’s the same, but could be.

Bower: [00:06:13] My dad was educated at Ohio State University and had graduated, and a couple of years after that, he was working for the Swift company, and all primed to go overseas. He wanted to be an overseas traveler, and towards that end he had studied Spanish and German so he could communicate with the customers over in Europe and South America.

Hartwig: What was his degree in, do you know?

Bower: [00:06:44] Business administration.

Hartwig: Did he have brothers and sisters?

Bower: [00:06:49] He had a sister. There were only two kids in their family, which in those days was unusual.

Hartwig: And was she local or did she--
Bower: [00:06:59] Yes, she was born in Bowerston, and she, too, moved to Scio. Grandpa bought a hardware store for her to run, right across the street from our grocery store. She sold large farm equipment like tractors, ploughs, and combines and corn huskers and so on--along with tools and small hardware equipment.

Hartwig: So how did your dad adjust to the transition?

Bower: [00:07:32] I wasn’t there in the beginning, in the mid-1920s when he went there. I didn’t come along until 1932; I think he had been in Scio for probably five to six years by that time.

Hartwig: Did he ever talk about it?

Bower: [00:07:55] No, not very much about the transition. He talked a little bit about his childhood before they’d moved to Scio, but not a lot about the accommodations he had to make to come and take over the grocery store when his father got very ill and became incapacitated.

Hartwig: How long did he run the store for then?

Bower: [00:08:31] Until he had his first heart attack in about 1961, but he kept on working until he had his second, and killer, heart attack in 1963 and died. My brother had taken over the store after the first one. My brother was two years older than me.

Hartwig: So he was born around 1930?

Bower: [00:09:00] In July, 1930.

Hartwig: What was his name?

Bower: [00:09:00] Robert. Robert Harry. He just passed away this month, July 7th, 2014.
Hartwig: So what did he do?

Bower: [00:09:17] Well, when we were little, we played and ran around together, of course. [laughter] He was my big brother.

Hartwig: So talk about growing up then as children in Scio. What was it like?

Bower: [00:09:34] Wonderful. From my point of view, it was grand. A little town of about, I'd guess, 800 to 1,000 people located on the edge of Appalachia, near the Pennsylvania-West Virginia-Ohio tri-state area. We were about 30 miles from Wheeling, West Virginia. You knew everybody in town, everybody in town knew you and your parents and siblings. It was very warm and wonderful when we were children. My brother and I played sports together and went hunting and fishing, learned to shoot rifles and bows and arrows.

Hartwig: Did you teach yourselves or did your dad teach you, or where did you learn to hunt and fish and play baseball?

Bower: [00:10:38] We learned it by ourselves or from older guys, fishermen or hunters or so on. Dad didn't participate in any of those sports; he'd never learned them as a kid. I was just speaking at my brother's funeral about this. One time we decided we were going to become muscle men, so through a pulp magazine we bought a book on how to build up our muscles. Charles Atlas was the man who promoted it in those days. I remember his advertisement, something like, “Don't let bullies kick sand in your face at the beach. Get up and fight 'em.” So we bought Charles Atlas' book on muscle building, and we would heave and grunt with the weights and try to build up our muscles.

Hartwig: What were you lifting? Did you have a weight set or--
Bower: [00:11:38] Well, we made our weights out of coffee cans and we’d put cement in them, let it harden with this broom stick in the middle, and then turn it over and put another coffee can on the bottom and fill it up and let it harden, so then you’d have one of these heavy barbells.

Hartwig: So about what age were you at the time?

Bower: [00:11:56] Eight, nine.

Hartwig: Did you see any results?

Bower: [00:11:59] No. [laughter]

Hartwig: Were you doing it right?

Bower: [00:12:03] Probably not. I remember my dad once bought us boxing gloves. In those days, Joe Louis was the premier heavyweight champion of boxing. We listened to all of his fights on the radio, and Dad somehow got boxing gloves for us. They were huge. They were about as big as watermelons, worn on your hands. We would flail away at each other with these huge pads.

Hartwig: So did he try to teach you technique or--

Bower: No.

Hartwig: --just have at it?

Bower: [00:12:35] Yes. We did that for a year or two. Then I remember one day my brother let go a haymaker that landed on the point of my chin and knocked me on my keister, and for the first time in my life I actually saw stars. [laughter] And I said, “I don’t think I want to box anymore. I’ve had enough.”

Hartwig: So you and you brother were definitely competitive.
Bower: [00:13:05] Not at all. We played together. He was my catcher for baseball. When I was about eight years old, I decided I wanted to be a baseball player. I saw a movie called *The Pride of the Yankees*, which was about Lou Gehrig, and he became my hero. I wanted to play Major League baseball, and so starting about age eight, I really devoted myself—hundreds and hundreds of hours—to developing my baseball skills. And then because Scio high school had other teams, I also learned basketball.

Hartwig: Did they have enough kids to form a team? Did they have a field?

Bower: [00:13:59] Well, we did have a field that we’d scraped and leveled off. Some adults had helped with that. I know in football our high school didn’t have enough players, so they had six-man rather than eleven-man football in those days.


Bower: [00:14:15] Oh, six-man is fun, everybody gets involved in every play of that game. I didn’t play football; I needed to protect my arm because I wanted to be a pitcher.

Hartwig: So you were always a pitcher?

Bower: [00:14:27] I was training to be a baseball pitcher, and I was a very good little pitcher. I didn’t want to injure my arm playing football. When I was little, nine, ten, eleven years old, my brother was my catcher. I spent countless hours throwing to him, trying out different curveball pitches. I developed a very big sweeping curveball when I was about ten years old.

Hartwig: Isn’t that a little early to be throwing curveballs? Is that bad on your arm?
Bower: [00:15:08] That advice only came along later. In the old days that kind of talk was for sissies. [laughter]

Hartwig: I mean, I played, and no one threw curveballs. They just threw fastballs.

Bower: [00:15:20] You had a deprived childhood. [laughter] So I developed a big sweeping curveball, and I could throw it overhand to get it to drop, three-quarters to get it to move out and drop, sidearm to produce a big sweeping curve, and throw it underhand to get a curve that would move up and out. Those pitches were sufficiently devastating that the players on the older semi-pro team in Scio--these were World War II veterans--asked me to pitch for their team. I was about twelve years old at the time.

Hartwig: And how old were they?

Bower: [00:15:58] Well, they were World War II veterans in their twenties and thirties.

Hartwig: A twelve-year-old versus twenty--

Bower: [00:16:03] Twenties and thirties, yes. Well, I could throw a hell of a curveball.

[laughs]

Hartwig: How fast were you throwing, approximately?

Bower: [00:16:09] Not too fast. No one could measure speed of pitches in those days.

Hartwig: Not too fast, but the curveball worked. What else? Was it just curveballs or what else?

Bower: [00:16:14] Oh, I'd throw a fastball on the inside or outside the plate, you know. Didn't want to get it over the plate too much for the hitters to feast on.
Hartwig: Were you pretty accurate?

Bower: [00:16:22] Yes, I could get the curveball over for strikes.

Hartwig: Did you ever have any coaching?

Bower: [00:16:28] No.

Hartwig: All self-taught.

Bower: [00:16:32] Yes, although I bought a book written by Christy Mathewson, who was an old pitcher in the 1920s, on how to throw breaking balls, and I think I also got a book by Bob Feller, a famous pitcher in the '40s.

Hartwig: “Heater from Van Meter.”

Bower: [00:16:50] That’s right.

Hartwig: Well, you liked Gehrig, but you’re from Ohio, so why not Cleveland?

Bower: [00:16:59] Yes, of course. By the time I was twelve and thirteen, we were big fans of the Cleveland Indians, and from 1945 through 1950, the Indians had pretty good teams. Lou Boudreau was their player-manager.

Hartwig: Yes.

Bower: [00:17:20] I’m surprised you know this. You’re one of the few people in the world who knows this stuff.

Hartwig: I grew up playing baseball, still follow the sport.

Bower: [00:17:28] Really?

Hartwig: Larry Doby, wasn’t he the second--

Bower: [00:17:30] Larry Doby was one of the first black guys in the majors after Jackie Robinson.

Hartwig: Yes, first in the American League, I think.
Bower: [00:17:35] Played in center field. That’s right. He was the first black that I can recall seeing play in the majors, and he was a good left-handed hitter, and then the Indians hired in Luke Easter. Yes.

Hartwig: Did you go to games in Cleveland?

Bower: [00:17:52] Not very much, because Cleveland was a long way away, 100 miles. Boy, in those days 100 miles was forever away.

Hartwig: Did you go on vacations, or did you do much to get away from Scio?

Bower: [00:18:06] My dad and mom didn’t go on vacations much. When you have a grocery store, somebody has to be there to tend to it, open it up every day. So they couldn’t leave for any long periods. Well, we didn’t open up the store on Sundays. We went to church on Sundays.

Back to baseball: I was allowed to play baseball on Sundays. When I got older, like, sixteen, seventeen, I grew bigger and stronger, and started to throw a lot harder, but in the process I lost my sweet curveball.

Hartwig: How? Just too strong?

Bower: [00:18:41] I was just overthrowing it too hard or something, and the curve no longer had that big sweep to it. I guess I should have slowed it down--I don’t know. So I just developed a good fastball.

Hartwig: So this was in high school, then?

Bower: [00:18:57] Yes. I also threw a screwball and a fairly okay sinker.

Hartwig: Did you have a changeup?

Bower: [00:19:04] Of course. I didn’t like to throw it very much. I got a bigger kick out of just blowing fastballs past the hitters. [laughs]

Hartwig: Do you remember your stats? What was your ERA?
Bower: [00:19:15] I haven’t any idea. No one kept such statistics for such little town teams.

Hartwig: But you were good enough to get offers to play.

Bower: [00:19:19] Baseball, with the Pittsburgh Pirates and with the Cleveland Indians.

Hartwig: Did they have a draft back then?

Bower: [00:19:28] No. The baseball scouts would just come around to watch you play in games. The Indians were interested in me. In my senior year in high school, I was voted an all-state player in baseball and in basketball.

Hartwig: What position did you play in basketball?

Bower: [00:19:49] I was a forward, and we had good teams. One of my co-players was really good, and he went on to play college ball for a while, but then didn’t last very long. Because I was all-state in baseball and in basketball, I got college offers to play basketball, too.

Hartwig: But you liked baseball the most?

Bower: [00:20:25] Yes. I knew I wanted to play baseball for sure, so I accepted a baseball scholarship that was offered to me by a school called Western Reserve University, now called Case Western Reserve.

Hartwig: Was that the only scholarship you had?

Bower: [00:20:49] No, I had some in basketball from Union College, Wooster College, and University of Cincinnati.

Hartwig: But you wanted to play baseball.

Bower: [00:21:00] Yes. I think the Cleveland Indians kind of bankrolled under the table the athletic scholarship I got from Western Reserve. The agreement I
had with them was that at the end of college or whenever I wanted to get out
and play baseball professionally, they would have first option on signing me.
And I did, indeed, want to continue playing beyond college. I had a good
career pitching baseball in college, and I was a good enough hitter that when
I didn’t pitch, they’d put me out in the outfield, hoping that opposing players
wouldn’t hit the ball in my direction too much.

Hartwig: Were you a good batter?

Bower: [00:21:47] Yes, I was a good hitter, a homerun hitter. I hit for a good average,
   too.

Hartwig: What did you hit in the order?

Bower: [00:21:56] Third or fourth.

Hartwig: Power hitter. Do you remember your stats as a hitter?

Bower: [00:22:02] I don’t know. Isn’t that strange? I can’t remember that anybody
   kept statistics, but of course the coaches probably did. I just never saw them.

Hartwig: What’s your favorite baseball memory or proudest accomplishment?

Bower: [00:22:17] When I was about sixteen, I was playing with the local semi-pro
town team against a team from another town 20 miles away, from
Amsterdam, Ohio. In three successive times at bat I hit homeruns clear out
of the park because Amsterdam had a baseball field with an actual fence
around it. [laughter] So one could hit it over the fence for a homerun, and I
put that sucker over the fence three successive times at bat.

Hartwig: About how far do you think they went each time?


Hartwig: That’s pretty good.

Hartwig: So do you think you could have played pro ball?

Bower: [00:23:26] Yes, I think I could have. But the military draft got in the way of those plans.

Hartwig: Did you play against any really good players that went on to play the pros?

Bower: [00:23:32] Not that I knew of. Bill Mazeroski played for the Pittsburgh Pirates, and I knew his brother Bobby. His brother was a pitcher. But Bill’s the only guy I know who got into the majors. There must have been more, I’m sure. But I don’t remember any.

Hartwig: So let’s go back a bit. So when was the first time you learned about psychology, or I guess back then psychoanalysis? How did you become familiar with Freud, and what set you on your future track?

Bower: [00:24:15] It was a teacher, of course. A woman teacher and her husband came to Scio, to this little town. Why, I’ll never know. The husband became the superintendent. Jim Wiggins was his name and her name was Virginia Wiggins. They must have arrived at the time I was a freshman or sophomore in high school.

Hartwig: Do you remember where they came from?

Bower: [00:24:53] No, I don’t. Some other place in Ohio. I know they both had graduated from Ohio State University. He was very impressive as an intellectual, very serious, very well-schooled in the larger national events and political issues. He also knew a lot about chemistry and biology, could teach
science. And Virginia just dazzled me. She was beautiful and brilliant, very witty, very fluent and very alluring and dramatic and cosmopolitan.

Of course, I fell head-over-heels in love with her like little schoolboys do with their beautiful teachers. Importantly, Jim and Virginia took an interest in my mind rather than my pitching arm.

**Hartwig:** Much to your dismay?

**Bower:** [00:26:01] No, no, no. They were some of the few people who cared about developing my intellect, I guess. Of course, my parents cared that I did something besides baseball. I think I had a minor interest in chemistry before then, and I’d done well in my science courses.

With some help from Jim, Virginia and I used to have many little heart-to-heart talks about my future plans. We eventually got around to thinking that psychoanalysis would be a fascinating field for me to go into. Virginia was really hot about psychoanalytic thinkers at the time, and it was she who got me reading Freud and Carl Jung and Alfred Adler.

**Hartwig:** What were your initial impressions of them at that time?

**Bower:** [00:27:11] Of who, of Freud?

**Hartwig:** Of Freud and Jung and the psychoanalysts.

**Bower:** [00:27:18] I thought it was absolutely fascinating. Freud was talking about sex, of all things, good lord. [laughter] You know, Freud told all these stories about oversexed or undersexed young neurotic women in turn-of-the-century Vienna.

**Hartwig:** Indeed, perfect subject matter for a young boy.
Bower: [00:27:43] Perfect subject matter indeed for a young boy. So I got to reading quite a bit of Freud, and got into a fair amount of Carl Jung. I read Jung largely because Philip Wylie liked Carl Jung. Philip Wylie was a popular novelist at that time and Virginia loved his novels, and so I picked up and read all of Philip Wylie. And because Wylie was nuts about Carl Jung, I read Jung. I didn’t understand all the details of Carl Jung, still don’t, actually. He was far out. Not crazy, just very speculative, more so than Freud.

So I became interested in psychoanalysis and decided I was going to become a psychiatrist, so that when I went to college at Western Reserve University on this baseball scholarship, I started out in their premedical program, because to become a psychiatrist you had to first have an M.D. So the first couple of years I was at Western Reserve, I was in the pre-med program taking all the required science classes such as physics, biology, and chemistry. I remember two of the most mind-numbing classes I ever had were in biology, one called comparative anatomy and the other, comparative embryology, in which we had to memorize the names and locations and be able to identify all the muscles and bones and nerves in four different mammals.

Hartwig: So you weren’t interested in memorizing at the time, and memory?

Bower: [00:29:57] No, I hated those courses.

Hartwig: Looking back, is that an interesting episode?

Bower: [00:30:05] No. It taught me if you have to memorize too many things, you’re in the wrong field. [laughter]

Hartwig: So what courses did you like that you took?
Bower: [00:30:15] History, literature, political science, and psychology, of course. I took as many psychology classes as I could. It was fortuitous that at that time, the Western Reserve psych department was populated with psychoanalytically oriented psychologists.

Hartwig: Was that unique?

Bower: [00:30:47] Yes, it was very unique at the time. The head of the department was Calvin Hall, who was a very dyed-in-the-wool Freudian who wrote books on Freud, one called *The Primer of Freudian Psychology* and another called *Theories of Personality*, co-authored by Hall and Lindzey. Calvin was a good psychoanalytic thinker. He took an interest in me because he was a rabid baseball fan, so he and I could talk about the Cleveland Indians, but he also would teach me whatever I wanted to know about Freudian analysis.

Hartwig: Could you apply psychoanalysis to baseball in any way?

Bower: [00:31:42] No. These were separate compartments in my mind.

Hartwig: So describe Cleveland at the time. So you’re coming from small-town eastern Ohio into Cleveland. What was it like?

Bower: [00:31:56] Cleveland was mind-blowing for a small-town boy. I was culture-starved, and I just went wild in the city. For example, I was crazy to go to the Cleveland symphony. Western Reserve students could usher at the symphony and get in for free, so I did a lot of ushering to see symphonies. I went to the ballet; I went to dramatic plays, art museums, to jazz bars to listen to bebop and Dixieland music. I went to the Cleveland Indians games, to the Cleveland Browns football games. I got involved in political campaigns.

Hartwig: And you had time to study?
Bower: [00:32:50] No. [laughter] Who wanted to study? I was getting broadly educated. I went to union meetings of the ILWU, International Longshoremen Workers Union, where they had socialist debates. These old-style Marxist guys would stand up and argue with each other about what was the right way for the proletariat to come into power. [laughter] I mean, these guys would be in their sixties and seventies, but sharp as a tack, you know, at least on the issues surrounding socialism, democratic socialism, and communism and so on. It was great. I loved it.

In 1952 there was a presidential election, I campaigned for the candidate of Henry Wallace’s Progressive Party. The candidate was Vincent Hallinan, an old San Francisco leftie who’d defended Harry Bridges of the ILWU. My campaign buddies and I would go knocking on doors, usually in the working-class section of Cleveland, and hand out flyers. To get informed, I also attended huge political rallies for Republican Eisenhower, for Democrat Stevenson, and a small rally for Vincent Hallanan. Eisenhower won the election handily, just crushing Stevenson, getting an enormous 55 percent of the popular vote. Our Progressive Party candidate, Hallanan, came in a very distant third with less than one-fifth of 1 percent of the vote. A bit of a disappointment.

Hartwig: Were you always political from then on?

Bower: [00:34:51] No, just for about two or three years in college. Those were the days of the Joe McCarthy era, and he scared off anybody who wanted to study Marxism or wanted to hang out with socialists and so on.

Hartwig: Did that have an effect on Western Reserve University in any way?
Bower: [00:35:13] No, not at WRU. McCarthy didn’t consider us big enough fish to come hunting there. But he terrorized Marxist friends of mine at Harvard.

Hartwig: So who were some of the other faculty that you studied with or influenced you?

Bower: [00:35:33] Psychology teachers named Morrow, Hertz, and Wallen. The teacher who I connected with and got me going was Charles Porter.

Let’s see. I have to back up a little bit. My deal with the Cleveland Indians was that I’d pitch baseball for Western Reserve, but in the summer time I was supposed to pitch in the semi-pro leagues in Cleveland or in some comparable league. In those days, most of our games were played at night, Thursday night and Sunday night, and so I had the days free. I thought that for my education I should meet some psychotics, somebody who’s really crazy, because if I studying to become a psychiatrist, I ought to know what these people were like. [laughs]

So I applied for a job at the Cleveland State Mental Hospital, which was just a 30-minute bus ride away from the Western Reserve campus. Because I was a big man, they put me on the back wards tending the more debilitated psychotic patients.

Hartwig: So you were like staff?

Bower: [00:37:03] I was a ward attendant, looking after the patients in their dorms, where they slept and lived and spent their day. We made sure they did what they’re supposed to do and not get out of hand. We’d walk them in a group to their lunch or their dinner or to dances. These were highly controlled.
social events between the men and the women. And for me that who summer was just a major learning experience.

**Hartwig:** Describe that. How so?

**Bower:** [00:37:42] Well, first of all, the first week or so I was there working on the back wards, I was terribly frightened that one of the patients would attack me, and so I always moved around with the wall at my back, with nothing behind me. I would walk around always looking over my shoulder to check whether somebody was about to attack me. In fact, with my paranoia, I think I was the crazier one in the group. [laughter]

**Hartwig:** Were you alone with them or were there security staff?

**Bower:** [00:38:23] There were three or four ward attendants on a given ward and a couple of nurses.

**Hartwig:** Wasn’t that dangerous?

**Bower:** [00:38:31] No. It turns out that the patients had no desire to harm anybody. Most of them were very depressed, very withdrawn, misinformed and very worried about their situation is in this large amorphous institution. They were frequently disoriented. Some didn’t know how to take care of themselves, even to toileting themselves. They often were very much absorbed within their own thoughts. Usually it was a depressingly bad scene.

However, there were a few of the patients on the ward who were more or less in contact with reality most of the time, and they would help out with chores like running the laundry room or handing out bed sheets and pillows and the like. I could talk to those people. I got to know several of them quite
well, and to like them, and 90 percent of the time they seemed like “normal” folks.

**Hartwig:** What kinds of treatment were they undergoing? So this was before psychopharmacology, so did they have group therapy or nothing? They were just being kept?

**Bower:** [00:39:59] They were just being warehoused there.

**Hartwig:** So how did this--I mean, so you’re studying--

**Bower:** [00:40:05] It was terrible for me. It was upsetting, depressing, demoralizing to my career aspirations to become a psychiatrist. I saw how the patients were warehoused. There was not really much being done for them because at that time not much was known about what to do with them. It was just very demoralizing to me, and I figured that we had to get better science about what was wrong with these people and how to treat them.

**Hartwig:** So this led you to turn away from psychoanalysis?

**Bower:** [00:40:50] Yes, because these patients were not at all like Freud’s patients in turn-of-the-century Vienna, who were highly over-intellectualized and probably over-wrought neurotics. So it was mind-altering for me.

So that was my first summer there, and then the next year, my sophomore year in college, because I had familiarity with the psychiatry staff and the institution, I was hired back by the psychology department of the hospital to serve as a research assistant. This was in addition to my going to college classes.

During that year I got involved in various projects. I remember one project. I was asked to help out this one guy who was getting his PhD at
Western Reserve University, one of those students doing their clinical internship out at the state mental hospital. This guy was doing a reliability check on the Blacky projective test for senile psychotic women. The Blacky was a new projective test that had just become available a bit earlier. At the time I didn’t know it, but it was a rather low-level, brainless project even for that time, but I thought, “Well, okay.”

Hartwig: Sounds fancy, but-- [laughs]

Bower: [00:42:37] Sounds fancy. So my job was to give the Blacky projective test to these old women. I would go around to one ward after another to test patients. The wards usually had a little testing cubicle where I could bring in a patient and test her. I would show them these pictures of Blacky, a dog, involved in various psychoanalytically relevant scenarios, like Blacky would be dreaming about her tail being cut off--that’s to evoke castration anxiety. [laughter] Or Blacky would be taking a poop next to daddy’s doghouse--that was to tap into anal aggression. And so on. [laughter]

So anyway, I’d show these pictures to the patient, and ask, “Make up a story about what’s going on here. Tell me what’s happening, what led up to this, what’s going on now, what’s going to follow this scene.” And one of the first obstacles I found was that in the hospital with older patients, they don’t see very well but they were not given glasses, so many of them would come in but couldn’t see what’s going on in the pictures. That defeats the purpose of a projective test where the patient is supposed to organize and interpret what’s happening in the picture. So they would say, “I can’t see it so well,
Doc. Could you tell me what’s happening? Then I'll make up a story.”

[laughter]

Hartwig: Did any of them just make something up?

Bower: [00:44:34] So I’d say, “Here’s Blacky dreaming about having her tail cut off.”

Or these old ladies, many of them from Eastern Europe or some old country, were very inhibited about talking about sex or all the psychoanalytically relevant functions of the body, and so they would hem and haw and eventually giggle and laugh as they came out with a story about these high jinx. It was hilarious for me and them—but it was also just very depressing for me. I would look at this and say to myself, “This can’t be what psychology’s going to be about, dealing seriously with this kind of data.” Nonetheless I’d hand over my recordings of this data to this PhD candidate.

Hartwig: And how did his research—or what happened to his—

Bower: [00:45:45] Oh, he got a dissertation out of it and went off to a clinical practice someplace. I haven’t looked him up since.

Hartwig: So did you change the course of your studies?

Bower: [00:45:55] Yes. So about that time that I became disenchanted with medical psychiatry—and this was in the days, as you pointed out, before psychoactive drugs came in, and those just washed through the ward—I decided that what was needed was more scientific studies of what was going on in mental illness and therapy.

Hartwig: Describe “scientific” here. What do you mean?

Bower: [00:46:30] Objective, laboratory-based, I guess, not based on just one’s intuitive beliefs and inferences about what patients are telling you. I came to
believe that psychiatric interviews are one of the more biased ways to get information about what somebody is like or what's wrong with them. I thought psychologists needed to start doing experiments on psychotherapy. You know, if you were going to assess, say, the effectiveness of psychoanalytic therapy, you would give that therapy to some patients, whereas in random assignment other patients wouldn’t get that therapy, and you later compare the behavior of the two groups to see which came out better. Thank heavens the Behavior Therapy Movement was just starting in the late fifties and through the early sixties, and they were doing the kinds of therapy experiments I thought should be done.

Hartwig: And was Porter one of these?

Bower: No. Charles was a straight experimental psychologist. He had just come in from Yale as a freshly minted PhD, and his primary research topic was the detailed study of hearing, audition, which I wasn’t interested in at all. But he taught a class in experimental psychology, which was the first time I’d ever encountered somebody doing experiments in psychology. I took that class, and he and I formed a real friendship because it was clear there weren’t too many people like me around Western Reserve who were really interested in that type of psychology.

So he kind of took me under his wing, he and one of his graduate students named Jim Carlson, and the two of them and I would talk about what experimental psychology was like and could do. Chuck, having come from Yale, had been very influenced by an important Yale psychologist
named Clark Hull, who indeed was the preeminent learning theorist of his
day for plus or minus ten years on either side of 1950.

So Charles and I tried to formalize and put in mathematical form and
symbolic logic the learning theories of Clark Hull. Hull himself was very
interested in just doing that, but didn’t have the horses to do it. Nonetheless,
Charles thought that he and I could do it together. So we tried to formalize
Hull’s theory of behavior using symbolic logic and mathematics. Which was
why I started taking logic and mathematics courses.

Hartwig: Were you influenced by philosophers, or where did this--

Bower: [00:49:55] Very much so, yes. At that time an important component of
theoretical psychology was the philosophy of psychology and methodology
of psychology. Charles Porter was very interested in what was called
philosophy of science in those days. Of course, I got interested in it, too, and
began reading all these books and papers on philosophy of science, written
by important figures of those days.

I got interested enough in that topic so that at the end of my
undergraduate training in psychology, I applied for a graduate fellowship
from the Woodrow Wilson foundation, which would allow me to take time
off and go to the University of Minnesota to study philosophy of science and
mathematics for a year or two.

Hartwig: So why Minnesota?

Bower: [00:51:10] Minnesota at that time had the premier program in philosophy of
science in the world. They had some stupendously good, famous people
there. Herbert Feigl was the premier leader there. He had been in the Vienna
circle of Rudolph Carnap, Moritz Schlick, Kurt Gödel, Ernst Mach, and
Gustav Bergmann and those people who had been in philosophy of science
in Vienna and had escaped from Austria and emigrated to America during
the Nazi regime.

So anyway, Herbert Feigl was the head of that program, and other
faculty members were Michael Scriven, Wilfrid Sellars, May Brodbeck, Paul
Rosenbloom, and Paul Meehl. Paul Meehl was a psychologist, a
psychotherapist, but also a very hardnosed methodologist in psychology.
Paul had written a couple of very important papers on what was the right
way to do theory in psychology, at least according to the philosophy of
science and operationalism.

**Hartwig:** So what did you take away from this one-year fellowship?

**Bower:** [00:52:42] I learned an awful lot about philosophy of science and
epistemology and philosophy of language. The main useful thing I learned
for my later career was mathematics. Along with the philosophy of science, I
was also taking many classes in mathematics and, importantly, mathematical
statistics, probability theory, and stochastic processes, and those really
prepared me for the work I did in the next phase of my life.

**Hartwig:** So did you have in mind that, “I want to apply mathematics to psychology?”

**Bower:** [00:53:23] Yes. That’s what Charles Porter and I thought we were doing, in
our rather futile efforts to formalize Hullian theory, and that’s what I
intended to do.

While I was at the University of Minnesota, I took a course called
“Mathematics in the Social Sciences,” in which the professor--Manuel
Donsker was his name—would review a number of topics from economics and sociology and psychology where mathematics sometimes was being used. In that class he talked about a paper by William Estes called “Mathematical Theory of Learning” or something like that. Donsker described the gist of that paper. I was very taken with it so went off to the library and got Estes’ paper and read it and said, “By golly, this is the kind of work I should be doing.” And at the same time I got other papers that Estes wrote and those were referring to still other papers by Bob Bush and Fred Mosteller.

Also at about the same time Bush and Mosteller published a book called *Stochastic Models of Learning*, in 1955. It was that year that I was at Minnesota that that book came out. I was so taken by it that I organized a seminar of graduate students mainly from the psychology department, but some from philosophy, and I led that student seminar. We went through that Bush and Mosteller book from cover to cover, and I was really quite taken by it all.

Throughout the year that I spent at Minnesota I’d always been intending to go on into psychology. I knew Minnesota was only a diversion to get tooled up to do what I really should be doing, namely, theoretical work in experimental psychology.

**Hartwig:** So did you then begin looking for PhD programs?

**Bower:** [00:55:41] So I looked for a graduate program and applied to Harvard and to Yale. At Harvard, Bob Bush and Fred Mosteller were there, and at Yale, Neal Miller was there. So I applied and was accepted to both graduate programs. I talked to both Bush and Mosteller, and then talked to Miller on the
telephone and decided he was the one I wanted to work with. So I decided to go to Yale.

Hartwig: Why?


Hartwig: Now, what would a Freudian say?

Bower: [00:56:25] No really relevant. Bob Bush was an ex-physicist and Fred Mosteller was a statistician, so you knew they had only a minimal interest in actually doing experiments. They would take other people’s data and model it, you know. Definitely not the best way to proceed, I thought.

The actual fellowship I was offered at Harvard was with Bob Bush and Dick Solomon. Solomon actually was doing avoidance conditioning with dogs. But I didn’t know him very well, whereas Miller, I knew some of his work. Although I could have flipped a coin, I didn’t. So I decided to go to Yale and I’m glad I did.

Hartwig: So describe the transition from Cleveland to New Haven, from Midwest to East Coast.

Bower: [00:57:32] New Haven is a one-horse town. It is all centered around the university, as far as I could tell at the time, that’s what it was. There were a lot of slums at that time around the university in New Haven. There was not much to do in New Haven except schoolwork. I think they had a live theater, the Schubert, which would put on plays, many of which were on their way to Broadway, but that was about it. And then the Yale football team, I think I went to their games, but it
wasn’t much compared to what I was familiar with in Cleveland and Minneapolis. [laughs]

**Hartwig:** Exactly, yes.

**Bower:** [00:58:28] The Cleveland Browns.

**Hartwig:** So describe your early research at Yale, what did you do, what you were trying to find. Talk about your methodology.

**Bower:** [00:58:38] When I arrived there with Neal Miller, I liked him immediately. He was so outgoing and sharing and listened to my ideas. He had just discovered a neural structure in the brain of cats where if you injected a couple of drops of saline, hypertonic saline, it would make the cats drink voluminous amounts of water. So you obviously were bathing just the right brain cells that controlled thirst, or at least caused drinking.

Miller was very interested in these artificial motivations that you could instill with a little drop of saline into the cat brain. He would say things like this, “Well, if this is real thirst as opposed to just some kind of weird reflex, if it’s real thirst, then it should serve as a discriminative stimulus telling the cat to do one thing if it’s thirsty, and to do something else if it isn’t thirsty. So let’s see if we can get a cat in, say, a T-maze, to go left if you’ve just put a drop of saline in its brain to get something over in that location that it wants, but if you just put water in the brain so it isn’t thirsty, and he uses that cue to go to the other side of the maze to get the reward. So the cat should learn to go one way for the reward if he is injected with saline in the brain, but go the other way for the reward if he gets water injected in the brain. That would say that what you’re putting in the cat’s brain is doing something that
serves as a discriminative stimulus or strong cue telling him which way to go
to get the reward.” Seemed reasonable.

Unfortunately, my cats didn’t really want to run, didn’t really want to
move through the maze. [laughter] I built something that would push them
out of the start box up to the choice point of the T-maze, yelling at them,
“Come on, and get your butt in gear.” The cats didn’t give a damn about the
reward that I was providing for their correct choices. The reward I was
providing—Miller had thought of this one—was to grab the cat out of the goal
box of the maze when it finished its run, and put it out on the floor and let it
romp around the lab room. Miller thought that, wow, a free romp in the
room would be very rewarding for a cat that usually confined to a cage.
Unfortunately my cats didn’t find it at all rewarding. What I found is as soon
as I opened up the top of the maze and reached in to pick up the cat, the cat
would flinch back in fear, so if anything, I was punishing it for going through
the maze. No wonder my cats were taking forever to move through the
maze.

Hartwig: Why cats?
Bower: [01:01:54] They have a big brain and they were cheap.

Hartwig: So relative size of brain in terms of--
Bower: [01:02:01] Yes. I don’t know why cats. They have human-like brains. Some of
the earlier brain-stimulation work that Miller’s group had done involved
injecting saltwater into the cat’s brain. I remember one good feature—the
cats were cheap. We got them cats from the city pound. They were strays.
One of them that I had been testing in my experiment got bigger and bigger,
and I asked Frank Beach, a faculty member who was a big expert on sexual behavior and maternal behavior, to come in. “Would you check that cat? I think maybe she’s pregnant.”

He felt around, and he said, “No, that pussy isn’t pregnant. There’s nothing in there.”

Then about a week later, the cat had a litter of four kittens. [laughter] I told Beach and we had a good laugh about it.

**Hartwig:** Did you ever ask him again for anything? [laughter]

**Bower:** [01:03:11] No. I never asked Professor Beach for that kind of judgment again. And the nice thing about the four kittens is I gave them to Neal Miller’s little kids. Neal had two little kids, Sarah and York, and they were happy to have the kittens. I’m not sure whether Neal or his wife, Marion, were happy about that.

Anyway, so I tested my cats later to see if they were still drinking when you gave them brain injections of hypertonic saline. Surprisingly, the saline injection had lost its ability to induce drinking. What happened probably was that scar tissue had built up around the tip of the injection cannula, the hypodermic needle that had been implanted in the brain, so there wasn’t enough infusion of saline into the critical neurons. So the cats were no longer any good for our purposes, and I said, “Neal, I don’t want to do this experiment any longer.” So we dropped it.

At that time Neal was getting interested in electric stimulation of the brain rather than chemical infusions. All of his students were doing work with rats, where we would implant an electrode down into the rat brain into
an area where we thought electrical stimulation might have a motivational effect. At that time there had just been findings on what’s called the reward effect that a psychologist named Jim Olds had found. He found spots in the rat’s limbic system, in the subcortical part of the brain, where electrical stimulation was rewarding or pleasurable, so that the rat would press a lever to deliver a brief shock to its brain.

So Neal had his whole group of researchers, including me, sinking electrodes down into rats’ brains looking for other brain spots that would produce some kind of motivational effect, either rewarding or punishing or that might produce thirst or hunger or the like. The idea was to be able to turn on that psychological motivation just with a switch.

It was in that context that I stumbled across some spots in the limbic system that had a dual effect, both rewarding and punishing. That is, when you turn on the simulation, it’s initially rewarding and the rat will do anything to get you to turn it on. But if you leave the stimulation on for more than half a second, it starts to get painful or punishing, and then he’ll do anything to get you to turn it off.

Surprisingly, the way we discovered this was because of another failure experience. One of the things you learn about animal behavior is if some stimulation is very aversive and you turn it on, the animal will learn some response to escape it, to turn it off. And if you give them a warning signal like a tone, say, five or ten seconds before you’re going to hit them with that aversive stimulus, and provide them with a response that they can make that will avoid it, then they’ll quickly learn to avoid the aversive stimulus, to do
whatever behavior, raise their arm or stand on their behind legs or so on, that the experimenter has determined will avoid the aversive stimulus on this occasion. That’s called avoidance learning.

Every aversive stimulus that’s ever been discovered that produces escape learning can also produce avoidance learning. Well, with my rats I couldn’t get them to learn to avoid this brain stimulation. I’d put them in a maze and give them five seconds to scurry down to the end of the maze. If they didn’t do that, I’d turn on the brain stimulus and they’d run quickly down the maze to turn it off. I’d put them back in the start box of the maze as though to say, “Now, get moving out of there, a brain shock is coming” but they would just sit there and wait for it to come on. Then when I’d turn the brain stimulus on, they’d run and turn it off. And that’s when Neal and I thought maybe there’s something rewarding about turning on that stimulation, but it starts to hurt after a brief time so they would want to turn it off. [laughs] So we put a lever in the start box of the maze and let the rats press the lever to get the brain stimulus. Damned if they didn’t press the lever to start the stimulus, but then they would run down the maze to turn it off.

So we set up a new test box which had a lever at one end which the rat could press it to turn on the stimulation, then he’d run across the little shuttle box to a wheel on the opposite wall that he’d rotate to turn it off. Then he’d run back, press the lever to turn it on, run over and turn it off, on, off, on, off all day long. The rats loved it, and it was interesting to watch them for
about ten minutes. After that you’d say, “Okay, what’s the next trick?”

[laughter]

But anyway, so that was the first time I think anybody observed and reported these dual positive and negative spots in the brain. I remember I gave my first paper at the convention of the American Psychological Association in New York City on these dual reward-punishment effects. In such speeches, you have fifteen minutes or so to describe what you did and found. I remember being very apprehensive, because right after me on the program, Jim Olds was speaking, the pioneer founder of this field, and here I was reporting something strange that he hadn’t reported. So I thought, “He’s going to murder me.”

So I described our observations, the dual reward-punishment results and all my various tests for it. When I finished, Jim Olds popped up and said, “I would like to confirm these important observations of Dr. Bower.”

[laughter] Hallelujah, sir. Turns out, at least he said, he had found the same kind of spots around the limbic system. He said he just hadn’t got around to reporting it yet. Oh, sure!

Hartwig: Interesting. Wow.

Bower: [01:09:49] Anyway, I’m unsure whether this was an important finding in the overall scheme of things. I think what probably happens is when you put an electrode down in the brain, you’re actually putting down two electrodes, a positive and a negative pole, and the wires are insulated down to the very tip, so the stimulation just travels across this tiny distance between the tips of the two electrodes. So you can think of there being beneath those tips a little ball
of brain cells that are receiving electrical stimulation. As the stimulation continues, the size of the ball of cells that are being stimulated expands. Probably what you hit originally are these reward fibers, and then right beside them are punishment fibers, and so the stimulation spreads out over time to recruit more clusters of punishment cells that are firing.

**Hartwig:** So talk a little bit about thinking up, designing, testing, reconfiguring experiments, apparatus. How did you go about that?

**Bower:** [01:10:53] You mean apparatus or experiments? Which?

**Hartwig:** All of it. I mean, you had to be a little bit of a mechanic to create your Skinner boxes.

**Bower:** [01:11:02] Yes. There were several kinds of standard equipment that animal-learning people were using in those days. One was a simple Skinner box in which animals could be is tested. When the animal depresses a lever inserted into a wall, closing a micro switch, an electrical mechanism drops a pellet of food down into a little cup inside the box for the hungry animal to eat.

Another kind of apparatus we used was a runway. We’d put the rat in its start box, open the start gate, and he runs down the runway to the goal box where he gets a bit of food or water or some other reward. You measure the time it takes him to run down to the end box. As they learn, the hungry rats run faster since the faster they run the sooner they get to the food.

One can also design equipment for delivering electric shocks to the animal as punishments. For example, the goal box of the runway can be rigged so that as the rat touches the food there he also gets an electric shock to his feet or his snout. That, of course, puts him on later trials into an
approach-avoidance conflict about running to that reward site. Or you can set up a runway so that the whole length can deliver a shock; put the rat in the start of the runway, turn on the shock, and he runs down to the goal box to turn it off. Or you can set up avoidance boxes, whereby the animal avoids a shock if it responds soon enough to a signal, perhaps by jumping across a small barrier between the two compartments. So those are some simple apparatuses. But experimenters will construct far more complicated apparatuses, for example, multiple T-mazes, or consecutive runways in a row, and so forth, depending on whatever question they wish to study. For example, the second attached runway could be used to measure a rat’s frustration and renewed motivation by noting its running speed following his not getting an expected reward in the first goalbox he’d just run into a few seconds before.

Hartwig: So you could reuse the same apparatus for different—

Bower: [01:12:23] Oh, yes. You reuse the same apparatus again and again for different experiments. At Yale I learned carpentry, metal cutting, electricity and electrical circuitry, and how to design electrical components like timers for timing how long an animal takes to do something. For example, to time how long a rat takes to run down a runway, we’d use a micro switch to start a clock when we opened the start box door, and a photo beam near the end of the runway that caused the clock to stop when it was interrupted. You know what a photo beam is. So you open the door, when the rat starts to run, he breaks his first photo beam at the beginning, and that starts a clock, and he runs down to the end box where he breaks a second photo beam that stops
the clock and delivers the food reward. That would require one or two electrical relays for the circuitry.

I learned in my carpentry and electrical shop course at Yale how to wire up relays and capacitors to make timing circuits, and how to use stepping switches to make counters. For example, in a Skinner box, you might want the rat to press, say, ten times to get a reward. How do you build a circuit that counts to ten? Well, there’s an electrical apparatus called a stepping switch that every time the rat presses a lever, that closes a micro switch and advances the stepper one notch, and when the stepper moves up to ten, bingo, that kicks in a circuit that activates the feeder, delivering a pellet of food to the rat, and resets the stepper back to zero.

Or you can build timers. Say you want the rat to get rewarded for the first press he makes after thirty seconds has elapsed since his last reward. So after his last reward, we start a timer going, and when it times out thirty seconds, it closes a switch, so that the next response the animal makes closes another switch that activates the feeder and delivers a food pellet to the rat and resets the thirty-second timer.

So what you put together are various combinations of steppers, relays and timers, components like that, so you can build up whatever elaborate circuit you want to achieve some goal. When I first came to Stanford, I set up my lab basically making the apparatuses out of plywood and electrical parts from old pinball machines. I would drive around to junkyards or to pinball arcades and ask, “Have you got an old pinball machine that you’re throwing out?” They’d sometimes have one or two they’d give me.
So I’d pick up the pinball machine--perhaps for a couple dollars--and bring it back to my lab and strip it down and take out all the racks of relays. The pinball machines usually had power supplies and power-transformers in them, and often they’d include stepping switches and condensers and capacitors so we could make up timing circuits. Then we would lash those all up together to do whatever we wanted to program for the rat or pigeon in the Skinner box.

In a Skinner box you can set it up so that with pigeons, let’s say, on one plastic key on the cage wall, the pigeon will peck and a back projector displays, say, a red light on the key. He pecks and pecks, and let's suppose that when he gets to a hundred pecks, he give him a bite of food. Then that red key light goes off, and then over on this second key on the wall he gets a yellow light, and there he has to peck but then wait fifteen seconds before he pecks again in order to get food. If he pecks too soon, the clock just resets, and he has to wait another fifteen seconds without pecking the yellow key in order to get food for his next peck. So when pecking the yellow-lighted key, the pigeon is learning to put long delays between his responses, whereas when pecking the first, red key he’s learning to peck as fast as he can. Pigeons will learn that discrimination, where you turn on the red key, and they peck very fast, but turn on the other lighted key and they’ve learned to respond with long delays between pecks. Of course, we always had some particular reason, usually theoretical, for studying these curious reward contingencies and how animals learn to adjust their performance to them.
**Hartwig:** So stepping back, what was the status of psychology while you’re at Yale here in the fifties?

**Bower:** Psychology at Yale during that period was great. It was probably the number one psychology department in the world at that time. I didn’t know that, but I knew the faculty around me were fairly famous. But frankly we didn’t think too much about how other universities compared to Yale.

There had been an effort at Yale to set up what was called the Institute of Human Relations. In fact, I was a part of that because the psychology department was in the Institute for Human Relations. The grand plan was that the Institute would bring together the social sciences—anthropology, psychology, sociology, economics, political science—and that psychology would be the base or foundation for making contact with the other social sciences. There were surely quite strong connections at Yale with anthropology and cultural studies, and there was a fair amount of that cross-cultural work going on in the Institute in the 1940s and somewhat later.

But social scientists were not people that I saw every day. They were sitting off in another wing of the building. One prominent program was called area studies of foreign cultures. Anthropologists would go off to visit foreign cultures, observe, record, collect, and bring back tons of records and data to store in these area study files at the Institute for Human Relations. Anthropologists from around the world would have access to those data banks. I believe those area files are still used to this day.

[break]
Bower: [01:19:11] So there was an initiative at the Institute to get psychologists more interested in cultural studies. There was also a strong interest in encouraging collaboration between experimental and clinical psychologists and psychiatrists. Neal Miller, in his earlier days, had been very interested in psychoanalysis. In fact, he had spent a summer or some time in Vienna being analyzed by some Freudian analyst just to see what it was like. Neal and his friend, John Dollard, who was a sociologist and psychoanalyst by training, collaborated and wrote books together. One was called *Social Learning and Imitation*, and another book was called *Personality and Psychotherapy*. In the *Personality and Psychotherapy* book they tried to translate Freudian concepts into the liberalized stimulus-response reinforcement approach that was then popular at Yale.

I remember when I took the class in learning with Neal Miller, what we did basically was to go through those two books, primarily the book on *Personality and Psychotherapy*. That provided me with the kind of background I needed later when I got interested in behavior therapy and started running around with that crowd of clinical researchers.

But anyway, Yale was very heavily dominated by theories of learning. Miller was the big Kaduna, and Frank Logan was the other central theorist and experimentalist. There was also a very prominent group of social psychologists there centered around a professor named Carl Hovland. He was quite an ingenious, energetic man who began a lot of the work in studies of communication and persuasive techniques --- that is, what to do in a speech to persuade people to your point of view. Another interest was how
do you inoculate somebody against a speech that’s coming from an opponent strongly advocating against your position? What aspects of a communication can you vary to get people to really change their behavior? Do you instill fear in them or do you instill just a little bit and give them ways to avoid their fear? Do you talk about what’s wrong with something or do you just talk about what’s the right way? Should you tell people the ten great mistakes of investment? I find that I’m likely to remember the investment decisions, but not that they were mistakes. [laughter] Your memory says, “Was that the thing you’re supposed to do or was it the thing you shouldn’t do? Which was it?” Turns out it’s very counterproductive to tell people the ten worst things they can do.

Anyway, so Carl Hovland had a group of social psychologists around him. Many would be young guys who were going on to academic fame elsewhere. So Yale was a great place. I loved it there.

**Hartwig:** Did the faculty share and work with each other and share ideas or were there separate camps?

**Bower:** [01:23:00] No, they were in separate camps. The social psychologists didn’t have much to do with the animal-learning people, and neither had have much to do with the clinical psychology people. Yale had a big clinical psychology program at the time. The students interacted some, but basically we kept ourselves separated. You could work with different professors within a given domain, but it was very rare that you switched allegiances.

**Hartwig:** Did you try to borrow from different camps?
Bower: [01:23:50] No, I didn’t. Maybe other people did. I’m trying to remember some of the people I went to school with.

Hartwig: Zimbardo was one, wasn’t he?

Bower: [01:24:02] Yes, Phil Zimbardo was one. There’s an interesting story there. The year I went to Minnesota, I had also earlier applied to go to Yale and Harvard and had been admitted, but I opted instead to go study on this Woodrow Wilson Fellowship I received to study at Minnesota for that year. Well, the person who was my backup, who was admitted to take my place in that year’s class, was Phil Zimbardo from Brooklyn College. What had been arranged for that student-slot was a research assistantship with a young assistant professor at Yale named K.C. Montgomery, who was studying exploratory behavior in rats. So Phil was admitted to the Yale program as K.C. Montgomery’s research assistant. Well, during that year, Montgomery became increasingly depressed and withdrawn and committed suicide about two-thirds of the way through the academic year.

Hartwig: How old was he?

Bower: [01:25:22] He was in his mid-thirties. So here’s poor Phil Zimbardo, first-year graduate student, and they shoveled all of Montgomery’s work onto him, saying, “You have to finish off his research program. You have to write a final grant report on Montgomery’s work for NIMH. You have to write up whatever research papers he was doing. By the way, you also have to look after the emotional grief of his widow and his children.” [laughter]

Hartwig: You’re joking, right?
Bower: [01:26:06] No; perhaps I’m exaggerating just a bit. Poor Phil, his first year, and he worked his butt off to do these extra chores. So comes the next year, I come waltzing in to Yale with this fabulous fellowship with Neal Miller, who’s a very big cheese, and I run into Phil, and his first words were, “You son of a bitch. It was you who got me that horseshit job with K.C. Montgomery.” [laughter]

Hartwig: Wow.

Bower: [01:26:40] That was my introduction to Phil Zimbardo. [laughter] We became good friends and eventually shared an apartment together during my second year in graduate school. We shared an apartment a couple blocks away from the department. Other people in my Yale graduate class were Ron Wilson, Dick Debold, Don Jensen, Tim Brock, Ted Coons, Britton Ruebush, Tom Storms, and Earl “Buzz” Hunt. Earl “Buzz” Hunt and Tim Brock actually became semi-famous as academic psychologists. Others were Lyman Porter, who enjoyed a fine career, Ron Rabideau, Dick Whalen, and Don Novak. There were many more of them I don’t remember because they were in clinical psychology, so I didn’t interact with them very much. Certainly not after our first year there because they often were outside in clinics and doing their practicum service.

Hartwig: So in 1957 you did the Social Science Research Council summer workshop.

Bower: [01:28:09] Yes. This was a continuation of my interest in mathematical models for learning. Yale didn’t have anything on that. At Yale I was learning how to implant electrodes in rat brains and how to do histology and cell cytology and all that, and how to build Skinner boxes. But I kept an interest
in mathematical psychology, and I noticed this particular program being offered and that the workshop had fellowships one could apply for. So I applied for an SSRC fellowship to be a student in the summer institute. It was held here at Stanford in the summer of 1957. Lo and behold, I got the fellowship, I guess because I showed how I had read all of this Bush and Mosteller and Estes stuff before. And Bush might have remembered me from my Harvard application. Happily, Sharon and I were married just before I got to that workshop.

**Hartwig:** Was this your honeymoon?

**Bower:** [01:29:26] Yes. Sharon and I had our honeymoon in Wilbur Hall dormitory here at Stanford.

**Hartwig:** [laughs] What a lucky woman.

**Bower:** [01:29:37] Indeed. We had got married during the break between semesters in January 1957, but she had to go back to finish up teaching at Louisiana State University, and, of course, I was stuck at Yale being a conscientious student. So that meant we were apart between January marriage and the end of May. So our first real time together was driving out to Stanford from New Haven. Along the drive out, we stopped for a wedding reception in her hometown in St. Peter, Minnesota. And then we drove out here and moved into Wilbur Hall for our real honeymoon.

**Hartwig:** So where did you guys meet? What year was that?

**Bower:** [01:30:32] Sharon and I met in 1952 in New York City. We had gone to a summer camp called the Encampment for Citizenship, which was a program put on by the Ethical Culture Society, which was a New York-based group. I
think they were Reform Jews who believed that college kids should get a deep grounding in the methods of democratic action and learn a lot about liberal democratic society and how to get things done, move forward to become political activists.

It was a fabulous place. There were about a hundred of us there, college students from all around the country, comprised of all makes, social classes, and races. Sharon was one and I was one. We saw each other that first night at the social mixer and grabbed on to each other and haven’t let go for over sixty years.

But it was a wonderful, wonderful thing, that Encampment for Citizenship. It lasted about six weeks in upper Manhattan. During the mornings we would have lectures by local politicians or people from the Urban League or various country’s embassies, or the NAACP, labor union leaders, or the National Association of Manufacturers. We also met our U.N. ambassador who came to our campus in upper Manhattan. We even got to meet and talk with Eleanor Roosevelt, visiting her at her Hyde Park residence.

So that was in the morning, and then after lunch we’d get on the subway and ride downtown Manhattan. We had divided ourselves into small groups who had special interests and activities planned for them to do, like we’d go to the U.N. or go visit the Congress on Racial Equality offices and see what they were up to, or go to the garment workers union hall to meet union leaders.

Hartwig: So how did you qualify for this? Did you apply or how were you selected?
Bower: [01:33:28] I just applied for it. I had heard a lecture by the leader of this group; Algernon Black was his name. He had come to Cleveland and gave a lecture, and one of my buddies said, “We ought to go to that. Let’s go hear what he’s got to say.”

So we went, and I liked Algernon and he liked me,

He said, “Why don’t you apply for a fellowship to the Encampment?”

And I said, “Okay.” I didn’t have to attend the summer camp. I think I only had to pay my bus fare to New York. It was great.

I know Sharon’s dad was very opposed to her going there because he thought she would be going into a hotbed of communists out in New York, who would brainwash her. [laughter] But he eventually relented and let her go. She said he felt a lot better about it when she returned home and said, “Dad, I’ve met a college baseball player there who’s now my sweetheart.”

Her dad was the executive head of the baseball team in her hometown.

Hartwig: So when did you meet him, then?

Bower: [01:34:46] Well, I came out to St Peter that September of 1952, met her dad. He gave me a tryout, had me out on the diamond pitching with a couple of his guys from the hometown team. I apparently passed muster enough so that they hired me for the next summer to come back to St. Peter, Minnesota to pitch baseball. That would be the summer after my junior year in college, and that was a great experience.

In those days, before televised major league games, every little town in Minnesota and probably Ohio had a town baseball team. That village team would play the baseball teams of other towns nearby--particularly if they
were approximately the same quality of players. So that next summer, 1953, I joined the St. Peter Saints as one of their pitchers, and we would play all of the town teams from within about thirty or forty miles. They had a league and we’d play regular games, usually two games a week.

Because I was on a college athletic scholarship, I had to keep my amateur status, so they couldn’t pay me to be on the team. However, they could provide me with a travel allowance, and so I got $45-a-week travel allowance. I would be attending summer school in Minneapolis and take the bus down to St. Peter, sixty miles away, to play in the ballgames. And I remember one of the deals was it was just me and one other guy were the pitchers for this team.

**Hartwig:** Two pitchers.

**Bower:** [01:37:16] Yes; but we only played two or three games a week. If I had to relieve him because he was off and getting beat up, I got $5 more an inning in relief. [laughter] So I was sort of ambivalently rooting for him to do well. I’m sure he did the same. Russ Gustafson was his name. He was a lefty.

**Hartwig:** That sounds like a Minnesota name.

**Bower:** [01:37:49] Yes. He pitched for the Gustavus Adolphus College team right there in St. Peter. Sharon, my wife to be, was a student at Gustavus Adolphus College. I haven’t thought about these things in years. I loved playing baseball there. It was good. And I did okay, not fabulous. I learned that some of those old country boys were damn good hitters.

**Hartwig:** Were there ever any fights?
Bower: [01:38:27] No fights. But I couldn’t just blow the fastball past them. So I think it was during those summer years, in summer of my junior and my senior years when I played for the St. Peter Saints, that I came to the realization that although I was okay, I was not super fantastic as a pitcher.

Hartwig: Humbling experience.

Bower: [01:38:52] Humbling experience indeed. I was okay and I would win more games than I’d lose, and I finished more games than I got knocked out of the box, but occasionally I would get knocked out of the box. That didn’t happen to, say, Bob Feller when he was playing semipro ball. So by the time I graduated from college, although I would have loved to continue playing baseball, and I think the Indians would have signed me to play in their Minor League system, I don’t think I was a super prospect for major league baseball. I would have liked to play professional baseball, but the Korean War was raging, and my military draft board basically told me, “Mr. Bower, you have two options. You can go on into post-graduate school and maintain your student deferment, or you can go to Korea as a guest of the U.S. Army. Which would you prefer?” [laughter]

So I went to graduate school, which was a good thing. Although I would have loved to try to play baseball, and if the universal draft had not been on, I would have done that for sure. But given the failure statistics for professional baseball players, it was very unlikely I would have made it to the major leagues. I would have probably ended up being a used-car salesman or running a bowling alley in Cleveland or some career like that. [laughter] Even
then, I knew professional baseball would be fun for a few years, but then I'd have to settle down to business and become a professional, psychologist.

**Hartwig:** I think you made the right choice.

**Bower:** [01:40:53] Well, I don’t know. I occasionally wish I could have struck out Joe DiMaggio at least once.

**Hartwig:** I think Bob Feller--how old was he when he went pro? Was that right out of high school?

**Bower:** [01:41:03] Yes, right out of high school, and he had a really strong arm on him, I can tell you. I met him at the Cleveland stadium. In those years of the early fifties, the Indians had a great pitching staff. I was invited to the stadium and pitched batting practice a couple times. I met the great pitchers of those days --- Early Wynn, Mike Garcia, Bob Feller, Ray Narleski, and a Don Mossi. Mossi and Narleski were relief pitchers. Mike Garcia and Bob Lemon and Bob Feller were the starters. Dan, I’m amazed you know about these players. You know their names. You are so unusual.

**Hartwig:** [01:42:06] Well, I met Bob Feller, got his autograph. I’m from Iowa, so I knew legends. I played baseball through high school, so I loved it.


**Hartwig:** I collected baseball cards too.

**Bower:** [01:42:31] Really?

**Hartwig:** Yes, for the longest time.

**Bower:** [01:42:33] I didn’t do that. I had an indoor baseball board game I played when I was a child.
Hartwig: Well, why don’t we call it a day and we’ll end it here. We’ll pick up a little bit next time with you graduating from Yale and pick up then on your marriage, and then your coming to Stanford, but let’s end it here.


[End of Session One]
Hartwig: This is Daniel Hartwig, university archivist. I’m here again with Gordon Bower. Today is August 27th [2014], and this is our second oral history session with Professor Bower. Welcome again.

Bower: [00:00:13] Welcome. Nice to be here.

Hartwig: All right. So I think last session we ended right before your coming to Stanford, so why don’t we pick up with the transition to Stanford.

Bower: [00:00:26] Right. I got hired through the old boys’ network. I had been here, attending an SSRC summer workshop in mathematical learning theory, when I’d come as a second-year graduate student at Yale. Pat Suppes was a member of that workshop, and the members were all people who became leading lights in the mathematical psychology movement of the 1960s, some famous, some about to be famous, but I didn’t know any of them.

So I just spouted off my ideas in my usual characteristic way of talking to people. I put out a couple nice ideas, and I sufficiently impressed some of the people there, including Pat Suppes and Bill Estes, that I might be a contender.

When I got back home, back to Yale, in my third year there, Pat Suppes called me up and offered me a job at Stanford. He had money from the SSRC to develop a program in mathematical models in the social sciences, and I was one of the first people he hired. So this was a year before I even got my PhD at Yale, so I never had to go through those onerous job
interviews and job talks and other anxiety-provoking episodes that most people did.

Hartwig: So what was your title?

Bower: I was assistant professor, and I was offered the grandiose salary of $5,000 to become assistant professor in Stanford’s psychology department. Suppes had talked to the head of the department, Bob Sears at the time, and said he would put up the money for this new position from SSRC. And Sears called up my professor at Yale, Neal Miller—they were old Stanford and Yale buddies—and asked about me. Sears probably talked as well to Bill Estes, another of my promoters. And they said, “This fellow’s okay,” and I was hired on the spot, sight unseen by Sears or any other psychologist, but that’s the way things were done in those days. Today it would provoke an avalanche of lawsuits about affirmative action, but in those days, executive heads did all kinds of unilateral things. Suppes was an associate dean at the time, I believe.

Hartwig: And he was in philosophy at the time?

Bower: Yes, he was in the philosophy department, although Pat’s a true Renaissance man. He became interested in mathematical learning theory and enabled a lot of money to flow into Stanford for the mathematics and social sciences program. Over the next two years he hired four new people on that money: Joe Berger, sociology; Herb Scarf, economics; Dick Atkinson and me in psychology. I guess he must have had good judgment, because three of the four of us were later elected into the National Academy.

Hartwig: Absolutely, yes.
**Bower:** [00:04:37] And Joe Berger, the fourth one, became chairman of the department and president of the American Sociology Association, so Joe had many achievements. But anyway, that’s the way I got hired.

On the way out here from Yale, I had a summer research fellowship working at a VA Hospital research team in Pittsburgh. They liked me so much, they offered me $10,000 to stay on there, double what I was going to get at Stanford. [laughs] But I didn’t want to work in the VA research team.

**Hartwig:** Given the past experience in those type of environments or--

**Bower:** [00:05:22] Oh, it would have been very different. What I was doing at the VA Hospital in Pittsburgh was working with my buddy Larry Stein, learning all about operant conditioning procedures to study rats learning in Skinner boxes. That was something that I made use of very quickly once I came to Stanford.

I know when I first came here, at my first interview with Suppes, I’d said we had a child on the way out here in the summer, and so I was a new father. And he said, “How much are we paying you?”

And I said, “Five thousand bucks.”

He said, “Well, you’re a young father. We’ll goose it up to $5,500.”

[laughter]

I said, “I think we’re going to have some more kids, then.” [laughter]

**Hartwig:** Five hundred a kid. [laughs]

**Bower:** [00:06:31] So, I got a 10 percent raise just by showing my face. [laughter]

**Hartwig:** So was it an easy decision to accept the offer and move out here or--

**Bower:** [00:06:42] Well, I didn’t care to have a competing offer at the time.
Hartwig: This is before the PhD, correct?

Bower: [00:06:47] Yes, a year before my PhD.

Hartwig: So did you wait then until--

Bower: [00:06:52] Yes, yes, I waited till I got my PhD. I knew I wanted to get that out of the way before I came here.

Coming here was a bit of a stretch, because I was a Yalie on the East Coast, and in those days Stanford seemed to be so far away out in the boondocks. It had the reputation that all people do there is drink Tequila Sunrises and fool around and not do any important science and nobody ever hears about them again. That was a very popular view of things from the East Coast in those days. It was terribly unfair because Stanford’s psych department had a lot of ex-Yalies in it. Bob Sears, Jack Hilgard, Doug Lawrence and Don Taylor were al from Yale. Golly, these were all famous psychologists who had come to Stanford from Yale.

Hartwig: Did you reach out to them to get their impressions or recommendations?

Bower: [00:08:12] No. I just came on Suppes’ offer.

Hartwig: So you got here in 1959. Describe the university, the department.

Bower: [00:08:25] I didn’t know very much about the university at all, other than what it looked like around here and how to get to it from Palm Drive. I never researched how good Stanford was in the overall ratings of universities. I knew it had an up-and-coming reputation, but I didn’t know a thing about it, other than I knew a couple of people who were in the psych department. I knew the names of Bob Sears, Jack Hilgard, Doug Lawrence, and Leon Festinger, but that was about it.
So when I arrived, I had to learn who would be my colleagues. And in those days, the psych department was in Cubberley Hall. They had joined forces with the School of Education, and the psych department had offices in Cubberley Hall. I remember my first office in Cubberley was right beside the men’s restroom. Between classes when many guys were heading to the men’s room, I would have guys running into my office unzipping their fly. I’d have to say, “No, no, it’s next door, next door.” [laughter] Fortunately, nobody peed on me. If I was holding a meeting with a student in my office, we’d have to stop talking while all the toilets were flushing. [laughter] So that was my first office at Stanford.

Hartwig: How long were you in that office for?

Bower: [00:10:31] Only about two or three years, I think. Then they moved me down into the basement of Cubberley, into a corner office where all the exposed pipes from the plumbing ran through the ceiling. But at least I was no longer next to the restrooms. I was in there, oh, maybe six or seven years.

Hartwig: And did you have a lab down there then too?

Bower: [00:10:58] Yes, I had a couple labs there. I remember one lab I often used was built as an anechoic chamber for some psychologist who had been doing research on hearing, where you need to have a very quiet environment. Whoever had done that research was long gone, so I got to use this anechoic chamber. You put a human subject in there, and he can’t hear any outside noises. And it used to freak out some of my student subjects, “Wow, why is all this insulation around here? What are you going to do to me, Doc?” [laughter]
“It’s very simple: just learn a few words here we’re going to give you.”

But anyway, that was one of my lab rooms down there.

**Hartwig:** So were you working with Pat then initially [unclear]?

**Bower:** [00:11:58] No, I didn’t work with Pat ever. I started teaching classes in mathematical learning theory along with classes in just straight learning theory. I also taught an undergraduate lab course in operant conditioning and learning, and that was fun, I remember. I had never done that before.

Every student in the class was given a rat to use as their subject in the upcoming experiments they were all going to do. So the first week I had the students go out in the animal labs handling their rat, getting them and the rat it used to handling. They’d wear these thick gloves, to reduce the chances they’d get a rat bite. In addition I made sure every student had a tetanus shot in case they should ever get bitten. And the students would learn to pick up their rat, so the student himself was getting deconditioned of his fear, and the rat was getting used to being picked up and held. The rats were also put on a deprivation schedule, so they’d be hungry and ready to work for food rewards when students got around to training them.

Then every week in the class I would lecture about classical conditioning, and all of the variations on that theme, and the students would start doing classical conditioning of their rat. They’d press a button to activate the feeder, which made a sound, and it would drop a pellet of food into the food cup in the test chamber. The rat would then go over to the food box and eat the pellet. So pretty soon that sound, the chime of the
feeder, would cause the rat to scurry right over to eat. So the students could see that happening already.

Then the next week the student would be shaping the rat to press a lever in the chamber to get the feeder to give him a pellet of food. Then the next week the student would pay off the rat with a pellet only every third bar press, and then advance the required effort to get a reward --- make it every seventh, and then every fifteenth, then every twenty-fifth bar presses to get the reward. The students would conduct these experiments each week. They were required to a brief paper on their observations of their animal, record and report its number of bar presses per minute, and so on.

**Hartwig:** Was it a popular course?

**Bower:** [00:14:39] Yes, and strangely enough, PhD students in the communications department were required to take my course.

**Hartwig:** Why was that?

**Bower:** [00:14:56] I don’t know. They didn’t know either. [laughter]

**Hartwig:** Did they enjoy it?

**Bower:** [00:15:04] I think so. I got to know many of them, and some of them actually became later faculty members here.

**Hartwig:** Was it a lab requirement?

**Bower:** Yes. Don Roberts was one and Henry Breitrose was another of my lab students who were in the communications program who got their PhDs here. They returned to the faculty here later in communications and journalism. Every time we’d see one another, we would talk about the old rat experiments that they had done with me. Anyway, those are old memories.
But that was a good course. I liked it. I’m not sure the students thought it was very relevant. I know at one class I talked about paramecia research. That was a very hot topic in those days. You can take these little one-cell organisms, paramecia, and condition them. You had to do it under a microscope, practically, because you couldn’t see them with the naked eye. So you could, say, turn on a bright light and shock them, and they would swim away; and they’d learn to swim away to the light alone before the shock came on.

I recall one of the weird phenomena of those days was that you could grind up a trained paramecium and feed it to another, untrained paramecium, and that second paramecium would quickly pick up the classically conditioned reflex that had been trained into the first one. I remember describing the phenomenon as something that was incredible. A few of my students dropped out of the class at that point. [laughter] They figured this stuff was just too bizarre.

**Hartwig:** So what was the explanation as to--

**Bower:** [00:16:56] The hypothesis was that you’re transferring the altered DNA from a paramecium who’s been conditioned into the paramecium that hasn’t, and that transferred DNA holds and brings along the altered response to the light-flash when the donor-DNA mixes with the receiver DNA. Or something like that. It’s still pretty bizarre.

**Hartwig:** Is this a learned behavior? How could a learned behavior be transferred?
Bower: [00:17:07] DNA does wonders. [laughter] I think it was replicated many times over, and then it failed to be replicated, and then everybody forgot it.

Anyway, excuse me. I’m digressing.

Hartwig: Absolutely. How long did you teach these conditioning courses?

Bower: [00:17:33] Oh, I taught that for about eight years. And I also taught a class in theories of learning. That led Jack Hilgard, who had a very popular book called *Theories of Learning*, from which I taught, to ask me to be his collaborator and coauthor on that book.

Hartwig: What year would have this been?

Bower: [00:18:05] 1965 was the first revision, and then we did one in, I think, ’73 or thereabouts, and then another revision 1981, which was the last one I did. Jack lost interest in working on learning, and so he didn’t keep up.

Hartwig: Why?

Bower: [00:18:29] He got interested in hypnosis. In 1959 when I first met him, he was mainly interested in human motivation, and had got off into educational policy. He did a lot of writing in education, but then he got into hypnosis. He made hypnosis research respectable; he had a big lab here, used to hold weekly research lab meetings, and I attended those because I wanted to learn about hypnosis. You know, who wouldn’t? It was kind of weird stuff.

So he taught me how to do it, and I actually later used a lot of hypnosis in research I did on emotion, because hypnotic suggestions are a great way to get people into some emotion like feeling happy or sad or fearful or angry. And I used that in work I did later in the 1980s, on emotion and memory.
Anyway, back to my teaching. I taught a class on learning theory, and I taught a class on mathematical learning theory. Then about 1960 or ’61, Suppes got Dick Atkinson and Bill Estes to join the faculty. Atkinson came in with, I think, a joint appointment in education and psychology. Estes came in straight psychology. Estes was the big kahuna in those days. He was Mr. Math Psych.

Hartwig: Did you guys collaborate a lot?

Bower: [00:20:39] Only slightly. Actually, Estes and I never published anything together. We just talked together a lot. I had first met him in that 1957 SSRC summer workshop I mentioned earlier, and he and I took a liking to one another. He liked my ideas, and he gave me advice about writing up some of my ideas for two chapters in a book that came out of that summer workshop. Bill and I kept up a very lively correspondence for two years while I was a third-year and fourth-year graduate student at Yale.

Hartwig: What were you talking about?

Bower: [00:21:30] Bill was an old rat runner, doing animal conditioning, and that’s what I was into at that time. He was also starting to do this mathematical psychology stuff, and I bought into his way of doing things. I would work out how his theory could apply to the data of one or another kind of conditioning experiment, and that’s what Yale psychology was all about, animal conditioning experiments. I would show how his theory explained, say, how the amount of reinforcement would affect learning or the delay of reinforcement affected learning or the effects of the animal’s level of motivation on its performance, and so on.
I was into that topic big time at Yale, but Yale had a particular way of theorizing about it that differed from the way that Estes did his stimulus sampling theory. So I recast all of these findings from conditioning with animals into stimulus sampling theory, which was the Estes approach. And Bill really liked that. It was right down his alley. [laughs] And I would write him these long letters explaining the way his theory could explain behavioral effects of stimulus intensity of the CS, or explaining drive discrimination, drive generalization, transfer between hunger and thirst of the rat’s habit, and so on.

Hartwig: Did you publish on these?

Bower: [00:23:30] No.

Hartwig: Did he?

Bower: [00:23:34] I think he published some papers on drive effects on rats’ behavior. He was a very inventive guy. I loved reading his stuff and listening to him.

Anyway, so all this stuff I did was never published. I’d talk about it in my graduate class, like this is the way to deal with intensity of a Pavlovian conditioned stimulus upon learning. This is the way you could deal with amplitude of the unconditioned stimulus in determining the amount or level of conditioning. This is the way, using a probability learning model, you could conceptualize drops of saliva as a measure of a salivary conditioned reflex, and so on. But none of that was ever published.

Hartwig: So what were you researching and publishing then at the time?
Bower: [00:24:34] At that time, I was doing work on discrimination learning, on escape learning, and on correlated reinforcement schedules.

Hartwig: Describe a little bit of these.

Bower: [00:24:58] Okay. In correlated reinforcement training, you correlate the reward that an animal gets with some intensive aspect of its response, such as how fast he runs to a goal box or how hard he works. And you could set that correlation to go any way you please, since you, the experimenter, are controlling it. You can set it up so that the faster the rat runs to the goal box, the bigger is the reward he gets. Or you can reverse it: the faster he runs to the goal box, the smaller is the reward he gets. Or the faster he runs, the longer he has to delay to get the reward once he gets to the goal box, and so on. You measure how long he takes to run down a runway to a goal box, and then you control how long he has to wait after his response to get the reward on this trial.

You can also correlate the reward with the amplitude or strength of a rat’s bar press. Another arrangement I studied was this: I’d make a ½-inch slot in the side of the Skinner box where the animal would be working, and he could stick his nose in different locations along this slot. You can vary where in the slot he had to stick his nose to get a reward. For example, the farther down the slot to the left that he stuck his nose, the bigger is the reward he was given. Or he had to stick his nose in exactly a specific location to get the reward. [laughter] Things like that. Correlating the reward with the speed, amplitude, or spatial location of the animal’s response.
So my dissertation was on how much a reward was delayed in the goal box depending upon how slowly the animal had run down there on that trial. The animal was really hungry and wanted to get to the goal box as fast as he could and get the reward. However, you can set it up so that if he takes more than, say, three seconds to get from the start box to the end box of an alley that was about six feet long, you impose a thirty-second delay once he gets to the goal box. And that’s almost punishing for the animal to have to wait that long to get his pellet of food; so he eventually learns to take his time going down the alley to get the reward. To go nice and slow. [laughter] You can observe his conflicted, herky-jerky movements [demonstrates]. He wants to get there, but on the other hand, he learns that he can’t get there too soon.

So of course, animals try to adjust their movements so that they get to the goal box at just more than the three seconds, just after the deadline. You can plot the distribution of their response times around the three-second deadline. The frequency of their overall response times came to be peaked, right around three seconds. The rat learns he’ll get punished with the long delay if he goes under three seconds, so that pushes him to go slower. On the other hand, he wants to get there as quickly as he can just over three seconds, and so that pushes his response times to be shorter. So the experimenter can vary how much time the rat has to wait to get there as well as how big a delay the animal gets if he’s arrives too soon.

Animals are very rational in the way they perform, in one sense. If the deadline time to get to the goal box is just too darn long, they won’t wait to
get there. They’ll just run quickly down to the goal box and just accept the
delay that we’re going to give them for going too fast.

**Hartwig:** [laughs] No matter what?

**Bower:** [00:29:26] Well, no; it depends systematically on the deadline and the delay of
reward that’s imposed if they run too fast. They get very good at almost
rationally choosing the speed they will go at in order to minimize the total
time to get the reward. It’s a bit like a singer who chooses the vocal
amplitude that’s most effective in singing a given tune. Or it’s like a person
who’s, say, running a long distance race who learns how to alter his speed
depending on the terrain and the competition.

Anyway, I did a lot of work on that kind of thing, where I’d manipulate
the amount or the delay of reinforcement in correlation with the animal’s
speed of running down a runway.

**Hartwig:** And how was this received within the profession in comparison to other
research going on at the time?

**Bower:** [00:30:26] I think people said, “Yes, yes, that’s okay.” It also explained the
general view that animals will run faster to the goal box the better the reward
they get there: that’s because in the usual circumstances, the faster they get
down there, the sooner they get the reward and that’s what they want to
minimize. But other animal-learning experimenters weren’t thinking of, for
example, what happens if the faster the animal gets down there, the longer
the reward is delayed or the worse is the reward? My approach could deal
with all those kinds of more complicated but realistic circumstances. As
another example, my social reward for talking to you requires me to control
my talking so that it abides by a certain speed and loudness, and its
effectiveness doesn’t increase with the amplitude or rate of my speech. This
approach was called the micro molar view of behavior; namely, you focus on
and reinforce the micro parts of the speed or intensity of the behavior. I
would say that learning theorists accepted that, but then ignored it since they
were interested in very different matters.

Hartwig: Why?

Bower: I don’t know. It is the case a lot of it depends upon what you call
“the response” that the animal is learning. One thing I noticed is if you’re
training the animal to take time to move down a runway, he doesn’t just run
slowly; what he does is run fast in spurts and then stop near the end box and
put various backups and little interruptions into his behavior stream, and
after those hiccoughs, he finishes by racing into the goal box. So it is as
though he really wants to get there very fast, except he knows he has to add
these hiccoughs and interruptions in order to take time off the clock.

So the rat puts these various superstitions into his response chain in
order to slow him down, or to at least lengthen the time between when he
moves out of the start box and when he gets to the goal box. One could look
at that and say he’s not learning to run slow; he’s learning to take time for
getting from start to finish by, for example, turning around three times as he
moves down the runway.

Hartwig: And did you try, then, to construct different tests to change that type of
superstitious behavior or--
Bower: [00:33:29] You could try. You can try to do it so that the reinforcement depends upon him running slowly rather than putting superstitious hitches in it, but then you get back to the question of what is the response that you are rewarding. In those days the response was defined by whatever it takes to move from one place to another, or whatever force was required for a rat to get the lever pressed down or for a pigeon to peck the key. The response was defined in terms of the behavior impacting the environment in a certain way, like getting the lever down, closing the micro switch that operates the food dispenser.

You don’t distinguish among ways in which the end result is achieved, like whether the level is pressed with a lot of or little force, whether the rat pressed with the left paw or the right paw or his hind feet, or whatever. All of those activities that get the lever down are categorized together as “the response”, which you reinforce. That is, the contingencies of reinforcement specify the action that tells us when to give the reward. Is it when the lever moves down so that the micro switch clicks, or is it when the rat uses his left paw to depress the lever, and so on? This leads into a deep philosophical discussion regarding how a response is defined.

I don’t know why that approach never became popular. Not too many people jumped on that bandwagon of the micro molar approach to behavior. It was an animal-learning kind of issue. In the larger picture, consider that if you look at, say, studies of human motor learning, everybody understands that of course what the experimenter is doing is shaping the speed or shaping amplitude of the subject’s response. For example, every baseball pitcher
knows that sometimes you throw a slow ball, a changeup, and other times you throw as fast as you can, but there are different reasons and circumstances as well as reinforcements for altering those aspects of the pitching motion.

So everybody who studies human motor learning understands perfectly well that you’re using a, quote, “micro molar” approach. So for them it’s no big deal. It was just the people working on animal learning at the time who thought, “No, no, the bigger the reward the more excited the rat gets and the faster he’s going to run,” and so on. Anyway, my research there didn’t have much of a lasting impact.

Hartwig: So you also were studying mathematical models for human learning at the time, correct?

Bower: [00:36:30] I started that seriously when I got here at Stanford.

Hartwig: So, describe that research.

Bower: [00:36:40] Well, I started out studying what was then a very old-fashioned human learning paradigm, which is paired-association learning. The procedure is kind of like teaching someone a foreign-language vocabulary. You teach a child a word like the German word “Frau”. What does that mean in English? It means Woman.” Later you ask, “Frau means what?” You wait a few seconds, then later you say, “Frau means woman”. And you repeatedly study and test the pairs with flashcards.

So my earlier research was done with lists of 15 or 20 paired associate flashcards. I had undergraduate experimenters running undergraduate subjects. They might be learning a collection of words paired arbitrarily with
digits. “Use the digits from one to ten, and when I show you this word, ‘book,’ I want you to say, ‘Five.’ When I show you this other word, ‘Chair,’ I want you to say, ‘Eight.’” There’s no rhyme or reason to the pairings, they are just brute associations, but nonetheless I ask you to learn these associations. When I show you any one of these words, I want you to say its associated number. Just guess if you can’t recall it.”

This was a very standard procedure studied by psychologists for the past hundred years. I might have the subject learning, say, twenty words being paired with the first ten digits, with two words per digit. I was interested in what happens trial by trial as the subject learns each pair. Of course, subjects start by guessing correctly about one-tenth of the time, but as study trials proceed most people learn and increase their performance up to giving 100 percent correct responses.

The theoretical question is, how are we to describe what is going on there? People are studying each pair for a couple seconds and trying to hook up the word “book” with “five” and the word “chair” with “eight” and so on. The traditional and common sense theory was that people are building up the strength of an association or habit for each pair they study, and that it varies in strength from zero up to a maximum. So that every time the person sees the word “table,” he consults that habit and says, “Seven.” On this view, learning is just a gradual buildup of habit strength towards some maximum with each study trial.

I wanted to test that against another, weird notion, which is that people don’t learn a darn thing about any given pair for some trials. They’re just kind
of muddling along, getting nowhere, and then suddenly they learn a given association all of a sudden on one trial. That approach was called one-trial, all-or-none learning. And that was the idea that Bill Estes and I and several other people started working on.

Turns out that you can find a number of cases of paired-associate learning in which this all-or-none process seems to be what’s going on. Suppose an experimenter has the subject learning twenty pairs that he goes through many trials repeatedly involving a test then a study for the collection of twenty pairs. Subjects just start off guessing at what will be the correct response. Each time the subject studies a given pair, let’s hypothesize that there’s a certain chance (let’s call it c) that he’s going to learn it and remember it for the rest of the training session. One minus c is the probability that he doesn’t learn that association on that study trial, so that he just remains ignorant of it until the next trial with that pair. So the next time you test it, he’s still in a guessing state for that pair. However, after he guesses, he is shown the correct answer and has another chance to learn the association. Thus, each study trial is providing an opportunity for the person to learn the correct association for any pair that he hasn’t yet learned.

Surprisingly enough, when you have a certain fixed probability of learning an item in all-or-none fashion every time you study it, that process generates a learning curve that on the average over the twenty items looks just like the continuous learning curve that was postulated by the traditional theory. They two theories produce indistinguishable average learning curves, so that you have to look at other kinds of statistics of the data to find out
which one better captures the underlying process. Is it this gradual strengthening of the paired associations that has traditionally been believed, or is it just more failed study opportunities until the subject finally learns each item all at once?

So Bill Estes, Pat Suppes, and I developed several techniques for determining when subjects were showing all-or-none learning. The simplest way to think about it is that on the one-trial learning point of view, people start off in an ignorant guessing state. They don’t know the answer so they just guess. And then they suddenly learn the association on some one-study trial. What that implies is that if you find the point at which they make their last error and go into this errorless run of correct responses, up to that point, they’re just guessing on that pair. So if for that pair you plot a backward learning curve from the point of their last error, moving back towards the beginning of training, what you ought to observe is just chance responding. All those trials in which people are responding up to the point where they make their last error, the theory supposes that they’re just guessing. So, therefore, you should have a flat pre-criterion backward learning curve, that’s flat up to the point of the last error. And it’s pretty dramatic to observe when that happens. It means that at the individual item level, people aren’t improving at all up until the point at which they finally [snaps fingers] snap into the correct association and get it. Surprisingly, we observed that outcome in a number of experiments.

Another way to test the all-or-none theory is to look at how many errors a person is going to make on a given item, like “table” goes with
“seven,” and you look at any trial where he makes an error, and you say, from there on, the number of errors he’s going to make on that item before he learns it is the same as he would have made had he started afresh from that point with no study history. It’s as though every error is what is called a recurrent event; namely, that error kind of resets the subject’s learning history on that pair back to zero. Namely, the error tells us that he hasn’t learned that item yet. Therefore, the number of trials and number of errors he’s going to make before he finally learns this item is, on average, always going to be the same.

That prediction seems so strange when you say it. If I make an error on trial one, I’m going to expect to make, let’s say, five more errors before I learn. If I made an error on this item on trial ten, I’m expecting to make five more errors before I learn this item. If I make an error on trial twenty, I’m expecting to make five more errors before I learn. That sounds so bizarre. Of course, that statistic is conditional upon selecting different items or subjects that have made an error on trial one or on trial five or on trial ten because an item that has an error on trial ten is just one of those few that hasn’t been learned yet.

Anyway, Bill and I were looking for all-or-none learning using other methods. We were also doing little minimal experiments where we’d have somebody study a list of pairs, table-seven and dog-five, and so on, and then we’d give them two test trials. Test one, what digit went with table?, what went with dog?, and so on, but subjects were given no feedback at that point
about their answer. We’d accept without comment whatever answer came to
them.

Then we return and test them again in a few minutes. Here’s the
second test. What was table? What was dog? And so on. That’s a simple,
almost trivial experiment. But what the experimenter assesses is the
probability that a subject gets the association right on the second test, given
that he got it wrong on the first test. The all-or-none theory says if he got it
wrong on the first test, he hadn’t learned it, so on the second test trial, he’ll
still just be guessing, so he will get the correct answer only by chance.
Alternatively, you can create cases where it’s difficult to guess correctly, as
when one word is paired with another word, and there are many words to
guess among --- maybe several thousand. So if the subject doesn’t remember
the pairing on the first trial, we can be pretty sure he won’t be able to guess it
right on the second test. His percent correct on that second test should be at
zero. And that’s exactly what we observed. That’s the way those data always
come out; that is, given that you make an error on the first test and you get
no corrective feedback, your probability of making a correct response on the
second trial is just at the chance level.

There’s a few cases with word-word pairs in which subjects get the
right associate the first trial and they fail on the second trial because they
forgot. Or in cases with just a few digit responses, they guessed it right on
the first trial, but they didn’t guess it right on the second trial. So those will
be cases in which subjects went from correct to error, but with word-word
pairs there’re almost no cases, in which subjects go from error to correct.
Those were pretty powerful demonstrations of this all-or-none learning phenomenon, and we were getting those results in three or four experiments I published in the early sixties on paired associate learning. I also observed similar results in another kind of learning called verbal discrimination learning. In that task, you show a person, say, twenty pairs of words and say, “I’ve decided that one of the words in each pair will be right and one will be wrong. It’s arbitrary. So let’s start. For this pair, shoe versus shirt, which do you think it is I’m going to say is correct? The subject would pick one and I’d say, “It’s shirt.” Then I’d go to the next pair and the next, and after he sees twenty pairs we then come back around for another test. “Shoe or shirt, which one did I say was correct?” In that situation, people learn just fine and you also observe this characteristic all-or-none learning of the correct choice within each pair.

So I had a number of cases like that, and Bill and I and our associates were creating quite a disturbance in the human learning field, because up till then, the traditional view was that everything would be learned gradually. And that makes sense, doesn’t it? Everything is learned gradually. But here we were saying, no, at a microscopic fine-grained level, a lot of the pieces are being learned in all-or-none stages. I was even able, later, to create learning situations in which subjects’ learning involved two or three all-or-none stages over successive study trials.

**Hartwig:** Did you receive praise for this at the time, or did it--

**Bower:** [00:49:01] Praise?

**Hartwig:** Yes. Or did it take a little--
Bower: [00:49:02] We got some people who wanted to either support us or oppose us, so it created some controversy among researchers in human learning. For example, in the study-test-test kind of experiment, our critics would claim, “Oh, well, all you’re doing is selecting on the first test the items that are really hard, and they’re the ones you make a mistake on. Therefore, although they’ve learned a little bit, they don’t know enough to make a correct response on the second test. Or they’d argue, “The error on test 1 was made by your dumber subjects, so you’ve selected slow learners and hard items. That’s why they are not only making an error on the first test but on the second test as well. What can you expect?” [laughter]

But Bill showed mathematically was that that objection just can’t be right. It’s a mathematical fact that if you have probability P of making a correct response, and successive responses are independent, then the probability you make a correct response followed by an error is P (1-P). The probability that you make an error followed by a correct response is (1-P)P. So that simple math says you’re going to have the same proportion of subjects and items who respond correct-then error as subjects who respond from error-to correct. But that prediction wasn’t true. Even with guessing among a small number of responses, like the digits, here’s about a 20-to-1 difference in frequency of those two sequences.

There were several other experiments that I did on the issue. The other kind used a procedure wherein the subject is trying to learn paired associates but every time he makes a mistake on a pair, I switch the correct answer, change what will be considered the correct answer henceforth. So, for
example, the experimenter has come into this trial with the assignment that “table” is going to be paired with “seven.” But you can’t remember what was paired with table, and suppose you guess, “Five,” so I say, “No, that’s wrong. It’s one, table/one, is the pairing I want you to remember.” [laughs]. You’re going to have to learn the pairing table/one now.

Next trial around, what’s “table?” And if you say, “Seven,” I say, “No, no, it’s three.” [laughs] The procedure is for the experimenter to keep changing the answer for an item whenever the subject gets it wrong. You continue rewarding the same answer only if the subject remembers the last pairing and so gets it right. What you find is that if you change the answer every time the subject makes a mistake, the procedure does not slow down the subject’s overall learning. The only difficulty with running the procedure is that the experimenter has to remember that if the subject recalls what you last told him was correct, then you’ve got to say, “Right. Correct.”

Anyway, so that’s another illustration of people learning all at once, and up to the point at which you learn, the experimenter can be switching the responses and the stimuli all around, and your overall memory is so poor you don’t even notice the switching because you haven’t learned a darn thing yet. But that view of one-trial learning is completely antithetical to the theory that people learn each pair in a slow, incremental fashion. That incremental theorist would exclaim, “But, my golly, you’re changing the answer on the poor subject almost after every test trial. How can he learn anything at all about a consistent answer?” Our response is to say that he learns any given
association in a one-trial, all-or-none fashion, and is only giving you ignorant
guessing up until that point in the study-test series.

**Hartwig:** So how long did you pursue this type of research?

**Bower:** [00:53:25] For about three or four years. It was fun while we were at it. But then I was able to find or create cases in which there clearly were multiple stages in learning, and it wasn’t just one “big bang” of learning, but rather it was sort of “bang, bang,” that is, several different all-or-none stages in learning. As soon as you concede that, then all of the very dramatic predictions of all-or-none learning go away, and the overall learning progress starts to move closer to the incremental view.

I found cases in which you get somebody starting at zero probability of correct responding and eventually they learn up to 100 percent over multiple trials, but I would look for an intermediate stage of performance. We found several cases looking at responding between when subjects make their first correct response followed by some correct and incorrect responses, and then they make their last error and move into 100 percent correct responding. In those cases, we could examine performance over trials between the subject’s first correct response and his last error, and see whether performance over those trials is stationary, shows no improvement, like a Bernoulli series. We found a number of experimental cases where subjects produced response sequences like that; we found it with eye-blink conditioning with humans, with reversal learning of rats in a T-maze, with avoidance conditioning of rats, and so on. But there are many other experimental situations in which the data are such that you cannot do that analysis --- for example, situations
in which subjects begin learning with correct responses above zero probability, or they never get up to 100 percent correct responding.

**Hartwig:** So then you moved on to memory organization, is that correct?

**Bower:** [00:55:10] Not right away. Rather I began studying short-term memory. That was a very popular topic starting around 1963. The earlier stuff I’ve been talking about was where the human subject has multiple trials studying and trying to recall a collection of ten or twenty items. And in those cases we were analyzing what happens over multiple study-then-test experiences that somebody has with a set of vocabulary items or the like.

What became very popular around 1963 were studies of immediate memory. Immediate memory had been sort of looked at and studied for many years ever since Ebbinghaus’ studies. For example, in the digit span test of immediate memory, the subject listens to six or so digits and tries to repeat them back in order, and you vary the number of digits he’s to try to recall in serial order. Everybody knew there’s a limit of seven or so digits or unrelated words that subjects can recall correctly immediately.

But what started to get interesting was how fast that immediate memory disappeared. There were many experiments being done whereby, say, I give you only three unrelated words to remember, but I distract you by, say, having you count backwards by sevens from 900 for 15 seconds. So you count 900, 893, 886, 879, so on, and after you do that for 15 seconds, I stop you and ask, “What were the three words I just gave you before?” Most people will say, “I don’t know.” [laughter] So that was interesting to us --- how fragile this immediate memory was and how you can blow it away in just
a few seconds of distraction activity. And experimentalists got very interested in that, in how to understand that.

And along came this neuropsychological patient, HM, who Brenda Milner had discovered up in Montreal, Canada. HM had received bilateral hippocampal surgery to alleviate almost continuous epileptic seizures he was having, and the surgeons cut out his medial temporal lobe on both sides of his brain. HM then showed this amnesic syndrome. He had a fairly normal short-term memory, so he could even carry on a conversation with you and keep track of topics for a few seconds, but he showed absolutely no accumulative learning of anything for more than a few seconds. So he had good or at least acceptable short-term memory, but no ability to form a longer-term memory. That was the way Milner and we thought about it. It was as though he could hold things in a little box called “short-term memory” and spit those back out within a brief time, but he couldn’t transfer things to a bigger box called “long-term memory” and go back in and retrieve those.

So this sharp distinction between short-term and long-term memory came to be studied, and it was an important distinction made at that time that has persisted ever since. In one sense, we’d always had that distinction, but nobody focused on the significance of it until that time. People started thinking, okay, so I guess short-term memory is sort of like an entrance or anteroom of material going into long-term memory, because information you’re going to put into long-term memory have to first go through short-term memory. You have to perceive them, hold them and maybe think
about, re-circulate, and rehearse them. Those are activities you do to put the materials into your longer-term memory.

So several of us started proposing these theoretical models or mathematical descriptions of how information, say, in the form of paired associates or strings of words enters a short-term memory store. While the items are in the short-term store, you can repeat them back, put them back out, and you can also try to transfer them into a longer-term store.

I started working on models of that process in 1962, ’63, ’64 with some of my Stanford students, and Dick Atkinson also got involved with this. Estes wasn’t particularly interested in short-term and long-term memory, but Dick and I were. So we started doing a lot of work on trying to fit data that people were publishing on short-term memory. For example, we know that if you experience multiple repetitions of the items that are to be remembered--for example, I show you three words, wait a second, show you those three words again, wait a second, give you those three words again, then I start distracting you by subtracting arithmetic and I ask for recall of them after twenty seconds, that your recall at twenty seconds is much better if you have had multiple repetitions compared to a single study trial. Moreover, your immediate memory of the three words is also far better if you can inter-associate the three words, as would arise using a familiar three-word phrase like Happy New Year or New York Yankees. So the issue becomes how to think about that and what’s happening when you have multiple repetitions or what happens when you have a three-word phrase that you know already so
that you can replace three words by a pointer to a single word or a single node in long-term memory.

So my students and I did a lot of work on that, and I published some on it. Dick Atkinson and one of our students, Rich Shiffrin, who had worked with me, developed a terrific theoretical model that accounted for a wide range of data that they had collected on short-term memory. Their theory became a kind of focal point and benchmark for the next ten or fifteen years, I would say, for research on short-term memory, for how subjects transferred information from short-term to long-term memory, and the like. I worked in that area, too and had fun at it for a while.

I then got more interested in what exactly is going on when we say you’re transferring some information from short-term to long-term memory. I mean, the math model can describe it. The longer the information is held in short-term memory, the more probable it is that some of it goes something into long-term memory. But what, in fact, is happening? And that’s where I got more into a subject’s introspections, what’s he’s thinking about when he’s the doing this transfer. I became more concerned with issues of how the person is linking up what he’s now trying to learn with what he already knows, and how does that linkup occur and how important is that to his long-term learning. You could say that I was examining how subjects were using the contents of their long term memory to link up to items in short-term memory in order to transfer that information.

Those questions got me into the issue of organization in memory; that is, what is it about materials that you study or look at that makes them highly
memorable? Or what does your mind do with materials to make them more memorable, to put them firmly into memory?

Hartwig: So what were some the tools or techniques or means for organization that would make things more memorable?

Bower: [01:04:49] Well, I started studying very old mnemonic devices that magicians and stage performers had been using for centuries.

Hartwig: This is just finding old books? Or how did you--

Bower: [01:05:06] No. I knew about magicians and I knew of Dale Carnegie’s books, which talked about ways to remember a speech. There were books out by magicians and memory experts who knew some techniques that had been around since ancient Roman times. Several Roman treatises from 200 B.C. had introduced some of these techniques. One was called the method of locations, or method of loci. Over the centuries there had developed a lot of these mnemonic procedures. Academic psychologists had never studied them.

Hartwig: Why not?

Bower: [01:06:13] Well, these techniques were viewed as in the realm of magic and were for entertainment and showing off. Ebbinghaus and followers in his tradition had canonized the view, “We’re interested in catching people as they learn from ground zero, by brute force, just by the sheer heave of the will, studying hard, and rehearse, rehearse, and repeat the material to be learned.” But the magicians answered, “Who wants to do it that way? That’s not the way to learn the names of a hundred people who walk into your stage shows.” [laughs]
So I read a bunch of memory books written by mnemonists and started doing research on mnemonic techniques. That was unusual in those times, that an academic psychologist would actually try that approach, where you’re teaching people something to do to learn rather quickly instead of just telling them, “Here. Repeat this material until you learn it,” and let them go.

So one of the techniques that works very well is to have people learn a list of unrelated words by having them tell a story by which they link from one word to the next to the next and so on to the end of a serial list. You illustrate to them what you mean. The story can be as bizarre as they wish, but they have to make one up.

In one of our first experiments we had people learning twelve lists of ten words one right after the other. We’d show them the ten words of a list. Half of the subjects in the control condition were told, “Study the ten words on each page. We want you to learn them in order.” And they’d study them for two minutes, and then we’d say, “Okay, recall them,” and they would do that at a very high level, like 95 percent. And you’d say, “Okay, here’s the next list. Study it for 2 minutes”. And then you’d have them immediately recall that second list. Then they’d learn a third list and so on. They go that way through twelve lists of ten.

Then you say, “Okay, I’m going to give you the first word of each of the twelve lists. I want you to remember all the other nine words in order that were on each of the lists.” These subjects would groan, curse you out, then try really hard but could recall only about 15 percent of the 108 words from positions 2 through 10. This is like one word recalled out of seven.
[laughs] They are best on the twelfth list and the eleventh list—that’s a recency effect—but their recall was far worse than subjects in the experimental group. That’s the control group. Now for the experimental group of subjects you tell them, “I want you to learn the ten words of a list by making up a story woven around them in order. You start telling yourself a story using the first word in the list, and continue that story by extending and linking it to the second word, then do that to link into the third word, and so on until you’ve woven the story around all ten words of the list. The story can be as crazy and bizarre as whatever you need to link the words in order. You’ll give you two minutes to make up your story and after that time I’ll ask you to recall those ten items in order. Fine.” After they do the first list that way, we give them the second list, and later lists in order.” After these subjects had made up stories through twelve lists, we’d say, “Okay, now I want you to remember all twelve lists. Here’s the first word of the first list.” And they rattle off that story and they tell you the other nine words. “Here’s the first word of the second list.” They rattle off that story. [laughs] And these people continue to do astoundingly well. They remember like 85 percent of the words, seven times as much as the poor control subjects who just relied on their usual learning method of rehearsal. And these were smart Stanford students!

That was one of my first experiments, and when I sent it in for publication, the editor said, “You haven’t reported any statistics to show a significant effect here.” [laughter]
I said, “You don’t need statistics. Just look at the average recall levels.”

[laughter]

**Hartwig:** So did they publish it?

**Bower:** [01:10:31] Yes, they did. I thought they were pulling my leg or something.

**Hartwig:** How was that received?

**Bower:** [01:10:39] Well, very good. Other people picked it up and did it. The problem is once you do these demonstration experiments, and they have these huge effects on learning, other experimenters follow you but say, “Oh, okay. How do we follow up on that?”

We’ve done lots of work where we have people learning many arbitrary pairs of words like nouns. We just say, “Make up a sentence that connects this word to that word in the pair,” and so on, for, like twenty arbitrary pairs of words. Paired associates are learned quite well if you teach people to just to make up a sentence, any sentence you want that’s kind of reasonable, that puts this one word with the other word of the pair. That method produces a big impact. People do much better if they have to make it up themselves than if you give them a reasonable linking sentence, although if you give them a linking sentence one, it’s also better than if you just give them the word-word pair and say, “Here. Rehearse this until you learn it.”

So we got into studying all kinds of memorizing techniques. Here we would compare learning by subjects doing rote repetition to learning created by giving people sentences to link two words. We could ask, how does that compare with having subjects generate their own linking sentence, or how does that compare with having them make up an image in which the
referents of one item and the other are interacting in some way, and so on. You find a nice increase in degree of learning as subjects use more and more elaborative and visual imaginative methods for linking the word pairs.

I got interested at this time also in mental imagery. Mental imagery had been put on the back burner in American psychology after about the 1920s. Most studies of human learning at that time were very focused on either motor learning or on verbal learning, with big emphasis on learning by rote repetition. No researchers were working on imagery, and, of course, all these magicians and mnemonists, memory books, were always saying memory improves if you use imagery and imagination.

So I started studying the role of imagery in memory. Paired-associate learning was the simplest place to start, so we’d take two unrelated words like “dog” and “bicycle.” Subjects were to learn a collection of pairs such as dog bicycle. How do you make up an image to link those items? I think of a dog riding on a bicycle or maybe a dog chasing a bicycle, and I can visualize that scene, and so on. And when people do that, they greatly increase the amount they can learn. That is, when I later ask “What went with dog?” they can say, “Bicycle.”

So interactive imagery produces huge effects, and the vividness of the image they make up predicts quite well how well they’re going to remember that pair. The more vivid or the sooner it comes to them, the better is their recall later on. Imagery can nearly double or triple the amount of memory people get from a single trial of studying the word pairs. And what that means, of course, is that words that refer to concrete objects or words that
are highly imageable like “ghost” or “unicorn” even though they aren’t concrete, they’re imageable, and words that are highly imageable are also easily learned. Even if you don’t tell people to learn by constructing images for such concrete words, they often do it somewhat spontaneously. Although they remember far better if you tell or instruct them on how to do it.

Hartwig: What about chunking or multi attribute?

Bower: [01:15:00] Yes. Let’s see. Let me talk about chunking. One observation is that the size of the short-term memory box is determined by the number of chunks of information you can put into it without loss. It’s like six to eight chunks, and a chunk is a highly integrated set of units or elements. It could be a word or a word-phrase, it could be set of abbreviations or anything. Or it could be a set of digits or the like.

If you use written materials that a person can chunk into larger units, like unrelated letters chunked as words, a person will remember seven words as well as seven unrelated letters, so the words are just larger chunks. So the question becomes, what determines what’s a chunk? A chunk is obviously a highly inter-associated set of elementary units like letters in a word.

I got interested in the issue of perceptual factors that determine chunking. To illustrate, suppose I show you the abbreviations “IBM,” “FBI,” “UCLA,” you could remember those as three chunks. But if I screw it up and take one letter from the end and put it at the front of the string (thus creating AIB, MFB, IUCL) thus having the same set of letters except they don’t make recognizable abbreviations, then the material has been broken up into twelve
letters rather than three familiar chunks. And, of course, people immediately recall three units better than twelve units.

You can show that what determines those chunks are the spaces or the silent pauses, like you can say, “IBM,” pause, “FBI,” pause, “UCLA,” pause, and so on. It’s the pauses that help create the perceptual unit that is entered into short-term memory. If you put the pauses in the right places, everything goes swimmingly because they demarcate familiar units. But if you put the pauses in the wrong places, it completely screws up perceptual memory, because our perceptual system uses the pauses as markers for grouping elements into chunks. The main problem people have when they recall a string of items from short-term memory is moving from one chunk to the next. If you look closely, you can see that most of the errors arise when subjects move from the end of one chunk to the beginning of the next chunk in the series. Once their memory retrieves one chunk, they can quickly recall the elements within it. But they then have to move onto the next. That’s when we observe subjects having another recall gap in getting from this chunk to the next one.

In analyzing recall data, we calculated what are called transition error probabilities in people’s recall. Those statistics showed very clearly what people’s chunks are, what chunks they had adopted for encoding and recalling the series of letters or digits. One finding from our experiments, let’s say when you’re asking subjects to recall novel strings of twelve digits, we look at how much people improve in recalling some strings that are repeated every now and then. You find for that recurring string that if you
group those twelve digits into the same chunks every time you present that series to the person, he gets better and better at remembering it. In contrast, if you take the same recurring string of twelve digits but you group them differently each trial by inserting pauses at novel places or by printing the letter series out on a page with novel spaces between letters, subjects won’t get any better at all in recalling that series over its successive presentations, no matter how often they hear it. Even though as they’re trying to remember this string of digits, and they’re writing down roughly the same sequence in their recall, they don’t get any better at recalling it. It is as though when you change the grouping or chunking of the elements, it’s a new series—a new world for them.

We studied several perceptual variables that would cause different chunking. One was the physical similarity of elements making up a chunk. We would vary, say, the sizes or colors of triads of successive letters that would cause perceptual chunking of letters into either familiar abbreviations like “FBI,” “UCLA,” or the letter sizes and colors arranged slightly so as to break up those chunks completely. You can see it in reading text. If you vary the spacing or the size of the letters or you vary the color of the letters, so that the first three letters are green, the next three are blue, the next three are yellow, the next ones are brown, that too will determine the chunking and recall that people adopt.

We showed a similar outcome in free recall. In free-recall experiments, you present a whole bunch of unrelated words to people and ask them to remember all the words in any order that they want. It’s a task similar to
remembering all the movies you saw last year or all the people you met at a party. I viewed it as a matter of remembering chunks of items that you form as you’re taking in the words of the list. You inter-associate the words into recallable clusters. Imagine that you form a collection of people at a party, or a collection of movies you’ve seen, a collection of books you’ve read. The collections form into inter-associated clusters and you recall the collections by going from one cluster to the next.

One way you get better at recalling the collection is that your clusters grow bigger and more stable with training, so that eventually you remember bigger ones. In one of our experiments, we had subjects remember thirty-two words presented as eight groups of unrelated words. We just presented four words at a time and told folks to remember them, continuing up to eight quartets, or thirty two words in total. Then we’d say, “Now remember all the words you can in any order you want.”

In that situation, if you ask subjects to imagine quartets of words to put the items all together in a scene, and now here’s another quartet, put them in a scene, so on, they will later recall the 32 words in quartets, and do fairly well. If you repeat the same list of thirty-two words, showing subjects the same quartets to study again, they get better and better over trials. On the other hand, if you force them to reorganize and put the 32 words into new quartets every trial, so no word recurs with any other word in the reshuffled quartets, subjects do not improve their recall over study trials. So in order to improve, you have to keep the same groupings from one study episode to the
next. So that was one of the studies we did on chunking operations in free recall.

**Hartwig:** About this time, you attended the Lake Arrowhead Conference Center meetings.

**Bower:** [01:24:05] Oh, yes, those were wonderful summer conferences. We would meet for a week. The attendees were many of the fat cats of human memory research.

**Hartwig:** So who were the fat cats?

**Bower:** [01:24:23] They were the old duffers, the old geezers who had been very prominent in the 1950s and the 1960s. They were Benton Underwood, Leo Postman, Art Melton, Charlie Cofer, Rudy Schultz, Jim Jenkins, and Jeff Keppel and Ed Martin. These were the old-timers. They had persuaded the Office of Naval Research to support their individual research projects and also to give them money to have this at Lake Arrowhead Conference.

Lake Arrowhead is a resort east of Riverside, up in the mountains there, a wonderful, beautiful place. There’s a conference center of the University of California where we went. They had big conference rooms, places for us to eat and sleep, completely out in the boondocks, but we were well taken care of by kitchen staff. The group always brought in a lot of booze. [laughter] Those old-timers loved that.

**Hartwig:** Which I’m sure it made for lively discussion.

**Bower:** [01:25:54] Those old timers got the money to bankroll the conference. It was mainly originally for them, but then they started introducing some of us young Turks into the meetings: Endel Tulving, Ben Murdock, George
Mandler, George Miller, and me. And we started changing the complexion of what kind of topics were talked about and how we reacted to their work.

The way the Arrowhead Conference was set up was that everybody had a quarter or half a day to present their research, what they were working on and what they’d just done and what they were planning to do next, and so on. The rule was that anybody could say anything they wanted to about it at any time. So you could jump in and shout, “That’s horseshit. I don’t believe a word of that. More than that, I have data that completely kills that idea, so give it up.” [laughter]

This was standard behavior, except when we young Turks were added to the group, we really did more of it and didn’t always show appropriate deference to the older members and their research. Also we started talking about our own stuff so we got the old guys on board as being interested in what we were interested in. So they would start theorizing the way we did, Endel, Ben Murdock, and George Mandler and George Miller and I. Miller and Endel were particularly effective in this regard. So, we’d had this conference once a year, and it must have continued for, I don’t know, eight or ten years, and it was great. It was the way to really get your ideas out there to the attention of the leaders, so you could put your new ideas and findings on parade. Just the kind of thing that young researchers like.

**Hartwig:** And did this mirror maybe the changing landscape within psychology?
Bower: [01:28:46] Yes, it mirrored the advent of cognitive psychology, of the cognitive revolution, especially regarding human memory, because we were a bunch of memory researchers.

There are a few people who didn’t attend the conference. Dick Neisser was not in it, although he should have been, but Dick was not a memory researcher in those days. And Don Broadbent could have been in it, but he was not a memory guy. But the rest of us were nuts about memory research.

Anyway, the cognitive psychology approach to memory got started and consolidated at that conference, and the way that we talked sort of got accepted by the older people. They understood we were not going to do their kind of memory experiments; or, if we did, we would always reframe them in terms of our ideas.

For example, interference of memories was a big topic for these guys, whereas they never touched memory for coherent stories. So I did some research on interference in memory for stories. Most research up to that time had studied interference using lists of single words or the like, but I did some research where we had people remember stories. In this case, our texts were biographies of famous poets, and we made up three such texts. We had people study a biography and then try to recall it, study a second one and try to recall it, study a third one and try to recall it. Then at the very end, we had them try to remember all three of the biographies. I wrote the biographies in such a manner that the overall framework of the biographies remained the same; namely, the text would mention when the poet was born, what his father did, where he was raised, where he went to school, the name and
publisher of his first poem and so on. However, the three texts were clearly
distinct since all the details like names and locations and years were different.

So the overall framework was the same as the subject read about and
recalled each of the three poets except all the facts or details were changed--
what was his father’s occupation, where he went to college, who he was
married to, and what the wife did, etc. I was most interested in what happens
when subjects try to recall each of these three biographies? We see that as
subjects read then recall each biography, they get better and better at
remembering the right kinds of facts, the gist of the biographies. It’s just the
specifics of what details go with which poet that soon are getting very
confused in their recalls. So, over the three tests, subjects are learning what
we might call the macro structure of the biographies, that is, the kinds of
things mentioned in the biographies, but they’re getting more and more
confused about the micro structure, about the details of each. Memories of
those details are interfering with each other. So we observe learned
facilitation occurring at the macro level, while also seeing interference going
on at the micro level.

So if you look at just overall level of recall of how many correct facts a
person recalls about the third biography, it’s about the same as it was for the
first and second biography. So at that unanalyzed level, it looks like
everything’s okay --- no problem. Except if you analyze it, you can see a lot
of learning going on at the macro level, coupled with subjects’ increasingly
confused remembering of what details go with each poet in the macro
categories. So the net effect, that is, of no overall difference in recall of the
three biographies, was in fact a composite of two opposing tendencies: subjects’ increasing memory for the macro-structure framework of the biographies alongside increasing interference in discriminating among the details that each biography attaches to that macrostructure.

Anyway, that’s a piece of research I did that combined some new ideas with some old ones the geezers could relate to. It’s using interference notions, yet it’s looking at a new topic; namely, how people remember coherent prose—in this case, biographies that are similar in structure. And you can think of it in terms of facilitation caused by repetition at the macro level, alongside of interference among details, with the two levels interacting with each other in memory. Anyway, that was one research project I did from a mixed perspective.

**Hartwig:** Let’s end here and we’ll take up with session three.

[End of Session Two]
Hartwig: This is Daniel Hartwig. Today is September 2nd [2014]. This is the third session with Professor Gordon Bower. Good morning.

Bower: [00:00:08] Good morning to you.

Hartwig: All right. So, last session we were finishing up talking about chunking, so another one of your primary research subjects or fields was in conceptual hierarchies and iterative cueing. Can you talk a little about that?

Bower: [00:00:27] Yes. One of the limitations of short-term memory is that you can only hold four or five chunks of information in short-term memory. So, how do you overcome that barrier or that limitation? The way, of course, it’s done is to make each chunk be a code name for larger pieces or larger chunks of information. You can imagine that process going on iteratively; that is, you subdivide a category once, and then using the products of that subdivision, you subdivide those another time, and on the products of that, you do it a third time, and so on. And in this manner, you generate a hierarchy or at least a series of information chunks that are connected to one another.

Diagrammatically, it’s like you start at the top of the tree or chain, so to speak, and you download the chunks of that first chunk, and then you take each of those sub-chunks in turn and download those into what they consist of, and you keep moving down.

Although that always sounds very good in the abstract, I thought it would be nice to have experimental demonstrations of that process. So my students and I tried to arrange such a demonstration. We did it by first
composing several conceptual hierarchies in which we would begin with some very large category such as metals or animals or jewels, and then unpack each large category.

So metals can be man-made metals like steel or pig iron, or precious metals that you take out of the ground, such as silver and gold and platinum; and furthermore, the silver can be of various kinds that have been treated in various ways and made into things.

As a second example, consider the category of rocks which can be divided into building materials like bricks and slag in contrast to precious rocks, such as diamonds, rubies, or sapphires, and each of those can be subdivided in terms of their carats, size, or cost and what they are used for.

In each of these cases, you’re kind of going deeper and deeper into a person’s knowledge about animals, or metals or rocks, and as you move down each of these branches, you uncover more and more detailed knowledge that an educated college student would already have. You’re taking a given category and subcategorizing it and then subcategorizing the subcategories and so on. And very quickly, the number of items grows exponentially in terms of the amount of information that’s being contained in that top-level concept of metals.

In our experiment we asked people to study and recall four such hierarchies. We would spread out each category tree on a sheet of paper, with the words written and laid out like a tree of bifurcating branches at successive levels, and we asked people to study those for recall. After they’d study a tree of conceptual knowledge for about two minutes, they’d study the next tree,
and so on through four conceptual trees. Thus, these subjects studied what we called the well-structured organized conceptual trees.

For the alternate subjects, in the control condition, they saw and studied exactly all the same words that were in the original trees, but we scrambled them and mixed them all up. Although written physically as a tree structure, we combined words from different levels and from different conceptual trees onto one page. So these subjects studied these four mixed sets of words arrayed visually as trees. So the control subjects saw four pages that contained a totally disorganized array of information.

So subjects would study these four different arrangements for two minutes each, and when they’re done with all four arrays, we would ask them to recall the words in any order. The total numbers of words involved in those trees was about 120. And the people who got the disorganized lists could recall only about ten or so of all the 120 words on their first try. The few words they did recall were conceptually related, like a few words about precious metals or domesticated animals.

On the other hand, the people who saw the organized trees could output on the order of 80 of the 120 words after studying them each of the trees for about two minutes. And so the discrepancy in recall between those who studied the organized list versus those who studied the disorganized collection of the same words was on the order of almost eight to one.

What was interesting was to watch the way people recalled. The organized people would recall one tree of words at a time, even though they could have recalled them in any order they wanted. They would start their
recall with the top-level concept in a tree, and then recall its subcategories, then take each of those in turn and recall their subcategories, and so on. So they were just unpacking and moving right down the tree in their recall.

What they would forget might be one of the subcategories, and therefore all the items subsumed by that subcategory were forgotten as well, because they were unable to cue themselves to come up with those words. They tended to lose whole subcategories, but they were recalling overall by downloading subcategories in a recursive fashion, which is basically the way you unpack a tree of concepts and categories. You start at the top and you recall its subcategories. Then you take each of those subcategories in turn for recall and do it in a breadth-first manner.

We gave all subjects a second study trial, and the people who studied the organized categories got all 120 words on the second trial, whereas the people who studied the disorganized categories got a little bit more, maybe 25 or 30 words. It was a dramatic difference comparing the 120 words recalled by subjects who'd studied the organized trees to, say, other subjects in classical immediate memory studies whose short-term memory consisted of recalling 6 or 7 digits in order.

So we did the same kind of experiment not with categories but with just associative relationships. I recall one of our associative hierarchies started with “mouse.” [laughs] And mouse is associated to words like “trap and mouse,” “rat and mouse,” “cheese and mouse.” So that’s how we made up the first level of associates of mouse. Then you’d take “trap” and you look up words associated with trap, like “speed trap,” “rat trap,” “trap door,” and so
on. Or from the associate “cheese” you could associate “butter” and “milk” and “curds.” And then “butter” associates might be bread, cup, fingers, and so on. You can imagine how you’d fill out a hierarchy that is only associative in nature; the associates arise from all kinds of relations to the target word, and they are not within the same conceptual domain at all. They just happen to be all manner of word-word associations that people have in their head.

So we did the whole experiment with four hierarchies of just associates, having subjects study then recall either four organized or the same words in four disorganized arrays. It turns out you get rather comparable differences in recall. The differences aren’t as striking and huge as we observed with conceptual categories, but, nonetheless, I think we observed on the order of a five-or-six-to-one difference in recall, favoring people studying and recalling organized collections of associates versus disorganized collections.

So that was one way you could see how you can goose up the amount of remembering that people have for given arrays. So instead of the limitation of five or seven items that people can recall out of short-term memory, you’re now increasing their immediate recall up to 120 items, and that’s very impressive to observe.

Hartwig: So what years would this be?

Bower: [00:11:08] This was 1969, something like that.

Hartwig: And how was this received?

Bower: [00:11:17] Oh, it was very well received, and everybody wanted to have our experimental materials, so I passed it around somewhat, and after a spate of experiments by other psychologists, the field moved on to other things.
So that’s an example of hierarchies, but you see the same kind of thing with hierarchical recall of strings of unrelated letters or digits, where you group the overall string of, say, sixteen items into two big groups of, say, eight and eight, and then each string of eight can be subcategorized into two fours, and then each four can be subcategorized into doublets, and so on. So what started out to be, say, a series of sixteen digits or sixteen letters can be broken down into doublets so you have eight doublets, amalgamated into four quartets, and so on. If you simply space items that way and induce the subject to group the items in that way in their mind, their recall gets much better.

You can also see the way subjects are unpacking the chunked series as they recall. If after looking at the chunked series, the subject is asked, “I want you to remember these sixteen digits in order,” he’d recall the first quartet, and then begin making errors in moving to the second quartet and getting into it, or make errors in getting into the first doublet of the second quartet, etc. You can just trace the memory errors that people make in terms of where they’re having greatest difficulty --- in moving from one group to the next and to the next and so on, and doing this recursively as they recall --- starting from sixteen to eight to four to two elements of the larger string.

So, we did some experiments of that general kind, and I think it had an impact on psychologists’ thinking about limitations of short-term memory.

**Hartwig:** So in 1973, was it you and John published *Human Associative Memory*?

**Bower:** [00:13:51] Yes. That was, for me, a very important change in my conceptual thinking.
**Hartwig:** How so?

**Bower:** Up until that time, almost all of my research was in the tradition of having people memorize lists of unrelated words, or digit strings or letter strings or whatever meaningless materials were convenient. We could see that the way people were doing that is by relating what they’re seeing to what they already know and then setting up associations between chunks of information that they are learning, except that they’re learning a new configuration of elements that they didn’t know before. And how does that happen? How does that chunking process get involved with how people learn more meaningful material?

So John and I started doing research on people’s memory for simple sentences like, “Joe loves Mary,” or “Harry bought a bike,” or “Daniel owns a Buick.” How do you think about that in associative terms? The way we thought about it was that people first have a little linguistic grouping or chunking procedure called syntax that enables them to group parts of a larger sentence into smaller chunks, such as, “Dan owns a Buick, the Buick is red, and it has four painted hubcaps,” etc. And in each of these phrases, what you’re doing is putting together a specific configuration of already-known concepts. Like I know who Daniel is, I know what Buicks are, and I know what the relationship “owns” means.

So you can think of this new proposition, “Dan owns a Buick,” as setting up associations between new instances that I create in my memory—from my concept of “Daniel,” to a new instance of an “owning” relationship, and a new instance of “a Buick.” So your brain sets up a new associative
structure having subject/Dan, predicate/owns a Buick, and that predicate comprises a relationship of owning to an object, which is “Buick.” So you can think of that as the person recording in his brain a tree structure relating a subject to a predicate. This is all done by your brain setting up in memory new instances of the general concepts that you have, like “owning” or “Buicks.” And you are associating these concepts together in a structured way. That’s the most elementary basis for the theory that’s then to be elaborated for storing more complicated sentences.

You can use this elementary memory structure to answer questions, such as “What did I say about Dan?” And you can go into memory and pick up the most recent example of “Dan” in my memory and output the rest of that associative structure; namely, he owns a Buick. Or you can ask a different question, “Dan owns what?” or “What did I say Dan owns?” And so you probe your memory with “Dan owns—,” question mark, and using a matching process, your brain retrieves this associative structure which you had just set up, namely, “Dan owns a Buick.” So you say, “Okay, what does Dan own?” Answer, “A Buick.”

So you could see that memories organized by such subject-predicate structures enable you to answer questions about who’s doing what to whom, when, and where. All your brain has to do is to set up subject-predicate associations in memory that encode each of these bits of knowledge.

Okay. That works with an elementary proposition like “Dan owns a Buick,” but you can elaborate upon that, as in “My friend Dan recently purchased a big red fast Buick.” [laughter] And what you’re doing in each of
these cases is predicking something about concepts you’ve set up. So about Dan, you can say “my friend,” as a predicate. Memory will record “Red fast Buick” by connecting “red” and “fast” are predicates modifying “Buick.” But nonetheless, these are just associations you’re setting up in a structured tree.

Okay. To do this, what you need is to be able to take an input sentence and analyze or parse it from its surface structure into its deep structure of propositions that relate concepts. For example, “owns” is a relationship between a person and an object. “Red” is a color adjective modifying an object, “Buick,” and so on. The idea is that people use their knowledge of grammar to perceive and analyze this sentence into its atomic propositions and then are setting up these associative structures for the atomic propositions, which are bringing together concepts that they already know but in a novel configuration.

And those underlying conceptual associative structures enable you to transform what you hear into other surface structures. So you can go from a deep structure like “Dan owned a Buick” to something like “A Buick is owned by Dan”--from the active to the passive--by a simple linguistic transformation. Or “Dan doesn’t own a Buick anymore.” That that can be transformed into a proposition that asserts “It’s false that Dan now owns a Buick.”

So by using syntax, a listener or reader converts a surface sentence into a deep structure of atomic propositions; once you have that level of atomic propositions and conceptual language, you can transform it in any way you want to spit it back out or to answer questions about it, no matter how the
question is asked. Such as, “What did I say Dan owns?” or “List all the people you know who own Buicks,” so that you are making use of that conceptual memory structure in novel ways. And that’s one of the hallmarks of language, of course; we don’t store words, we store conceptual structures. And we don’t remember the exact words we read or hear, although we can, but we don’t usually. We remember conceptual propositions, the gist of what someone said.

Okay, that’s all sort of preliminary to the main point of my research with John Anderson. What John and I did was to carry out a collection of memory experiments showing that it’s okay to view sentences as a collection of associations amongst concepts, and that these conceptual associations follow all of the rules that associations since Aristotle have proposed, as well as all the laws that experimental learning theorists have been talking about over the last hundred years. And what we mean by that is an association will get strengthened by repetition, two items will get associated because they occur contiguous in time or in space, and once an association has been strengthened, it can decay, you can forget it, and we know how to make that happen rapidly, and so on. We know what cause you to remember one thing and not another, and why you associate one thing with another.

But in language it is the syntax, not temporal contiguity, that tells us what goes with what. Let me give you a trivial example in which a person’s syntactic knowledge overrides temporal contiguity in establishing associations. I read you a collection of short sentences like “John owns a Buick,” “Harry owns a cat,” “Bill owns a shoe store,” etc., and I repeat that
series many times over in exactly the same sequence. “A owns B,” “C owns D,” “E owns F.” After you’ve heard this for a few times, I ask you, “Okay, what does A own?”

And you can easily say, “B.”

“What does C own?”

You can easily say, “D.”

But contrast that to the following observation: no matter how often I repeat two sentences side by side, you will not know what is the word that follows Buick in this stream. You go from B to what? And people will not notice that the object in one sentence (like Buick) is always followed by a specific person in the next sentence (like Harry). The words are contiguous in time, as when I say repeatedly, “John owns a Buick,” “Harry owns a cat,” therefore “Buick” and “Harry” are nearly contiguous in time, but the syntax tells the mind that these items don’t belong together. Our syntactic analyzer breaks these utterances up into propositions, and you, so to speak, put a mental period or group boundary after “Dan owns a Buick,” etc. That’s the end of the group, and you don’t establish associations about groups from alien sentences.

Okay. So that’s an example in which syntax and our normal way of parsing sentences is overriding temporal contiguity in making associations. It’s a simple example of how syntax works.

Hartwig: So how long did you and John work on this, and what were some of the techniques or other areas of psychology or other studies that you [pursued]?
Bower: [00:26:59] Well, one of the things we showed there was that you can create very strong associative interference in memory. We did experiments in which we would teach somebody a collection of very simple facts about, say, people in a small village. Examples would be “The sheriff lives on Maple Street” and “The mayor runs a grocery store” plus many more facts of this kind. So we have you learn multiple facts about a character and/or learn multiple facts about a predicate, such as the predicate “owns a Buick.” “Person A owns a Buick,” “Person B owns a Buick.” And then I’d give you multiple other facts about what each fictional character owns.

So the experimenter has the subject building up a giant network of relationships. There’s a collection or fan of people who own one type of thing, and there’s a fan of one type of thing that many different people own. Such networks produce associative interference in people’s knowledge, so that they’ll get confused when we ask “Does Person X own a car?” or did I tell you that “The Mayor lives on Elm Street,” and so on.

The interference shows up even if you train people so their recognition memory is perfect. They, nonetheless, differ in how fast they can retrieve and verify that a certain fact was told to them. It’s as though they have to go through the recital of finding out all the things that Daniel owns or all the people who own a Buick, looking for the one specific association you’re asking about, whether Dan owns a Buick. Once you find the query’s association, you reply, “Oh, yes, you said, indeed, Daniel owns a Buick. Ah, that’s true.”
But all of that memory searching that goes on for the subject and for the predicate and the things that go together takes time, and so you’re slower to verify that fact. Even though you know the fact, you’re slower to retrieve and verify it. So if you measure time—latency, as we call it—the slowing of your answer is very predictable by the fan on the subject and the fan on the object, the so-called fanning effect that John has studied extensively.

We also did a lot of experiments in which we studied people’s memory for lists of more complex sentences. Like our prototypical sentence, “In the park last night, a hippie touched a debutante.” [laughter] After studying a list of such unrelated sentences, subjects would be cued to recall. “So what did we say about a park?” or “What did we say about a hippie in a park?” And we’d look at what people could recall when cued about that sentence after they’d study some twenty-five unrelated sentences of this kind. All the sentences weren’t that silly.

We would look at fragmentary recall, as when subjects would recall only parts of the sentence, such as, “There was something about a hippie in a park.”

And we’d ask, “Can you recall any more?”

And subjects might remember another piece of it, like, “Ah, he did something with a debutante.”

“A debutante?”

“Yes, I think that is what it was.”

“And when did this happen?”

“Oh, it was last night.”
So we observed fragments of this associative structure coming out in subjects’ recall. John and I developed a mathematical model of the learning and recall process that predicted the probability that various parts of the sentences would cue recall of other parts of it. The model made some strong predictions such as, if to a given cue a subject cannot recall part X of the sentence, then he won’t be able to recall some other fragment of the sentence.

It was pretty impressive, actually, when you think about it, because the data structures that subjects are setting up and retrieving are very complicated. I mean, if you look at the small details of it, it’s quite complicated, which parts of the sentence cue recall of other parts of it.

So the relationships can get quite complicated in the memory data, but here we are fitting a little dinky mathematical model to these data, predicting patterns within the data, and we were rather impressed with that. The book that we wrote had a collection of different things in it. Some were these experiments that I’ve talked about—the fan effect and the fragmentary recall effect—but in other sections of the book we also used the theory to explain a number of facts that were in the experimental literature.

**Hartwig:** Such as?

**Bower:** [00:33:18] Oh, the depth of processing results, or why it is that if you are learning paired associates of words and I ask you to make up a sentence relating the two words, your memory for the pair is far better than if I just give you two words to repeatedly go over, because the latter relationship, repetition, is confined to just one type. For example, if the item “dog” is
repeated next to the word “bottle,” “dog” next to “bottle,” that's a very weak relationship for encoding 25 or so pairs. And the relationship yields poor memory because you're using “next to” for twenty-five different paired associates. That’s going to lead to a lot of interference compared to having a unique verb, unique predicate, to put together the different pairs of words.

We wrote about a lot of findings. For example, why it is that if you study a collection of words, all of which are associated to a central word that is never presented, when I later test your recognition memory for words on the list, that central word turns out to be judged with a very high probability as having been presented, even though it had not been presented. It’s because all of these other words on the list are appearing and they are activating the unspoken central word, so it is later falsely recognized because it was so strongly activated in the context of hearing the word list. That was a well-known phenomenon that we explained.

But one of the good things that book did was review the history of associationism in philosophy and show how modern experimental learning theory was firmly in that tradition. Our work in this book was putting together modern association theory with psycholinguistics, with ideas of Chomsky and George Miller, and bringing this barren associationist doctrine into contact with language and human knowledge. So now we could say a little bit more about how it is that associations work in conveying knowledge and how people pick up knowledge, such as Jupiter is a planet or whatever, and how knowledge structures can get elaborated by adding more
associations to them. So the one thing we did was to try to relate modern associative learning theory to psycholinguistics.

We also touched on another topic, which was how people put novel pictures into memory? I show you a picture of a bottle on a table or the bottle next to a box. How does that’s scene go into memory? So John and I used the same old trick, say, “Well, I know that’s a picture of a bottle and I know this is a table, and I see that the spatial relationship is of a bottle on a table.” So I’m just sticking into memory that associative structure relating your concepts of “bottle” and “table”, and the spatial relationship of “on”. Once you have that associative structure, you’re able to answer questions about the picture. For example, I can show you the picture and I ask, “Is the table on the bottle?”

Your associative structure replies, “No.”

Or I’ll say, “Is there a dog on the table?” [laughter]

You say, “No.”

“What’s on the table?”

“A bottle.” I mean, that’s very elementary, but you could see how it can get elaborated to deal with larger scenes and to answer questions about familiar scenes. Like “What does Hoover Tower look like from the front?”

Okay. And so we tried to link up certain computer science programs that were becoming popular in those days to our associative theory of memory. The computer science programs were doing what was then called scene analysis, so that their robots could take a photograph and then analyze it into components such as recognizing, “That thing is a bottle and that thing
there is a table, and, by golly, they bear a spatial relationship to one another. This is a photo of a bottle on a table.” Sounds trivial, but let me tell you, in the sixties and seventies, doing scene analysis automatically was a huge problem in artificial intelligence.

In fact, artificial intelligence always had the hardest problem dealing with these rich cognitive skills that people all have, that we picked up as babies. Examples would be being able to recognize objects and see those relationships in the perceptual world and store up all kinds of facts about it. Babies learn physically how to handle bottles and how to deal with them before they ever learn that it was called a bottle. To translate that into our HAM theory, we would set up a concept of bottle in which it is associated to a picture or an image of a bottle, but the concept is also linked to a name, and the name exists in a big phonetic domain which can sound out the name, “bottle.” [laughter]

So you can begin to see how our little dinky associative network of propositions can deal with some language, with some perceptual integration and with the integration of perceptual knowledge, conceptual knowledge, and language. John and I were fitting experimental data that was coming out in those days. Many experiments, as I noted before, would show a subject a picture and ask a question about it. So you might show a picture of a little box and a little ball and ask, “Is the box above the ball, yes or no?” or ask “The ball isn’t above the box, yes or no?” “The ball is below the box.” “The ball isn’t below the box.” Subjects answer all these little dinky syntax
variations, and darned if people don’t differ in how fast they comprehend, make these decisions, and answer the questions.

And it turns out there’s a mild bias in using the relationship of “above” as primary whereas to understand “below,” people represent it as “not above,” and you hypothesize that’s the way the propositions get set up when people look at this picture. So when subjects are asked to verify a sentence like, “Is the box below the ball?,” they have to translate the query sentence into a form to compare to their encoding of the picture and see if they match. That translation and comparison takes more time if people represent “below” as “not above.”

While those are very simple experiments, they have important implications about how pictures are encoded and how they are related to sentences about the picture. That’s very important for figuring out how a young child learns syntax and how he learns the meaning of concepts and the reference of those concepts. That is, the reference of a concept is often depicted as a picture of what it looks like. But what we know about bottle as a concept can be far beyond its reference or appearance. So the difference between a concept and its reference was a distinction that John and I carried over into our HAM theory.

We were able to talk about these matters in a way in which people working in human memory had not done before. The old traditionalists would say, “We’ll deal with language later. We’ll deal with perception later. All we’re going to do now is experiment with memory for a list of unrelated words or nonsense syllables.” And after a while, that restriction turned out to
produce results that were not very interesting to other experimenters. So other experimenters stopped paying much attention to the field called “verbal learning”. “You guys just study memory for a list of unrelated words. Who gives a crap? Tell me about language. How do you remember a story? A text? A conversation? How do you remember historical facts? How do you remember something from the newspaper you just read?” And so on.

So I thought our book, *Human Associative Memory*, would be a major game changer, a major way for changing the game that experimental memory researchers were doing. Now, there were things wrong with that theory in *Human Associative Memory*. For one thing, we didn’t have a very rich syntax parser. Language understanding is bloody hard, and people struggled with it for some forty years before they eventually got to Siri, this little dinky microprocessor in your iPhone that can analyze and comprehend language fairly well

Back in the sixties, we didn’t know how to do that well. We had ideas about how to do it, but they needed a computer as big as this room in order just to barely understand a spoken sentence.

**Hartwig:** So who were you working with or who influenced you in terms of computer science and AI? You worked closely with the department here or--

**Bower:** [00:45:46] Yes and no. I got involved in computer science and artificial intelligence in a summer institute I went to at Rand Corporation in Santa Monica. That summer institute was taught by Herb Simon and Allen Newell and by some of their students. They were telling us about their early explorations of computer programs that would simulate some cognitive
behaviors—that is, the programs had output behaviors that were similar to interesting psychological phenomenon arising when you give a problem to somebody to solve and you look at the kind of responses they give you.

I think Al Newell and Simon were at that time working primarily on reasoning and problem solving. For example, they were concerned with how do you program a computer that will solve problems in symbolic logic, such as “P implies Q, and P, therefore what follows?” “Q.” What’s the consequence of those two lines of a syllogism? And Newell and Simon and their students were working on various computer programs for doing logical reasoning, for doing mathematics. Some of their students were working on simulation programs for doing trigonometry, some for doing differential equations. You know, give an equation to solve, how do you understand the equation and how do you call up your knowledge of calculus to solve the equation? How can you program a computer to do it?

So during that six weeks in Santa Monica, I got to know Herb Simon and Al Newell quite well and came to really appreciate the potential for psychological theorizing in terms of this computational programming that they were teaching us to do.

We all had to do a project, I remember, in a computer language they were teaching us called IPL 5, Information Processing LISP 5, a horrible programming language, but they used it for writing their programs and they were teaching it to us. We all had to program a psychological process in IPL 5. So, following my interests, I did a little memory model in IPL 5 of a short-term memory and a long-term memory with items of information being
entered into the short-term memory, being processed and maybe getting into long-term memory before they were forgotten. I got that all programmed up in IPL 5 and laid out four or five different variations of that model and even fit some data with one of them. But it died there in Santa Monica, never went anywhere, because the model was so simple you could do mathematics on it. Didn’t need a computer to grind out its implications.

The thing about computer simulation is it was a way of mechanically grinding out implications of a collection of starting assumptions, and you could set the program to go with some input, and it would run off its calculations or miniature decisions, and come up with an answer of some kind. You had to have each of the steps in your theoretical program well specified or the thing would just crash and say, “No, these assumptions are contradictory or so ill specified, that I can’t compute any farther.” So computer simulation was a therapeutic treatment for getting people to think very deeply about what pieces and what mechanisms and processes have to be put together to convert an input into an acceptable behavioral output. So it’s a very good tool for forcing theorists to think explicitly about things, and I took that to heart when I returned to Stanford. And it’s useful for cases in which the phenomena you’re dealing with are too darned complicated to push through a mathematical formulation. You’d think, what people are doing here is too complicated, there’s too many variables going on, so I can’t make a simple mathematical model of their thought processes or their behavior. Then you have to revert to a computer simulation model.
So it was after that that my students and I got into computer simulation more, thanks to Newell and Simon. One of the first computer simulation projects that I was associated with, was involved with, was with one of my students, Doug Hintzman. Doug and I were interested in paired associate learning at the time. And Ed Feigenbaum, who ended up being a computer science professor here, became a very famous guy, was one of the students of Newell and Simon, and he gave a talk at this summer institute that I heard there. I got to know Ed pretty well. At that time, he was up at Berkeley. He had developed this program for doing paired associate learning, had written up a simulation of it called EPAM, Elementary Perceiver and Memorizer, EPAM. Ed and Herb Simon had developed EPAM in a relatively rudimentary form, but, nonetheless, it would do paired associated learning, except it was deterministic, not probabilistic, and so they never could fit data. Besides, they were not verbal learning psychologists like I was.

So Doug Hintzman and I, mainly Doug, said, “Let’s formulate EPAM as a really simple theory, so that what subjects learn is a sorting tree of binary relationships developing into a hierarchy.” The model would learn paired associates like nonsense syllable items to be associated to a digit, so, “CHP is paired with the numeral three” and “HDL goes to the numeral seven” and so on. The model applied to the data of subjects learning lists of such paired associates.

Here’s how it worked: Our little system looks at the stimulus like CHP and might first ask, ‘What’s its first letter?’” If it is “C,” you move the processor down one branch and store “C__ is 7” at its end. And if it isn’t
“C,” then you move it down the alternate branch. Okay, if it isn’t “C,” the program then asks, “Okay, is it H?” If it is, you go down the left branch. If it isn’t, you go down the right branch. When the system reaches the end branch, it then looks at the first letter and stores that letter and its associated digit, e.g., “7,” so you’ve begun to classify the stimulus.

After finishing the first trial, the program will have a rather small sorting tier it can use to classify and respond to many of the stimulus items.

So that you can see how one slowly can build up a network of associations between what’s the first letter, what’s the second letter, what’s the third letter, and so on. And once you’ve sorted a stimulus to an end terminal, you attach a digit response at that mode. The program sorts the stimulus and then gives the response it has attached to that stimulus.

The program is a bit more complicated than what I’ve tried to describe here. The program builds up what’s called a discrimination net over successive trials of seeing this list of nonsense syllables paired with digits. Doug and I would take that model and use it to explain almost every significant finding in the verbal learning literature that was extant at that time, which was voluminous, but also understandable. For example, we were able to show that the more similar are the stimulus items—that is, the more they share letters with one another but depend on the configuration in which the letters occur—the more similar are the stimuli, the more often the program confuses items, so the harder that list it is to learn. The more similar are items in a given list, the more a second list will interfere and upset the learning you’ve done with the first list of items.
If you assume that you can have a push-down stack of responses at the base of this stimulus discrimination net, the model can deal with learning of multiple lists of the same stimuli so that in the first list “three” goes with “CHP,” in the second list the same stimulus is to be associated with “five,” and in the third list the same stimulus goes to “nine.” That creates a push-down stack; the program can use that stack to remember what was the first associated response, what was second, what was third. This system responds okay, but if you allow the push-down stack to be perturbed by intervening events, then the system produces many inter-list intrusion errors and interference errors.

I can’t remember all of the findings Doug dealt with, but he explained a whole laundry list of relevant phenomena in the verbal learning literature using that little discrimination net learning model. Doug called the model SAL, Stimulus and Association Learner, and published it. He got the Dissertation Award that year from the American Psychological Association. APA used to have these awards for the best dissertation of the year being submitted to the committee, and I submitted Doug’s. He got the prize. Unfortunately, there wasn’t any money for him attached to the prize.

Anyway, that was one of the first computer simulation models I was affiliated with, with my student Doug Hintzman, who’s gone on and done wonderful research in his career.

**Hartwig:** Did you do a lot of computer simulation?

**Bower:** [00:58:48] I didn’t.

**Hartwig:** Students or--
But my students did. John Anderson, who did the book with me, or I did the book with him, carried on big time into computer simulation and developed our HAM theory in very many ways. One of the things John and I hadn’t done was say how do you get this big associative structure to move and do things, such as to draw inferences or to put together several facts in a row to come to further conclusions. That is, how do you get the program to reason? How do you get it to do things like put a goal into its memory, and plan an action to achieve that goal? The goal might be something like “Memorize this list of words and recall them.” The associative network in HAM needs some way to represent goals and carry out a long sequence of actions to achieve that goal.

So what John did was to develop the notion of goals that sit in a short-term memory structure. He also developed the idea of productions, which are like little steps in a program, which effectively move the program towards a goal. For example, a specific goal in my working memory might say, “Call up this step and do that.”

What productions do is to move you along in a sequence. For example, suppose I want to start here and walk four blocks. I set a counter that says blocks walked so far is zero. So I walk, step, step, till I get to an intersection. When I get to the intersection, I stop, look both ways. And if no cars are coming, cross the street. If something’s coming, wait, and then look both ways again, and if nothing’s coming, cross. Update your counter. You’ve now walked one block.
To continue, now what is the goal? Walk four blocks. So walk another block, get to the intersection. Stop, look both ways, and cross, and continue till you’ve walked four blocks. That’s a trivial example of how you would write a program that does things like walk x blocks and not get killed in the process.

Each of those steps in that program is called a production. Productions are things that take in an input, a stimulus, if you like, or look up in short-term memory that you’ve got a stimulus, and then the production takes some action. They’re like stimulus-response pairs, except they’re much more powerful than a stimulus-response bond. If such and so is true, then do X. The condition can refer to what’s in short-term memory. If this structure is active in short-term memory, then do so and so.

Anyway, productions underlie computer programming. Each instruction in a computer program is a production. So productions you can think of as habits, although very flexible habits. These habits can vary in their strength if they’ve been practiced and used a lot. So the theory postulates a learning algorithm for learning productions, learning how to go from here to there, and here to there.

What John did in his subsequent work was develop goal structures and production systems that could do various interesting things, psychologically speaking, and thereby start to get closer to a very objective scientific explanation for various behavior regularities. One of the directions in which he took it was towards education via tutoring. You start with a model of what a kid knows as he learns, say, algebra. Algebra itself is a collection of
rules, and there’s a collection of procedures that a knowledgeable person uses to learn algebra and to use it to solve equations. So if I ask you “4x plus 8 equals 16,” what’s x? Well, one of the rules is you can take any number and add or subtract it from both sides of the equation.

So one of the heuristics you learn is to collect all the constants on one side of the equation. So 4x plus 8 equals 16. Get rid of that 8 and leave only 4x on the left. How do I do that? “So 4x plus 8” is sitting in my short-term memory, and it calls up a rule, “Get rid of the 8 on the left side,” and so next step is add minus-8 to both sides, and now I get 4x equals 8. Seeing that, I have another rule in algebra which says I can divide both sides by the same number, so I divide 4x by 4 and 8 by 4, and I get 2 here and x here, so I have “x equals 2.” Problem solved.

What the program does is invoke a series of productions. The first production creates an intermediate result, and that intermediate result evokes another production, and that invokes another production, and eventually, you get to the end and you’ve achieved your goal. That, in general, is the way a person should be solving algebra problems, making use of rules of algebra, like you can divide each side of an equation by the same number.

There are various heuristics that people learn to use; that is, rules of thumb that help them get to the answer quickly. Like the first thing you do with a long equation is distribute it; that is, multiply out all the terms and collect all the constant terms together that modify x and collect all the remaining constants together on one side, and so on.
John programmed a set of those heuristics into a computer program that solves algebra problems, so to speak, the ideal learner knowledge of algebra. And then you can use the ideal program to teach a kid algebra. You can set that kid to work on various problems. You observe what he does as he types his intermediate results and answer. The computer program sees what steps the student is taking since he types it into a typewriter. The program might reply, “Okay, you’ve screwed up here. You forgot to do so-and-so.” It can give the kid feedback about what he’s done, and then the kid can try again.

What the computer program is doing is watching the kid, his answers, and comparing it to the ideal, and giving feedback to the kid about where he is screwing up, and in that way, the program is tutoring the kid. Each kid has his own individual tutor for getting into high school algebra. And you keep track of the program of different students. When they come back tomorrow, you start off them at the place they left off, maybe with a little review, and then take them farther through the lessons. That’s the way tutoring programs work. The good thing about John’s tutoring programs is he has a model of the kids’ knowledge, so he has a model of what different students know so far.

**Hartwig:** How was that created?

**Bower:** The program keeps track of the errors the kid has made and what he’s learned up to that point. So you know what he knows, and his performance is to the ideal model, so that you can say, “Okay, Daniel’s doing
well up to this point. Bill needs work back here because he still makes occasional errors on using this rule.”

So anyway, John developed his algebra tutoring program to such an extent that the Pittsburgh school system uses it. He’s at Carnegie Mellon University in Pittsburgh, and he put it in the high schools in Pittsburgh. John also developed programs for teaching plane geometry and solid geometry. His students and other people working with him developed programs for other topics—differential equations and trigonometry and so on. I don’t know that they were quite as scrupulous as John was in modeling the student, but, nonetheless, the schools could use them as tutoring programs.

Pat Suppes and Dick Atkinson at Stanford developed computer models for tutoring spelling and reading and elementary mathematics. They didn’t have a model of the student so much as John did but they kept track of where has the kid got so far in math. Does he know 2 plus 3, does he know 2 plus 4, and so on. Their programs provided students with drill and practice on mathematics, reading, and the like. They developed an entire curriculum for elementary mathematics and elementary reading.

Bower: [01:11:56] John Anderson has developed the most comprehensive, thorough-going, explicit model of the human cognitive system that’s ever been devised. It’s called ACT, Adaptive Control of Thought. It has in it almost every component of cognition we know about. The theory is very explicit, detailed and accurate in predicting results of many different experiments. In addition, it operates successfully across a wide variety of areas of cognitive psychology,
not only reasoning and problem-solving, but also learning, memory, and certain aspects of perception and performance and attention.

More recently, he has been extending that computer simulation model so that it is being coordinated with brain structures that are being activated as a person engages different parts of, say, a problem-solving routine. So John can look at FMRI data, and be able to say, well, during this initial part of a long algebra problem, the student is just seeing and taking in the program, and so this part of the visual cortex is going to be lighting up in that first second. And then in the next part of the problem, he’s going to do some collecting of terms and moving around constants, so the subject has to go into memory and call up that heuristic production. So during this part some specific areas of his brain ought to be lighting up at that point. Then the next part of solving the problem, the subject will be using other algorithms to move along towards solution, and so this other area of his brain ought to be lighting up now. What John’s been able to do is to show those correspondences between cognitive acts and brain area action in detail that no one else has even thought of doing. I’m very impressed with it all, and I’ve told him so.

So he and I still talk a fair amount, and I promote his work whenever I get a chance because I’m so proud of him. He was elected to the National Academy very early and has done exceedingly well. He’s now one of the probably top ten people in psychology in terms of his theoretical power, and he’s far exceeded me, and I’m very happy about that.

Anyway, I’ve even used productions to talk about neurotic thinking.
Bower: [01:15:57] I was giving a lecture at the American Association for Behavior Therapists, and I was explaining concepts of cognitive psychology that should be relevant to these behavior therapists. I was also trying to get them moving more into cognitive therapy rather than behavioral therapy. And I said, “You know, you people are already using productions. You just don’t know it.” Aaron Beck and other therapists believe that people have habits of thought that goes from thinking one thing to thinking another thing. For example, I learn that I just bounced a check. That leads me to thinking, “I don’t have any money in the bank. That means I am poor and a failure. That means nobody wants me anymore. That makes me sad and I might as well be dead,” and so on.

Aaron Beck and other therapists have identified and laid out these rules of thought that a neurotic evokes to go from, say, one bad thing happening to him which sets off a chain of negative inferences until he arrives at “I ought to kill myself.” It’s a chain of associations that is using these productions. And I illustrated how such maladaptive chains of inference could happen with a depressed person or an anxious person.

Hartwig: Well, this fits right into your work on emotional influences on cognition, does it not?


Hartwig: Do you want to talk about that?

Bower: [01:17:56] I got into that because I was very fascinated with a phenomena called drug-state-dependent memory, whereby you could teach an animal one
habit or conditioned reflex, let’s say, when he’s in a normal state, and you could teach him a second, conflicting habit when he’s in a drugged state. Let’s say a rat is filled up with alcohol, although he still can move. So he learns the two conflicting habits: one that operates when he’s awake and the other that operates when he’s drunk. And the awake habit, he doesn’t have access to when he’s drunk. And the drunk habit, he doesn’t have access to when he is awake. They are dissociated. They are like two different dogs in the same brain, and the switch is the drug state.

The way this would typically be studied would be, say, teaching a rat who’s hungry that when he is sober, he goes left to get food, and when he’s got a lot of alcohol in him, he goes right in a T-maze to get food. You put him in the T-maze, and the choice he makes depends on whether he’s drunk or sober, and there’s not a lot of crosstalk or interference between these two habits. That phenomenon, called drug-state-dependent memory, has been shown for a large number of psychoactive drugs: marijuana, heroin, cocaine, scopolamine, methamphetamine, amphetamine, and other drugs like that. I was always fascinated by that dissociation. It’s as though different parts of the brain get engaged when you’re drunk or when you’re sober, or when you’ve got amphetamine in you versus when you are sober.

And there’s a lot known about drug-state dependence. For example, the greater the dose of heroin you give to a rat, the easier it is to teach him these two conflicting habits. It’s as though you’ve got to make a really big change in the physiological state to get this clear dissociation. Makes sense. So I wondered, “Gee, I wonder if we could get something like that with
different intense emotional states, something like drug-state-dependent memory.” And I got thinking and wondering, “Well, maybe we could do this by inducing strong emotional states in people using hypnosis.”

I learned hypnosis from a Stanford colleague named Jack Hilgard, who was the world’s number-one hypnosis researcher at the time. Jack and I were good, good friends. In fact, he and I wrote a book together called *Theories of Learning*; it went through several revisions.

Anyway, Jack had taught me how to do hypnosis with Stanford students, and I was interested in various aspects of hypnosis. One was whether or not you could produce genuine hallucinations or genuine deafness or genuine blindness. By “genuine,” I meant that you could change the brain response to a click or to a flash of light. Well, I’m getting off the track here.

**Hartwig:** That’s okay.

**Bower:** [01:23:20] Well, it was fascinating work.

**Hartwig:** How long were you exploring hypnosis?

**Bower:** [01:23:34] Oh, sporadically for several years. A friend of mine named Karl Pribram was here, and he had a new big machine called a computer for averaging transients that measured the evoked response in the brain to a discrete stimulus. You could put electrodes on somebody, say, the back of their head above the visual cortex, and you flash a light at them. And every time you flash a light, you start the computer recording the evoked electrical responses of the visual cortex, except you’re recording them from the top of the skull so they’re kind of noisy and weak. But you have a repetitive flash,
and over time you accumulate and average the responses together. By pooling together the weak responses evoked by multiple flashes, you get a pretty stable average of what the response evoked in the visual cortex from that flash of light. You can do the same thing with auditory clicks or with other stimuli like touches, and you get measure responses from different parts of the brain.

What I wondered is whether you could modify the evoked response in the brain with hypnotic suggestions, if you instructed hypnotic subjects to act blind or deaf. So we got a couple of good subjects, and fastened electrodes on their scalps. I would hypnotize them and tell them they’re blind until I woke them up later from the hypnosis. And then they’d stare at a spot, and although you’re blind, you can keep your head fixed and your eyes fixed on that spot. Then I start flashing a light, going flash, flash, flash. And we’re measuring their average evoked response in the visual cortex, or in the auditory cortex as a control since you wouldn’t expect much activity in the auditory cortex to visual flashing. You ought to only get flash responses in the visual responses.

Then we’d say to the hypnotic subject, “Okay, now you’re deaf. Now you can’t hear anything. You can see okay, but now you’re deaf. You can’t hear anything.” And we’d present an auditory click, click, click to the subjects’ earphones. And you look at the evoked response in the auditory cortex and make sure that the clicks are not causing evoked responses in the visual cortex. That’s the control.

**Hartwig:** So what did you find?
Bower: [01:27:08] The first guy we ran on this, first subject, gave us beautiful data.

The evoked responses were greatly diminished to the flash when we told him he was blind and to the click when we told him he was deaf. I thought, “Wow. This is great.”

So we ran another subject, and he didn’t show the same reductions. So we ran another subject, and he showed the reduction somewhat for vision but not for hearing. We ran another subject, and he didn’t show any changes. We ran another subject. He showed a little bit for hearing but not for vision. And I thought, “I don’t understand what’s going on. I think I’m going to stop this because I don’t see any regularity or consistency in what’s happening.”

Then I had second thoughts about the significance of the hoped-for demonstrations. Maybe what people are doing is just changing their peripheral receptors so that when I tell you you’re blind and I start flashing things, what you do is try to reduce the size of your pupil. Perhaps you look off to the side so the flash only hits you in the periphery of the retina. Or when I tell you you’re deaf, what you do is change the tension on your tympanic membrane, the eardrum, so that you don’t get such a big bang going through into the cochlea and up into the brain from each little click.

In fact, a friend of mine had already conditioned a tympanic membrane response to a light flash. He’d play a light flash and follow that with a hell of a loud noise, [demonstrates], and pretty soon, the response of the tympanic membrane to changes to the light flash. The membrane loosens up when he sees that flash so the person doesn’t get such a huge barrage of noise going
into his cochlea. So you can learn to change the tension of the muscle on the tympanic membrane with a visual stimulus. Wonderful.

I thought, okay, maybe something like that’s going on where our subject is reducing the evoked response in the auditory cortex, to a click when we tell him he can’t hear anything. So maybe that’s all it is, those peripheral tricks, and some subjects hit on those tricks to use, while other subjects don’t, but it’s not a central brain mechanism that is causing reductions in the evoked cortical responses.

I know that subsequently my friend Dave Spiegel returned to this phenomenon and succeeded in demonstrating the hypnotic reductions in a reliable way, and he didn’t think these peripheral explanations of muscles changing could explain the reductions in responses that he was observing.

Hartwig: Did you continue, then, hypnosis to study emotional states?

Bower: [01:30:40] Yes. So back to the topic here, emotion. I saw through work that Jack had done and just through reading the literature on hypnosis that hypnosis was a great way to induce emotional feelings in people. You can do it in a variety of ways. One is to get them to remember a specific time in their life when they felt very sad--like the death of their mother or their grandmother or their dog or something--and to go through that occasion very slowly and really get deeply into it emotionally. That’s one way to do it.

Another way is just to tell people you want them to get sad or to get happy or become frightened of something. Or you can describe to the subject a scenario in which your dog is injured and you eventually have to kill the dog because he’s broken his leg. You take him to the vet, and the vet has
to kill him, and the dog is looking at you forlornly. You can go on and on for several minutes and have the subject really get into it when he’s hypnotized, paying attention only to this scene and to vividly imagine it, visualize it happening. Subjects are asked to add their own details as to what the dog looks like and what the vet looks like and so on, so that they really get into it. And you say, “Okay, fine. Now I’d like you to memorize some words for me.”

**Hartwig:** Poor guy.

**Bower:** [01:32:40] And I leave you feeling sad, and I say, “Okay, that’s the end of that experiment. Thank you. Now we’re going to do a memory experiment. Remember the words ‘paper, globe, computer, chair, rug,’ etc.” Then once they hear twenty of these words, you ask them to recall them in any order they want. Subjects get out a certain level of recall, say, recalling 50 percent of the words.

Then, in the same session, you get subjects into an alternative mood, the opposite mood, now happy. You say, “I want you to get happy,” and do it by going over an event where you were happy. Or they listen to this happy music or watch this happy TV show or watch Robin Williams doing his standup comic routine. After doing that for five minutes or so, the subjects usually [fall] into a very elated, happy state. And then say, “Okay, that’s the end of the experiment. Thank you. Now I want you to learn another list of words.” So they learn a second list of words, one list happy, soon after they had learned a first list when sad.
Then you remove their mood, and kiss them goodbye. You bring the subjects back a couple hours later, and induce one or the other mood in them, say, sad. You do it by using another method to get them sad, so the method is not identical to what was used originally, but they get sad again. Perhaps they do it watching a sad movie or they do it by imagining a scene that you describe rather than remembering one of their scenes. So once the subject is sad again, you say, “Okay, now remember the word lists you learned earlier. Now remember the first list you learned today.” Later you ask, “Now remember the second list you learned today.”

What you find in that circumstance, or what we found in three different experiments, was that if they’re in the same emotional mood state when they are recalling, they do best at recalling the list they learned when they were in that mood state. So if they are sad at the time they’re recalling, they do best at remembering the word list they had learned when they were sad. And vice versa, if they’re happy, they remember best the list they learned when they were happy.

So what was important was not so much whether they’re feeling happy or sad when they’re recalling, but rather the matching between the way they felt when they learned and the way they feel at the time they’re recalling. So it was very much like the drug-state-dependent phenomenon. I learned sober and I tested drugged. When I learned two lists, one when sober, one when drugged, and I recalled both later when either sober or drugged. So the phenomenon was very good, very close to the drug-dependent effect.
The problem was I couldn’t always produce the result if all I did was to train people on one list in one mood state and then test them the next day either in the other state or in the same state. I didn’t get much when I did it that way, and I never quite understood it very well. What worked best was to teach them two lists, one sad and one happy, and then test them when they’re in one of those moods, happy or sad, and recalling both lists, and that produced the effect fairly reliably.

We then extended this work to other kinds of experiments. One was make people happy or sad and ask them to remember episodes from your childhood, any incidents you can recall from when you were little, say, in elementary school, and we would record what they recalled. And what we found is the subjects showed a strong bias towards remembering incidents that have the same emotional flavor as what they were feeling right now. So if they’re sad, they tend, like 75/25 percent, to remember incidents that are sad or that are unpleasant, memories that kind of support their sadness. Vice versa, if they’re happy when we ask them to recall, they tend to remember very pleasant, joyful, elated experiences from their childhood.

We got the same thing when we asked people to record in a daily diary a few events that happened to them each day, write a two-line description of who was involved and what happened and when did it happen and how did you feel on a plus-5 to minus-5 rating scale, pleasant to unpleasant, happy to sad. We asked subjects to do that every day, record events in a daily diary, for three weeks.
Then we had them come into the lab, and we would make half of them happy, half of them sad, and then ask, “Please remember now as many of those incidents you had recorded as you can. Just write brief descriptions so we can identify them.” And they would get out, say, 50 percent of the incidents that they had recalled.

And then we would ask, “How do you rate then now on a scale, happy, sad?” And what subjects who’d been made to feel sad now remembered selectively the sad incidents that they had recorded when they were sad…and those presumably had originally made them sad. And vice versa, our subjects who’d been made to feel happy remembered more pleasant incidents from their daily diary.

They also changed their current rating of the incidents they recalled, because we asked them, when you’re recalling, would you rate the incident you’re recalling. If they were happy and were recalling a happy incident, they gave it a very high positive rating like they had before. If a happy subject is now recalling a sad incident, he’d rate it a little happier than he had originally. So if they had originally rated it as a minus-4, they now rated it as a minus-2 rather than a minus-4, so the emotion attached to the memory of the incident shifted in the direction of their current feeling state.

But what was important was that people in a happy or sad mood tend to remember selectively incidents from the past few weeks that are either happy or sad in agreement with the mood you’re in. And that makes good sense because that’s mood-state-dependent memory. The results also help interpret a phenomenon that psychotherapists know called mood
congruence. If a patient comes in to be treated for depression, and in the course of your interview with him, you ask, “Tell me something about your childhood,” what patients come up with are memories of predominantly terribly bad events and very bad descriptions of people from their childhood. You get the impression that they’ve had a very deprived, depressing, upsetting, and frustrating childhood.

That led many therapists to say, “Wow, that lousy childhood. Who wouldn’t be depressed if I had a childhood like that? This just confirms my belief that childhood experiences make the man.” Except later when you’ve got the patients remitted out of their depression and feeling better, either with drugs or with psychotherapy and you ask them, “Would you recall your childhood now?” Now this second recall is much rosier, much happier. You say, “What happened to that lousy childhood?” [laughter] The answer is nothing happened; you changed the mood of the person recalling, so he calls up different events and gives them more positive evaluations.

A lot of behavioral therapists and psychotherapists got interested in mood-dependent memory, because it helped them explain some observations, and it helped them with doing treatment--

**Hartwig:** Absolutely.

**Bower:** [01:43:17] As part of their treatment, therapists would now ask their patients, “I want you to record what’s happening to you over the week, but I want you to record selectively cases when you were happy or when you showed competence and showed strength and did well and you felt very good about
some interaction or incident, I want you to record events. And when you come in to see me next week, we’ll go over those.”

And what that does is to prime and make more available the good things in a person’s life, even though he might say, “I didn’t have anything good happen.” Pretty soon, the therapist can look over the patient’s records and say, “Well, there’s an incident where you did well, there’s another one where you were happy, and there’s another one where somebody surprised you with a wonderful compliment.” In that manner the therapist changes the mental set of people to emphasize different kinds of experiences in their life, and that has a real therapeutic effect on them. Anyway, those are examples of state-dependent memory.

Another phenomenon we found was what we called mood-congruent influences; that is, when you get a person feeling happy or sad or angry or frightened, their emotion primes and makes available memories that they had poured into their brain over time in association with the emotion that they were feeling. Remember, in the human associative memory theory in HAM, you are always storing experiences as propositions. Previously it was simple association like that list contains the word “dog,” or “I thought of ‘table’ in the context of studying the list.” We now add the idea that you can associate the mood that you were in. So, for example, sadness becomes associated to each word on the list as does the list tag. Sadness is going to all those list items so when I turn on sadness along with instructions to recall all the list, those two associations converge on the list words. And that’s why you can get adding up of associative strength from the list cue and the mood. Thus,
the list cue plus the mood of sadness aid one another in producing recall of
the list words. On the other hand, if I make you happy and ask for recall of
the words you’d associated to sadness, your mood during recall does not
combine with the list cue to aid recall. You have to rely on what you can get
from the list cue alone.

Anyway, so the idea was that mood-congruent influences arise because
when I make you happy or make you sad, you activate all of the concepts
that are already associated to happy or sad, all of the themes affiliated with
happiness or sadness, all of your associations that are happy or sad. That
theory set us to doing many experiments showing many influences of
happiness and sadness on several kinds of cognitive processes.

Take as one example the determination of word associations. Suppose
you’re sad and I say, “Here’s a word. Tell me what word it reminds you of.
Lake.”

“Oh, drowning.”

“What?” [laughs]

“Well, my friend drowned in a lake,” or “I nearly drowned in a lake.”

“Basket.”

“Rotten apples. I remember once stuck my hand into a rotten apple in
a basket.”

So people give you associations that are biased in the direction of the
emotional state they’re in when they’re giving you free associations. You
haven’t told them to come up with a sad associate. They just do it that way.
Same thing happened when we asked people to tell us stories. We’d show them pictures from the thematic apperception test, a projective test. That test consists of these great photographs or drawings of people in significant life situations, like an older woman leaning over a young man, and he’s looking downcast and the woman’s looking over his shoulder.

We’d say, “Tell us what’s going on here. Give me a story. What went before this? What’s happening now? What’s going to come out?” And we surreptitiously make people happy or sad before that, and you get very congruent stories out of them. Subjects will weave a story about how the guy in the picture has just failed his bar exam and he’s not going to be able to make a living, and the mother is trying to console him, he’s so sad, and so on.

Those are themes that are affiliated with the emotion that subjects are feeling. When I look at a picture, that’s what comes out of my memory. The same happens when people judge their life. How well are things going in your life? How’s your marriage going? Do you like your job? How’s your car running? Tell us about your friendships. How’s your relationships, etc. People’s top-of-the-head evaluations are biased very strongly in the direction of the emotion that we’ve just instilled.

Joe Forgas and I did a lot of this evaluation work. Joe would have his assistants stand out in the lobby of a movie theater, and they’d catch people as they’re coming out from seeing a comedy or coming out from seeing a real tearjerker, a very sad movie. And you say, “Would you please help me a moment? I’m doing a research project for my honor’s thesis, and I need a few people to answer a little questionnaire about their life.” Or you’d ask,
“How long have you lived where you lived? Oh, you’re married. I see. How long have you been married? How happy is your marriage?” “How satisfied are you with the marriage?” You don’t launch into such sensitive issues right away, but after asking a few innocuous questions, you very slowly move to more sensitive questions, like, “How satisfied are you with your life, with your life’s achievements? If you had it to do over again, would you do something different?” Or, “How’s your marriage going? How’s your career going?” And people turn out to be quite biased by the movie they’ve just seen. It’s strange. They don’t associate the movie they’ve seen with their way of answering these questions. They think they’re just telling you like it is, except like it is depends on the temporary mood they’re in.

I should have mentioned how I started a research collaboration with Joe Forgas. The initial work I’d done was all on mood and memory, remembering incidents from the last two weeks, remembering several lists of words. Joe was a social psychologist who came to Stanford on a sabbatical leave from the University of Sydney, Australia. Joe got me interested in how moods might influence more social things, social influences, social attitudes, and social impression formation.

One of the first things Joe and I did was one of the more interesting ones we did. We looked at how your mood influenced the way you saw yourself performing in a videotape interview. It was a two-day experiment. A college student would come in and be interviewed, asking him many questions about campus life and how he liked his teachers or his opinion
about campus issues, I forget, but all kinds of questions about his ongoing college life. That was done on day one.

Day two, we bring the subjects back, we teach them a little bit about how to look at a videotape of an interview and to record positive or negative prosocial or antisocial acts of the character in the videotape. Every time they hear a little beep, they were to record something, every five to ten seconds, on a minus-5 to plus-5 scale rating the behavior of the person being interviewed.

Having taught them how to rate prosocial or antisocial behavior, we say, “Okay, here, we want you to listen to some music and tell us how you like the music and be able to talk about its quality and characteristics of the music and the musicians.” Then we’d play either very happy music or very sad music, like Mussorgsky’s “Russia Under the Yoke of the Moguls” played at half speed. [laughter] And they listened to that piece of music for five minutes.

Then we would say, “Let’s go on now to the next experiment. We would like you to rate yourself in this interview for prosocial or antisocial. Here’s your interview from yesterday.” And the video would last for ten minutes or so. “Remember, every time you hear a beep, rate your behavior on a plus-5 to minus-5 scale. Were you showing prosocial behavior or were you showing antisocial behavior or were your neutral, showing neither?”

Half the subjects were happy, and half the subjects were sad on this second day when they rated themselves. We observed this complete crossover interaction; namely, if they were happy, they saw lots of prosocial,
wonderful things they were doing. They were suave and debonair and answering questions competently. But if they were sad when they looked at themselves, they saw just the reverse. They saw themselves as antisocial, as avoiding eye contact with the interviewer, or they were being hostile and uncooperative, and so on.

So we observed this immense difference in ratings of that interview and the specific acts in the interview, depending upon the person’s mood while rating. And of course, it’s the same video; if you have independent judges rate the behavior of the interviewers, they see a little bit of each one. In contrast, people rating themselves when they’re sad or happy see primarily bad or good things in themselves, even when they’re looking at themselves videotaped just the day before.

So that struck me as interesting and powerful as well; it is as though the good or bad behavior is in the eye of the beholder. That’s the phrase I used, “the eye of the beholder” or the mood of the judging eyes. Social behavior is like a Rorschach card. It is a blob upon which we project our ways of thinking and our ways of categorizing behavior, and that projection depends in part upon our mood state. We were not the first researchers to say a given bit of behavior could be seen in two different lights, depending upon the attitude you start with or when your goal is to get evidence for or against this person. But I thought it was neat that we could show the effect with a person judging himself rather than another person, although it also works when people rate other people.
[End of Session Three]
Interview Session Four
September 10, 2014

Hartwig: Today is September 10th [2014]. This is Daniel Hartwig with Gordon Bower. This is our fourth session of his oral history. Good morning.

Bower: [00:00:09] Good morning.

Hartwig: So, last session we were talking about your research, and we were about up to your work on narrative memory and mental models. Do you want to explain that now?

Bower: [00:00:21] Yes. I was doing work in human memory, of course, and one of the problems that was confronting that field was that we were getting a tremendous amount of information about how people remember things like lists of words or lists of sentences or collections of unrelated pictures etc. And the reason for doing that was it was relatively easy to score recall by essentially counting the number of words somebody was remembering and so the scoring problem is easy.

The field postponed getting into studies of memory for coherent text or movies or videos because in a sense we did not know how to score recall data accurately when people were recounting stories, when they would be dropping out parts of it or paraphrasing the story, rearranging the order of mentioning items, combining pieces of information into their gist, summarizing things, making digressive comments on what they had read, etc., etc. And experimenters would look at that big mess and say, “God, I don’t know how to score any of this.” That partly was what held up a lot of research into studying memory for text and the like.
But around late 1960s and early 1970s, several of us decided we would grab the bull by the horns and just start looking at the way people recall simple stories. The operative word here is simple stories: folktales, elementary narratives that are short and understandable. And it turned out the scoring issue was not nearly as difficult nor as complicated as people had always assumed it would be. Without too much effort, you can record what people recall in terms of basic or atomic propositions and examine to what extent the elementary propositions in the text show up in the subjects’ later recall of it.

And as soon as we started doing that, it turned out it was not so hard to score people’s recall of text. The scoring issue could also be circumvented if you tested subjects for recognition of specific facts. You make up a lot of questions to ask the subject, such as asking, “Was this fact explicitly stated in the text? Was this exact verbatim sentence in the text? Was this topic discussed in the text? Yes or no?” and you measure memory accuracy that way.

So in the late sixties, early seventies, we began doing studies of text recall. In the beginning, a number of us decided to study elementary simple folktales. Why? Because much of what people recall from a text or a story they’ve read depends upon their background knowledge relevant to the topic of the text. We tended to stay away from studies of subjects’ recall of technical subjects like Middle-Eastern history, or engineering or biology, or physics or microeconomics. Why did we not work with such texts? Because different subjects recall widely differing amounts.
What you find out very quickly, of course, is that what a person can recall depends enormously upon their individual knowledge and familiarity with the topic that the text is about. Therefore, and if you want to study recall of, say, thirty people for an experiment for a passage, you try to make sure they have roughly the same level of familiarity and knowledge about the passage and about the concepts and ideas involved in it. Otherwise, you will observe rather marked individual differences that will overwhelm whatever text variable you intended to study. For doing experiments, you prefer that your subjects are “homogeneous” or roughly the same relative to the topic of your interest.

So one of the first reasons for studying memory for simple folktales and simple narratives is that understanding those depend very much upon people’s knowledge of folk psychology, common sense psychology that everybody learns from age five, about how to interpret people’s action, that people do things in order to gain a reward or avoid a punishment, that there’s reciprocity rules in human relations. That’s the principle that “You scratch my back, I'll scratch your back,” or, “I will give you what I expect you to give me in return.”

People understand those elementary principles of folk psychology, and the plots of folktales usually revolve around such conflicts. So when you give a folktale, Hansel and Gretel stories or whatever, to college students, they understand immediately what it’s about, and so there’s considerable homogeneity in the subject population in understanding stories of that kind. So that’s one reason to do it.
The other reason is that with simple stories, you can write them yourself and you can manipulate the way in which pieces of the story fit together or the order in which sentences occur, or the complexity or concreteness of the language in the story. You can write stories in which the texts are the same except for one or two sentences that different people read, so you can examine the way in which their inferences are altered depending upon which of those two sentences they read.

So that’s the kind of thing that an experimental psychologist likes to have, control over the material, so he or she can isolate and examine the consequences of particular aspects of the stimulus material, in this case the crucial sentences in the text or story that the person is reading. And that ability to control the stimulus helps a lot when you’re doing experiments.

Okay. And, of course, there’s a good reason for using narrative stories with subjects in experiments—because they’re interested in them and they understand them. They’re used to reading stories and summarizing stories and recalling and evaluating them. So in some sense, you’re tapping into things that people do routinely in their daily life. In fact, you can study almost every important issue in cognitive psychology in the context of people understanding stories, recalling stories, drawing inferences from stories, or reasoning from the information they’ve been given in stories.

So, in one sense, it’s a little microcosm or test bed in which almost every topic in cognitive psychology can be studied. At least that’s what I thought, and I think other people thought the same way, and so a group of us, four or five psychologists around the globe, got going.
Hartwig: Who are some of these other individuals?

Bower: One of my students, David Rumelhart, was one of them. He had moved at that time down to UC San Diego; Walter Kintsch, a fellow at the University of Colorado, who had spent a postdoctoral year here with me; Roger Schank, who had been an assistant professor here in computer science, artificial intelligence. I got to know Roger quite well, and he was moving lots of his theorizing into stories. He was at Yale at that time, and he worked with another Yale psychologist, Bob Abelson. I knew Abelson quite well since he was one of my teachers at Yale.

So Schank and Abelson were very strong in the story-understanding game. Others were Dave Rumelhart, Walter Kintsch, Art Graesser, and then other researchers got involved as we went along. One of my students, Perry Thorndike, was one of the very first people working with me on the topic; John Black was another, and these people continued with good careers in psychology.

So, what research did we do? How can I break this down? One of the first things we looked at was what are called story grammars. The notion is that simple stories are like problem-solving stories where there’s a single protagonist who’s given a problem he has to solve, like catch the cattle rustlers if you’re the sheriff, or solve this murder if you’re the homicide detective, or get the cows to market if you’re a rancher.

And it turns out these stories have a very simple structure, and good stories follow this structure. For example, they introduce a set of characters in a setting in which they’re going to perform, and then along comes a
problem or some event that creates a problem for the main character, and
then he sets off to try to solve that problem by his actions, and those actions
might generate another problem for him, and that might generate another
problem, and then he tries to carry out effective actions to solve the string of
problems.

You find out very readily that by virtue of having heard hundreds of
stories in their life, people have this mental structure--we called it a story
schema or parts of a story grammar--regarding what a good story should
have to be coherent. And you can think that as people are hearing a story or
reading a story, they call up this framework or this schema that they can
impose upon what they’re hearing or reading, so they can say, “Yes, yes,
we’ve got to have a character. Okay. Dan is going to be the main sheriff, he’s
a character in the story, and sheriffs have routine collections of duties that
come with their role, and that is to keep the peace and catch bad guys and
robbers.” So then up comes the first event. Black Jack comes in and steals a
bunch of cattle, all right? So we know that’s going to set up a goal for Daniel,
the sheriff.

And so you can just sort of see people understanding these elementary
things. You can almost list out what are the goals that a character of that kind
is going to have and that that will be presented to him in, say, an old-
fashioned western story or the like. You can easily write those kind of simple
stories, that are about ten lines long, and you can vary how coherent they are-
-that is, what an event leads to, or what wasn’t explained, or the extent to
which the setting is important or not to later events in the story.
We were especially looking at what people recall after reading such stories, and you get a variety of elementary findings. One is that there’s an enormous difference, of course, between people remembering a coherent story as opposed to trying to remember the same sentences except they’re just all scrambled up in order, so there’s no “coherence” to the sequence of sentences. And what does coherence mean in that circumstance? It means that you read things in an order in which concepts are slowly introduced and then they are talked about for a while, then that concept or that person is kept in the foreground from one scene to the next to the next and so on. More than that, you know or can figure out what are the goals of the main character. Often the goals are stated or implied very early in their reading.

Some of the very early experiments we did simply removed the goal of the main character. There would be sentences about all these things that he did, but you could not infer why he was doing them, because we didn’t tell them. Moreover, the stories were written in such a manner that the subjects couldn’t infer why the heck the guy was doing these things. So what you find, of course, is that scrambling sentences really makes everything in the story fall apart. I mean, their comprehension and memory for it is very bad. It’s like just a series of descriptions—he did this, he did this, he did this, he did that, etc.—and so it’s almost worse than reading a series of unrelated sentences.

So we did a number of experiments in which we manipulated the structure of simple stories, leaving out the goal or putting it at the end and so on. Or we’d have subjects study stories that were very tightly knit, and if you
left out the goal, it became really hard. Let me give you an example. One of
the stories that we worked with was called “The Old Farmer and His
Donkey.” The farmer had a donkey that was very stubborn. One day he
wanted to get the donkey to go in the barn, so he pulled on the donkey’s
reins, and the donkey refused to budge. So then he pushed the donkey, but
he couldn’t push the donkey into the barn. So then he thought he’d get the
dog to bark at the donkey, to scare the donkey into the barn. So the farmer
went to the dog and said, “Would you please bark at the donkey?” [laughter]

And the dog said, “No, I refuse. I’m not going to bark.”

And so then the farmer thought to himself, “Well, I’ll get a cat to
scratch the dog so the dog will bark.” So he went to the cat and said, “Will
you scratch the dog for me to make it bark?”

And the cat said, “No, I would like you to get me some milk first.”

And so the farmer said, “Okay.” And he went to the cow and asked the
cow, “Could you give me some milk?”

And the cow said, “No, I want you to give me some hay first.”

So the farmer went and got the hay and gave the hay to the cow, and
the cow said, “Thank you,” and gave him some milk. And the farmer took
the milk and gave it to the cat, and the cat was happy, so he did as the farmer
asked and scratched the dog, and the dog was hurt so he barked, and the
donkey jumped into the barn. End of story. [laughter] That’s what we call a
very tightly-knit series of goals and sub goals. You can almost hear them
coming in and getting lined up almost like a line of standing dominos. And
when the farmer knocks over the first goal by giving the hay to the cow, then
he can do the next, then the next, and so on until he gets the donkey into the shed.

Now, that story becomes pretty incomprehensible if you scramble up the story lines or remove the goal. People will fail to remember whole subparts of the story or will get things out of order and so on. So that’s an illustration of a story that can be screwed up easily—in which all you have to do is remove a goal or put it in a non-optimal location in the story, and comprehension and memory fall apart very rapidly.

We did a whole series of experiments of that general kind, and we were able to identify the differing levels of importance of information in a story, going from very general statements about characters, situations and plot structures, and then moving down into more and more specific detail. And we were able to show that the level of detail that people can recall depends very much upon those details being causally connected to items that went before them in the story.

Okay. Let’s move on. It turned out that the story-grammar approach is very limited in what things you can study with it. That’s because what’s really important in stories is the content, the meaning of the story, rather than the abstract syntax of the story. It’s the content that sells stories to Hollywood or to publishers, and they want to know far more than the abstract structure of the story. So you have to look closely at what makes content interesting as well as how do people knit together the different parts of a story.

One of the problems with story grammars is that they dealt only with the surface structure of the text as what was explicitly stated or written in the
text. But it turns out that what’s important isn’t that syntax, isn’t always what’s stated or what is written in the text, but, rather, what the person thinks about as he reads and what he infers about what’s going on in the story. So if you really want to get into understanding what people are doing when they comprehend, you have to look at the inferences people are making as they’re reading or hearing a text.

So then the question becomes, how do you measure or look at inferences readers make while reading, and there’s a collection of methods you can use for that purpose. One way that inferences occur is by the reader using his knowledge of human goals and plans and actions undertaken in the service of such plans and goals. The way a reader will understand an action is in terms of what is the implied plan and goal that that action is satisfying, and if you figure that out, then you’re kind of dumbfounded.

So we did a bunch of experiments in which subjects would read about a goal a character has and then an action that he performs, and we would sort of ask, can readers fill in the gap inferentially that says why that action was performed to achieve that goal? I will give you a simple illustration. Let’s say, “Daniel was hungry, and so he ate a pizza.” That action doesn’t require much inference to see how it satisfies his goal. On the other hand, if I say something like, “Daniel was hungry. He got out the phone book and looked up Domino’s number.” Now, there, “Why would he get a phone book?” “To look up a phone number.” “Why would he look up a phone number?”
“Well, I guess he’s going to call Domino’s; Domino’s sells and delivers pizzas, and Dan intends to order a pizza from Domino’s, so when the pizza is delivered, he’ll eat it.” Blah, blah, blah.

So just from that example you can appreciate what cognitive acts we were investigating. People understand goal-to-action steps like that by filling in the inferential gaps, using their knowledge about standard plans that most people in our society know.

So we did several experiments in which we would basically ask people, “Do you understand that action in light of what goals he has?” The method was to introduce a character, assign a goal to him, and then say that he performed some specific act. “Does that act make any sense, given what you know about him? Or does the act not satisfy the goal, is totally off the wall?” For example, “Daniel was hungry, so he took up the carpet.” That act is unrelated to the goal and is far-fetched, as contrasted to having him call up Domino’s. So subjects should reject that as a reasonable thing to do.

We were able to show that the time people take increases with the number of hypothetical steps of reasoning they have to make inferentially in order to link up the goal and action, so they were able to say, “Yes, I can dig that. I understand why he’d do that.” So we completed several experiments of that kind.

The other kind we did varied how many goals we’d assign to a given character. He’s hungry and he wants to exercise, so he has to decide whether to go to the cafeteria or the gym. We would then state an action that the character performs that can satisfy some of these goals or none of them. We
found that the more independent goals that a character has, the longer it takes people to understand any given action. It’s as though they have to consider one goal after another until they find one that satisfies the goal, and it takes more time to do so the more goals there are to consider. On the other hand, you can select actions that satisfy two or three of the goals together, in which case you respond quickly since the act satisfies whichever of the character’s goals you first consider. So we did experiments of that kind.

Let’s consider another set of our experiments. In some we looked at the consequences of the point of view that a reader takes as he reads a given text. We know that what people will pick up as they read a text depends upon the interests they have in different parts of it. Suppose I have different people read a story about the national budget coming out of Congress. Then we know that academics will care about and focus on how much research money is going to NIMH and NSF. Farmers are going to care about what’s in the budget about agriculture policy and farming. Artists are going to care about whether there’s any money in the budget for PBS and the National Endowment of the Arts. That is, what readers get out of reading the budget report depends very much upon the interests they have as they read it over.

But more than that gross effect of interest, your memory of a story also depends upon the particular perspective and point of view you adopt while reading. To show this, we did experiments in which subjects would read a common story but different subjects would read a short lead-in before that
common story. Different lead-ins were designed to induce subjects to adopt one or another perspective as they moved into reading this common story.

For example, the story might be about two guys meeting a female videographer at a boat dock on a lake and going out and shooting an advertising videotape of one of the guy’s waterskiing. One guy’s driving the boat and the other guy is being pulled behind on water skis and the girl is in the boat shooting video of the water skier. The story describes a number of events that happen as the boat drags the skier around the lake during the video shoot.

Before subjects read that common story, they read a brief paragraph setup. The setup is either about Harry, who is the long-time boyfriend of the videographer (named Cindy), or the lead-in is about Rich, the model hired to be the water-skier. Cindy is the video person who’s working for an advertising agency, and she’s going to be inside the boat videotaping Rich, the skier while her boyfriend, Harry, drives the boat around the lake. In one setup you begin with Harry getting up in the morning and going out to meet his girlfriend Cindy to do a video shoot at the lake. And you say some things about Harry and kind of get inside of his head. The other lead-in begins with a focus on Rich. Rich is relaxing in his swim suit in a lake-side bar when Cindy comes in looking for a hunky guy to hire as the skier for the ad. Cindy eyes the muscles of this rather good-looking guy, Rich. She hits on him and hires him to come along as the water skier model. The story makes clear that he goes along because he’s interested in her.
After reading one of those two set-ups, everybody reads the story that follows. The story describes a series of mishaps, like Rich letting go of the towrope or often falling off the skies, or Harry’s difficulties getting Rich back onto the towrope after the spills, and the near-accidents Harry has with other boats as he pulls Rich around the lake. Significantly, the story leaves ambiguous or vague regarding who, if anyone, is at fault in causing the mishaps and screw-ups.

After subjects finish reading the story, we wait a day, and have them return to the lab to recall the story. We also ask them many questions about what they remembered as having been stated in the story. We’d ask: Was this stated verbatim in the text? Was the gist of this implied in the story? We also asked about their private experiences while reading, such as: Who was the main character? Who did you identify with? Did you have any imagery while you were reading the story? Where were you stationed in your imagination as you read in the boat with Harry or on Rich’s shoulders on his skies? Or shooting video alongside Cindy?

What we you found, first of all, is people identify with the character who they had met in the lead-in to the main text--whether Rich the water skier, or Harry the boat-driving boyfriend. Secondly, the character they were identifying with determined the imagery they had while reading. So if they’re identifying with Rich the water skier, it is as though in their imagery they are perched on Rich’s shoulders looking at the boat as it pulls away. On the other hand, if they were identifying with Harry, their imagery was like they’re sitting on Harry’s shoulders looking back at Cindy at the back of the boat
and at the water skier. Most of our subjects’ identifications followed the lead-ins except for a few female subjects who identified with Cindy, the videographer. We were interested in those who identified with the water skier or those who identified with the boat driver.

Thirdly, if you asked them who’s the main character in the story, of course they say, “It’s my guy.”

“What was your guy like? Give me a personality description of him.” And they give you very positive, sympathetic descriptions of their character while they think the other guy is a schlemiel, clueless, inept, incompetent, or dull and bland.

And their memory of what was written in the text was very much colored by or filtered through their positive or negative evaluations of these two characters. They tend to remember good things about their character, things where he was said to be competent. For example, at one point in the story Rich falls off his skis into the water. He has to be picked up again, and Harry has trouble maneuvering the boat close enough so he can grab onto the towrope and get back up on his skis.

We would ask subjects about specific events, as, “Was this sentence written? Was this said?”

And people who identified with the water skier will say, “Yes, I recall Rich couldn’t get a hold of the towrope because this damn driver wasn’t good enough to bring it near him.”
Or vice versa, for subjects identifying with the boat driver, you’d say, “Yes, that guy Rich was so incompetent, he couldn’t even grab hold of a rope tow that practically hit him in the chest,” etc.

There were strong biases in what they remembered they had read, and their general belief was, “My guy is good and the other guy is no good. My guy rarely screws up, and if he does it’s because his job in that unfavorable environment is tough. On the other hand, the other guy screws up because he’s inept and incompetent.”

And this is a well-known principle in the social psychology of attribution. That is, you attribute your mistakes to the difficulty of the environment; you attribute your enemy’s mistakes to his flawed character, namely, that he has no ability to do anything. The bias is called the actor-observer difference in attribution style. It’s a well-known phenomenon. And we were showing the same kind of bias when people were recalling a common story. We often asked subjects to remember exactly what was stated in the story and, nonetheless, over time their memory of the events shifted in the direction of the actor-observer kind of biases. So that was kind of interesting.

That experiment shows that the principles people use in understanding other people and their social actions are used the same way when we understand stories about human actions, so the same biases show up with all their consequences. It also illustrates that people aren’t just remembering the surface sentences of a text. In fact, often they don’t remember very much
about the exact text. What they remember is the kind of dramatic scenario or constructions that they made from the text.

We did further experiments of this general kind, studying what we called the “soap-opera” effect. In these experiments we would have people read exceedingly bland, boring little vignettes about a girl or a guy getting up in the morning, coming down and having a rather routine breakfast, and then going to a college class, sitting through a lecture, and then waiting to talk to the professor after the lecture, then going to the grocery store, and then going home.

And again, you set it up with two different characters before you start. One character is Jack, who is on the college’s wrestling team and wants to build up his strength; that’s about all the few lines of the set-up text said about Jack. The other character that other subjects read about in their lead-in is Nancy, who wakes up feeling sick and she wonders whether it’s somehow related to the professor that she’s been seeing. And of course, undergraduates who read this will develop this full-blown scenario that she’s been having an affair with the professor, and that she’s pregnant or she’s worried that she might be pregnant. None of that was written in the text; that kind of scenario is just vaguely hinted at or triggered by these lead-in sentences.

Then all subjects read these rather boring stories about Nancy or Jack, they are getting up, having breakfast, going to class, wanting to talk to the professor but they were not able to, going to a doctor visit, going to the grocery store, then to a student party, and then home.
Again, you take similar measurements of memory. You wait a day or so
and then have subjects come back to the lab and try to recall everything they
can of the text. And further they were asked to judge whether or not
particular statements had been in the text.

And you find these enormous discrepancies in recall of the two groups
of subjects. Subjects who started with a lead-in about Jack the wrestler tend
to remember items of the text relevant to his needs, such as how big a
breakfast he ate, what kind of food he picked up at the grocery store, how
much weight the doctor told him he’d gained, and how many hors d’oeuvres
he ate at the party. In contrast, with the lead-in of Nancy, subjects assume
that she’s pregnant from her affair with the professor and distort their recall
in conformity with that script. For example, they would recall how little she
ate for breakfast because she had morning sickness, and that the professor of
the class she was attending was definitely the scoundrel who had knocked her
up; she tried to talk to him after class, but he pretended to be too busy to
talk, so he obviously was ignoring her. At the doctor’s office, she had some
unspecified tests and the doctor told her “Our expectations have been
confirmed.” So our subjects believed that she’d had a pregnancy test and that
the doctor told her that she was pregnant. And at the party she couldn’t get
him alone to talk to him about her predicament, so he was rejecting her
appeals for help.

Our subjects’ memories composed a complete soap opera out of their
recall of this small text about routine events, even though they were told,
“We want you to remember exactly what was written.” Remember the text
described deliberately boring episodes about getting up, eating breakfast, going to class, visiting the doctor, shopping at a grocery, and so on. So these results show that what people remember from a narrative depends heavily upon the interpretive themes that they bring to it, what kind of schemata they’re using to fit these facts into, and what kinds of elaborative inferences they’re constructing and saying to themselves.

Before that experiment, we’d already done experiments on having people reason about or remember routine scripts, as we called them. We would write stories about routine stereotyped events but with many elements of the script missing though implied. People readily understand the event descriptions anyway. For example, they might read: “Dan went into the restaurant, ordered a steak, ate it, paid, and left”. That’s the text for what we call the restaurant script. Every American understands what a standard restaurant script is, and so you can quiz the subject hours later regarding what precisely was written in the text they had read.

We’d ask something like “Did Dan read a menu?”

“I don’t remember, but, yes, he probably did.”

“Did he talk to a waitress?”

“Yes, yes, he must have. Yes.”

“Did he get a bill for the food?”

“Yes, of course

“What did he eat?”

“The steak, wasn’t it?”
In other words, people can answer many questions based on their general knowledge of what happens at a restaurant. And they tend to remember the minimal text as having been elaborated with some of its more stereotyped parts.

People know hundreds of these kinds of little scripts from having lived in our culture. There’s scripts for making a telephone call, for going to the doctor, or to the dentist, going to having your car repaired at a garage, filling up your car with gas, buying shoes at a shoe store, grocery shopping, etc. We have all of these knowledge bundles in long-term memory. In a story about, say, somebody going to his dentist, storytellers will mention only a few items of that episode, but yet readers can imagine all the stuff that’s missing. And when they later try to remember that little routine story, they may confuse the actual text with the elaborations they imagined about the situation. They will think that the text stated that the character did or said so-and-so. So we’d done a number of experiments on these scripts and the way in which people use cultural scripts to embellish, fill in the gaps, of a narrative they’ve listened to.

Hartwig: So how long did this research last?

Bower: [00:44:23] Oh, this lasted for twenty years or more. Eventually we got into mental models, the notion that people are not only remembering propositions stated in the text, but also are building up imagery of what is going on and kind of seeing, in imagery, important parts of a narrative. It is almost like they’re seeing a motion picture unreel in front of them. The text
is sort of like instructions from a director as to what to put in the movie and how the action moves from one scene to the next and eventually to the end.

It isn’t only imagery because people will remember what motivations and goals the characters have, and, of course, they can’t see those important aspects of the situation; you just know that characterizes him. Or if the character has hidden a weapon, the reader knows it’s there, even though he can’t see it. Nonetheless, the reader’s imagery is a very good way to think about the mental model that he makes as he reads a story.

People don’t remember the text. What they do is remember the movie they made out of the text, at least for narratives. They remember a little bit about specifics of the text, perhaps where on the page a given statement was made, or even verbatim memory for a few specific statements. But by and large, people will import many other things into their memory that were not in the text. The mental models that people make up are based on their cultural knowledge about human actions and stereotyped situations. They bring that knowledge to bear and use it to generate the internal film, if you like, that the text has been describing to them.

One thing that’s very much a part of the mental model is, of course, the physical locale, the locations in which the action takes place, the settings. People, when they are composing and filming a narrative in their head, they will momentarily notice the physical situation in which the characters reside or are moving around. As the character moves farther along, that first place fades out of their short-term focus and the new focus is on where the character is now. So if I tell you a story in which the character moves from
one place to the next to the next, your focus of attention in your film--in your mental model--just sort of moves along with the successive locations the character visits.

We got interested in the notion that the things a reader might know about a given locale, about a given environment, become more activated at the point when he is currently focusing his attention; namely, on where the character is now located. So we did a series of experiments on the reader’s focus of attention within his mental model and how a text or story moves that around through a known environment. And one way to study that is to start out your experiment by teaching your subjects a certain location or set of locations, and how they’re laid out relative to one another. And for that purpose, I brought you a little picture to look at. This is an example.

**Hartwig:** So describe this for the listener, then, since they can’t see it.

**Bower:** [00:49:12] Okay. What I’m showing Daniel is one example of a layout of a research building that our participants would study. There are ten rooms in the building. Rooms are connected to adjacent ones by doors. There’s a reception room into which the fictional subjects of the narratives would arrive and be met. There’s an experiment room that it’s attached to. There’s a lounge that’s attached to that. There’s an attached repair shop and a washroom. There’s a library attached to a laboratory, and so on. Our subjects were told to study this spatial layout and to learn the names of each of the rooms and how they are connected to one another and their access. For example, according to this diagram, you can walk directly into the library
from the conference room or from the laboratory, but not from the other rooms.

Each room is furnished with several visible objects, and subjects learn what those objects are. For example, the lounge has a vending machine and a magazine rack. The repair shop has a storage bin for tools and a camcorder sitting on a table there. The laboratory has a bed and a thermometer in it. The library has a telephone and a radio in it, and so on.

So subjects are first learning the spatial layout of these ten rooms, their room-names, access doors to move from one room to an adjacent one, and they’re learning what objects are in each room. So this takes subjects on the order of fifteen minutes to learn, and they would be tested in several ways. “On this blank sheet of paper, please draw what you remember of the rooms and their names and the layout of the building, and how rooms are connected by doors to what other rooms, and also what named objects are in what rooms.”

So that one map is quite a complicated spatial array to learn. In some experiments our subjects would learn the layouts of two such buildings. If we taught two buildings, they’d be much simpler, of course, maybe four rather than ten rooms per building.

We wanted to make sure that our subjects knew that spatial layout, had it firmly in memory, so that when they start to read a story about some character coming into that building and walking from one room to the next along a definite path, they would have a strong memory, probably even a visual memory of the layout, so they would be able to visualize the character
moving around the building. Also they would be able to answer in which room particular objects were to be found.

Okay. So once subjects have memorized that building map, we begin having them read stories in which we introduce a character that is given some goal that motivates him or her to move throughout the building, going from one room to the next, in pursuit of that goal. The goal might be to clean up all the rooms in the lab because the board of directors are coming tomorrow for a site visit, and he’s got to get everything in shipshape. So the story describes the protagonist moving throughout the building, cleaning up, putting everything in its place and so on.

The subjects read the story line by line on a computer, hitting a key that moves them from one sentence to the next, through about fifteen or twenty sentences, until they get to the end of it. After reading the story subjects’ comprehension was tested by asking them a couple of easy questions about what the story character did. They would then read another story about a different character who was given a different goal—for example, to look for a stranger who’d got into the building. Subjects read through some ten or twenty short vignettes in just that manner.

As the subject paced himself reading each line of the story, we would periodically interrupt him and insert a question such as “Is the telephone located in the lab room?” or “Are the telephone and the radio in the same room with one another, yes or no?” Subjects almost always knew the correct answer since they’d recently memorized the map. However, we were interested in how fast they could retrieve those facts from memory. The
questions were carefully written to refer to objects that exemplified various spatial relationships relative to the character’s current location in the story. The objects tested might be in the room the character has just moved into, or they could be in the room he has just left, or they could be some other room in the building at various distances away from the character’s current location.

So if we were to ask, say, “Is the telephone in the library?” subjects would be very fast to answer that question because they’ve just moved into the library and that presumably brings into focal attention the subject’s spatial memory of that place that we had just taught him earlier.

On the other hand, we might ask the subject, “Is the computer in the office?” Although the answer is “yes,” the computer is located two rooms back from where the character is now, so it takes subjects longer to retrieve that fact and answer affirmatively. We found a difference in reaction time for people to verify a fact depending on how far away the object was in the building from the character’s current location. The character’s location is presumably where the reader is focusing his attention within the mental model that’s in his head. So we found a regular distance gradient for memory retrieval times. The general idea is that wherever the subject is attending in his mental model, items in memory there will be activated or “lit up” and so are retrieved more readily. Objects in the room one ahead of where he’s going are also pretty fast. Objects in the room one back from where he has just gone through are retrieved fairly fast. So we observed this nice spatial gradient of what we call priming or activation of different items in memory.
We’re able to show several further facts about that distance gradient. One is what we called the “intermediate room” effect. For example, according to the map I showed you, the character was able to move from the lounge through the experiment room into the reception room. And at some point during the story, subjects might read the sentence, “Wilbur went from the lounge into the reception room.” We’d interrupt with a question like, “Is the telephone in the reception room?” That’s answered very fast since that’s where the character is now located. Or we’d ask “Is the vending machine in the lounge?” That’s answered fairly rapidly since the character just left there. Or we could ask, “Is the Xerox room in the experiment room?” While that’s true, neither the Xerox machine nor the experiment room have recently been mentioned explicitly in the text. However, according to their memory, readers know that to get from the lounge to the reception room, the character will very probably go through the experiment room.

What we found is that the time subjects take to answer questions about objects in that unmentioned middle room is midway between the times for answering questions about objects in the room where the character currently is and the room where he started this movement. We call that the intermediate path effect. Intermediate rooms are those along the path that the character has moved through and presumably activated to an intermediate amount. The result suggests that the reader has imagined seeing the character moving through that path because he knows that path must be taken, and so it’s as though the film shows him moving from the start room
through the intermediate room to the end of the walk. And the objects in those rooms become activated to varying degrees in the subject’s memory.

We also were able to show that if you have the character pick up an object and carry it around with him—let’s say the character picks up a magazine in the lounge and puts it in his pocket, carries it around with him—and then we ask you, “Was there a magazine in the building somewhere?” readers answer very quickly because the magazine is with the character now, even though it was some ways back that it was last mentioned. So, when the focused character carries some object around, your attention follows that character, and that object incidentally remains in the focus of attention, you find that readers are very quick to answer questions about the carried object.

If the narrative has a major and a minor character, people will tend to follow mainly the major character and where he’s going. They’ll answer quickly questions about rooms in which the major character has just moved, but more slowly for questions about objects in rooms into which the minor character has just moved. Readers also tend to activate rooms and objects in them according to route distance through the building’s access doors, not according to Euclidian distance as the crow flies. For example, in terms of Euclidian distance, looking at this building layout, you can see that the telephone is very close in direct distance to the vending machine as the crow flies, but it’s on the other side of the wall. In contrast, the two objects are very far apart route-wise. That is, the character would have to pass through a number of doors and rooms to move from one object’s location to the location of the other object. People tend to answer questions about objects
that are closer according to route distance, not Euclidean distance from the character’s location. So if the character is now in the library, he will answer more quickly a question about an object in the next room along his walking path than a question about an object that’s farther along the route even though the queried object is closer to him in a Euclidean sense, but he’d have to break through a wall.

Our further interests concerned retrieval times to answer questions relevant to the character’s goals. Like we would say, “He’s in the repair shop and he wants to videotape his practice speech that he will be giving to the board of directors tomorrow.” And then you would ask, “Where’s the VCR?” If you’ve just mentioned that the character wants to videotape something, then objects relevant to that goal, like the VCR, get activated, and subjects are very fast to answer, “The VCR’s in the experiment room.” Okay.

On the other hand, if the story says, “He wants to make a telephone call. Where’s the VCR?” subjects are much slower to answer. So people are activating items throughout the building depending upon the goals that they know the character is thinking about.

[break]

**Bower:** [01:01:44] So we’re finding that what’s on the character’s mind are things like objects that he’s carrying, goals that he is considering, and what he’s seen as he’s in a particular location. All these things are in the reader’s mind as well as in the character’s mind. It’s as though the reader is thinking about very much the same things that the storybook character is thinking about or noticing or paying attention to, and this shows up in terms of the amount of
priming or activation of these concepts in the person’s memory. When a goal is achieved and completed, then it drops out of the character’s short-term memory and drops out of the reader’s short-term memory as well. So goals that are achieved sort of slowly deactivate and fade away.

We were able to show that the language the narrative uses in describing the passage of time has a big effect upon this deactivation process. We know that as the character moves from one room to the next, the objects where he used to be are deactivated and fade out of short-term memory, and while the reader is activating the things where the character has just moved.

How fast prior-room objects fade depends upon how much time in the story has elapsed. So if the character goes from room A into room B, and I test you about some object in room A, you will answer at a certain speed. But if after the character moves, the subject reads several sentences about some jobs the character did in room B--such as to sweep and mop the floor and so on--and then we ask about some object in room A, subjects will be somewhat slower to retrieve the location of that object, even though it’s the preceding and close room.

We were able to show how fast the prior-room objects are deactivated depends upon what the story says about how long these actions took the person to do in room B. So if we say, “He went from room A into B and he did some cleanup work there, and **two minutes later** he was finished there. Was object X in room A, yes or no?” We’d compare those answer times to “He went from room A into room B and he did some cleanup work there, and **two hours later** he was finished there. Was object X in room A?” So just
depending on whether the story says “two minutes” or “two hours” later, you activate far more for objects that were in the room you had just left two hours ago. It’s as though you are changing the scene in your mental model if you took a long time or you’re said to have taken a long time, as opposed to a short time after moving into room B.

Anyway, there are various cute and clever things of that kind that we found. Okay. Enough. I’ve talked enough about mental models. Among other things, that’s the kind of studies I was doing near the end of my career. It was one of the main sets of experiments that my group, Mike Rinck, in particular, and I were doing at the end of my career.

Hartwig: All right. So let’s step back a bit now. So talk then about approaching retirement and reflect a little bit upon your career as a teacher. What was your philosophy as a teacher?

Bower: [01:06:46] My philosophy as a teacher was to give students the information that I think they need. I was primarily a graduate-student teacher. For most of my life I taught graduate lecture courses and graduate seminars, and so in those cases, you presumed the student was interested in learning the technical material we were going over. So my main function was to lecture about the material and get students to engage with the topics and with the principles that were involved in technical papers that we would be reading.

I tried throughout my lectures to sprinkle in things that I had done or that my students had done on a particular topic, and we had, indeed, done a large range of things over the course of my life, so it was relatively easy to
say, “Oh, by the way, I or my students did some experiment on that. Let me tell you about it.”

I suppose I was a rather conventional teacher. I prepared very hard for every lecture, I know, even when I was giving the same class again. I would, nonetheless, spend four to six hours preparing, because I was always slightly anxious that I was going to be asked something over the materials that I wouldn’t know the answer to.

My wife, Sharon, said, “Gordon, you don’t know you have to know the answer to everything. Just say, ‘Okay, I’ll look that up,’ and go on. Stop worrying so much about it.”

But I’d say, “No, no, I have to know everything,” and worry, and more than that, everything that I’m asking them to read, I have to read again and refresh it in my mind. And so I’d spend lots of time preparing every lecture, every class, every seminar meeting. I would be very well prepared to do battle with the forces of ignorance.

Hartwig: Did your methods and techniques change over the years, and if so, how?

Bower: [01:09:32] No, they didn’t really change. I guess I learned not to give quite so much detail, understanding that what students might be interested in was the big picture rather than what’s the nitty-gritty detail. But graduate students are supposed to know and care about the nitty-gritty details, because that’s where the science is, not at the big global picture of things.

I did change my teaching a lot when I was teaching undergraduates. The first undergraduate class I was teaching my first years here was a laboratory class in conditioning and learning. I think I mentioned that before,
where each student would be given a rat to put through its paces over the course of the quarter.

The other freshmen seminars that I taught were often on practical psychology. For example, I taught a freshman seminar on applications of behavior modification principles that taught students principles of behavior and reinforcement theory to use in their daily life. So I would go through some textbooks on behavior mod in applied settings, lecturing about it, and have the students carry out little behavior mod projects about every two weeks. Examples would be teaching them self-control or helping one of their friends control some behavior they didn’t like or control their eating or their drug use or their study time or their time management. I would teach the students how to use behavior modification techniques to help themselves or help their friends to alter behavior. I would try to schedule it so that the technique being used would be the topic that we were reading about that week or over the last few weeks.

They would also have to do a major project on themselves during the course of the class and write it up and present a talk about it in the class and so on. Students really got into that. They liked it and would be quite happy talking about it. Now, whether they used it after the class was over, I don’t know, but at least they were taught how to deal with many behavior problems. Examples were things like control temper tantrums or aggressiveness of their kids or control eating behavior of their adolescent child or getting their little kids to go to bed and sleep or getting their adolescents to get enough sleep, etc. So the students were at least taught how
to do it. I don’t know whether they remembered to do it when they became parents later on. Some of my undergraduates went on to graduate school, so I kept up with them, but most of them became doctors, lawyers, and Indian chiefs, and I never knew what happened to them.

**Hartwig:** So let’s talk a little bit about the history and the evolution of psychology here at Stanford. So you got here in 1959. So describe some of the major changes within the department.

**Bower:** Well, the faculty grew larger over time. We changed our location. When I arrived in 1959, psychology shared space in Cubberley Hall with the School of Education. About ten years later, around 1970, we moved into our current Jordan Hall Building. We had applied for and been awarded funds from NSF and NIMH to renovate the old biology building and put us in there. The building was always Jordan Hall, named after the first Stanford President, a biologist named Jordan. The good feature of that building was it was big with lots of rooms, so everybody had enough space to stretch out, have their graduate students in rooms adjacent to them, have their laboratories pretty much near them, and so on. Because everybody’s space needs were satisfied we didn’t have turf wars and territorial wars, which many university departments here or elsewhere would have. So all that space led to a certain friendliness and collegiality of the faculty and graduate students.

One of the things we tended to stress, certainly when I was chairman and then dean--

**Hartwig:** So at what years were you chairman?

**Bower:** I don’t know. I think like 1980 to ’84, or ’79 to ’82.
Hartwig: I think that’s right.

Bower: [01:15:57] So I would always meet with and emphasize to any new faculty member who came in the following: “This is a collegial place. We don’t fight with one another. We don’t have turf wars. Don’t gossip about or run down other faculty members to graduate students. We don’t try to poach and steal each other’s graduate students. We also treat each staff member with respect because they are a very important part of the machinery that keeps this place going, so be nice to your secretaries and to the janitors. Don’t ever talk down or poke fun at your colleagues, either within the university or when speaking outside of the university. Your colleagues are your friends, and you should treat them like they’re friends.” And that ethos kind of permeated the place, I think.

Hartwig: Were there ever issues?

Bower: [01:17:05] We tended to only have tough problems when we had to fire somebody, like terminate an assistant professor. That’s always a very anxious period, certainly anxious for the assistant professors. But also there would occasionally be disputes among the faculty as to whether some faculty member was worth promoting or worth keeping. We tended very much to rely upon the faculty members in the area of an assistant professor to give us an honest evaluation of that person. We also relied upon other people, certainly those who were in the nearby substantive areas, to read that person’s work and form an opinion about it and maybe even talk to other folks around the country about this person who’s under consideration.
And we always took it very seriously, and everybody got to air their point of view. I was, I would say, one of the opinion leaders in many of those discussion, I believe. At least that’s what everybody told me. Amos Tversky was another strong well-informed faculty member who had very good judgment about research quality. Leon Festinger was also a very good judge of talent and was very helpful before he left Stanford for a job in The New School in order to get back to New York.

Hartwig: So talk a little bit about your colleagues over the years.

Bower: [01:19:05] When I came, it was a very wonderfully warm, clubby group of people, and they just kind of took me and my wife, Sharon, into their bosom and treated us like we were their kids—or no, not their kids, but their adult children, and they were very, very friendly. Particularly friendly was a man named Paul Farnsworth. [And] Quinn McNemar, Jack Hilgard. Leon Festinger was one of my best friends. Doug Lawrence. All these people were very, very nice to us, and we would be invited to dinner at their house. Hilgard had a cottage over on the ocean, and he would have us over there with our kids to run and play on the beach. There was always a large number of parties going on. In those days, there were cocktail parties, a lot of heavy drinking in those days. I didn’t do a lot of it, but I did some of it.

Importantly, people lived on the campus or near campus, so everybody came to work every day, and everybody came to parties when there was a party, and so we socialized with each other and we saw each other and we relied upon each other for socializing.
I think the only person who didn’t do a lot of that was Leon because he loved to go into the city and drink or meet up with friends in the city. But he was still a big party guy even when he was here. Leon Festinger is a very, very famous social psychologist and one of my best supporters here, and I loved him. He and I used to talk psychology quite a bit. So when he would write a paper or book, he’d ask me to make comments on it because he liked what I had to say about it, I guess.

It was just wonderful in those days. Now, this is talking in the late fifties, early sixties, through the sixties. We had rather spartan laboratories. My lab was in Quonset shacks out behind Cubberley in a sort of a parking-lot-like structure back there, and they had up these old Quonset huts without any air conditioning or any heating in them, but they were great for me. I was able to build my apparatus, my rat boxes and running boxes and Skinner boxes, and keep my rats out there and kept them happy. Great time.

Then after a while, they tore down the Quonset shacks and gave me labs up El Dorado Avenue in what is now a fraternity house, I believe. But that was okay, and then they moved us over to Jordan Hall. And by that time I was getting out of the animal conditioning work and working more with people, human learners, and Stanford undergraduates. But we have lost that cohesion, I believe.

Hartwig: What do you think are some of the reasons?

Bower: People living in the city, commuting, working on their computers at home, not coming in, not living day to day with their graduate students, kind of flying in. I think the young people still do that because they know
that they can’t be running off too far, but a couple of them still live in the city.

**Hartwig:** Is there more or less collaboration between faculty?

**Bower:** [01:24:17] I think that hasn’t changed very much. In the old days, there was not a lot of faculty collaboration.

**Hartwig:** And why?

**Bower:** [01:24:29] Well, first of all, there weren’t very many of us, and we were spread across many areas. Secondly, you tended to do all of your research with your graduate students. I would run my own experiments for the first couple of years I was here, but by the third year I was here, I was having undergraduates helping me run experiments, or graduate students increasingly helping me do my ideas, or the two of us kind of jointly thinking up something to do together and publishing together. But I didn’t collaborate with anybody. Dick Atkinson and I and Ed Caruthers wrote a textbook together because we were jointly teaching a class on mathematical learning theory. So I would do some parts, and Dick Atkinson would do other parts, and Ed Caruthers would do a part. The three of us wrote a textbook.

Jack Hilgard and I collaborated on his book *Theories of Learning*, but we didn’t talk very much about it because Jack wasn’t doing any learning theory at that time. He was off doing hypnosis, and I was doing all the learning research, and so I became the majordomo or the major reviser of revisions of that textbook.
Hartwig: What about interdisciplinary collaboration between other departments, schools, here or other places?

Bower: I think in the old days some people had joint appointments in education. I know Hilgard did and Dick Atkinson did, and they would tend to do essays and write position papers in education, but I don’t remember them doing much research of an experimental kind. Atkinson and Pat Suppes got involved with computer tutoring of elementary grades in math and reading. They had a few education graduate students who came and helped them with those tutoring projects, but I don’t think there’s any faculty member in education.

I know Bandura had some work with one or two people in education, but mainly using modeling to model study habits or using modeling to--what comes to mind is the moral rules, ethical rules that the model used when the observer kid had to do something.

But there wasn’t very much. There was some. We had people who had joint appointments, like in the School of Business. Al Hastorf had that arrangement, as did Alex Bavelas. Nowadays the department has a few social psychologists that have joint appointments in organizational behavior or in marketing.

What’s happened over time with those joint appointments is that they get much more money from the business school than they get from humanities and sciences, so they take their full 100 percent appointment in the business school, but they will cross-list their classes in psychology. But we’ve lost two or three of our young professors to the business school. They
came in the psych department and got transitioned into the business school. That didn’t use to happen. That used to go the other way. History came the other direction.

**Hartwig:** So maybe talk a little bit about the history of funding over the years, how it changed.

**Bower:** [01:29:25] I was lucky in that I came in the period during which Stanford was growing exponentially, the psychology department was growing, and national funding from the government was growing exponentially. It was in the post-World War II era of Sputnik—I forget, when was Sputnik? I’ve forgotten.

**Hartwig:** I think ’57.

**Bower:** [01:30:11] That scared the hell out of the government, and they decided they were going to fund lots of educational research, particularly in physical sciences but also in social sciences.

Also, the psych department in the fifties and early sixties had a clinical psychology program to train clinical therapists, and after World War II, the VA system had lots of clients, lots of soldiers who came back with PTSD or psychological problems of various kinds, and so there was a great demand for psychotherapists at VA hospitals, and we had two here, in Menlo Park and Palo Alto. So they said, “Goose up your clinical program, please, so we can get more clinician interns.”

So in the mid to late fifties through the early sixties, Stanford psychology department got lots of money from the VA to hire adjunct associate or assistant professors to help run our clinical program, right? So we got a few more of those, and the clinical program grew. But also the VA
just threw a lot of money at the psych department, I think, for all of us to use in research. Good deal.

Also, NIMH grew because National Institutes of Mental Health, they knew there were lots of soldiers with mental health problems, and so they were throwing money around, particularly at departments that had any kind of mental health programs or clinical programs. But then the NIMH saw that it was important also to fund basic science in psychology, thank heavens. NSF got into social science funding too. Social Science Research Council was funding. The Ford Foundation had a lot of money. They funded the Center for Advanced Study in the Behavioral Sciences up here with a huge endowment and so on.

So there’s a lot of money sloshing around, and Stanford knew how to get it, and we were taught how to put in grants. So one of the first acts I performed upon starting as a psychology teacher here was to write a grant proposal to NIMH, and darned if I didn’t get funded for doing research on observing behavior in rats. Now you could teach them how they would learn to get information that was useless, but they wanted to know anyway what was about to come down the pike at them. I had a whole variety of things, interesting things I did in those days with rats and cats.

But anyway, so that was the beginning of my funding from NIMH, and it started in 1960, and I was lucky or whatever, persuasive. I have continuous NIMH funding for the next forty years--

Hartwig: Oh, wow.
Bower: [01:34:13] --from 1960 through until I quit in 2008. The reason it doesn’t quite work out is I had three years off to go to NIH and a year here or there when I went someplace else, so I just put it on hold. But if you add the active years, NIMH funded my research all my entire career, sometimes with honorific awards. Big deal. It was a big deal. It gave you ten years’ funding where you don’t have to write grant proposals. I think I had to write one every five years to tell them what I’d been doing.

But anyway, so the funding situation really was very good, and everybody on the faculty in the physical sciences, certainly, but also in the social sciences was expected to apply for and hopefully get research funding from the government or from one of these foundations like SSRC or the Ford Foundation or the Rockefeller Foundation.

That funding was terrific to have around, I must say, and it was through that that, for example, it was SSRC funding that got me my job here. Pat Suppes had got that money from the Social Science Research Council to develop a program in mathematics in the social sciences. I think I talked about this earlier as to how I got my job, and that was on soft money from SSRC. They brought me in and Dick Atkinson and Herb Scarf, an economist, and Joe Berger, a sociologist, four of us, I think. And all of us got tenure here, and three of us got in the National Academy, so Pat had decent standards or a good eye for talent on the hoof.

The Center ended up being a great place for us. It sort of became our union hiring hall. People come to the Center who are pretty good, and we would have them down to talk, and we would get to know them, and they
socialized with us, and those who came were psychologists, and so we would hire a few of them now and again. And so it’s like we were getting exposed to the cream of the crop, and they were getting exposed to Stanford and what a wonderful place this was. So we hired three or four people out of there, like Al Hastorf was one of those ones that we got. I think Bavelas was one we got, and Leon maybe. Tony Deutsch, Karl Pribram came, Gig Levine, and maybe others came through that door.

But it was wonderful, the money that was flowing through the departments and the university in those days. This is how the administration, especially Provost Fred Terman, was able to kind of build up the university, because endowment money was coming in. The deal always was, “Give us soft money, and we’ll hire some assistant professors using that soft money, and pretty soon they’ll be able to pay for themselves, or we’ll get some big donors to endow Stanford, and then that’ll pay for them. If not, then we’ll kiss them goodbye.”

That growth model worked very well in those days. I think it probably still works pretty well, obviously in computer science and in certain kinds of hard engineering and in biotechnology, and probably molecular genetics and any of those growth areas.

The problem you have with that mode of funding of people is that people will buy out their teaching and just live on grants, and so we become a research institution with people living on soft money. That has two problems with it, which you find out very quickly when you’re in the Dean’s Office. Problem number one is you have a lot of people who are around here taking
up space who are not teaching, and so although we have lots of famous people around, they aren’t filtering down to the undergraduates to teach it. So we had to kind of say, “No, no, you can’t just have research associates. You have to have a teaching component to these people.”

**Hartwig:** Approximately what year would you say that this started to be a concern?

**Bower:** [01:39:47] Oh, it was a concern in the sixties and seventies, seventies for sure, because this place can expand only so much, and lots and lots of very famous scientists would like to come here and set up shop, but you don’t have space for all of them. So that was one problem.

The other problem is they take up space and they’re on soft money, and sometimes the soft money stops and they can’t get it that year, so they will turn to their chairman and say, “Would you please give me some money? Because I’ve been throwing a lot of money at Stanford with my overhead, so you kind of owe me, don’t you? Give me some money. I’m out of money for the moment,” or for the year. And that could be a problem if it got too bad.

What would happen with many people was they rode a crest that was very popular for a while, but then their time passed them by and they’d lose their funding, and then you’d have to say, “Kiss them goodbye.” And occasionally people would sue you, the university. This happened a couple of times with people who had appointments in psychiatry in the VA Hospital, and they would say the psychiatry department had run out of money, didn’t want to fund them anymore, but they would say, “You owe me. Didn’t you sign something over that said I’m a real honest-to-God member here?” And they would have this fight because the VA might not want to support them
any longer and their department didn’t want to. Anyway, it could get very combative, enough so that it would go to court sometimes.

Anyway, I’m emphasizing too much the problems, but there were problems with soft-money funding of scientists around, and so each department, particularly those in engineering and computer science, were told to shut down or at least stop so much growth in their soft-money positions and start requiring more teaching from people in those departments. “We don’t just want you to do robotics and teaching a couple of graduate students in your lab. We want you to also do other things.”

The other thing that showed up a lot with people in molecular genetics and biotechnology was patents. They would invent something, invent a drug or a process or a software, piece of hardware, and go out and patent it. And Stanford would say, “How about me? How about us? We aren’t chopped liver.” Or NSF or the DOD, DARPA, would say, “You know, we bankrolled you. How about giving us a piece of the patents on the hardware?”

So that all in the sixties got resolved, and I think Stanford eventually got it working okay. At least you don’t hear any great blowups anymore through the Office of Science and Technology, or whatever they call themselves, Patent and Technology here.

[End of Session Four]
Bower: [00:00:02] One thing I forgot to mention last time when I was talking about teaching was that my primary function as a teacher at Stanford was as a PhD advisor. I was renowned for being able to take students through their research projects or suggest research projects for them to do, to grow in their knowledge of an area, up to the point where they could do a dissertation and move on to be researchers. So I was rather successful in turning out PhD’s, many of whom ended up as very good scientific citizens in the academic community. Somebody once counted up that I had on the order of fifty-four PhD’s, far more than anyone else in the department. A large proportion of those had gone into academia, and, of those, a significant number had become famous as researchers.

Hartwig: Why don’t you list some of those individuals?

Bower: [00:01:32] Bob Sternberg was one. John Anderson was another. Keith Holyoak was another. Steve Kosslyn was another. David Rosenbaum was another. Mark Gluck, John Clapper. Can’t recall now all of them. I also had many postdocs who did very well in their careers. Mike Rinck was one of those.

Hartwig: Did you continue to work with them after--

Bower: [00:02:09] Yes. Dan Morrow is another. Tom Nelson is another. I can’t generate a whole lot at the moment.

But somehow or other I was lucky or talented at being able to attract good students and to help them along towards developing into mature
researchers and getting them good jobs and well on their way towards academic success as professors, so I’ve even received awards from various professional organizations for being a good mentor, and that’s a good feeling. I say to myself, when I’m feeling down, that my best legacy to the field is those graduate students.

Hartwig: And they were involved in the various Bowerfests. Correct?

Bower: [00:03:14] Yes, indeed. In fact, they organized the Bowerfests as well as these various parties for my seventieth, seventy-fifth, and eightieth birthdays.

Hartwig: What were some of the highlights from those events?

Bower: [00:03:30] [laughs] The roastings, of course, were always the most enjoyable for all of us at the banquets, but the conferees also gave scientific papers over a two-day conference. Many of those papers were published in a book dedicated to me. In fact, I had two different Festschrift volumes in my honor, the first one that came when I turned age fifty and ten of my ex-PhD students planned and produced a volume describing my influence on their research. Each chapter started off talking about me and my role in their education and professional development. A second festschrift volume was published in 2005. It was edited by Steve Kosslyn, John Anderson, and Mark Gluck. It’s also been Steve, John, and Mark who have organized the birthday parties in my honor, my seventieth, seventy-fifth, and eightieth birthdays, and those have been very gracious for me and Sharon.

With some of my PhD students, I’ve been able to keep in touch with them as they advance through their professional life. They will call me and we talk about whatever professional crisis they are having at the moment.
And they write nice things about me, which makes me feel good. It’s one of the things that professors live off of, that kind of praise.

At any rate, I wanted to add that information about helping graduate students through their dissertation. I was a good classroom teacher but in a rather traditional sense. For example, I would stand up and lecture, hand out my lecture notes, use slides and media and ask the class some questions, try not to talk all the time. I also gave midterm and final exams and had students write term papers, and I would give feedback on their papers. So I did all that, which is sort of the traditional way of doing the job.

When I did a quarter teaching at Stanford in Vienna, Austria, in 1969, I gave a seminar on memory, but the main class I taught there was called “Psychological Conceptions of Mankind.” I organized that according to what is called the Personalized System of Instruction, PSI. The basic idea there is to divide the course material into ten to fifteen units. Each unit consists of a collection of readings, which I had summarized and annotated to a great extent and gave them ideas of what they should look for in those readings. I would also identify key concepts that they should know when they’re done with that unit.

And when they’re done with each unit, they take a test over it, and they have to get 85 percent right on that test before they can proceed to the next unit. If they don’t achieve 85 percent, I talk with the student about what they’re messing up and what they need to bone up on. They go home and study and come back and take a second, alternative test over that same unit.
Usually students pass on the first or the second exam, and so they go on to the next unit and so on.

One of the features of that class was that while I held two sessions a week for us to all meet together, I did not give lectures in class. Rather, I did demonstrations of interesting psychological phenomena with them. I would do hypnosis. I’d do progressive relaxation and imagine desensitization, which nowadays they call mindfulness. I would do other demonstrations like Spot the Liar. For that demo, I’d take four kids out of our classroom and tell them a 2-minute story about events leading up to an alleged car accident involving teenagers, and ask one of them to confabulate and add a few different details to his version of the story. I’d then have the four return to the classroom and have each of them at random tell their “truthful” version of the accident events they’d been told about. Unknown to the class, among the four “witnesses” was my designated Liar who told essentially a similar story but with some details missing or confabulated. The rest of the students sitting in the class listening to these accounts had to spot who was the Liar, who was modifying or making up things.

In another demonstration I would take one student out of the class and have him imagine a story in which he breaks into a house, notices many valuable items there, and actually steals a golden watch. When he returns and is put in front of the class as a prospective thief, I would give him a free-association test with certain words related to items he’d imagined seeing in the house but had not stolen. Included would be words related to the gold watch he’d imagined having stolen. The student was instructed to give his
first free-association to the prompts I was giving him. The class would observe him carefully, especially his word-association times, and try to infer what if any of the household items the student had imagined stealing.

**Hartwig:** And did you use this method only while you were in Vienna? And why?

**Bower:** [00:10:09] Yes, it was only in this Vienna course that I did live demonstrations with my students. I recall one we did on rumor spread. We start with six students sent outside of the classroom and I’d bring in one, and I’d show him and the class a picture up on a screen in the front of the room. The picture was of a fight between two men on a crowded subway train with people of all racial features in the seats around the fighters. Our first subject would describe to the class what he sees in the scene. Then in comes another student (who is not allowed to see the pictured scene), and the first student tells the second student what the picture was about. This type of retelling continues—the second student tells the third, the third tells the fourth subject, and so on. The rumor spreads along the line. The students sitting in the classroom could compare the rumor spreaders’ descriptions to the picture on the screen and see how the descriptions change as they proceed from one recaller to the next, how the accounts are shortened, modified, conventionalized and schematized. It makes for an impressive demonstration of the unreliability of word-of-mouth rumor spreading. I did several classroom demonstrations like that.

**Hartwig:** Do you think this method of teaching was more successful than traditional lecture-based teaching?
Bower: [00:11:11] The students loved it, because the rule was you don’t have to come to these meetings; they’re for fun and inspiration about psychology. If you’re around and want to be interested in something psychological, come and watch.

The other big feature of the class was students could go at their own rate, so they could finish several sessions in rapid order. After the first unit, they could get the second unit; they could get that and pass that test quickly, and go on to the third session. So in the first week or two, if a student wanted to, he could get through three or four of the twelve units. He could go as fast as he wanted through the units, so there’s nothing holding him back. The condition was, if you get through everything, passing every unit by 85 percent and you get to the end of all twelve units, you’re going to get an A for the course.

You tell that to Stanford students, particularly if you also say they don’t have to attend class but they can take off for Paris or Berlin or Rome or Florence or wherever they want through the week, they’ll do it. [laughter] They will whip through that material as fast as they can and then have three weeks off to go on the train to Paris, Rome and Florence. So they loved it.

The other professor who was with me didn’t love it, because he was giving the conventional class, and so students had to show up at all his lectures, etc.

Hartwig: There are a lot of components in this method to the contemporary flipped classroom or the Massive Online Open Course, where you can proceed at
your own pace, lectures are mostly online, done at home, people can proceed at their own pace, so it’s definitely an anticipation of this.

**Bower:** [00:13:25] Same thing, except on the MOOCs that you see today, the kids have to take tests. That is, I think some of the courses you have to take tests. You get feedback about the test, but you go on to the next unit even if you didn’t do very well. Nevertheless you go at your own pace. In the case where students are going for credit, some teaching assistant is keeping score of how well students do, especially on a midterm or final exam, but that’s all.

The problem with MOOCs, of course, which we knew back in the sixties, is motivation, keeping students coming back to it and working at it persistently, because they don’t have that human connection of somebody who’s sitting on top of them saying, “I want to see and test your every outcome, your every result. Show me what you’ve done so far,” or, “Let’s talk about what you’re learning.” So that human motivation is missing. The problem that has come up with the MOOCs is they have like an 80 percent, 85 percent dropout rate. Many people who sign up don’t even get through the first unit of the material and very few people end up going all the way through for credit, so the administrators of MOOCs are worrying about the same thing we worried about in the 1960s--how do you get people motivated enough to keep coming back and to stay with it.

My way of doing that PSI class, of course, is the students remained very motivated to get to the next unit by passing the test on their current unit. In order to do that, they have to do the readings and learn the things that I tell them to learn they should really know. So each unit, I would write
out notes for students, e.g., reviewing the biological psychiatry view of mental illness. I would give students well in advance a fairly detailed outline of what I would have lectured about had I been doing it, referring a lot to the readings that they were doing for that unit also highlighting the main ideas they should pick up, learning what was called the behavioral educational objectives for each unit, and they would be tested on just those things. And they liked that, in the sense that it was very specific as to what it is I want them to know and what they are going to be tested on. I indeed tested them on those ideas, and if they could do it, I’d say, “Good. Move on to the next unit.” And students loved that part of it. So anyway, that was my one-time unconventional teaching. [laughs] I never gave that class again.

**Hartwig:** Why not?

**Bower:** [00:16:29] It was more appropriate for students who were not involved with our regular psychology curriculum because I was touching a little bit on a large number of topics, on psychopharmacology, you know, drugs, which kids were very interested in in those days, and sleep and dreams, Freudian psychoanalysis. After all, we were in Vienna, so I had students go out to Freud’s old museum. I also covered radical behaviorism and Existentialism in psychology, that is, existential psychoanalysis. These are topics I’d never teach here. [laughs] At least I don’t think I would.

**Hartwig:** You never know.

**Bower:** [00:17:23] Yes. So it was fun. It was a hell of a lot of work for me, now that I think about it. I had to write out all these lecture notes for twelve units, as I
recall, and read all this new stuff. God, I worked hard at it. But I learned a lot, too.

Anyway, back to my starting point, I thought I would mention my graduate-school mentoring PhD students. I was mentoring my students the way I had been mentored at Yale by Neal Miller and Frank Logan. I became, so to speak, students’ colleague. Miller and Logan came to treat me as an equal by the time I was in my third year, maybe even my second year of graduate school. They didn’t give me things to do, experiments to do. I had my own ideas that I would pass by them, and I tried to craft my experimental ideas so they would be interesting to Neal Miller or to Frank Logan. That was easy because I was interested in most of the things they were doing. But they pretty much felt comfortable saying no to some ideas, “That has this complication so it won’t work right, we think,” or, “That’s something we already know. It’ll just be redundant, so try working on something else.” Or then they’d say, “Now, this was an original idea, so, yes, so that looks interesting. Go on and do that. Here’s the money to pay for it.” [laughs] Or, “Here’s the rats to test in it,” or the apparatus or the labs that we all used.

**Hartwig:** Was it more effective to be a little more hands-on, encouraging, or to be a little more hands-off in terms of mentoring?

**Bower:** [00:19:23] You start hands-on and you gradually release one finger at a time, so to speak, and start encouraging them to come up with things on their own. One of my rituals was to have thinking sessions, either one-on-one or with a larger group, where we’d try to think of something to do, some new idea, some new direction to go in on our research. Most quarters we’d have
research group meetings once a week in which I or one of my students would talk about the work we or they were doing, and we all would critique it and ask questions and ask for clarification and say, “Why didn’t you do it this way rather than that way?” And when a given experimental idea had been discussed, the next question was, “Okay, what’s the next step? What would be interesting? Would this become a dead issue that nobody wants to work on further? Is everybody bored with it, or do we want to try to uncover something new here? Let’s think.” A group-think period.

There’d often be a period at the beginning of the session during which we didn’t criticize anything that was said; just let any idea be expressed. And then after a while, we’d get more critical about what has been said, always trying to come up with an idea or two for people to continue working on.

One of the techniques I taught to my students was how to do analogical thinking. Suppose they’re working on topic A and I know things about topic B, and there’s an analogy between those two areas, then it might be possible to take what’s been done in area B, some technique or some new variable studied there, and transpose it onto topic A then perform a similar experiment on topic A.

Example. Let me give you an example. John Bransford, a psychologist, and Jeff Franks had done research in which they showed that you can write prose passages where each sentence is perfectly coherent, but that from one sentence to the next you can’t figure out what the heck the text is about. Students don’t see how the sentences hang together globally until you give them a clue. If some people had been given the clue before they read the
passage, it makes perfect good sense. So Bransford and Franks were using material like that to demonstrate the fact that language is often very cryptic and obscure until you have some way of calling forth your knowledge relevant to what’s being talked about, at which point it all becomes transparently obvious.

I remember one of the ways Bransford and Franks showed that was by having students read a passage which is a very abbreviated description of doing the laundry, mentioning that you first sort things into piles, and if you got enough of the [things] in one pile, then you can start working on that pile, while you create the other piles, and so on. The text continues that way for ten or so sentences, describing how to do your laundry, but the text never makes it very conspicuous as to what the character is doing until you hear a clue, like, “This is about doing laundry,” and then it’s obvious how the vague description fits with the laundry script.

Well, Jeff Franks and John Bransford did that and showed the enormous impact when past knowledge is brought to bear in understanding obscure language. I looked at that and I said, “That’s really interesting. I bet we could show the same thing with respect to vague, nonsensical pictures, drawings.” That is, I can make up drawings that just look like nonsense on the page until I give you a clue as to what the drawing is depicting.

So I made up a whole bunch of drawings. They were very much like a book that was out at that time called *Droodles*. A droodle is a very obscure drawing that if you’re told what it is, it hits you immediately as to that’s what it is. So I made up a bunch of such obscure drawings, had subjects try to
learn them, and ran the same kind of comparison that Branson and Franks had ran for text, except we were doing it with drawings. So some subjects were given the clue with each drawing, other people weren’t. After they had seen fifteen or twenty of these things, we asked them to recall by drawing the original drawings. As expected, there was this enormous difference in terms of how many drawings subjects were able to recall. Those who originally got the clue with the picture remembered far more.

What we were able to do further was to show the role of clue comprehension not only for a single drawing, but also for pairings of drawings, of this diagram paired with that diagram. Again, you could make up two drawings that don’t look like anything until I give you a clue, and then you see what they are, how they fit together. So, after study, we would give one of a pair of drawings and ask the subject, “What drawing was this paired with?” Subjects’ memory is greatly improved by being given a clue that brings up their schematic knowledge that knits together the two drawings.

Let me give you an example. I show you something that looks like fog, and then disappearing into it is a picture that looks like this curlicue, and it’s paired with another drawing of that fog, and it’s paired with a diagram that looks like a circle with two little holes in it. That’s it. [laughs] And what you say is something like, “The first one is the back end of a pig disappearing into a fogbank and the second drawing is of his nose coming out the other side of the fogbank.” Makes sense. So I’d make up a bunch of these funny pairs of drawings.
Hartwig: That was just one way where you would encourage application from a different concept or field?

Bower: [00:27:21] Yes. So we’re taking it from language and using it with drawings or pictures, because people at that time are claiming that pictures were far better remembered than words, and that’s often true. If I show you a bottle as opposed to I just say, “Remember bottle,” the object or picture will be remembered better. But it turns out that superior memory for pictures depends upon the facts that we can usually interpret the picture and assign a concept or a conceptual description to it that makes it perfectly understandable. On the other hand, we can draw pictures that are nonsensical, are incomprehensible, that we can’t remember worth a darn.

So it’s not the case that we remember pictures of anything better than words or descriptions; it’s rather that pictures enable us to get a conceptual interpretation of it. So we can store in memory not only the picture and its name and the conceptual interpretation, but we can also store the perceptual image of it, so we have multiple ways of representing and getting back to that memory. So anyway, that’s what I was trying to clarify, that pictures are not very well remembered unless you can interpret them. That’s the same point that Bransford and Franks were making about memory for obscure texts.

Hartwig: What were some of the other disciplines or research areas that you brought in throughout your career in terms of teaching or your own research?

Bower: [00:28:52] My students and I brought in a lot from linguistics. We would look for things that linguists were writing about that we thought were interesting, and we’d try to do something on language comprehension with those ideas.
But we always tried to have memory involved in the project, so we’d do experiments in which the person had to remember something.

One demonstration involved the way people understood “bring” versus “take.” Better not go there--I remember the gist of it, but I can’t now recall the details.

**Hartwig:** We can come back to it, if you want. But you also brought in, I mean, computer science.

**Bower:** [00:30:12] Yes.

**Hartwig:** What about medicine or neurology?

**Bower:** [00:30:17] No.

**Hartwig:** Cognitive science?

**Bower:** [00:30:21] I picked up many ideas from artificial intelligence, particularly the work on language understanding. I was always interested in computer models of language understanding, how it is that you can take that page of print in front of you, put it into a computer, and then have the computer understand it conceptually, be able to transform the sentences, to answer questions about them, to draw inferences from what has been read, be able to knit together successive sentences and paraphrase them, and the like.

Roger Schank was one of the people here at Stanford in computer science, working on computation linguistics in the early sixties through mid-sixties. Roger and I became good friends. I would sit in on his lecture classes and have my students sit in them too. Wait, I’m getting the dates wrong. It wasn’t the mid-sixties. It was the seventies, the early seventies. Late sixties through the mid-seventies.
We did some work with Roger’s ideas. Roger was interested in decomposition of words into more primitive concepts. Consider a word like “give,” as in “John gives Mary a book.” “Give” can be decomposed into a set of primitives so that “give” means something like physical transfer of an object, in this case a book that involves differences in possession, like John possessed it at one time. The recipient, Mary, possesses it at another time and so on. Thus, “give” contains in it something like transfer of property and movement. Consider now a word like “sell.” What’s the difference between “sell” and “give”? Well, “sell” involves giving, but where there is a transaction in which the recipient gives something like money back to the giver and so on.

In experiments we were able to show that after subjects study sentences like, “John gave Mary a book,” they will get confused in memory between “give” versus “transferred” the book to Mary or “loaned” Mary the book. We found that verbs that are equivalent in terms of the state changes that they produce in the story world, those verbs would tend to be confused when people try to remember exactly what was said or written. We did experiments using many different verbs that Roger had decomposed into primitive concepts.

From Roger Schank I also picked up the notion of scripts. Each person in our culture has many hundreds of these very well-memorized routines of the way things work in the world—the way you put on your shoes or the way in which you make a telephone call or the way in which you turn on your computer or buy a meal in a restaurant, etc., etc.
Roger developed that idea quite a bit in his artificial intelligence programs for understanding short stories and simple stories involving collections of scripts, and my group did quite a bit of research on it. We showed that people, indeed, do in memory remember the bare bones of a text based on a script, but then they fill in all the details that were missing in the text. Or suppose I tell you a story about a guy going to the doctor and another guy going to the dentist. These two scripts are really examples of a more abstract script of a person hiring a professional or going for an office visit to a professional. You can show that there's a lot of crosstalk and confusion in memory between two such stories that you’ve been told, between the guy going to the doctor versus the other guy going to the dentist. So the things you said about Harry at the dentist get confused and imported into your memory of the story about John going to the doctor. That’s a kind of high-level transfer memory or contamination, if you like, of one memory by another because they are making use of the same abstract knowledge structure, in this case knowledge about visiting a health professional.

So Roger stimulated me with lots of ideas. Roger and Dave Rumelhart also stimulated me to move into research on story grammars and story memory. That’s something that psychologists weren’t doing much of before I got into it from the work in artificial intelligence.

**Hartwig:** Were there aspects of memory that totally baffled you or you could never figure out? Were there things that you would still like to work on today that you couldn’t do before, in terms of technology?
There’s little I could do with FMRI that I couldn’t do behaviorally. The kind of conundrums and problems of memory that we were concerned about and that we tried to design experiments to make distinctions among them would still be there if you just run those subjects through FMRI. You still don’t know what’s happening enough to understand what’s going on.

Here’s a simple example to illustrate my point. In recognition memory, this is where I show a person, let’s say, a list of words, and then later, half an hour later, I say, “I want you to remember that list of words that I showed you before. I showed you twenty words. Here are forty words, twenty of the old ones and twenty new ones I’ve scrambled up so they’re all mixed together. I want you to go through and look at each word and say was that on the old list that I gave you before, or not--just say “Old” or “New.” That’s what’s called a recognition memory experiment.

One theory of recognition performance simply says that what the person’s doing as he’s looking at each word in the list, is setting up a simple association such as “In the list context, I saw the word ‘bottle.’ In the list context, I saw the word ‘table.’ And so on. And then later when we showed him ‘table,’ he asks himself, ‘Do I have an association from table to the list context?’ And if I do and if it’s strong enough, then I’ll say, ‘Yes, it was there on the list.’” That’s one view.

Alternative view is that all of these words that are hooked up to the list somehow or other get activated, the whole shooting match. When I show you a word like “table” or “bottle” and ask you, “Was this on the list?” what that view says is, is that the entire ensemble of all those memories from the
study list get activated, and by my having a particular one of these word memories in there, it enables the whole ensemble to be larger in terms of total activation than if that text word was not on the list. And many theorists argue for this whole activation point of view and adduce evidence for it. I don’t believe it happens that way.

It turns out that if you do FMRI on a person as he’s studying the things or as he’s being tested for recognition memory, you can’t tell which of these theories of view is right. You just can’t tell. You know that, yes, when a person is studying an item to remember, things happen in the hippocampus and the medial temporal lobe that you can measure, and when you test the person, something further happens in the frontal lobe and the hippocampus. Consider items that the subject either can remember versus those he fails to remember. If you back-sort the data and look back at what was the hippocampus doing when he saw that word that they later remember, you can see that it was having a bigger activation in the hippocampus compared to another studied item that wasn’t remembered. So you know words that they remember produce a bigger activation when they studied them in the hippocampus than words they failed to remember. And that’s okay. You say, “Yes, that’s reasonable. That’s probably what happened. But how does that distinguish what I was worried about between my two theories of a context-to-word association versus a large global activation of the whole list?” And it doesn’t.

Or take another example. In simple recognition memory, there’s this theory that at testing the subject tries to retrieve an association of the word
to this context. Another theory says that in addition to that context
association, each word presented, like “bottle,” has its activation or
familiarity enhanced simply because it was presented. Thus, presented words
will be more activated than words that were not presented. And it’s assumed
that subjects use that activation to say, “Yes, I think that word was on the list
I just studied, even though I do not specifically remember it.” So that’s a
particular theory about recognition memory, that subjects not only try to
recover a specific context memory, but also just look at a word in memory
and see if it feels more activated and familiar.

Now, it’s hard to distinguish those two points of view. Do you really
need the notion of familiarity and activation of a presented word over and
above just retrieving an association? Well, here’s an experiment that I did
with my student, Katarina Villanova that I think speaks to that issue. The
“familiarity” theory says that activation to a given word node in memory
occurs every time you see and read that word in the current session that
you’re in. You activate it every time you see it and read it, and then you also
“get” that it belongs to the list.

So one of the experiments I did with Katarina—and I think John
Anderson and I did an earlier one—we gave people a list of words to
remember, say, “Here’s the words we want you to remember.” And you say,
“Dog, sled, table, notepaper, blah, blah, blah,” etc.

But before they study that list, we give them a set of instructions that
include some of the words that could have been on that list, like, “We want
you to listen to these words, which will come one at a time.” And at the end
of them studying the list, there’s the list to study, and then there’s post
instructions about how they are to be tested; namely, “There’s a sheet of
paper that you’ll be getting, and we want you to check yes or no, I heard that
or didn’t hear that,” and so on. We give standard instructions for
recognition/memory testing. In the text that follows we include a number of
test words that had appeared in the instructions, either before the list to be
learned or after the study list. And so those instructional words had very high
frequency and high recency in subjects’ memories. We then see whether or
not those lures, or distracters in the recognition memory test suffice to
convince the subject to believe they were on the list because they should be
highly familiar to the subject.

We did that experiment in many different ways, but the answer always
was no, subjects rarely get confused at all about that word that was in the
instructions being on the list that they were told to remember, even though
in some strict sense we have activated and familiarized them with that
distractor word “paper” or the word “listen” many times over. It’s just that
during instructions subjects are not at that time hooking up the words to the
list context that they’re supposed to remember.

So it’s as though subjects can kind of put barriers between that box of
words in instructions and the box of words in the list you’re supposed to
remember, and memories of the words don’t sneak back and forth very
much. That kind of experiment says that recognition memory for a list of
words isn’t much affected by overall familiarity or activation of that word by
virtue of it having been used frequency in the recent past.
Anyway, to draw my point to a close, I don’t think there’s much in the FMRI studies of memory that will distinguish between a sense of familiarity with an item versus being able to retrieve it and its association to list. Anyway, that’s another example, I think, where FMRI isn’t helpful in deciding between two theories of memory performance.

Hartwig: What’s most interesting to you right now in terms of psychology?

Bower: [00:47:37] Why it is so hard to persuade people out of their firmly held beliefs, why they can continually ward off arguments against the position that they have. I’ve been studying religions and, in particular, the debates that go on between atheists and religious people. I am an agnostic skeptic or, I guess, atheist, and I cannot understand how atheist thinkers, of whom there’s five or six on the current bulletin board—Richard Dawkins is one—how they can, in my mind, utterly demolish the arguments of theists for religion, for belief in God or belief in Jesus Christ, and yet it has no impact whatsoever on those devout believers. The same with arguing with rabbis or arguing with Muslim imams. The arguments pass right over their head. I don’t know how they do it. It’s irrational, and I think people shouldn’t be irrational. That ought to be one of the first canons of the good life; believe things only because you have good reasons, not because it makes you feel good to believe in certain things. So I’ve been watching all of these fruitless arguments go on with the new atheism because I’ve been an agnostic for, I don’t know, fifty years, and it’s still puzzling to me.

The other puzzle is why it is that every American politician has to pay lip service to religion every time they give a talk. “God bless America and
God bless us.” And the Muslims, “Allah be praised. Allah will care for us.”

So the brittle persistence of closely held beliefs is one of the interesting things in psychology.

I guess I haven’t kept up with recent developments in psychology since I retired. I go to the psychology department colloquia that are here, particularly if it’s on a topic I’m interested in, or the person giving the colloquium is a friend of mine and I have to show up to say hi or support them, go out to dinner with them, or the like.

I guess I’m interested in what we call willpower, self-control, or self-regulation we used to call it in the 1960s, that is, what people can do to change and control their behavior. I used to teach a similar class not on willpower directly, but it was on emotion regulation and on impulse control, because those are the kind of topics that willpower’s all about. How do you stop eating all of that ice cream that’s in the refrigerator, or how do you stop having sex with every woman that’s willing to go with you, or how do you get out and exercise enough when you know you should, or start eating your veggies, and so on.

Issues of willpower are interesting because you have treat yourself as though you’re two personalities: one is the wanting and the other one is the controlling part of you. You have to help the controller develop strategies for keeping the willing or the glutton down, or keeping down the sex fiend or the drug addict. So I’m interested in that.

One of our ex-PhDs, Kelly McGonigal, who works in the Health and Wellness Center in the medical school, teaches a class on willpower.
Bower: [00:53:37] I took her class, and she was great. I complimented her on numerous occasions. Anyway, I’m drifting off here.

One of the main topics I’ve been interested in for scholarly reasons, I guess, is the beginnings of Christianity, what’s also called studies of the historical Jesus. There you have regular biblical scholars who are serious historians looking for the evidence that Jesus of Nazarene really existed and, if so, what he might have done. As you probably know, most of the New Testament of the Bible is a conglomeration of many different parts and pieces. Historians are not even sure how the parts were put together. The gospels Mark, Mathew, Luke, and John—the first one of those, I guess, was Matthew or Mark. That book wasn’t even written down until about A.D. 80 or 90, which is like sixty years after Jesus allegedly died. So there’s a sixty-year gap during which all there is, if there’s anything, is word of mouth and rumors, and the stories are being filtered through people who are big believers and who are proselytizing others to their beliefs. And they obviously are adding all kinds of mythic aspects to the story to attract more believers among the pagans.

So there’s many questions about the gospels. First of all, we don’t know who wrote them. Secondly, it’s clear that they were probably patched together by a committee, if you like, even the first gospel, and that later ones were just copying parts of the first one and elaborating a little bit more on their own. And then many writings later from 200 A.D or so are clearly just going off the original sources.
And if you look hard for what is the evidence in other extant literature around the time Jesus would have been born and raised and so on, there is practically no mention of anybody like that. In fact, there’s none. All the Romans that were writing, all the Greeks who were writing at that time, all the Jews that were writing, such as they were, gave no mention of any Jesus. Nothing big going on down there in Judea.

And it’s clear that at that time in Judea, there were many claimants for the role of the messiah and there many religious mystics who had other stories and miracles to tell besides the ones attributed to Jesus. Moreover, the mythological elements, the reports of miracles, that are in the Jesus story had appeared for years and years in other cultures. The stories about creation or about famous gods in Egypt or in Persia about whom the same claims had been made, the divine birth, the divine mother who had this divine child, about his precociousness in interpreting the holy scriptures of the cult, about his being persecuted and crucified, and about his transformation into the spirit world, rising from the dead. And, of course all the miracles the god-like person performed. All of those mythic elements show up in other cultures, in other religions, or other pagan belief systems years before Jesus allegedly was born. So it’s as though those various pieces of pagan myths were just sort of selected to compose the string of stories about Jesus.

So there’s a real debate between whether Jesus was ever a real historical person. Some historians say there was no Jesus, it was also an overblown conglomeration, and, therefore, there’s no God. There’s quite a little group of biblical historians who argue for that position. But then there are others
who say, “Oh, yes, there was a real person named Jesus. It’s just that we know very little about him. And even if you discount the miracles and the mythological elements and the resurrection, etc., he was a person and he was the promulgator of many useful ethical principles. So let’s treat Jesus as like Buddha, a provider of ethics,” and so they continue arguing along that line.

Excuse me. I’m getting off. I wish I could remember more about what it is I read, because there’s three or four biblical scholars that I have read and find really quite interesting.

**Hartwig:** Did you ever explore religion or religiosity in terms of experiments?

**Bower:** No. No. I wouldn’t know what to do. I guess you could do it in terms of whether these people are already susceptible to delusions, illusions.

There’s an anthropologist here who studied religious experiences. Tanya Luhrmann. Tanya Luhrmann’s a friend of mine. She’s an anthropologist psychologist. She’s the wife of Richard Saller, who’s the dean of H&S. Tanya does very interesting field studies, and the particular ones that I was interested in are where she’s observing a particular religious evangelical cult called the Vineyard group. They’re highly religious, but they also place great emphasis on communicating with God, talking to God in prayer, of course, but also conversing with God, and they believe he is answering back. That delusion is kind of interesting to Tanya and me. And what Tanya has done is interview many of these people about how they became enlisted into this community and into the beliefs that these people have, and, in particular, how they learn to pray and how they learn to interpret signs in their
environment and in their daily life as the Lord is answering, talking, giving them clues in answering their prayers.

And as a psychologist, you can just see the kind of group pressure exerted on new members of the cult, because they will often pray in groups. For example, a group will pray that Daniel will be able to believe in their doctrines or that Daniel will be able to pray effectively or Daniel will be able to hear from God. So there’ll be a lot of group pressure and a lot of modeling of how to pray properly. They really get into it big time. Some entire days may be taken up in praying and seeing things that happen around them. The pray-giver may meet people and see [things] on the TV that they interpret as signs from God answering their question.

And I wanted to know practical advice, like, “Does God tell them where to invest in the market?”

And, “No, of course not. That’s too mundane.” [laughter]

So Tanya has been studying some of these people, first in Chicago, but now she works with a Vineyard group out here in Palo Alto. She has enlisted some of them to do a kind of ritual practice and ways of training one’s mind, ways of imagining things, ways of obsessing about a particular biblical passage, ways of imagining oneself talking to Jesus or to God and so on. And you look at who among these participants eventually becomes very proficient in talking with God. One of the hypotheses that Tanya and I have discussed was that the people who are very good at conversations with God also have very good imagery. We know that people differ greatly in ability for imagery, vividness, and controllability of imagery. Psychologists have been studying
that topic for quite a while. And so we thought, the people who can develop
good conversations with God differ in their image ability, those who can
really get into it and who come from the training as believers and so on.

Since I’d done a lot of work on imagery in memory, Tanya and I said,
“Okay, let’s try having these people learn some things using imagery. Maybe
they ought to be much better.” And so we did an experiment of that kind
with these people.

Hartwig: And what were the results?

Bower: [01:05:57] Oh, they’re much better at memory for imagined scenes, of course.

[laughter] They’re much better with things that they learned in imagery than
people who are not good imagers.

Hartwig: Very interesting.

Bower: [01:06:09] So that’s as close as I ever got to doing an experiment on religious
people.

Hartwig: All right. Let’s change gears a little bit. We’ve talked a little bit about your
evolution as a teacher. Let’s talk about the evolution of the university. So you
got here in ’59, and you described the department a little bit. Maybe talk a
little bit about Sterling and Terman at the time and what was Stanford like in
1959.

Bower: [01:06:40] I didn’t know either Wally Sterling or Terman. I think I might have
met them once socially, but I can’t remember having a conversation with
either of them. I know they were exceedingly important administrators in the
late fifties, early sixties, but they were not on my radar screen very much. I
can’t even remember who was dean. I’ve forgotten. He was not on my radar
screen. That wasn’t until later when Bob Sears, who was head of psychology, went into the deanship, and then I knew him, of course, but I don’t know when he went into administration.

**Hartwig:** What was the mood, or what was Stanford’s reputation in 1959?

**Bower:** [01:07:47] I think it was kind of considered to be a moderately okay school. Some cynics from the East Coast looking out here would say, “Oh, well, Stanford’s off of the radar screen.” People at Yale and Harvard and NYU would say, “You know, they’re just surfing and drinking Rum Collins and taking it easy out there.”

But in fact, once I got here, of course, it wasn’t like that at all. Everybody was pretty hopped up about doing their scholarship well. There was still in every department some dead wood, some old people who had just been around a long time and who had to be retired off. But new people were brought in who were pretty good, they sort of set the standards higher so faculty and students you got were better and better. Those who weren’t very good got washed out and sent away.

I don’t know much about the university from my early years. I was so wrapped up in what I was doing in my lab and in my teaching in psychology, I didn’t pay much attention outside. I mean, I went to some football games, I think, but that was about it.

Then in the seventies, all the building started, and all the money came rolling into Stanford for building up engineering and physics, and psychology got a new building, Jordan Hall. Big new libraries were put in. Green Library
is now sitting where my old rat labs used to be. The Quonset shacks where I had my labs were near the coffee shop and Green Library.

**Hartwig:** What was it like during the campus disruptions during the sixties and seventies?

**Bower:** [01:10:20] Bad. Yes, very scary. It was bad in the sense that practically everybody was antiwar, Vietnam War. I don’t think I knew anybody who was in favor of the war at that time, and so we were very sympathetic with the students, particularly those who were being drafted. I remember I once offered some draft-able men some biofeedback training to control their blood pressure for when they went to the draft physical, so that they could flunk the physical. [laughter]

**Hartwig:** Did it work?

**Bower:** [01:11:15] Only one came to do it, and I don’t think he used it when he went through. I think he claimed to be a conscientious objector, and he got out of the draft that way.

**Hartwig:** So as a faculty member, were you vocal?

**Bower:** [01:11:41] I wasn’t, no. I was not a major celebrity on campus ever, and certainly not then. My students knew that I was on their side and that the way to try to deal with changing Washington policy was through the ballot box. So I would do things like take my little card table out to White Plaza and try to get people signed up, voter registration, or hand out campaign literature for antiwar candidates, and they would see me do that and say, “Oh, good going, Gordon. Good boy,” man. So that’s about the only thing
they saw in me. They knew I was antiwar. In fact, our opposition was openly
discussed sometimes with groups of graduate students.

We were very upset by the violence that went on around the Stanford
campus, by the sit-ins that occurred at the electrical engineering department.
Students were upset about the DOD funding of research on surveillance,
radar stuff, that was at that time being sold to the Iranian Secret Service, the
SAVAK, doing surveillance of the revolutionaries in Iran. And I thought that
U.S. policy was pretty bad, and so I supported some of those sit-ins. Some of
my students would go to those sit-ins. I didn’t go, but I knew they were
there, and I would talk to them about it and say, “I’m supporting what you
do. Just don’t wreck the place.”

And I was in favor of protesting some of the military research, the
DOD research, moving it off campus over to the Stanford Research
Institute, SRI. At one time--I forget what year--I was on the University
research committee that oversaw where the research was being done on
campus and what kind of research could be done here. So in one sense, I was
sympathetic to all the EE people who were doing research that didn’t have
military applications. Some clearly was being funded only because it had
military applications, and that I thought they should just get off campus to
SRI or wherever DOD wanted to do it.

I’m trying to think of what were the other issues at that time. One issue
was whether or not, say, a graduate student who was working with a given
faculty member on a research assistantship, could opt out of working on
military research with that professor. The professor could obviously tell the
student, “I got money from DOD to do this research, and I’m giving it to you. So you do it or get something else.” I remember that was an issue, and we tried to figure out a way to do sort of nice funding so nobody got too upset in that conflict.

But I think things occasionally got out of hand, like there was a firebombing of one of the faculty members’ houses; thank goodness it didn’t succeed. Scared the hell out of everybody, someone threw a Molotov cocktail up against Sandy Dornbusch’s home. Sandy was on some powerful committee at the provost’s level at the time.

I used to go to the mass meetings that the Students for Democratic Action and the Venceremos group would hold with the board of trustees and observe the trustees being pilloried and taking all this horse manure that students were shouting at them. And many of the trustees were pacifists, too, or some were industrialists like David Packard or Bill Hewlett, and you kind of felt sorry for them, but you felt sorry for the students as well because they were going to be taking the brunt of military service.

I tried very hard to point out to them that going on strike from classes is just cutting off your parents’ nose. Your parents paid for your university classes, and your absence isn’t going to have any impact on the Department of Defense or on the conduct of the war. So I was very much against those kinds of protests.

Hartwig: How do you think Lyman handled these difficult years?

Bower: [01:18:03] I think Lyman did a good job actually. He was good. He got beat up pretty bad now and again because he’s in a very, very touchy situation. He
believed in freedom of speech, letting students have their say, letting protests happen, but don’t shut down the place because some students wanted to go to class or hold classes, and many faculty wanted to do their research. So Dick was, I thought, very good at that.

They then brought in a guy named Ken Pitzer as university president. He only lasted a year, and the antiwar people chewed him up very rapidly.

Hartwig: Were faculty against his appointment?

Bower: [01:18:58] Some people were. I guess they were upset by Pitzer. Lyman upset some people. I wasn’t one of them. I thought Lyman did as best as could be done with what was a very fraught situation, anxious, and very difficult situation to go through.

It’s interesting; one of the groups at that time was Venceremos. I think it was a Latino-sympathetic group. But an aside, I go out walking now days. One of my buddies and I go walking. He at that time was a policeman with the Palo Alto Police Department, and he was an undercover guy observing goings on at Venceremos. [laughter] And also with the drug culture, undercover. He is Mexican, so he could speak Spanish. He was kind of raised on the streets, and grew long hair and a beard and had the whole hippie protester persona at the time. He tells me interesting stories about those times.

Hartwig: I bet. [laughs]


Hartwig: So let’s talk about something, picking up a little bit on Lyman, so maybe give us your impressions of Stanford presidents and provosts over the years, how
effective have they been, or what have been some of the major changes as a result of their leadership.

**Bower:** [01:21:03] I think Don Kennedy was a great provost, and I think he was a great president, too. The shit hit the fan when he went to Washington to have to deal with the indirect-cost-recovery business, but that would have hit anybody in that position.

**Hartwig:** And what was the faculty perception at the time regarding this?

**Bower:** [01:21:29] People in humanities thought he was terrible, and people in the sciences who were paying into and living off of the indirect-cost-recovery funds from research grants were reasonably happy about him. As always, you could do Monday-morning quarterbacking after the fact and say, “Well, he should have been talking this way to that congressional committee rather than that way,” and so on.

But I think the anxiety levels were so high at that time that it would have been hard for anybody to do well, and it was easy for whatever that congressman was to make political hay out of Stanford’s owning a yacht that some donor had given us and we hadn’t yet sold and so on. But Stanford came out of that reasonably well.

We all got to know our governmental research grants officers quite well and tried to coordinate with our grants officers so they wouldn’t get upset with Stanford. I would sort of separate what was happening in Congressional hearings from what was happening with my local grant and the overhead I was paying Stanford on my grants. I was never a big grant getter. I always had grants, but I never received a lot of research money. I might have money
for my summer salary and for maybe one or two graduate students, a little bit of money for the computer programmer in the department and for my secretary. But compared to physics and engineering, mine was a peanuts kind of grant, and that's where I always kept it. I never wanted to administer great big grants.

But back to what faculty thought about it, I wasn’t able to know much of those things about Stanford's administration. I think I just didn't know. I never talked about it very much with other faculty, so I’m a very poor observer and respondent to those matters.

**Hartwig:** Well, talk about your time as chairman and then talk about your time as associate dean.

**Bower:** [01:24:02] Being chairman was easy. What year was it, '78 to '82 or something like that?

**Hartwig:** I think so.

**Bower:** [01:24:11] Seventy-nine to '83, something like that. I know I didn’t want to do it because I thought it would slow down my research, and it did slow me down some. I couldn't spend quite so much time with my graduate students. One of my first rules was to not use the chairman’s office for anything.

[laughter]

**Hartwig:** Why?

**Bower:** [01:24:46] I didn’t want to put that spatial distinction between me and my faculty.

The other thing that I did was to set it up so that there’s two obligatory faculty meetings every year, one at the beginning of the year when we kind of
get together and count heads and say, “Here we all are. Aren’t we happy?
Let’s have a party,” and there’s another one at the end of the year where you
have to evaluate each of the graduate students and say how he or she’s doing.
What work and papers and experiments have they done? What have they
found? How’s their progress towards their dissertation and getting out of
here? Do you want to keep them? Who will work with them, if not you? And
so on. So those were very important meetings.

We also had faculty meetings if we were appointing or promoting
somebody. So all those meetings were obligatory, and so obligatorily we had
at least two faculty meetings and maybe three or four if we considered
promotions or new appointments. What I did as chairman was to say,
“Those are going to be the only faculty meetings.” [laughs]

Hartwig: But what about issues that came up? How were those handled?

Bower: [01:26:20] In the hallway by small groups of faculty, whoever had a stake in
that issue, if it was an issue. There would be things like what computer do we
buy now for the department. There were only about three people who knew
anything about that, so those three people would get together with me and
we’d talk about it.

Hartwig: Were there major issues that came up during those years?

Bower: [01:26:54] No. It might have been my slovenliness not wanting to do much.
There was an Undergraduate Studies Committee that would kind of discuss
course requirements or the like. There was a Graduate Curriculum
Committee, and they would decide what it was that would be done, and they
would talk to other faculty members, people on that committee, and say,
“We’re thinking of doing this. What do you think of that?” And then they’d get back together and chat and do it or not.

Maybe I’m exaggerating. Well, I don’t think so, not very much. So we didn’t have very many faculty meetings, and when we had a faculty meeting, it was for a real purpose, not just to hear whatever somebody’s worried about. So that was another policy I had.

A third policy I had was to take some of the people who were not producing very much and try to get them more involved in writing research grants and getting money to do research and pay for graduate assistantships. So I helped one person write grant proposals for himself and his group, because he had been out of funding for three or four years. And believe me, for a research scientist, if you lose funding, you drop out very quickly, and it’s very easy to become a piece of dead wood and not produce anything. So it’s very important to try to get those people back up on their feet and sending in grant proposals. Sometimes it’s not on the original issue that they wanted to work on.

I remember this one man had not had funding for two or three cycles of applying, and I went to him and said, “We’ve got to try to help you get something. Let’s do something else.”

He said, “No, I don’t want to. I want to do that staff I applied for. I want to do it that way.”

And I said, “Well, you aren’t looking too good in that regard. You aren’t getting the expertise you need to do that kind of work.” But he didn’t want to change; he wanted to keep butting his head. So I said, “Well, okay,
I’m going to help this other person,” and I did. And the first guy just faded away.

Hartwig: And as associate dean?

Bower: [01:30:02] One of the ways I got into the dean business was by my working on the Appointments and Promotions Committee of Humanities and Sciences. This was, in one sense, the most powerful committee in H&G. They look at all the cases of tenure promotions and appointments to the tenure line that come to the H&S team. All the departments in H&S would have their faculty meeting and might vote to give tenure to a faculty candidate and send the case up to the Dean’s Office. The Dean would hand it to H&S and say, “What should we do about this candidate? Do you think he or she is okay or not for tenure at Stanford?”

And most of the time, we would look it over and say, “Yes, it’s fine.” But we worked very hard on every case. We’d usually look at about five cases every two weeks, and we had to read all these thick files and try, insofar as we could, to read what the person was studying and doing and get a sense of how we evaluated their publications, read all the outside and inside letters. If you wanted to, I would call other people around the country to ask about this person, and so on.

You very quickly established very high standards for what ought to be the achievements of somebody who’s going to become a tenured faculty member at Stanford. Some departments almost had a reputation of being super clean, super neat, and great, and everything they put through was just squeaky clean and most excellent. Other departments were just the opposite;
they would promote anybody and everybody who was warm-blooded. But most other departments were in the middle. So you sort of quickly learned, by yourself or from others, to identify the departments that you have to look at very carefully. Unfortunately, some of those were in the humanities. And all of us thought, incorrectly of course, we could read humanities, “Why hell, it’s just the English language. It’s not mathematics or physics or molecular biology. We could read that.”

So we would get stuck a lot on cases coming out of the humanities and history, political science, the softer social sciences, because we could read it and evaluate it and ask, “My gosh, is that really that important?”

And occasionally the dean would have to say, “Enough, already. Come on. We have to have a department of x. You can’t throw everybody out.”

We would occasionally turn down some candidate, and the chairman or a couple of the big shots of the department would come to our A&P meetings and harangue us about what a bunch of crumbums we were to think we knew more than they did about standards in their field, etc. [laughs] Those were always fun to go through.

Occasionally the dean would almost always take our recommendation, once he had tweaked us a little bit, and the provost and his committee, the Executive Committee of the Provost, they almost always took our recommendations as well, because they knew we were guardians of principle at the gate, trying to keep down the riffraff and barbarians. But occasionally we would turn somebody down and the provost would step in to reverse us for what I would say were largely political reasons.
**Hartwig:** Was there ever the occasion where it was the right choice, looking back, or no?

**Bower:** [01:35:20] No comment. No comment.

**Hartwig:** So how long were you on that committee?

**Bower:** [01:35:31] I was on H&S, I don’t know, about six years, and then I was an associate dean for about three years. Let’s see. One of the things I didn’t like about being a dean was having to lie.

**Hartwig:** About what?

**Bower:** [01:35:52] Occasionally one of the faculty in one of the departments would get a great offer to go someplace. Duke wants this comparative literature professor or Yale wants this Greek professor, and they’re offering him beaucoup dollars, so we have to really respond strongly to that offer, “Dean, give us money or give us something to give to him, like a new position he can appoint. And you also, Mr. Dean, have to tell him how wonderful he is and how important he is to the lifeblood of the department and the university.”

So I’d read the person’s work, and I was sometimes not too enthusiastic about it, and I would kind of internally say to myself, “I don’t care if this guy goes to Podunk University. I don’t care. But I’ve got to go talk to him and tell him how wonderful he is and how wonderful his life will be at Stanford if he stays, and we’ll give him more money,” etc., etc. And I had to do that fairly often, and it kind of galled me. I was never allowed to say, “I think you’re crap. I wish you’d leave.” [laughter]
**Hartwig:** Well, why did you have to do that? Couldn’t you bring in somebody else or somebody better?

**Bower:** [01:37:45] I was the dean in charge of that particular department, and the assistant deans divided up the programs in the departments, so those were our fiefdoms. I had about fifteen of those programs. A couple of them were these small temporary interdisciplinary programs that got started in the sixties to satisfy the revolutionaries saying, “We want relevance, relevance here in our education.”

And these programs--I will not mention them--had become moribund. There was nobody--faculty or students--who much wanted to work with them. A few administrators were diehard workers with them, or a few people had their appointments with them and required those programs to keep going. And so we’d say, “We’re going to close you down. At the end of next year, this program is over, and any faculty will be moved someplace else, put you in some other department. Or if you don’t have tenure, well, you can be an adjunct in humanities. Or we could just say goodbye.”

All you had to do was threaten to shut down a program, then the people who wanted to preserve that program would write to everybody who was a friend of that program, every graduate of that program who thought it was wonderful, and they would try to mount a very strong resistance movement to keep it going.

And we deans used to say to ourselves, “It’s a very weak program indeed that cannot mount fairly strong support for its survival.” So you have
to go on your own judgment about these things, plus the enrollment figures and what’s become of the students who went through that program.

That was never any fun, shutting down programs and firing people, but it had to be done, because Stanford’s H&S was accumulating a lot of deadheads on the fringes of the major departments.

[End of Session Five]
Hartwig: This is Daniel Hartwig. Today is September 18th [2014], and this is the sixth session with Professor Gordon Bower. Welcome.

Bower: Yes. I wanted to say a few words about Stanford and my time here. I consider Stanford one of the truly great universities of the universe, and it has become great over the last fifty years or sixty years. It has been just an absolutely marvelous place for me to have had my career, to do research and to teach here.

The department I’m in, psychology, has been rated number one in the world for many years. I don’t know if we still are, but we’re always very close to number one, and we’re kind of number one in the particular area I was working in, which was cognitive psychology. So we tended to attract some of the most brilliant students going into graduate school in psychology, and many of them became independent thinkers by the time they were in the second or third year of their graduate training. Such students were a delight to work with, and it was wonderful just to sit and think about experiments with them, make up new experiments to do.

The university, I think, has played such a unique role in the development of Silicon Valley. Of course it has. I mean, in terms of Provost Terman and Bill Shockley encouraged many EE students to start companies, and a number of people went out from here and set up information technology, computer, and hardware companies--Hewlett and Packard, Varian, and Fairchild Semiconductor. Many of the top EE people at
Fairchild Semiconductor Company left to set up one business after another, from Cisco, Intel, Apple, Sun Microsystems and so on.

My Stanford cohort has been a recipient of the treasures that were made available by this development in Silicon Valley, usually by Stanford graduates. I think that milieu, that intellectual atmosphere of creativity and development and the dream that we’re on the cusp of great new things here, just permeates the whole Peninsula intellectual community. That dream and beliefs even trickle down to psychologists and certainly to our graduate students. They jump right into the flow of things.

I think one of the nice things about the university is when I came they had a lot of land that they could develop for faculty housing. A faculty member could lease a plot of land nearly on campus and build a home on it, which I did in 1964 when I achieved tenure. That home makes you very close to your campus office and to the community here so that you can take advantage of the cultural developments and intellectual events going on there—lectures, workshops, art, music, exhibits, drama, dance, Gilbert and Sullivan plays, and the like. And, of course, athletic competitions.

As you know, Stanford has been an enormously successful fundraiser. I think we’ve set records for fundraising in university campaigns, and all those funds have gone not only to develop professorships and endowed chairs, but also to develop the cultural institutions here, to develop the music school, the science and engineering buildings, new libraries, medical school, the new concert hall and athletic facilities, of course. One thing we still are waiting to be developed here is a major theater stage for putting on live theater. We
have limped along with Memorial Auditorium and with the Little Theater, and the Pigott Theater. The first is huge and the others are very, very small, and it would be nice to have a mid-sized performing arts theater or stage someplace, but Stanford has not yet constructed one with all of their gracious money.

I think being able to live on campus, being five minutes away from my lab, my office, and where I teach, has been a great benefit for me. My family lives inside in this bubble, which is just very endearing, very warm and creative. The community has a very low crime rate, very little hassles from protests and the like, so scholars can really concentrate on your intellectual work and development.

**Hartwig:** Do you think proximity, especially among different disciplines, different faculty, having all of the schools within relative proximity to each other, do you think that was a contributing factor to this innovation?

**Bower:** [00:07:20] It certainly was in my case, and I don’t know about others. People in the engineering departments at Stanford had wonderful relationships with the hardware and software companies in Silicon Valley. Those companies would pay Stanford to have some degree of ongoing access to the professors and the graduate students doing research here in engineering, hardware, computer hardware and software.

I think the same thing happened for molecular biology and biotechnology, with the development of places like the medical prosthetics laboratories and the Bio-X research facility here. It gave kindred faculty and graduate students here access to the research and development arms of these
companies, like Google, Cisco, HP, Varian, Xerox, Apple, Firefox, etc., where there was a very smooth interchange going on between our students and professors and the R&D departments of local companies like Xerox PARC. PARC was one research venue that I knew very well.

When I was chairman, I set up a liaison connection with Xerox Palo Alto Research Center, which hired many cognitive psychologists, but also people in applied artificial intelligence. My students and I got very interested in their doing applications of cognitive psychology to solving computer interface problems. This is, how to design an interface with software that people would be able to, say, use the computer or understand a computer program or software right away with only minimal instructions. Those would have to be written up and presented in a certain way to achieve that end.

Also I think the people working at Xerox PARC got a lot of benefit from our graduate students who went over there to do internships or do some research. I know two or three of my students did their PhD dissertations with some employees from Xerox PARC being on the dissertation committee. I was the nominal thesis advisor, but we’d have one or two scientists from PARC on the dissertation committee, or sometimes just occasional outside nonacademic advisors. Nonetheless, their scientists were very substantive people to have around.

I think those kind of relationships have developed more fully in the School of Engineering and computer science departments here and have been very important in helping develop entrepreneurs here in the business school, in the engineering school, and also in biomedical research programs.
Hartwig: Has that entrepreneurial spirit translated or been taken up by the social sciences and humanities? Maybe describe the relationship between science and engineering, on one hand, versus the social science and humanities over the years.

Bower: [00:11:39] No, is the most direct summary. A few psychologists have moved into the human factors branches of places like HP and Apple and Google and Sun Microsystems and Facebook. Each of these companies have human performance sections into which psychologists go and work. The psychologists themselves have not become entrepreneurs. They don’t set up new companies, I guess because they don’t have the underlying expertise to do so, nor the content to set up an ongoing, thriving company. Several of our ex-PhD students have set up consulting firms, and consult with information technology companies or with just any company or organization out there in the world that will hire them, Proctor & Gamble or whatever.

Hartwig: Psychology was pretty strong at the time you arrived, and it’s continued to be strong. What are some of the factors behind that success?

Bower: [00:13:23] I think it’s partly because we do not have a clinical psychology program. We have a straight research-based program. We got out of clinical psychology in the early sixties, because it tends to produce a bit of a dumbing-down; that is, clinical programs produce a dumbing-down of the graduate academic research component of a department. It’s very lucrative for departments to have a clinical program. However, you have to deal with practitioners who are not very interested or good at doing research, and
therefore, they don’t get much research funding from the government. But anyway, so that’s one factor.

Hartwig: Was that a difficult decision, or controversial at the time, to give up clinical?

Bower: [00:14:34] No. We had only a miniature clinical program in the early sixties. It mainly relied upon temporary appointments or arrangements we would make with clinical psychologists at the VA Hospital, out on Junipero Serra or in Menlo Park. Those people would come in and teach the clinical-oriented courses on mental testing or doing psychotherapy. The department itself had only a kind of skeleton crew of actual clinical appointees on the faculty here, usually one or two assistant professors and an associate professor. There would be a turnover about every two to four years because they wouldn’t or couldn’t produce research papers, so, of course, they’d be terminated. So that became kind of a revolving door for assistant professors in clinical.

The American Psychological Association, which was administering accreditation of all American clinical training programs, was putting a lot of pressure on the Stanford department to make more tenure-level appointments for clinical training of clinical psychologists. And the powerful senior members in our department basically said, “No, we don’t want to do that.” People like Quinn McNemar and Leon Festinger were completely unsympathetic to strengthening or continuing our minimal clinical program, and I think they just said, “Let’s get rid of it.” So there was a vote of the senior faculty in the early to mid-sixties to shut it down our clinical program, and so we did.
Another thing we did to make this a good research training department was that we abolished all graduate student prelim exams, major comprehensive written exams to qualify for beginning work on a dissertation. We discovered that those exams made students anxious and took them away from productive research. The exams were indeed onerous, students would spend three to four days just writing the exams. If they flunked it, they’d have to do it again in the next year, and if they flunked it too many times, they’d be thrown out of the program. We discovered that students got so anxious and uptight about those written prelim exams, they were cutting into the student’s research time and productivity. After all, what we really wanted to do was develop experimental and theoretical research students. So we did get rid of prelim exams, but required students to spend a lot more time doing and handing in experimental research, and I think students just took off with that kind of program.

Hartwig: Was that met with any resistance by the administration or the school?

Bower: [00:18:17] No, that was all done within the psychology department. I don’t know what other departments did. So with your students doing research, two or three experiments a year, the professors had to keep on top of their work and keep feeding them information, feeding them ideas, and talking to them about research ideas and so on. You taught students how to assemble a research program. I would say, “Think of your research like a conveyor belt in which you have different objects or projects that you’re developing and working on going down the conveyor belt,” and you’re just doing a little bit at each point, developing an idea, getting your equipment ready or getting the
subjects, doing some pilot-testing, running the complete study, analyzing the thing, and writing it up. In all those stages in a conveyor belt, you’re mainly working on your own ideas. We taught them basically how to be productive, how to organize their work-time to be productive. They don’t sit around in a beer hall just thinking and waiting for some creative idea to hit them. They should think along with their experimental work. Anyway, excuse me, I’m getting off the track here a little bit, but this is partly, I think, how we remained a top-rated research department.

We also had pretty good judgment about people we hired. We tended to be very selective in hiring assistant professors, some of whom we’d promote. Other of our top faculty we’d just hire at the top level. So at one time we had lots of the psych faculty as members of the National Academy of Sciences. Estes and Hilgard, of course, and Bob Sears, and Leon Festinger, and then later it was me, Roger Shepard, Amos Tversky, Dave Rumelhart, Eleanor Maccoby, John Flavell, Brian Wandell, Claude Steele, Lee Ross, Carol Dweck, Hazel Markus, and Ellen Markman, so on.

The National Academy is about the highest honor a psychologist can get. A few of us got other awards, but it’s a big deal for a psychologist to get elected into the National Academy, and at one time we’d have about half or more of our faculty as elected members of the National Academy. I think when a lot of our Academy faculty members retired, there was a momentary lull in the development of the department. Eleanor Maccoby retired; John Flavell retired; David Rumelhart got Alzheimer’s disease and retired; Amos Tversky died; Roger Shepard retired; I retired. So the average external
recognition of the department went down there for a while before we got replacements like Wandell, Steele, Dweck, Ross, Markus, and Markman.

I don’t mean to brag about the department, but we had a pretty powerhouse line-up of very famous researchers, and they were famous in the right way. They just were very productive of good research papers and they were going to national conferences and giving research talks everywhere and setting the agenda for the field. Those were very important activities for us to have done. Many of us also became elected leaders of professional organizations, of the American Psychological Association, of the American Psychological Society, the Cognitive Science Society, and so on. Yes, so I’m bragging. Excuse me. I shouldn’t do that.

Hartwig: That’s okay. There’s a lot to brag about.

Maybe talk a little bit about Al Hastorf. What was his role as chairman and then as provost, and how did that translate maybe to the success of the department?

Bower: [00:23:30] Al was a terrific administrator. He wasn’t a fabulous researcher, wasn’t all that productive, but he was very good as a research advisor and chairman. He was hired in the School of Business, and with a part-time appointment, I believe, in psychology. When Bob Sears, our executive head, moved into the H&S Dean’s Office, they convinced Al Hastorf to come and take up the chairmanship in psychology. He did and he was good. He had a good nose for academic talent, so he could easily get on board when we were hiring stars, like Phil Zimbardo, Lee Ross, Roger Shepard, Amos Tversky, and Bill Estes. He was involved in most of those decisions, and so he was
able to hire into the department very good researchers. Good researchers tend to hire other good people because they have high standards, and so the cycle it just feeds on itself. Hastorf also seemed to have been able to foster a feeling of collegiality and friendship among the faculty. I think that was partly because we socialized together because most everybody lived on campus or near campus in those days.

When he was the dean of H&S, Al was very good. I didn’t pay much attention to that job at the time. I can’t remember the order in which he was the dean versus Halsey Royden. But anyway, my experience with the upper administration started when I was on the Appointments and Promotions Committee of H&S, and that’s when I first noticed the dean as somebody I dealt with on a weekly basis, and that was with, I believe, Halsey Royden.

**Hartwig:** Did you continue to work with or consult deans after that?

**Bower:** [00:26:25] Oh, when I was chairman, I consulted with primarily Norm Wessells, who was the big H&S dean at that time. We consulted about appointments that psychology wanted to make or about somebody we wanted to fire or advance in rank. I became an associate dean when Norm Wessells was our big dean. I had been on the A&P Committee, and I guess the deans liked my judgments about academic talent, my standards and way of reviewing cases for promotion and my work habits on the A&P Committee.

When I became associate dean, the other associate deans were Bill Chace, David Kennedy, and Carolyn Lougee, from history. They were good folks to work with. I got to know them quite well. Each one of us would
have about fifteen or so departments and programs that we had to oversee and take care of.

**Hartwig:** You mentioned some of the programs that you had to evaluate and were cut. Were there other issues in terms of administration during that time?

**Bower:** [00:28:14] Oh, we had to deal with cuts and budgets, budget cuts. The Provost would say, “You have to eliminate 4 percent of the budget from H&S.” Where are we going to take it from, and where’s there any fat? In those days, if you vacated a professorship, someone left or retired, that professorship tended to go back into the dean’s pool, and the dean and his committees would decide which department got that professorship slot to fill.

**Hartwig:** It didn’t stay with the department?

**Bower:** [00:29:03] They tended to have an advantage in getting it back. Of course, otherwise you would have had widespread revolution. But we always could say, “No, you don’t deserve that. We’re going to give that to this other department or program that we think is developing well and needs growth possibilities. So we’ll give it to them instead of back to you.” That, of course, would create some hardship and animosities within the department that’s lost something, and so as dean you had to deal with that.

**Hartwig:** Was there one that was particularly tough, or, looking back, you may have made a mistake or you were definitely maybe justified in how it turned out?

**Bower:** [00:30:04] I don’t know. I didn’t pay attention after about three or four years. What you need to know is, well, what happened fifteen years down the line.

**Hartwig:** Exactly, yes. [laughs]
Bower: [00:30:17] Sorry. I should have kept track, you know, but probably somebody kept track.

Hartwig: Well, on the subject of administration, maybe talk a little bit about, as a faculty member or as an outsider, the styles or influence or changes that, say, the presidents and provosts effected on the university at large scale but then maybe within H&S.

Bower: [00:30:48] The simplest answer is I don’t know because I didn’t pay much attention to what the president and the provost were doing unless it involved tenure-line appointments in the psychology department. So I had very insular interests, shall we say. I never was interested in going to the Academic Senate, for example, and voting on things. I was always quite humbled by the fact that the presidents we had were usually very good at rounding up money from donors, and, of course, as a faculty member, I was the beneficiary of some of that money that they would bring in.

I was very much in favor of the Administration developing these programs to bring Stanford faculty and graduate students into closer connections to information technology companies. I saw that as a great benefit to this university, not directly to psychology but a little bit for even psychology. Some of our presidents have been very good at developing those connections. I know John Hennessy is especially good at it, maybe because he came with that IT business experience as a professor. I remember John when he came up for tenure. Back in the old days, computer science used to be in H&S, Humanities and Sciences, and I remember John’s dossier going through our A&P Committee, and at that time, he was primarily a hardware
engineer, but, you know, he went through like a greased pig. He was so obviously very talented and productive.

But even then, I think John was starting his MIPS company and doing well at it, and so he had one foot outside of Stanford, so to speak, as he went through the ranks here. It was very easy for him to talk to people outside of Stanford and to get them interested in programs that he and his administration wanted to develop, not only in engineering, but also in the business school or education school or H&S and the like.

I think Etchemendy has been doing a very good job as provost. I like John and I knew him fairly well before he became a provost. I knew him as a professor in philosophy. I was interested in the work he had been doing on writing computer programs for teaching logic--he's a logician--to Stanford undergraduates. I knew about that research and work that he was doing, and I kind of admired it from a distance. So I was surprised he became provost. I didn't know he had those administrative interests, even, because I don't recall that he was ever chairman or had served in the Dean's Office. I think he just got elevated into the provostship after Condi Rice left.

I knew Condi Rice just slightly because I was in the National Academy, so they'd drag me out with other Academy members on occasion, and the provost would be meeting us or having a party for us and so on. I was on the A&P Committee when the promotion for Condi Rice went through. She was in political science. So we got to know each other. For example, when I got the Presidential Medal of Science and was at Washington for this big award ceremony, in which several of us received our medal personally from George
Bush, the son, Condi came to that ceremony and afterwards we talked a bit about Stanford.

**Hartwig:** Well, let’s pick up a little bit, then, on some of the awards. You mentioned the Presidential Award of Science, 2005. You were also president of the American Psychological Society from ’91 to ’93. Maybe talk a little bit about that.

**Bower:** [00:36:49] Being in the American Psychological Society?

**Hartwig:** Mm-hmm.

**Bower:** [00:36:52] APS was a splinter group that broke off from the American Psychological Association, APA. I had been the chairman in APA’s Division of Experimental Psychology, and was also on the Board of Scientific Affairs and other committees with APA in Washington, but I tended not to be at all interested in going any higher in APA.

In the late sixties through the early seventies APA was growing by leaps and bounds, but all in the clinical psychology area. More and more clinicians joined, but fewer and fewer experimental academic psychologists were joining APA. As one consequence, APA had to become more concerned with professional psychology issues such as insurance payments for psychotherapy, or getting privileges for giving drugs to patients since psychologists are not M.D.’s, or can we set and enforce licensure standards, how to qualify for a license if you’re in Montana, Hawaii, and so on. Well, you have to set up a Hawaiian branch of APA. So that grew to all these state branches of APA developing, and each of them got votes in the APA House of Representatives.
So this unchecked growth in Divisions just sort of overwhelmed our small group of academicians. Divisions were also created for minority groups, for gays, for military, etc. Of course, this led to more and more disheartened feelings of alienation among the academicians, and so there were various attempts over the years to set up other groups, like the Psychonomic Society, composed of strictly experimental psychologists and neuroscientists. That group split off from APA in the sixties, and I was one of the people involved in that early split. But the Psychonomic Society was always a very small organization, a couple hundred, growing to maybe a couple thousand, and was very centered around experimental psychology—sensory, learning, animal learning, neuroscience stuff. So a large number of people who were social psychologists, personality researchers, industrial psychologists, consulting psychologists in education, etc., they were excluded from that, and so they decided, “We want to have our organization, too.”

So starting in, I think, the mid- to late 1970s or maybe it was in the 1980s. I’m very bad on dates. I’m sorry, I just am. It would have been in the eighties, a group of people who had been in the APA became so disaffected that they split off and established the American Psychological Society, APS, and asked everybody to join, even clinicians who were doing research. They asked me to get in on the founding of it. Although I was not one of the rebellious kind who left APA to join and start up APS, I was friends with all of those rebels, and they kind of dragged me along as one of the founding members of APS. I’m unsure when it started, but it was in the eighties, like ’85, ’83. We had our first meeting outside of Washington, I think, in
Maryland somewhere. I know it was on a college campus because we weren’t big enough to be anywhere else.

I remember our first board of directors, or whatever we were called, our founding committee meeting. We met on the lawn under a shade tree at this university, sat around having a bag lunch, talking about what are we going to do, what were to be the rules of the organization, what would be the requirements for membership, how do we see our development in coming years, etc.

Two of the leaders of that splinter group were Janet Spence and Jim McGaugh. Janet became our first president the first year, Jim for the second year, and then they got me after McGaugh, so I was the third president. In those days, they set up the elections so that you had a three-year cycle. You were one year as president-elect, one year as president, and one year as past president, so there was this slow handing off and continuity at the head of the organization.

That society just kind of took off and grew because it provided a home for not only experimental psychologists and neuroscientists, but also for academicians in social psychology who did research: personality researchers, educational researchers, people from the business school who worked in industrial psychology or in marketing or organizational research and so on. So it grew very rapidly and became sort of the voice of scientific psychology in Washington.

One of our major decisions was to have an APS office in Washington and to set up a lobbying arm. They were called advocates for science because
a scientific society cannot have lobbyists. We had advocates who talked a lot to Congress, to congressmen, about funding research in psychology and training programs in psychology. We were politically astute enough to know that you had to have a presence in Congress if you’re going to continue to get funding from the government for psychological research, for educational research, for neuroscience, and any other kind of research. You had to have a presence in Washington where you could advocate for funding for all of these interest groups and argue why psychology’s important to the national interest.

So APS grew. I, at the beginning, predicted we would asymptote around 15,000 members, which is a pretty small organization in Washington. But as our group grew up to that ceiling, we noticed that lots of people from Europe, Western Europe in particular, wanted to come to our conventions, and so we thought, “Okay. Well, let’s not make it an exclusively American organization.” So we changed our name to the Association for Psychological Sciences, APS. So we kept the same initials, APS, but now it’s called Association for Psychological Science, and we invited research psychologists from outside America. The Association immediately attracted large numbers of psychologists from Europe and Asia, particularly experimental psychologists and cognitive scientists and neuroscientists. So the group expanded greatly to, like, 25,000.

**Hartwig:** Now, at the time, was this causing shockwaves with APA?

**Bower:** [00:46:39] Yes, of course.
Bower: APA was very unhappy about this competing organization because they lost members, and APA became even more of a clinically oriented organization as its academic contingent dwindled. I kept my affiliation with APA because a lot of my friends were in it and I thought it was important. Why just have one lobbying arm? Have two lobbying arms, because psychologists need all the voices we can muster with Congress, I thought, and with the federal agencies: NIMH, NSF, and the Department of Defense and the Justice Department and the Department of Education and Air Force Office of Scientific Research, DOD, ARPA, etc. All of these places were sympathetic to psychologists coming and talking and lobbying and saying, “Yes, Department of Education, let’s get funding for your projects,” or, “Department of Health and Human Services, let’s fund...” What’s it called, the programs for preschool infants?

Hartwig: Head Start?

Bower: Head Start. There, that’s the word I was looking for. I’ve just exhibited my anomia. Head Start, yes.

Hartwig: Was there competition for dollars, though, between research and clinical practice?

Bower: Yes, yes, there always has been. So one of the things that we were always doing was trying to show how basic research feeds into clinical practice. In fact, in the 1980s, I was enticed into working for a year for the government at NIMH, the National Institutes of Mental Health. I took a leave from Stanford and went and worked for the government, for NIMH, and NIMH paid my salary that year I was there.
Hartwig: This was you were chief science advisor, is that correct?

Bower: [00:49:43] I was the chief science advisor to the director of NIMH. Fred Goodwin was his name. He was a psychiatrist, a biological psychiatrist, big on drugs for the treatment of mental illness. He and I would often have an argument about the efficacy of drug treatments because in those days I was reading all the negative evidence showing that many of the drugs of that era weren’t any better than placebos. Fred didn’t want to believe that, and so we had that argument periodically.

While I was at NIMH, I was asked to carry out a review of the role of and the contributions of basic behavioral research to the mission of NIMH, which centered on mental health. The way those things were set up is that our lobbyists--excuse me--our advocates in Congress would get some sympathetic member in Congress to suggest that we provide them with a report about this, and so the word would come down from Congress, typically a committee that was dealing with funding of research, requesting a report on what basic behavioral research contributes to clinical practice and clinical outcomes and so on.

So through that authorization, I set up a committee. I and some of the staff people at NIMH set up five different subcommittees to put together little sub-reports. We then integrated those into one big report to Congress, on basic research in psychology. We included research on social status and the family, on social psychology, on basic research in animal learning and human memory and reasoning, decision-making, some on sensory psychology, and especially some research that was dealing with
psychotherapy or basic research on drug addiction and the like. I kind of forget all the topics we covered. We’re talking of events thirty-five years ago, in the 1980s.

So our group put together a report. These five subcommittees would write up what they thought was wonderful research in their area supported by NIMH. Of course, being academics, they wrote in too much jargon and too long, and I had to tell them, “No, no, we’ve got to cut it down,” etc.

A science writer on the staff at NIMH, and I and another guy, John Kihlstrom, put together this final report that we sent to Congress. Then Fred Goodwin, our Director, and maybe one or two other people from the Mental Health Committee went to Congress and presented this research and our report and talked it up there and so on. One of the things every such report said was, “We need more money for our kind of training, and we need more money for our kind of research, and all kinds of benefits will flow to the national program working in mental health if you give us some more money.” I think every report in every field says exactly that. [laughter] It’s just a matter of how strong are the arguments you can make based on evidence. I think it produced some little increments in behavioral science funding for the next few years, and so agencies would do another report five years later, etc. That’s one of the ways funding from the government works.

Then along comes a president like Ronald Reagan, who says, “I want to zero-out all social science research at NSF,” so social science suffers for a while. But then you wait and hunker down until he gets out of office and you elect in a Democratic administration that will be good to you.
Hartwig: On the whole, though, is it a positive experience? What did you learn and what did you take away from it?

Bower: [00:55:50] No, I hated it. For one thing, when you work for the government, you have to show up every morning at eight a.m., and that was such an unusual routine for me. As a professor, I kind of mosey in around ten o’clock in the morning and stay till six or seven at night.

They said, “No, we want you here at eight a.m., Bower.” [laughter] My god.

We rented an apartment in downtown Washington, because my wife, Sharon, wanted to attend things on the Hill and big events going on at the National Archives or the Library of Congress or the Smithsonian or whatever. So we lived downtown, which meant to get out to NIMH headquarters, which are out in Rockville, Maryland, I had to take the train or drive out there, and so I had to get up at six-thirty to get to work at eight a.m. I hated that aspect of the job.

The other thing I did not like was although a budget was given for this study that I was in charge of, that budget was administered by the director of the Division of Behavioral and Social Sciences. I didn’t particularly get along with him. We had minor disagreements all along. One thing that really rankled with me was that he wanted to sign off on every dollar I committed to be spent on my subcommittees. On my five or so subcommittees, I had about fifty psychologists and social scientists who I was telling, “Yes, yes, come in to Rockville. We’ll pay your way this way or we’ll buy you meals, but
you just get out that report. Oh, you need a secretary? Okay, we'll give you
some money for that secretary.”

And my boss would look at that and he say, “No, you can't do that.”

[laughs]

I said, “What?” So he had to sign off on almost everything. That ticked
me off.

Hartwig: A lot of bureaucracy and accounting.

Bower: [00:58:32] The other thing was that I was dealing with academics who were
teaching and had graduate students and so on, and so they were willing to
come to Washington or to Bethesda, Maryland on weekends. They would
come into Washington Friday night, and then we'd meet Saturday and
Sunday, and they could return home Sunday night. That's sort of a standard
scheduling that I thought would be just fine for academic committee
members. That's the way conventions were. And my boss said, “No, they
have to come during the week so that our staff from NIMH can attend those
meetings.”

And I said, “Well, have the staff come on Saturday and Sunday.”

He says, “No. They're civil servants. They don't work on Saturday and
Sunday.” [laughs]

And that was a major pain in the neck to me and to all of the fifty
people that I'd lined up to come in and talk about what they should do in
their report and so on. I would say to the director, “Well, the staff aren't
doing anything. They're just sitting there listening. Why don't we record it
and let them hear the record?”
“No, they have to be there. They might ask a question.”

So, anyway, I did not enjoy my year in NIMH, and I was so happy to get back to Stanford where I could come to work at ten o’clock in the morning and, more than that, talk to really smart graduate students and colleagues. [laughs]

Hartwig: And not have to deal with red tape.

Bower: [01:00:34] I maintained friendships with the people at NIMH because I knew they were partly responsible for funding my research, and I always got along fine with them. They were good people. And by virtue of keeping good friends with my NIMH contacts, I had wonderful funding for the last twenty or so years of my academic career.

Hartwig: You also were an editor for quite a while. Talk about your experience as an editor.

Bower: [01:01:12] Editors. Well, there are several kinds of editors. One is you can be an editor for journals, and that was fun at first, but then it gets to be onerous because if you’re any good at reviewing research papers, you get more people wanting you to review for their journals or their tenure cases. So after a while, you learn to be assertive and say, “No, I can’t take on any more work.”

But in the beginning, it was really exciting to me because you’re seeing new research, or at least new work that these people think is worth reporting. Reviewing hones your analytical skills and hones your ability to criticize research and say, “This was not done right,” or, “You should have done this and that. Here is an alternative way it could be done.” Or you say, “This has already been done. This is not an advance on what we already know,” and
you have to cite chapter and verse of research that’s been done before, that is so relevant that this thing is redundant with it, and the journals don’t publish redundancies.

So you get very sharp, or at least I did. Excuse me, I’m bragging again. But you get reasonably astute at detecting errors and in detecting flummery in cases where somebody’s trying to play up a result that isn’t all that great, you think. Often you think that their result has alternative explanations and they should get rid of those alternative explanations before they get up on a hobbyhorse, publicizing their interpretation of their results.

So that’s journal publishing. The only problem I had with that was during my career I had a lot of different areas I worked in, in emotion, in behavior therapy, behavior modification, in neuroscience and physiological psychology, and in experimental studies of learning and memory, even in Skinnerian research. So I’d made contributions to all these different areas, each of which had its own journals, they would ask me to be on their board of editors or reviewers. So I very quickly would get on five or six different journal review boards, and that level of review work turned out to be counterproductive. I just couldn’t keep it up along with my own research, so I cut back after a while to reviewing for just one or two journals. A couple journals kept listing me on their masthead as an honorary member, but they didn’t send me any papers to review. [laughs] I said, “Okay, you can list my name, but don’t send me anything to review.” I think I finally got off even those. Okay, well, that’s journal reviewing.
One can also review book-length manuscripts that people send in to publishers for publication of books. I didn’t like to do that. For one thing, you have to read the darn thing, and it’s very long, most of these books, and I’d rather write a book than to read a book. [Hartwig laughs.] So I was not much for reviewing books. I would review short proposals for prospective books. Many authors would write a two-page summary of what they intended to write about, and send it to a publisher, and the publisher would then send it to other people like me to get their opinion about it and about the proposed author. That wasn’t very hard, except you did have to know the book literature then, so I would be unable to say, “Well, this overlaps with this book and that book.” So I wasn’t very good at that.

Then the other reviewing job I had was as senior editor of an annual series called The Psychology of Learning and Motivation. It got started in the mid-sixties. It was going to be run by an acquaintance of mine, Kenneth Spence, and his wife, Janet Spence. I contributed a long article for the first volume of it in 1966, ’67, it came out. Kenneth and I always got along very well. He was a big power player and contributor in learning theory. Unfortunately, he got prostate cancer and died after the first volume of the series came out. So, Janet, who kind of was struggling with the second volume, asked me to come on as an associate editor, and so I helped her compile and get out the second volume. And then she quit, said, “Gordon, you know what you’re doing. I don’t. You know all the people in this area.”

So by that time in the late sixties, I was a young Turk and I knew large numbers of interesting psychologists who were doing nice work in learning
and motivation, which is what this series was about. And it was up to me to
go out and pick people I knew--no, I didn’t even have to know them
personally--people who I thought were doing interesting research and that
they would like to have the pages in a popular edited book to fully describe
their program of research. The pages allowed were far more than what one
gets with a scientific journal where space is greatly limited. One of the first
things journal reviewers say to writers is, “This paper is okay, but you’ve got
to cut it by a third,” or something like that. Whereas with my series, I said, “I
want you to write. You can use up to fifty typewritten pages or so, describing
your research and telling people in the field why you think it’s significant.”

At that time, such an invitation to write was a godsend to many people.
They loved it. So I would get five, six, seven relatively famous learning or
motivation psychologists to write chapters, some of which became golden
chapters in citation indices. And because some of the people I got in the
early years of the series were really good and wrote important chapters that
were often cited, it was easy to persuade others to contribute to later
volumes.

In fact, I would invite the researchers I wanted to hear from. I had a
list of contributors three years ahead of time, so I could always say, “Your
chapter will be coming up three years from now, so plan for it. Okay.” And
as time came near, I’d remind them frequently. I never had much trouble
with lagging contributors because the people I invited were very reliable as
well as interesting at least. I never had any major--what shall we say--doubts
or second thoughts about people I’d invited when they wrote something for
this book. I would collect about six or seven chapters every year, and so we were always on time getting the volume to the publisher.

If somebody didn’t come in with their chapter, I’d say, “Okay, you’re gone. Bye. Next year, maybe.” And so I always had enough prospects so that if somebody didn’t show up, I’d have a replacement author. If somebody came in a year or two late, I could often accommodate them if they submitted a good chapter. The number of chapters per volume was elastic, although I always wanted to have at least five, if not seven or eight.

All these many contributors and their associates I got to know very well, more than I knew them before. So I quickly had this large collection of researchers in my field who I knew, and to whom I could talk about their research. They knew and respected me and what I was doing for our field. They saw me as a good guy because I was enabling them to expand their wings and write whatever they wanted to about their research in a popular, accessible publication. And I didn’t break their balls with editorial criticisms and tell them to cut it down or say, “That’s too speculative. You can’t speculate like that. Gut it,” etc. I said, “No, no. Go ahead and say what you want to--but you’re professionally responsible for what you publish.”

Hartwig: What were some of the most important chapters or research that came out of those series?

Bower: [01:11:46] One of the chapters I had in my second volume was by Atkinson and Shiffrin, called “Theory of Short-Term Memory,” and that became the standard theoretical document for many years thereafter.
Research by Endel Tulving and his associates was published. His research on organization in recall took off, and his chapters were very important. Work by Mike Posner on the use of reaction time for studying elementary cognitive processes involved in perception was very important and got used and cited a large amount.

A paper by Bill Estes on probability learning got cited a lot. A paper by Tom Trabasso on narrative understanding, on the use of causal chains, causal analysis of stories to predict what a person’s going to remember from a story and what they’re going to put into a summary of that story, that became a bestseller, so to speak.

Wonderful chapter on categorization by Doug Medin was in that series, and that set a standard for people talking about theories of concepts and categories. In fact, Medin became a major figure in cognitive psychology. Later, after twenty-seven volumes, I finally decided to turn over the editorship to someone else. I persuaded Doug Medin to take on the editorship, and he carried it on for another four or five years, and then he enlisted one of my ex-students, Brian Ross, to take on the editorship.

I think the series is still going, although it’s kind of dwindled in its impact and its visibility after some thirty years, but I think it’s still there. I never made much money out of it. I got a tiny percentage, half of 1 percent or something like that. People tended not to buy it after a while. Their library would buy it, and then they’d borrow the library copy. Or they would call up their friends, “Send me a reprint of your paper,” the way cheapskate
academics do. [laughter] Academics didn’t have any money for these kinds of books.

So I think the publisher after a while was saying, “We’re not selling enough of these,” or libraries were not buying enough of them, or something. I ought to look it up to see if it’s still going.

One of the nice things about it is if I wanted to publish a chapter in it by me or one of my really good students, I could always slip another chapter in the volume. I always made sure they were the really good students: John Anderson, Larry Barsolou, Doug Hintzman, Mark Gluck, and Mike Rinck. I mean, these are sterling research characters.

So anyway, editing those annual volumes was easy. Basically what I had to do was just keep tabs of who’s on track to get their chapter in. I would read the chapter. Occasionally I would say, “I don’t think you want to say that here. Why don’t you revise or cut out that section because I don’t understand it.”

But by doing that annual volume, I became even more comprehensive in my interest in psychology. I would try to cover the waterfront in reviewing work across the spectrum, in memory, learning, performance, perceptual learning, decision-making, and in emotion and motivation. So I was stretching my knowledge by inviting interesting contributors from all these areas then having to read and understand their chapters. I had to judge that it was worth my time and worth the readers’ time to read specific literature. So the volumes served like an annual review of psychology, but in specific areas
of learning and memory and motivation. So it kept me apprised of
developments in these areas.

Hartwig: That’s a nice segue. Let’s talk a little bit about your reflections on the
profession. So who were some of the colleagues that made the biggest
influence or impact on you and why?

Bower: [01:18:37] First, Bill Estes. He was the number-one thinker and researcher in
mathematical learning theory. I met Bill in 1957, and while I was at Yale we
started a correspondence while I was still a graduate student. Then I was
hired here at Stanford and he was hired here, and so we kept stimulating one
another, although it was probably more a one-way street. I got more from
him than he got from me because he was very creative, I thought, and he
would say something that would trigger useful associations in me on how to
explain something or which way to go in research. So Bill Estes was one.

Neal Miller, my old mentor from graduate school, just taught me good
work habits and ways to run a research group that I used, ways to relate to
graduate students, which I used a lot. So those were two.

A third one who was very helpful to me in my early years was Leon
Festinger, who was on the faculty here. Leon was a hard, ball-crushing critic
of everything in psychology, and he took me under his wing and liked the
work I was doing, for some reason. Perhaps because he was trying to do
something by stretching outside of social psychology into applying
dissonance theory, his particular bailiwick, to a well-known phenomenon of
annual learning, namely, the partial reinforcement extinction effect. And he,
for some reason, got interested in that with another faculty member named
Doug Lawrence, so Lawrence and Festinger did a bunch of experiments on that effect and published book called *Deterrents and Reinforcement*. Leon wanted me to admire that work and, in particular, wanted me to criticize it, which I did, and gave him back what he had given me on numerous occasions. But he and I developed a fairly close intellectual bond while he was here. Unfortunately, Leon left to go to New York in the late sixties or early seventies, and so our contacts dropped off.

Endel Tulving was another person who influenced me. He and I hit it off well in the mid-sixties. He became the most famous researcher in human memory. In the beginning, I did not agree with his point of view and I would argue with him, and he loved that. He loved anybody that would argue with him.

In fact, when I was an early assistant professor here—when was that? 1962 or ’63, Endel at that time was at University of Toronto, and a couple of other people there got the psych department at Toronto to offer me a full professorship when I was still an assistant professor here. Thank God I said no. [laughs] I didn’t want to move to Toronto. I preferred Stanford—and my wife said, “If you go, you commute. Damn it, I’m not going.” [laughter]

So Endel Tulving and I became very close friends always talking about our research, and we kept up that research talk through the sixties, seventies, eighties, and the nineties. I still talk to Endel, although we don’t talk much about research. It’s rather, “What aches and pains are you having today?” [laughter]
I helped him get through his wife’s dying of Alzheimer’s disease last year. He and his wife, Ruth, and my wife, Sharon and I were very good friends as couples. They would often get out of the Toronto snow country in the winter and come to California, UC Davis, or go to Florida, University of Florida. Endel’s wife, Ruth, was a terrific painter. She was in the artists’ national academy in Canada, so she was a terrific painter, very creative, very beautiful woman. I spoke at her memorial service after she died; it was a very, very hard time for us.

Endel and I published a couple of things together because we just thought, “Well, we ought to. We’re such good friends.” But he was always in some sense more creative than I was. I was more analytic, wanting to dot every “i” and cross every “t” and he didn’t care so much as I did about details.

Who else were very important people for me? Roger Schank was very important. Roger was a computer scientist, and he did natural language understanding by a computer. Fifty years ago, he was aiming to create whatever it is, Apple’s Siri in iPhone, that understands speech and understands what you’re asking and gives you an answer and so on. Well, Roger, very, very early was into that, although I’m sure he would think Siri is actually pretty dumb and that it didn’t really have much knowledge programmed into her database.

So Roger influenced my research, particularly once I started getting into psycholinguistics and narrative memory, narrative understanding, since Roger
was working on understanding and memory for narratives. So Roger was one
big influence on me.

Tom Trabasso was another. He was a very close postdoc with me, and
Tom and I kept talking back and forth for some 45 years, especially once he
started doing work on narrative understanding and narrative memory just like
I was. We published a bunch of articles and an early book called *Attention in
Learning Back* in the sixties together, which was on the research we were
doing then on concept identification. That book was okay, but we both left
that specific area and went into other research areas.

John Anderson was a very important person in my intellectual life. He
was a graduate student with me here, and got his PhD in 1973 or so. We did
a lot of research and published papers together when he was in graduate
school. We wrote the *Human Associative Memory* book together. I spoke earlier
about the content of that book. John and I just kept going with it for a while,
but after he left Stanford he continued developing his computer program for
learning and memory, and kept developing it more and more. I just sort of
left that to him, although he and I could still talk and get ideas, though they
were mainly ideas about the work he was doing. He didn’t have any real
interest in the later work I was starting in, say, emotion and memory. He
basically said, “That’s yours, Gordon. I don’t have any ideas about that, other
than what you’ve told me.” I’ve been one of his strong supporters over the
years. I invited him to write a couple of chapters in the Psychology of
Learning Motivation series, so he has been a great boon and source of
stimulation to me, at least, and I think to the field of psychology.
Who else? I don’t know.

**Hartwig:** You’ve talked a lot about Dick Atkinson thus far.

**Bower:** [01:29:28] Dick was great in psychology up through about 1975. He was here working with Bill Estes and Pat Suppes and me. Although Dick and I never published together, we talked together a lot, especially the work he was doing with his short-term memory model and the work I was doing in short-term memory, with my ideas and theories at the time. So through the sixties, Dick and I talked a lot together. In the seventies, Dick was getting more and more wrapped up in computer tutoring of reading and math in school systems in this area. Although I could understand why that could be interesting, not to mention lucrative, I wasn’t interested in it myself. I said, “You guys are way ahead on that. I’m going off to do my thing” because computer tutoring became a pretty big thing.

But Dick and I have remained good friends. He went off in 1975, I think, to NSF as its assistant director. Very shortly thereafter, the director left, and Dick became director of NSF. He was director of NSF for quite a few years, maybe five years or more. It was during the Senator Proxmire years, I remember, and the Proxmire’s Golden Fleece Awards. Dick was always ticked off about that, because Proxmire had a talent for picking out the most embarrassing title for psychological research projects funded by NSF and then lampooning them in the press. Dick tried to make sure thereafter that all titles of research at least were neutralized and sanitized. And I even know several psychologists who got picked for the Golden Fleece Award.
So anyway, Dick was at NSF for a number of years, and then he got a job as chancellor of UC San Diego. He was there quite a long time, ten or fifteen years, and was very central in developing UC San Diego as one of the premier scientific research universities in America, certainly on the West Coast. He kind of professionalized the place in terms of research funding, and by the quality of the appointments they made, and so on. He was adept at resisting the pressure from the UC board of regents to admit more and more and more students. He’d say, “Enough of those. Let’s get some more graduate students and research departments.” So he was very good at developing a research university, and I think he was also pretty good at relationships between UCSD and San Diego.

Then he was elevated to the job as the president of the whole University of California system and moved up to Kensington near Berkeley, where we used to visit him and Rita. He and I, or the couples—his wife, Rita, and my wife, Sharon, were very good buddies, and so the couples would get together periodically to go up to the wine country together or just to get together for dinner.

So we’ve kept up talking. Every time he saw me, he’d say, “Okay, Gordon, give me a synopsis of what’s going on in psychology now.” And I’d reply as best I could on the spur of the moment. He was interested in experimental psychology and learning, motivation, and so on. And of course, many of the people that I had trained and he had been involved with had gone on in their careers, became famous contributors in psychology. So he
would say, “Whatever happened to Doug Hintzman?” or “Whatever became of Jim Hinrichs?” or “Where’s Bob Sternberg nowadays?” and so on.

I followed the careers of many of our ex-students, so I could tell him, and I could also give a fairly decent summary of some of the more exciting developments in memory theory and in cognitive science, and so he kind of relied on that to get himself up to date in cognitive science or whatever.

So we would get together periodically, the Atkinsons and the Bowers, and have our conversations. Rita and Sharon would go off walking. They would in their younger years go with these trekking camp trips in the Sierras, but they don’t do that anymore. They’ve become too old.

Then when Dick left the presidency at UC up at Berkeley, he retired to a magnificent palace in La Jolla, and we still see them and talk to them. We just saw them in early May (2014). Dick came up here for my eighty-first birthday party. We went out to dinner with Pat Suppes and Dick, and we had a jolly old time, reminiscing about old times and what’s become of one acquaintance or another. Pat doesn’t do a lot of that, but Dick does. He likes to reminisce about the good old times when he was a professor. But I could get him talking about the political problems he would have as president of the UC system--

Hartwig: I can only imagine, yes.

Bower: [01:36:38] --and how he had to get along with this or that governor, or this or that hothead on the board of regents, or whatever they’re called in Sacramento, or what problems are boiling up from the faculty. He didn’t divulge any you’d call confidential information, but he would let me know
some of the crap he was having to put up with. He would say, “Gee, Gordon, I wish I was back being a professor again.” [laughs]

**Hartwig:** I bet.

**Bower:** [01:37:21] But he was going for big-time stars. And he’s written some memoirs about that period. I think he sent me some memoirs; first of all, a series of essays about higher education and some of the fight that he had getting the SAT 2 passed in the University of California system, and as well as many then just essays he’s written about education. In addition, I think he’s written a memoir of his time mainly as the president at UCSD and the UC system. I must confess I haven’t read them because I’m a slow reader. I move my lips when I read.

**Hartwig:** So, looking back, what are some of the accomplishments or achievements that you’re most proud of and what has been, I guess, the most rewarding and fulfilling part of being a professor or working here at Stanford?

**Bower:** [01:38:35] I think the most rewarding part has been being associated with exceedingly bright graduate students who have gone on into the field and made lots of contributions to cognitive science and to psychology, and made a big name for themselves. I’m not sure I influenced some of them very much, although they were my PhD students and we talked a lot together. But many were very much independent spirits, almost from the time they came into graduate school. Many of my PhDs have done very, very well, so I often say they are my legacy to the field. They are my jewels, my gems, and I have cherished them very much. So those are the main things I think I’ve contributed, is teaching those people.
I think that some of the work I’ve done on emotion and memory has had a big impact, especially as I moved it over into how emotion affects social cognition and social impressions, and social memory underlying impression formation. Those have had, I think, a fairly major impact.

The work I did early in mathematical models of short-term memory I think has lasted a fairly long time, and people either criticized it or picked it up on it and carried it forward and so on.

I don’t know. Some of the areas that I went into that I loved and in which I published a lot kind of died. I mean, not many people followed it up. Maybe researchers just couldn’t get into it, I don’t know about it anymore.

One of the areas was on narrative understanding and narrative memory. I think most people didn’t follow into that area. There’s a small coterie, a small group of people who do, but the area hasn’t exploded the way, say, that research on emotion in memory did. That really took off.

Hartwig: Is there anything you would have done differently, aside from playing baseball?

Bower: [01:41:52] I think I could have very early gone into immunology and conditioning of immunological reactions. This was an idea I had in the early 1960s, of all things, but I didn’t have the expertise in immunology to work on it. I remember I talked to a couple virologists, immunologists over in the medical school and said, “Would it be possible to do classical conditioning of immunological reactions? Sort of the way Pavlov’s dog heard a bell paired with food, producing salivation; the bell itself eventually starts to trigger salivation. Could you do that using, say, a distinctive cage in which an animal
gets a typhus shot and gets all of his immune reactions coming up in that cage? Then later when he gets better, can that immune reaction be triggered just by putting him back into that distinctive cage, into that specific environment?” And I would describe the kind of things I was thinking of maybe doing if I knew how to do it, and they said, “That’s crazy. That’s utterly crazy. That’s not the way the immune system works. Don’t you know anything?”

I said, “Well, I guess I don’t know anything. Sorry. Sorry to bother you.”

So I shifted out of that. And then later, along came a researcher named Bob Ader, who I didn’t know, he didn’t know me, who did experiments like that, and damned if it didn’t work. He didn’t do it with positive release of antibodies; he did it with suppression of an immune reaction. Ader would give cyclosporine to a rat, and that knocks out most of his immune response. Surgeons use cyclosporine, for example, to stop organ rejection in transplant patients who are getting a heart or a liver or kidney, because a major problem surgeons have with that is rejection of the transplanted organ by the person’s immune system. So what you want to do is to dampen down their immune response. Cyclosporine is a drug that will do that.

So Ader would have a rat drink a very distinctive tasting drink and be in a very distinctive environment when you give him cyclosporine, and it makes their immune system collapse there. Take them out, let them return over several days to normal in their immune competence, put them back into that environment, and look at what happens to their immune reaction to some
antigen challenge. Damned if he didn’t get conditioning of suppression of the usual immune reaction to the antigen. Way to go, Bob. And by then he was already well into doing the next thing and following up with a large program of research.

One of the first things you learn if you want to have your research noticed is don’t do too much following someone else’s path. I used to tell my students, “Remember, only the lead dog in the pack pulling the Eskimo sled can see open space.” [laughter]

Anyway, so I wish I had gone into that immunology research early, so I could have been more hooked into the biomedical research community, and I think maybe I should have been earlier into getting into the use of fMRI, because I heard about it very early on, even before the functional aspects of MRI started, but with just MRI machines.

There was a period during which I was offered a job as the director of the Beckman Institute at the University of Illinois. Arnold Beckman had gone to school at Illinois, and Arnie had given them many millions of dollars to set up a research institute on intelligence, whatever that meant in its multifarious properties and aspects. So they offered me the job because I had broad interests, and they said, “We want you to be the director here.”

And I thought about it for a while because it was a very plush job and I’d be dealing with and learning a lot of different sciences and disciplines, as well as having my own research group. But eventually I said, “No, I didn’t want to live in shampoo banana,” as we called it. [laughter] Shampoo banana, Champaign-Urbana. [laughter]
Bower: [01:47:56] Shampoo banana?

Hartwig: Yes. I’m from the area, but I’ve never heard that.

Bower: [01:48:00] It’s what my little kids called it. “I don’t want to live in shampoo banana, Dad.”

Anyway, so as next best thing, they said, “Well, then help us pick the director,” and I did. And then, “Will you be on the board of directors?”

And I said, “Oh, sure. I’ll do that.” And in that situation, I got to hear a lot from the computer science people and the biomedical research biophysicists, who they brought into that institute. And the institute included the number-one guy in developing magnetic resonance imaging in that institute, he was a guy at the University of Illinois, and so I heard it from the get-go as to what scientists could do with MRI. You really can see the brain. Hot damn.

But I said, “Nah, what you really want is the brain functioning as the person is making a decision or is remembering something or is expressing a preference for this versus that option in a decision,” or what have you. Those kinds of studies came along a little later with the development of functional MRI. But I didn’t grab on to the tail of that. I said to myself, “That’s very expensive research to get into,” as it was then. Because in those days there might be one MRI machine on the whole campus, and it would be in the radiology department, and you have to get money for the care and feeding of that machine, giving it to technicians who keep it alive and well. So that meant you had to have ideas that NIMH or NIH would fund so you can get
enormous quantities of money on a continuing basis to keep that machine running.

And I said, “I’m too old to chase that kind of money now.” You know, I was in my late sixties at that time, and I was in the middle of ten-year grants from NIMH for doing my behavioral research. So I said, “Let others do that. I’m not going to chase fMRI in the brain, when my adult subjects are doing many interesting behavioral things I can study.” So I didn’t grab on to the tail of that tiger or try to get on top of it. I said to myself, “Let the young people do that.”

Several years later NIMH became willing to fund functional magnetic imaging research, and I was involved in hiring a couple of young faculty here who did that kind of work, including John Gabrieli, who was the first fMRI researcher we brought into the psych department. And it’s clear the graduate students at that time could see this technique as the wave of the future, and so John became a magnet for attracting graduate students, including some of my students, I must confess. But it was a great opportunity for those students, and some of them have become stars in their own right. And it was then I also began to think I’m in a research area that’s settling down, maybe not the lead bandwagon anymore. So I didn’t go with fMRI.

Also, in the early years of fMRI, I thought the work that they were doing was not particularly interesting. What fMRI researchers were doing then was localization, showing that when subjects look at faces, this part of the brain lights up, and when they look at open spaces, this other part of the brain lights up. Or when subjects are memorizing a set of words, this brain
area lights up, and so on. And I said, “Yes, that’s fine. So what? What’s connected to that? What happens when I use one or another piece of knowledge to come up with a behavioral memory?” And it was quite far in the future that those questions got addressed.

Let me elaborate on that. As I said, in the early fMRI research, what researchers were exploring was what we call localization. So when a person is doing this mental act, like memorizing something, this part of the brain tends to be more active than other parts of the brain. That would be a typical research report in those days.

Well, I would look at that and say, “Oh, okay. Activity in that brain area is correlated with this piece of psychological functioning. So what?” We always knew that when somebody carried out some cognitive maneuver, some part of the brain would be more active. That’s called the psychophysical hypothesis that we’ve all believed since we were in diapers. So what this research has shown is that that when you’re memorizing, something happens in the brain: moreover, it happens kind of in this general location. So what else is the value of that?

And they could say, “Well, we know that if you blow out that part of the brain with a stroke, people have trouble.”

I’d say, “Okay, that’s an interesting fact.” But I didn’t need fMRI to find that out. Anyway, so I was kind of a curmudgeon, being skeptical of some research in fMRI. I would basically keep saying, “For example, I have this psychology conundrum. I can’t decide whether complex stimuli are processed in serial or parallel fashion. What do you know from your MRI
work that will resolve my conundrum? Like how do people remember serial lists of words or digits and how do they accumulate knowledge with repetition or how does the serial-position effect in recall arise, and where’s that show up in the MRI?”

And they basically would say, “We don’t know. That’s not what we can study yet. We’re looking at what lights up when you’re doing this or that,” and so on. So they didn’t have the fineness of detail to answer the behavioral questions that I was asking. I probably gave some grief to some people about that, being an old troglodyte and curmudgeon about it. But it also goosed them to keep moving forward so that they would investigate things that were more interesting to a behavioral psychologist.

**Hartwig:** Absolutely.

**Bower:** [01:56:25] I think I might have had some little impact in moving some of the Stanford MRI researchers over in that direction. I know it was true with my students who were doing fMRI, and they’ve become some of the leaders in that field these days: Anthony Wagner and Sharon Thompson-Schill and Alison Pearson, and many others who work in that field.

Anyway, excuse me. That’s quite a digression.

**Hartwig:** Oh, no, that’s perfect. That’s perfect. Is there anything else that you’d like to talk about that we haven’t talked about regarding your career at Stanford or your life?

**Bower:** [01:57:02] I think I’ve led a charmed life here. I’ve had a job that I love. My wife loves me and loves my job. Our kids turned out very well. They liked living on Stanford campus. Two of our three kids went to Stanford and got
degrees here, and my wife got her degree in counseling psychology here and
became an assertiveness trainer and was into behavior modification with
clients.

**Hartwig:** And you two got to collaborate too.

**Bower:** [01:57:42] Yes, we wrote a book together basically on her therapy work and
ideas, not from me. I was just a collaborator. Oh, I knew enough about
behavior therapy, so I could help write it.

And Stanford land is a perfect setting for a university. It’s beautiful. It
is flat, you don’t have to walk up and down hills to get anywhere on campus.
It has a great reputation overall, has a great reputation in psychology, and
they’ve treated me well. I’ve had many offers to go elsewhere to Ivy League
schools and so on, and they’ve just never appealed to me compared to what I
have here. And the deans or the provosts have always said to me, “Don’t
ever let money be a reason for you to leave here. What do you want? You
want a secretary? Here you are. You want some money for your
extracurricular research and for going to conventions? Bang. Here you are.”
They just never gave me any reason to want to leave, and there’s been every
reason in the world to stay here. I get to know more of my university
colleagues now that I’m retired, and I still have some friends living around
here, and so on.

Oh, there’s one thing I think I might mention. I think Stanford has not
had very enlightened policies about how to deal with retirement of their
senior faculty. I mean, there’s an emeritus group and we get together and talk
to each other, but the university has not built particular housing for their
emeritus faculty. At one time, we thought the Hyatt Vi over here was going to be a viable retirement option, but it turns out, no, Stanford washed their hands of that project and just leased the land to the Pritzker Foundation in Chicago that’s developing it. They’re putting the screws to everybody who might want to move in there, in terms of very high prices.

And if you look at the Stanford campus where many of us live, the old faculty who built up this place’s reputation have died, and what’s left are their widows or a few widowers, living in their big house. So 40 percent, I’ve heard, of houses on campus are occupied by widows who aren’t working here. So all of these campus houses, which should have been going to new faculty, are being occupied by widows. And wouldn’t it be nice if they could be moved out into some smaller attractive facilities? And even Gordon Bower and his wife could move out to the smaller on-campus premises, and we didn’t have to lose an arm and a leg paying the Pritzker Foundation to go to the Vi or another organization to go out to the Sequoias, which is too far away. So I think Stanford could have been more enlightened in building housing for senior faculty to go to, and that’s a minor pet peeve because I’ve complained about the Vi. It is too bloody expensive, and you basically lose your shirt.

Hartwig: I’ve heard, yes.

Bower: [02:01:40] Or your heirs lose your shirt if you go there and die. They cost a million and a half dollars or two million to get in, and the day you sign up, you’ve lost 30 percent of your money. That’s $600,000, bang, and you might only live there a couple years, and then you die. Too bad.
Anyway, so I’ve complained about the Vi. Sequoias is similar. You pay about $500,000 to go there. Suppose you only live five more years? Because the older you are when you move in, the fewer years you have to live.

Suppose Sharon and I live five more years. If we go out to the Sequoia, we pay them $500,000; but we’re losing $100,000 a year plus what we pay them monthly to live there.

Hartwig: Has the Emeriti Council taken this up as an issue?

Bower: [02:02:46] Of course. But we have zero power. We can try to shame the administration or the board of trustees to do something about it, but they don’t care. They say, “Thank you. You were well compensated while you were here on the active faculty as a professor. You’re retired now. Goodbye.”

[laughter]

Yes, I don’t know. Stanford is presently developing new housing units over on California Avenue near Xerox PARC, over in that area. So we’ll see what that becomes, but that isn’t going to be finished for another two or three years. They haven’t broken ground for it yet, but they’re planning to. And that’s going to be housing units. I don’t know whether they’re going to be rental or for sale, and whether they’re going to be restricted to active faculty, and whether old geezers like us can’t get in it. They’ll probably say, “You’ve got enough. You sell your house, you’ll have $2 million. What else do you want?”

But Stanford has been just a gloriously good place for me to be and for my wife and my children to be. I can’t imagine a better university. We have
the problems of increasing population density here and more and more traffic and congested roadways, but you just learn to negotiate around them.

And there is a wonderful cultural setting here with many events to attend, plays, dance, music, and the evening classes in continuing education. We go to those every quarter. We sign up for two or three. I’m signed up now to start one on “World War I and its Aftermath,” taught by Jim Sheehan, who’s a fabulous lecturer on just that topic.

Then my number two class is going to be “Classics of the American Short Story,” taught by Michael Krasny of Forum fame in radio. He is a professor of literature at San Francisco State College, but he also does many other things. He’s great in the sense that whenever he talks about a short story and the author who wrote it, turns out he’s often interviewed that author on Forum, so he can tell you many details about the writers and their associates. So he makes these classes very interesting, so whenever Michael teaches, I try to show up for it.

Bill Chace teaches in Continuing Studies, and he’s a fabulous lecturer. He’s one of my very good friends from the time we were on the A&P Committee together and later in the Dean’s Office together. We’ve remained friends, even though he ran off to where, Wesleyan College, and then to Emory University as president, but he came back to Palo Alto to retire. But he’s still teaching in Continuing Studies, and he’s a fabulous lecturer, and I like to listen to his lectures.
So we always get involved in something, classes on modern China or a
class on photography or on ancient history or Renaissance art in Venice or--
there’s so much we’ve taken, I can’t remember it all now.

Hartwig: That’s a lot.

Bower: [02:07:30] We go to a lot of history courses. I like European history, political
science classes. I like classes on the role of war in politics, you know, how for
many years nations used wars as a way of solving political problems and as a
mode of diplomacy. I also take a lot of classes in literature, such as Russian
literature, Dostoyevsky, Gogol.

Hartwig: Is there something you want to do that you haven’t done yet? Do you have a
bucket list?

Bower: [02:08:18] Yes. I would like to go to Angkor Wat, which is where there’s a
huge Buddhist temple in Cambodia, but I think my wife can’t go to places
like that anymore. She’s not very ambulatory. Or I would have liked to have
taken the river tour up the Mekong River or the river tour up the Amazon
River in South America or would have liked to have seen Victoria Falls and
parts of mid Africa and southern Africa. I don’t think we’re ever going to get
there because we’re now in our early eighties and not too ambulatory. I’m
okay still, but my wife is sort of confined.

Things I would like to have done. I’d like to learn to play the piano. I
love music of all kinds, and I listen to music almost all day long. Particularly
popular music I like. I don’t keep up with the latest “boy bands” or the latest
girl singers; I don’t care about them. I like old-time stuff from the sixties,
seventies, and eighties, and I record all that and play it quite a bit, always play it on my iPod when I'm out exercising.

But when I sit down at a piano, I can pick out tunes, and when my kids saw that, they bought me a little electronic keyboard, and they said, “You learn that, Dad.” And I took a lesson or two, but I said to myself, I know to become proficient at this, so that it’s reinforcing to listen to myself, requires about five to ten thousand hours of practice. I don’t have that kind of time yet to live, and I can’t go through the burden and the pressure to force myself to practice when I know how terrible it’ll sound even when I’m this close to death. Given that I’ve got only five or so years to live and maybe I’ve got a total of a thousand hours of leisure time, I don’t want to put that into learning a skill that takes ten thousand hours, so I haven’t done it. I still finger tunes on the piano, put a few old Barbra Streisand songs on my iPod, and I’ve lately discovered Susan Boyle. Remember Susan Boyle?

Hartwig: Yes.

Bower: [02:11:55] “I Dreamed a Dream.” That’s the latest obsession I have, listening to her. She has a gorgeous voice. I like to listen to women who have great singing voices. I like operatic arias. I don’t care about opera particularly, but I like soprano, or mezzo soprano arias, particularly if they’re beautiful and sonorous, Puccini-like. I love those, and so I often sit and listen to Maria Callas or someone like that, Joan Sutherland, singing sweet music.

I know many opera lovers hate the kind of music I love. I like schmaltz. I like things that are just slow and beautiful, and you want to hear them, from La Bohème or Madame Butterfly or Turandot, some of the parts
of that, and some Wagnerian things, some parts, overtures. So I love to listen to operatic arias by sopranos, mezzo sopranos. I love some popular music, a lot of it, actually.

I wish I still played a trumpet, and I can’t. I don’t have the embouchure or the teeth to do it. I have a lot of false teeth in the front, and if I played the way I used to play, those teeth would come out in the first minute. [laughter]

So other things on my bucket list, I don’t have very many.

Hartwig: Well, that’s good.

Bower: [02:13:59] My wife and I did lots of traveling when we were younger, in our thirties and forties. I never got overseas until I was in my thirties. My first sabbatical was the first time I travelled outside of United States. I went to London. At the time, I was thirty-one, thirty-two. But while we were residing in London, we’d take off and go all over Europe on trips, leave our kids with my mother in London.

Then over the years, I would get invitations to come talk at the convention of the Italian or the German Association of Psychologists or come to this conference we’re putting together in Oxford or in Tokyo, and I would say, “Oh, yes, sure. You pay my way, right?”

“Yes.”

“Good. Thank you. How about my wife?”

“No, can’t pay for her.” So we would attend the conference and always try to get a week or two on either side of the conference that was paying my way there. So I’d give a talk and then we would take off and go all over Germany or all over wherever, Italy, or Japan, India, Vietnam.
Hartwig:  Do you have a favorite place or particular memories from those places?
Bower:  [02:15:42] Those places?
Hartwig:  Yes. What stands out or what was your favorite?
Bower:  [02:15:48] Going through East Germany when the Russians still occupied it. That was very exciting. Getting in and out of Berlin through the corridor, I guess that was just scary.

I remember very well the trip we had through India. It’s strange the way it came about. I was at my health club taking a hot tub in a Jacuzzi, and there was a couple of guys from India there. They were graduate students at the university here. And I said, “You know, my wife and I are going to go to India, but we don’t know what to do or where to go.”

And one kid said, “Well, my father runs the largest travel agency in India, in Bombay. I’ll have him set you up with a tour.”

I said, “Well, yes, thank you.”

And so he did, and the father set up a magnificent tour for us very inexpensively. We stayed in five-star hotels as we went around seeing all the great sights of India, all the caves and great sculptures that you should see, and all the big palaces and old fortresses and rivers and where the British colonialists stayed in the summertime, etc. And it was just a highly memorable trip, like the city of Varanasi on the Ganges, where all the pilgrims come down to pray and bathe themselves at dawn in the Ganges. Those events created very memorable images for me.
Of course, you meet Indians at Stanford here all the time, and I could say, “I’ve been to India,” and I would numerate the places we’ve been, Amritsar, Srinagar, New Delhi, Mysore, and so on.

They’d say, “I haven’t been to those places. You’ve seen more of India than I have.” So I liked that. That was very memorable.

Also I enjoyed my tour of Vietnam. I had been invited to speak at a conference in Hong Kong. That group was paying my way to Hong Kong. Before that conference, I took time to fly into Vietnam, went into Ho Chi Minh City. At that time, I was a big cheese in APA and APS, and APA had an international program. I talked to the person who oversees that and said, “Could you give me the name of the person who runs psychology in Vietnam? I want to write to him and tell him ‘I’m coming to Vietnam and could you introduce me to some psychologists there?’”

Their top man, I forget his name, set up groups of psychologists to meet me at every city on the tour that I was taking through Vietnam. I bought the tour over here, so I was able to tell him where I was going to be when, and every place I went, there would be psychologists meeting me at my hotel, taking me out to dinner, talking to me as best they could about psychology in Vietnam in their part of it, or arranging for me to go to their universities and meet people. And that was very good and memorable for me, getting to know some psychologists there and what was their familiarity with modern psychology. They were very eager to find out about what’s going on in American psychology. I couldn’t begin to tell them all of it, they were so uninformed about it, but I enjoyed meeting them and talking to
them about some of the trends in American psychology. I also suggested to
them how they could get a little more research money out of their
government.

Vietnam was under very strict communist rule in those days, probably
they still are. When I got to Ho Chi Minh City and went to all the war
museums around there showing the great imperialist U.S. warmongers. Went
to Da Nang to where the U.S. had very large bases, and then to Huê. Huê is
where the old imperial parts of Vietnam used to be, with a lot of palaces and
those places, and then to Hanoi and the areas outside of Hanoi, Ha Long
Bay, and--what do they call it, the Hanoi Hilton prison that John--what’s his
name--

Hartwig: McCain.

Bower: [02:22:09] McCain. John McCain was in the Hanoi Hilton. And other such
places. So that was great. I enjoyed going through Vietnam.

I would like to go more through Thailand. I’ve seen southern Thailand,
but I haven’t seen northern Thailand. Also I’d like to see, as I said, Angkor
Wat in Cambodia. Those are very primitive societies still. But I guess that’s
one that I'll probably miss. Oh, I’ve never been to Taiwan, except stopping
through at the airport and then taking off in Taiwan.

And I haven’t seen all the Philippines. I’ve seen most of the cultured
Philippines areas. I’ve seen a lot of Australia, but I have no great desire to go
back, except I have a few collaborators who are there. They want me always
to come back. I haven’t seen a lot of South America. I’ve seen Patagonia
territory, but I haven’t seen Chile, Paraguay, Uruguay, or Ecuador or those places, Amazon. I’ve been to Buenos Aires and parts of Argentina.

And I haven’t seen many of the islands of the Caribbean, particularly eastern Caribbean islands like the Grenadines or those areas. It would be nice to see if I can persuade Sharon to get on a cruise ship with me. I think she likes cruises, so we could do that.

Okay. I’m running out of gas here, as are you, probably. Good god.

**Hartwig:** Any final words of wisdom or message you want to pass on to your students or your colleagues?

**Bower:** [02:24:40] Life is a blast. Keep your mind involved in learning a lot of activities. Enjoy everything you can. Don’t go through depression. Stay away from booze and drugs. Keep on moving, intellectually speaking. Don’t get sedentary in the topics that you are interested in or studying. I don’t have any real words of wisdom. Sorry.

**Hartwig:** That’s good. It’s been an honor and a pleasure. Thank you so much. A little over twelve hours.

**Bower:** [02:25:37] God, are you still recording all this?

*[End of Session Six]*
Gordon H. Bower  
Curriculum Vitae

EDUCATION:
- Western Reserve University, Cleveland, OH. BA 1954
- University of Minnesota, Woodrow Wilson Fellowship studying Philosophy of Science & Mathematics 9/54-6/55
- Yale University, MS June 1957; PhD 1959, Psychology, graduated with distinction

PROFESSIONAL INTERESTS:
- Conditioning, Learning, Memory, Language Comprehension
- Mathematical Models, Computer Simulation of Memory, Behavior Modification

PROFESSIONAL EMPLOYMENT:
- Stanford University Department of Psychology, appointed as assistant professor 9/1959, promoted to associate then full professor (1966), retired to emeritus status 01/2008
- Visiting Professor of Psychology, Phillips University of Marburg, Germany, Spring semester 1987
- Chief Science Advisor to the Director of the National Institutes of Mental Health, Bethesda, MD. 3/1992-3/1993
- Visiting Professor of Psychology (the Holtzman Chair), University of Texas, Austin, TX Fall semester 2003 & Fall 2005

PROFESSIONAL AFFILIATIONS:
- American Psychological Association, since 1960
- Western Psychological Association, since 1962
- Psychonomic Society, since 1960
- Society of Experimental Psychologists, since 1965
- The National Academy of Sciences, since 1973
- American Academy of Arts and Sciences, since 1973
• Cognitive Science Society, founder, since 1967
• American Psychological Society (Now Association for Psychological Science)
• International Society for Research on Emotions, since 1987
• Society for the Study of Text and Discourse, since 1968
• American Philosophical Society, since 2004

PROFESSIONAL SERVICE:
• Member, Experimental Psychology Review Board, National Institutes of Mental Health, 1968-1971. Many reviews of grant proposals since then
• Member, Psychology and the Educational Process Committee of the Social Science Research Council, 1970-1972
• Member, Executive Committee of Division of Experimental Psychology, APA, 1974-1976. Chairman, 1975
• Member, Governing Board of the Psychonomics Society, 1972-1976. Chairman and President, 1976
• Publications Committee of the Psychonomics Society, 1974-1978. Chairman, 1978
• Chairman of the Stanford Psychology Department, 1978-1982
• Associate Dean of Humanities & Sciences, Stanford University, 1983-1986
• Member, “Forum on Federal Research Management” Committee of the Federation of Behavioral, Cognitive, and Neural Sciences, 1986-1989
• Member of the Advisory Board, The Institute for the Learning Sciences, Northwestern University, 1989-1996
• Member, International Union of Psychological Science (US Committee), 1991-1999
• Conducted reviews of research programs throughout psychology for the Office of Naval Research, National Institute of Aging, and National Science Foundation.
• Founding Member of the International Society for Research on Emotion, 1987
• Science Advisory Committee to Board of Directors of the American Psychological Association, 1989-1990
• Chief Science Advisor to the Director of APA’s Science Directorate, 1985-1992
• Board of Scientific Affairs, American Psychological Association, 1991
• Board of Directors, American Psychological Society, 1989-1993
• Warren Medal Committee, Society for Experimental Psychologists, 1991-1993
• Convention Program Committee, Western Psychological Association, 1993-1994
• Organizer and presenter, Presidential Symposium on “Neuropsychology” at Convention of the American Psychological Society, June 1993
• Reviews of several grant proposals to the R.K. Mellon Foundation from the Center for the Neural Basis of Cognition (CNBC) Carnegie Mellon University/ & University of Pittsburgh
• Chairman; External Advisory Board for the CNBC (above), 1994-1999
• The President’s External Advisory Committee to the Psychology Department; Carnegie Mellon University, 1993-annually through 2004); the Dean’s Arts and Sciences Advisory Committee at Carnegie Mellon University, 2000
• Presidential External Review Committees on Departments of Psychology at New York University, Northwestern, Princeton, Harvard, and University of Minnesota and South Florida
• Chairman of the NIMH Task Force (50 members) that produced “Basic Behavioral Science Research for Mental Health,” a 137-page report to Congress, assembled during 11/92-6/95
• Advisor to the Exploratorium Museum in San Francisco, CA, on human memory exhibits, 1993- 1996

EDITORIAL CONSULTANT:
• Journal of Comparative and Physiological Psychology, 1963-1970
• Journal of Experimental Psychology, 1965-1972
• Journal of Mathematical Psychology, 1964-1970
• *Journal of Experimental Analysis of Behavior*, 1965-1969
• *British Journal of Mathematical & Statistical Psychology*, 1965-1974
• *Cognitive Psychology*, 1969-1975
• *Cognitive Therapy and Research*, 1976-1980
• *Journal of Verbal Learning and Verbal Behavior* (now *Journal of Memory and Language*), 1968-1989
• *Cognition and Emotion*, 1986-2006
• *Connection Science: A Journal of Neural Network and PDP Research*, 1988-present
• *Consciousness and Cognition*, 1989-2007
• Reviewed grant proposals for National Science Foundation, NIMH, NIE, and Canadian and Australian Research Councils
• Senior Advisory Editor, *Encyclopedia of Psychology*, by Oxford University Press and the American Psychological Association. Published 2000
• Senior Advisory Editor, *Encyclopedia of the Social and Behavioral Sciences*, by Elsevier Publishers. Published 2001

**HONORS AND AWARDS:**
• Woodrow Wilson Fellowship for Graduate Study at University of Minnesota, 1954-55
• NIMH Fellowship for Graduate Study at Yale University, 1956-1959
• NIMH Postdoctoral Research Fellowship, Pittsburg VA Hospital, 06-08/1959
• Society of Experimental Psychologists, elected 1965. (President, 1989)
• NIMH Postdoctoral Research Fellowship, University of London, U. K. 1965
• Fellowship at The Center for Advanced Study in the Behavioral Sciences, 1973
• Fellow of Division 3 (Experimental) of The American Psychological Association, elected 1973
• American Academy of Arts and Sciences, elected 1975
• President of Division 3 (Experimental) of the American Psychological Association, elected 1975.
• Albert Ray Lang Professorship Chair, Stanford University, awarded 1975
• Sir Frederick Bartlett Lecturer, British Experimental Psychology Society, 1975
• Distinguished Scientific Contributions Award. American Psychological Association, Sept. 1979
• The Howard Crosby Warren Medal, The Society of Experimental Psychologists, awarded 1986
• William James Fellow Award for significant scientific contributions, the American Psychological Society, 1989
• Elected President of Western Psychological Association, 1990-1991 (President Elect, 1989-1990)
• Invited Keynote Speaker to numerous conventions, including the American Psychological Association, Western Psychological Association, American Educational Research Association, British Psychological Association, British Experimental Psychologists Society, American Association of Behavior Therapists, European Association of Behavior Therapists, and German Psychological Association.

• Elected President of American Psychological Society, 1991-1993

• Honorary Doctorate of Humane Letters, University of Chicago, June 1991

• Honorary Doctorate of Science, Indiana State University, May 1993

• Senior Science Advisor to the Director of the National Institute of Mental Health; Bethesda, MD. 4/92-4/93

• Board of Scientific Advisors, Science Directorate of the American Psychological Association. 6/93

• Chief Scientific Advisor, American Psychological Association. 1993-2006

• NIMH-MERIT Award for his research grant (5 years initial funding; grant was renewed for 5 years through 2002). Had 48 years of continuous research funding from NIMH, 1960-2008

• Wilbur Cross Medal for Distinguished Scientific Contributions and Career; Yale University Graduate School. May, 1995

• Keynote address to The Third International Conference on Human Memory in Padua, Italy, July 12, 1996

• Keynote address to convention of American Psychological Association on “Emotion and Social Judgement.” August 19, 1996

• Panel to advise NIH director’s office regarding support for behavioral science research within the NIH. February 1996

• The John Kendall Honorary lectures at Gustavus Adolphus College; St. Peter, Minnesota. April 1996

• Keynote address at opening of German Psychological Association, in Dresden. September 1998

• Advisory Committee to Director of NIMH on Psychology and Public Health Initiatives. October 1999-2000
• Presidential Citation for scientific contributions. American Psychological Association, 2002
• Keynote address to the plenary session of the Psychonomic Society in Vancouver, Nov. 2003
• Honorary Doctorate Degree, University of Basel, Switzerland. Nov, 2003
• American Philosophical Society, Elected Oct. 2004
• Elected President of the Western Psychological Society, 2004 (for second time)
• Lifetime Contributions Award, Western Psychological Association, 2006
• Received United States’ President’s National Medal of Science, 2005 (Public ceremony, July 2007)
• Elected Distinguished Fellow and Founding Member, Cognitive Science Society, Initial year, 2008
• Outstanding Graduate Student Mentoring Award, Western Psychological Association, 2011
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Stanford University--Center for Advanced Study in the Behavioral Sciences
Stanford University--Committee on Undergraduate Standards and Policy
Stanford University--Cubberley Auditorium
Stanford University--Dormitories-Wilbur Hall
Stanford University--El Dorado Avenue
Stanford University--Electrical Engineering Department
Stanford University--Emeriti Council
Stanford University--Executive Committee of the Provost
Stanford University--Graduate Curriculum Committee
Stanford University--School of Medicine-Health and Wellness Center
Stanford University--Humanities and Science (H&S)--Appointments and Promotions Committee
Stanford University--Jordan Hall
Stanford University--Little Theater
Stanford University--Memorial Auditorium
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Sterling, John E. Wallace (1906-1985)
Sternberg, Robert (1949-)
Stevenson, Adlai Ewing (1900-1965)
Stimulus and Association Learner (SAL)
stimulus sampling theory
Stochastic Models of Learning (Bush and Mosteller, 2012)
stochastic process
Students for Democratic Society (SDS)
subject-predicate structures
Sun Microsystems
Suppes, Patrick Colonel (1922-2014)
Taylor, Donald
telecommuting
Terman, Frederick Emmons (1900-1982)
text recall
draft, the (Selective Service System)
Psychology of Learning and Motivation, The (Elsevier)
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theoretical psychology
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theory of behavior
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Thompson-Schill, Sharon L.
T-maze
Trabasso, Thomas R.
transition error probabilities
Tulving, Endel (1927- )
Tversky, Amos Nathan (1937-1996)
UCLA Lake Arrowhead Conference Center, Lake Arrowhead, California
Underwood, Benton J. (1915-1994)
United States Department of Defense (DOD)
United States Department of Defense (DOD)-funding
United States Department of Education, Washington, DC
United States Department of Health and Human Services [formerly Health, Education and Welfare]
United States Department of Justice
universities and colleges--research
universities and colleges--faculty
Varian, Inc., Palo Alto, California
Venceremos Brigade
verbal discrimination learning
Vi at Palo Alto (formerly Classic Residence by Hyatt)
Vienna Circle
Vietnam War
Villanova, Katarina
Wagner, Anthony D.
Wandell, Brian A. (1951-)
Wessells, Norman K.
Woodrow Wilson Teaching Fellowships
word association
World War II
Wylie, Philip Gordon (1902-1971)
Zimbardo, Philip George (1933- )