

## SRCD ORAL HISTORY INTERVIEW

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Interviewed by John Waggoner  
Between 1992 and 1994

**Waggoner:** Okay. Anyhow, general intellectual history; describe your family background along with any childhood and adolescent experiences that may be of interest. Where were you born? I think I know the answer to that one. Where'd you grow up? I think I know the answer to that one. What was your schooling like? And there were some interesting things about that one, and military or early work history--

Palermo: Well, my mother and father--my father was an immigrant from Italy. He came over when he was two years of age. And in--when he was in high school his high school principle took an interest in him apparently and helped to get him a scholarship at Colgate University, so that's where he went for--to college. And he graduated from Colgate after his first year at Harvard Medical School. I guess he went for three years to Colgate and then was admitted to Harvard Medical School and, after one year at Harvard, he got his undergraduate degree from Colgate.

**Waggoner:** So he was actually in graduate school, in med school, before he had his--

Palermo: Undergraduate--

**Waggoner:** --bachelor's?

Palermo: --yes, right.

**Waggoner:** Oh.

Palermo: And he got his MD at Harvard, and then went to Albany, New York, for his internship, and there he met my mother, who was the daughter of Horatio M. Pollack and his wife, whose name was--her maiden name was Schaffer. I can't remember what her first name was now.

**Waggoner:** Her last name was Pollack?

Palermo: Yes.

**Waggoner:** Well, that's kind of--actually my aunt married a Bill Pollack from somewhere around Albany, New York.

Palermo: Oh, is that right?

**Waggoner:** That'd be pretty strange if--

Palermo: Yes.

**Waggoner:** --they were related somehow.

Palermo: It would be. But at any rate, my grandfather was the chief statistician in the New York State mental hygiene department, and retired from there after--I don't know, about 50 years. He got his PhD at--in Leipzig at the time that Wundt was there.

**Waggoner: Oh, wow.**

Palermo: I think he got his degree either in sociology or statistics, but I think he always considered himself a statistician, and he taught for a while at Union College, sociology and statistics.

**Waggoner: So that would have been late 1800s, early 1900s?**

Palermo: I think he got his degree--hang on a minute, I actually have some of this. Oh, my father got his PhD in Leipzig in 1897 and then came back to this country and had a variety of jobs before he joined the New York State mental hygiene department, one of which was teaching at Union College. I can't—I don't remember all the others.

**Waggoner: So you have a long—**

Palermo: But—

**Waggoner: --you have a long family background of academics?**

Palermo: Right. So he met my mother--my father's parents, of course, were not well educated. My grandfather on my father's side was a blacksmith in Italy when he--when the family came here he and his sons ran a grocery store, a neighborhood grocery store, and they lived over the grocery store in New Jersey. And most of the rest of the family grew up in New Jersey with the exception of one brother of my fathers, who ran away from the family because he couldn't stand all the fighting and went down to Texas--

**Waggoner: Well, big Italian family.**

Palermo: Right. My aunt on my mother's side was--one of them was an art therapist also. My mother got a master's degree in psychology and so there is some--

**Waggoner: Yeah, and what's all psychology--therapy--**

Palermo: --history of psychology in the family. So I was born in 1929, August 21<sup>st</sup>, 1929, and in Norwood, Massachusetts, which is where my father and my mother went after they were married. And they had to elope because my grandfather didn't approve of my mother's marrying a poor Italian boy. But they went to Norwood, Massachusetts, where my father did another internship or residency.

**Waggoner: Even though he was a doctor they didn't approve?**

Palermo: Oh no.

**Waggoner: Even though he was an MD?**

Palermo: No, not even an MD from Harvard was enough to convince my grandfather that he was worthy of his daughter, although once my father became a success then my grandfather approved of it.

**Waggoner: Well, that's usually the way things were.**

Palermo: So they were doing--he was doing another internship at the Palmer State Hospital near Boston, and I was born in the Norwood Hospital, Norwood, Massachusetts. And shortly thereafter my parents moved to Springfield, Massachusetts--

**Waggoner: Right. I knew that's where you--I thought that's where you grew up.**

Palermo: --which is where I grew up, yeah. And as a child my mother thought that I was brighter than average, and therefore ought to start school earlier than other children. It turns out that my date of birth was close to the date which you should enter school, but I shouldn't have entered in September. I mean, I shouldn't have entered until the September after my birthday, which is in August.

**Waggoner: Right. They gave you the option.**

Palermo: Right--

**Waggoner: Yeah, that's--**

Palermo: --no, they didn't give the option.

**Waggoner: Oh, they didn't?**

Palermo: No, what, what happened was that my mother went and got me tested with the Stanford Benet and in the fall of the year, I guess after my fifth birthday, and I got a high enough score on the Stanford Benet so that I entered kindergarten in January. At that time they had a 1A, 1B, 2A, 2B kind of system so that I started six months before, or actually eight months before I should have started the following September. Then, when I was about--let's see, I was in sixth grade--no, fifth grade, the end of my fifth grade I had a double mastoid operation. I had earaches all through childhood with a great deal of regularity, and the doctor would come and slice my eardrum, and drain my ear, and relieve the pain and that was the standard procedure. Well, after a while it just--the earaches got so regular. I was getting an earache a month or something like that.

**Waggoner: That didn't affect your hearing, slicing your eardrum?**

Palermo: Oh yes, it did to some extent. My eardrums were all scarred and it has in fact affected my hearing a little bit, not too badly.

**Waggoner: No. I never really noticed anything.**

Palermo: And while--after I had the double mastoid, which took me out of school for about six weeks in fifth grade, just at the end of fifth grade, then they changed from 1A, 1B business to a regular school year, and so you couldn't enter anymore in the middle of the year. So then I had to do a year and a half in a year in order to catch up, so that put me another six months ahead. So when I graduated from high school I was 16 instead of 17 or 18--

**Waggoner: Yeah, boy, hat's off to you.**

Palermo: --and, well, to back up again, I wasn't doing very well in school and--

**Waggoner: Underachiever?**

Palermo: --I was an underachiever, and I was also not as mature as my classmates--

**Waggoner: Well, that's--yeah, that's true.**

Palermo: --and so I wasn't doing very well, so my parents decided they'd send me to prep school thinking that that would help, so I went to Loomis School in Windsor, Connecticut, for my junior and senior years in high school, and I didn't do all that much better there to be perfectly truthful. I did a little better, I'm sure I learned more than I would have, but I still was an irresponsible youth, if you will, and managed to get involved in a few things, not as bad as some of my classmates that actually got kicked out, but--a few days before they were supposed to graduate in my senior year. But at any rate, at the age of 16 I graduated from high school and was admitted to Colgate University--

**Waggoner: Yeah, I knew you went there.**

Palermo: --which of course was my father's alma mater--

**Waggoner: Yeah, I never knew that. Now, that makes sense why you'd go there.**

Palermo: Yeah. And after two years I'd accumulated one B, eleven Cs, seven Ds, and an F, and I got the F in philosophy and religion. I didn't like the sex fiend who was teaching the course, and he was, in fact--put a little more emphasis on sex than most people teaching that course might have.

**Waggoner: In philosophy and religion?**

Palermo: Yup. He managed to get it in one way or another.

**Waggoner: I suppose you could talk about primitive fertility rights or something like that, but jeez. See, I didn't know you were 16 when you went to Colgate? See, that explains--**

Palermo: Well, actually, by the time I arrived I was actually 17--

**Waggoner: Yeah, but still.**

Palermo: --because my birthday was in August. But--

**Waggoner: Still though, that's--**

Palermo: So the--at the end of my second year at Colgate my parents received a letter indicating that I was no longer welcome at that institution, that I'd flunked out. And I remember I got home--I went to visit some friends before coming home, so by the time I got home my parents had received the letter and knew what I didn't know upon my arrival. And my father came home from a party when he found out that I was home, and--

**Waggoner: Oh, so he left the party?**

Palermo: --he left the party to come home and have a discussion with me, a discussion which consisted of his being extraordinarily angry that I, who had all the benefits of his much better financial condition than his parents had been, who didn't have to work to get through college, had flunked out. And his solution to this problem was to buy me a gas station in the poorer section of Springfield and set me up in the gas station and then never hear from me again. Fortunately some of the friends of the family who were also at the party probably recruited by my mother realized that something might be going on, and they came over and broke up this affair. And subsequent to that, why I was try--various members of the family and other friends tried to get me into colleges that I might continue my education. So my grandfather, for instance, tried to get me into Union College where he had graduated and gotten an honorary degree and that didn't work. And some people, friends in Springfield, tried to get me into Springfield College, and that wasn't working out very well for some reason or other. I've forgotten why now.

**Waggoner: Well, I guess with an academic record like that though it's not going to be too easy to get in. But--**

Palermo: And my cousin, who was a lawyer in New Jersey, thought that he could--he was only a few years older than I am--he thought he could get me--use his pull with the legislature to get me into Rutgers, and that didn't work, or at least nothing happened soon enough. So finally he decided that he'd take me down to Washington and Lee, which is his alma mater, and see if he couldn't get me in there. So we drove down one weekend in the middle of the summer to--actually, it must have been

the latter part of July, and we met with the admission officers at Washington and Lee and they were perfectly happy to take me until they discovered that I had flunked out of Colgate, I wasn't just transferring, at which point they cooled. So my cousin said, "Well, aren't there any small schools around here that might give this guy a second chance?" And he suggested Hampden Sydney, or Roanoke or Lynchburg College, so my cousin said, "Which is closest?" and Lynchburg was closest, so we drove down there and entered the Grand Old Mansion, an old hotel, which was the main building on campus. It happened to be the women's dorm and the offices of the president and deans and so on, and we met the dean of men, who discussed my reasons for my failing at Colgate, and I told him it wasn't any particular reason other than that I didn't pay too much attention to my studies, had a lot of fun drinking and playing pool, and bridge and--

**Waggoner: Yeah. Well, that sounds like a typical freshman college year, actually, even today.**

Palermo: Well, I did work on the college magazine, and that was about the only thing I did that was of any note other than academic, which wasn't of any note. So he wondered--I don't know whether I should go into this, but he wondered whether I had any trouble reading. And I said, "No, I don't have any trouble reading," and he pulled a book off his shelf, and he handed it to me and he said, "Here, read," so I read, and they said--and he said, "No, you don't seem to have any trouble reading." And so we talked for a while, and then the dean of women came in and--or waddled in. She was quite a heavy woman, and turned out to be a wonderful woman that I got to know later. But she allowed as how they weren't very happy to take us city slickers from the northeast into--

**Waggoner: I was wondering about that, a Yankee going to a southern school--**

Palermo: --a southern school with religious ties to the Church of Christ, First Church of Christ--

**Waggoner: And you flunked philosophy and religion--**

Palermo: --well, yeah. I didn't point that out. But at any rate, they couldn't make any decision about me until the dean, the head dean, came and he wouldn't be back for a couple of weeks. In the meantime they decided they'd better give me an intelligence test, so they thought--they called in the secretary and she brought her little Otis Quick-Scoring Test, which is a half hour test, timed test--

**Waggoner: What's--a version of the Otis Lenin or something or--**

Palermo: --maybe. Yeah, I don't know.

**Waggoner: Yeah, I don't know. But--**

Palermo: But at any rate, she took me into the president's office and sat me down at the president's desk, who happened to be away, and told me I'd have a half hour and not to worry, I wouldn't finish the test, and just do the best I could. So in about 25 minutes I was done with the test, and she was very upset that I didn't take the full 30 minutes. Well, while they were scoring that test they decided to send me out on campus, and they talked with my cousin, and I wandered around campus, and met some students, and discovered that, even though there was no smoking in the center of the campus and there was supposed to be no drinking on campus, that people did it anyway, and got away with it, and that the general school wasn't going to be quite as straight-laced as I thought it was going to be from the description of the deans.

**Waggoner: It's usually that way.**

Palermo: Right. So I was going to get drafted--

**Waggoner: I was wondering about that because I knew this was going to be the--**

Palermo: --on August 21<sup>st</sup> if I didn't have an acceptance into college, and yet the dean was not going to be back in time to give me acceptance by August 21<sup>st</sup>, so I left--we left the campus with them saying that they thought there was a good possibility, but they couldn't approve it until the dean came back. And so we worried about that all the way home, and then I took a train home from New Jersey to Springfield, and when I got home on Monday interestingly enough there was a letter there saying I was accepted on probation.

**Waggoner: That's fast. That's faster mail than today.**

Palermo: Yeah.

**Waggoner: What year would that have been, like '46? '47?**

Palermo: Well, I got--no, I got my high school--I graduated from high school in '46.

**Waggoner: Okay. That would have been '48.**

Palermo: And then '48 is when I was--this was happening.

**Waggoner: There was still a draft then even though the war was over?**

Palermo: Oh yeah. And that was the Korean--the Korean War was--

**Waggoner: Oh, I didn't think that had started yet. I thought that didn't start till '50 or--because I wasn't sure--I knew you were too young for World War II, but I didn't know if you were one of those people who fell in a crack between the two or was--**

Palermo: Actually, I wasn't too young for World War II. One of my friends was in World War II who was the same age as I.

**Waggoner: Really?**

Palermo: Yeah. A fellow by the name of Jack Armstrong, the all American--

**Waggoner: The all American boy, yeah---**

Palermo: Yeah. His birthday is actually about 20 days after mine. We were the same age in school. In fact, there were the group of seven of us in junior high school and high school who hung around together. We used to have a regular poker game, and we played, and we did things together, and had a basketball team, and we played in the YMCA league and things of that sort. And all seven of us went to college, and all went to different colleges. And Jack Armstrong, the all American boy, went to Westland where he was an all American soccer player and distinguished himself in a number of other ways, eventually went to Harvard Business School, and he got his degree there after Westland. One of those seven died of cancer at about the age of 35, two children. At any rate--this isn't very systematic.

**Waggoner: Well, it's not supposed to be. Actually, I think you've already sort of jumped ahead to number two, which was what early adult experiences were important to your intellectual development, and then it said collegiate experiences. Actually, we haven't done that yet. So--because you haven't said much about intellectual developments considering how you flunked out of Colgate and just got into Lynchburg.**

Palermo: Well, when I was at Lynchburg I did much better. The competition actually wasn't as hard so it was easier to do better, but I also paid a little more attention to my studies at Lynchburg than I had done at Colgate, and recognized that if I didn't make it this time I was in serious trouble.

**Waggoner: Yeah, I'd say.**

Palermo: So at Lynchburg I not only did much better academically, but I was involved in a lot of affairs on campus. I was the president of the photography club, and I was in plays and I was the managing editor and then editor of the college newspaper--

**Waggoner: Well, what was your major? Was it psychology?**

Palermo: Well actually, it wasn't psychology, because they didn't have a major in psychology. They don't have enough courses to have a major in psychology. So I majored in education and psychology, and I minored in physical education.

**Waggoner: I think that was kind of common to be education and in psych. It was that way at Bloomsburg for a while; there was no separate psych department.**

Palermo: So I--but when I got around to--well, at Lynchburg I then met my first wife, or wife to be at that time, and we got married the--she was one year behind me, and we got married after she completed her degree on her mother's insistence. Her mother had never gone to college, and her father had never gone to college, and her father was a boxer and a shoe salesman, and her mother was much brighter than her father and with a--eventually rose from secretary to owner of a real estate agency in Washington, D.C., a real estate management firm actually. So her mother would not let her daughter get married until she'd graduated from college.

**Waggoner: Well, that's understandable, first generation college student--**

Palermo: Yeah, it was smart in any case.

**Waggoner: So you were gone for a year then before--**

Palermo: So I was gone to U Mass before we got married. We got married at the end of our--of my getting my master's degree.

**Waggoner: You did it in one year?**

Palermo: Mm-hmm.

**Waggoner: Boy, I'm ashamed now [laughter]--four, five, six.**

Palermo: Well, I was--they had a master's program, and it was basically set up to do in one year, and if you kept your nose to the grindstone you got it done in one year.

**Waggoner: And that was in psychology?**

Palermo: That's right, in psychology. But U Mass was the only school that would accept me. I got turned down by University of Minnesota, where I subsequently worked--

**Waggoner: I was going to say, you were back to there for a job.**

Palermo: --I got turned down at--well, a whole bunch of reasonably good schools to which I applied. But I finally got accepted at the University of Massachusetts, where they had only a master's program at that time.

**Waggoner: It was in what, experimental or--**

Palermo: Experimental psychology, yeah.

**Waggoner: --because I knew you later on, your dissertation was on rats jumping. You were more or less a rat runner.**

Palermo: Yup, my--this thesis was on fixated behavior of rats, like the affects of electric convulser shock on the fixated behavior of rats, an R.F. Mayer type problem of the time. And that was--and it had a--had the wonderful experience of having my first master's thesis proposal appear in the literature before I had to do it, because my thesis advisor, Claude C. Neet who was chairman of the department at U Mass, wanted to do an experiment with rats in a water maze and I wanted to do this like I wanted a hole in the head. Fortunately, some guys at the University of Pittsburgh did the experiment before I had gotten really into it, and so I managed to do a study of the affects of electric convulser shock on fixated behavior of the rat, which was an area that my dissertation advisor and Bob Feldman, another member of the department, were working on at the time. And so my thesis grew out of some of their work.

**Waggoner: And which water maze study was that? Because when I was taking Walt Weimer's course he used to always talk about how a water maze study sort of was used as argument against the idea that the animal was learning specific muscle movements because obviously it can swim to it. It's not the same muscle movements as the--as walking to it as--**

Palermo: Well, this was done back in 1952--

**Waggoner: Yeah.**

Palermo: --so--

**Waggoner: Yeah. I just remember him always talking about that, it's--**

Palermo: --probably long before Walter's concern with water mazes--

**Waggoner: Yeah, but I think in history he was talking about how that was used as an argument against the idea of learning specific muscle change responses.**

Palermo: Yeah, that's certainly an interesting argument. I don't know the full details of it, but--

**Waggoner: Yeah. Well, I just--**

Palermo: --but clearly if you get transfer from a water maze to a running maze, why, you can't use the specific response as a transfer of the specific response.

**Waggoner: Yeah, that's what I thought that was, I--**

Palermo: Interesting.

**Waggoner: --I don't know, that's what I--that's what he always talked about it as. So how'd you get to child development from--**

Palermo: Oh, well, I was always interested in children right from the very beginning I--when I was in junior high school I had two newspaper routes. I had the evening newspaper and the Sunday newspaper, and the people at the end of my route used to complain because I didn't arrive very early on the evening route, in particular, because I was playing with all the kids along the way, and the kids used to flock out to meet me when I would bring the papers, and I'd have to stop and talk to them and play a game with them, or toss a ball with them a little bit. And it was a big event to have me come around delivering the newspaper because I paid some attention to them, and played with them, and



talked with them and so on, so my interest in children went back a long time. In fact, long before I went to college and was saying I was going to be a doctor, like I thought my father wanted me to be, that I would be a doctor of children from the ages of 8 to 18, which of course there is no such specialty. But that was what I had in mind as the ideal age to be a physician for, and of course, when I went to Colgate I was premed, and wasn't very much interested in particularly the chemistry involved. The biology was interesting, but the chemistry was awful as far as I was concerned.

**Waggoner:** Yeah. You probably wouldn't have gotten much of anything about kids either from--

Palermo: No.

**Waggoner:** --from that.

Palermo: No, so I took some psychology, and while I was--well, in the middle of my sophomore year I decided I wanted to change my major to psychology. And I had made a good friend of a guy in the art department, a fellow by the name of Kruckoozan, who taught a course in jazz that I had taken, and I liked it very much, and I'd always liked jazz. I'd gotten interested in jazz when I was in prep school. There were some friends who had wonderful collections. And I took his course in jazz, and got to be friendly with him, and I remember going to him and telling him that I thought that I'd like to change my major from premed to psychology. And his question, which took me back at the--took me aback at the time, was, "Do you want to change into psychology because you think it'll help you or because you're interested in psychology?" It was a good question.

**Waggoner:** Well, think a lot of people get asked that question when they become psych majors.

Palermo: Well, they probably don't get asked it often enough.

**Waggoner:** Yeah. But it's--yeah, that's true.

Palermo: At any rate, I did want to change, and didn't get the chance because, of course, I flunked out. But when I went to Lynchburg, of course, I did change insofar as that would allow.

**Waggoner:** Did you--

Palermo: I could do it.

**Waggoner:** Were there any real psychologists on the faculty at Lynchburg, or were they all educational type--well, I shouldn't say educational is not real psychology, but I mean straight psychology, basic developmental basing--

Palermo: No, there was one--

**Waggoner:** --experimental--

Palermo: --one fellow that came to the department while I was there who only lasted one year because his method of teaching was to read the book and rehash it for the class, and I got very upset with that, and--as did a number of other students, but after the first course when I used to sit in class and take notes, after the first course I went to the back of the room and slept through the class. If we were sort of required to come to class, and he took attendance, but I refused to participate, because it was such awful--

**Waggoner:** Yeah, if it comes straight out of the book, I mean, you know, because if anything it's not in the book or you're just reading the book, why--

Palermo: My philosophy as an undergraduate was, if you go to class and listen you can--you're guaranteed a C, and anything you do over that is gravy. And at that time, of course, I was doing more over that than I did at Colgate. So--but I--to be perfectly truthful, I never really got involved intellectually in any of my academic work except sporadically until I got into graduate school. And then--

**Waggoner: Well, it seems kind of--it seems strange to me that you would go to--would want to go to grad school for psychology if you weren't really that much into it as an undergraduate--**

Palermo: Well, I was into it as an undergraduate, but I didn't--the teachers didn't do anything to--

**Waggoner: --okay.**

Palermo: I subsequently met this fellow at a meeting after I'd gotten my PhD. I went to a meeting of-- at Washington University in Saint Louis celebrating something about some series of dates related to Freud, Wundt, and somebody else. At any rate, it was a conference, and this fellow showed up at it, it's the guy who was such a lousy teacher, and he wanted to know if he could take credit for my getting a--stimulating me to go on to get my PhD--

**Waggoner: Boy, talk about a question that requires some tact, huh?**

Palermo: --yes. Well, I was a hypocrite and said, "Oh yeah, I guess so."

**Waggoner: You actually, I think, went on to question number three, what are your origins of your interest in child--I just heard the clock chime at quarter to five already, and we're only on question number three in the first part. But what are the origins of your interest in child development, what individuals were important to your intellectual development, and obviously not that particular individual? Who were your research mentors and significant colleagues?**

Palermo: Well, Kruckoozan at Colgate was certainly an important figure in terms of acting as an advisor to me, and although he wasn't a psychologist, he was in art; he was a sympathetic and interesting person that I enjoyed very much. In fact, he's subsequently--about 20 years later, I guess-- it might have been a little more than 20 years, committed suicide, and the--I wouldn't have known this but that the Colgate alumni news merely mentioned that he died, and I wrote them and said I was very upset because I thought he was a particularly effective teacher, and a marvelous individual and I thought they should have given much more information about his dying, and they wrote back and said that he had committed suicide, and that was why they hadn't--

**Waggoner: Yeah, they wouldn't do that.**

Palermo: --and I had an exchange with them about further elaborating my feelings about him. But at any rate, he was very important to me. There was a sixth grade teacher I had who was very important to me, a Miss Pomeroy, who was one of the people who managed to turn me on intellectually as a child, one of the few teachers I had who turned me on, and I did much better work for her than I did for anybody else in school. Then, when I got to--I wouldn't say there was anybody at Colgate other than Kruckoozan who really took an interest in me or in whom I took any particular interest. When I got to University of Massachusetts Al Goss was very important to me in many ways, and he took an interest in me and I in him. And at the same time Lew Lipsitt was getting his master's degree at U Mass, and he and I became very good friends then. In fact, we both got married at the end of that year. Lew got married to his wife, Edna, the week before I got married. And when we came back from our honeymoon a week after getting married they had returned to Amherst, and they had a key to our apartment, and they went in and pied our bed. When we returned to our apartment we were most surprised by that. But at any rate, that's where my friendship with Lew Lipsitt began, and we've been very close friends ever since. But Al Goss took an interest in me there, and introduced me to experimental psychology in a serious way. I had, when I was at Lynchburg College, gone to Randolph

Macon Women's College and taken a course in experimental psychology, because they didn't offer one at Lynchburg.

**Waggoner:** Well, I guess you can't have much of a major without a course in experimental.

Palermo: But Al Goss really got me involved in experimental psychology, and that was much to my delight. I learned Hullian theory under his tutelage, and a little philosophy of science of the Birdman variety since Al himself had gone to Iowa and received his PhD at Iowa.

**Waggoner:** Good positivism.

Palermo: Yeah, right. Good logical positivism. And when it came time for applying for graduate school to go on beyond the master's degree I really wanted to go to Iowa. I thought that was the--on the basis of Al's background I wanted to go to Iowa, and that was the first place I sent away for an application. But--

**Waggoner:** Well, that was the big school then, wasn't it? I mean, that was--

Palermo: --well, certainly one of the big ones, yeah.

**Waggoner:** --one of the top five or six in the country for--

Palermo: Yes, for experimental psychology within the Hullian tradition at any rate.

**Waggoner:** Right, yeah.

Palermo: Yeah, kind of--with it there. Of course, he was a--and Hull had died the spring in 1952--I believe it was the spring of 1952 that Hull died I remember, anyway. We had a closing of the curtains in Al Goss's class when he heard that Hull had died. So I wanted to go to Iowa but I didn't think I could get in, and so I applied to all kinds of other schools. I applied to University of Tennessee was my backup school. I thought I could get into Tennessee if I couldn't get into anywhere else, and the first place I got turned down was the University of Tennessee. And I never sent in the Iowa application until the very last minute. I--it was only a two page application basically, and so I decided at the last minute what the heck, I didn't have anything to lose. I'd send it in. And I, of course, got accepted at Iowa, and was given an assistantship, and I was absolutely delighted. And so my new bride and I, after we got married in June, went out to Iowa. We hooked up a trailer on the back of our car that my parents had given us as a wedding present, and headed out to Iowa, naïve young couple that we were.

**Waggoner:** Who needs a trailer?

Palermo: Yeah. And when we got to Iowa we were able to get into married student housing, which at that time the part that we got into was a trailer, which had no running water, and was ten feet, literally ten feet from a railroad track, and about 50 feet from the highway that went through town.

**Waggoner:** Nice and quiet.

Palermo: Right. So each time the train went by, and fortunately it only went by twice a day, it would shake the trailer, and all the curtains would have to be--I mean, all the pictures would have to be straightened on the wall and things of that sort.

**Waggoner:** Grab your papers and stuff on the table before they fall on the floor.

Palermo: And we had two trailers nearby that were washing facilities, so every time you wanted to go to the bathroom you had to go out to a trailer and--

**Waggoner: Just like being at camp.**

Palermo: --right, it was. And if you wanted to wash the dishes you had to go get water and bring it back to your--put in the sink, and catch it in a bucket underneath when you were through. And there were several times when we forgot to put the bucket underneath. And we had a gas heater--

**Waggoner: Just a gas heater in Iowa in the winter?**

Palermo: Mmhmm, for the--

**Waggoner: Oh boy.**

Palermo: --for the--to keep us warm, and the couch used to fold out to make the bed. We didn't have a separate bedroom in this. It was just a small trailer. And in the wintertime all the local bums used to come and sleep in the bathtubs in the--in the johns. Unfortunately, the women's john was the one that had the bathtub, so--but they adapted. They got--all the women got so they didn't pay any attention to the bums sleeping in the bathtubs.

**Waggoner: Jeez. Boy I thought I had--I thought I had it bad when I had a three-bedroom house in graduate school.**

Palermo: Oh, that must have been tough.

**Waggoner: Yeah. Boy.**

Palermo: Well, when my wife worked at the hospital. She worked in the otolaryngology department as the secretary. And I had an assistantship, so that my assistantship paid \$90 a month, and her job paid nearly twice as much, I think, as I was getting. And there I met Boyd McCandless, Al Castaneda and Charlie Spiker, all of whom now are dead. Charlie Spiker just died a week ago Sunday.

**Waggoner: I was going to say, because I know you were just for a conference to--for him a couple years ago.**

Palermo: No, he died of thoracic--of cancer of the throat.

**Waggoner: Oh, jeez.**

Palermo: Something that he really didn't want anybody to know about. He was a very private individual. So they were--Boyd McCandless had taken over the Iowa Child Welfare Research Station about two years prior to my arriving, I think it was two years, and he had hired his two former students, Charlie Spiker and Al Castaneda.

**Waggoner: I better stop this. Okay, we should be rolling again. So you met McCandless, Castaneda and--**

Palermo: Yeah, it was McCandless had hired his two former undergraduate students from San Francisco State College, Charlie Spiker and Al Castaneda, that he'd sent down to graduate school. And Charlie had gone to Iowa and was--and had gotten his PhD in the Iowa Child Welfare Research Station, and Al Castaneda had gone to Ohio State where McCandless himself had subsequently gone. And so when McCandless, who himself had graduated from the Iowa Child Welfare Research Station when Kurt Lewin was there, had returned as the director of the Iowa Child Welfare Research Station--

**Waggoner: And that wasn't part of the psychology department?**

Palermo: No--

**Waggoner: It was completely separate?**

Palermo: --it was in the same building as the psychology department, but the Station was in the west wing and had the fifth, sixth, and seventh floors of the west wing. The psychology department was in the center of the building on the first and second floors, and they had some rooms in the basement, as well. But it was also ed psych, which Lindquist was in, was in that building underneath the Iowa Child Welfare Research Station was the ed psych department. So we had--and the psychology department also had the east wing of the building. So we were close enough to the psychology department to be, you know, the students all did things together, and all the ed psych students really didn't interact much with the other two groups interestingly enough.

**Waggoner: It's pretty much the same way here, though, from what I remember when I was here. They didn't really--they took our courses--they took courses here, but they didn't really do too much otherwise.**

Palermo: Yeah, that's true. So what these three individuals wanted to do was to start--and what they did do was to start the so-called experimental child psychology movement. Prior to McCandless's taking over--he took over after Sears had left, and Beth Wellman had died at--who were significant figures, and of course, Lewin had gone and actually was dead by the time McCandless came back, but McCandless was one of the experimenters in the famous democratic/autocratic, laissez faire--

**Waggoner: Oh, leadership styles, yeah.**

Palermo: --leadership style study. He was one of the leaders as a graduate student.

**Waggoner: One of the confederates--**

Palermo: Yeah--

**Waggoner: --experiment, yeah.**

Palermo: --so, but at any rate, the three of them along with Howard Meredith, who was the physical growth man, who was sympathetic to their position. The four of them really started the experimental child psychology movement.

**Waggoner: Meaning that before that it was mostly, like, naturalistic observation based--**

Palermo: Yes.

**Waggoner: --or more descriptive rather than informational?**

Palermo: --yes, much more "developmental" in the old use of the term, that is, when I was a student we made a distinction between developmental psychology and experimental child psychology.

**Waggoner: Oh really? Okay.**

Palermo: And developmental psychology was the form of psychology advocated primarily as the major spokesperson was Dale Harris at Minnesota. And McCandless, and Spiker and Castaneda in particular--Meredith was supportive within the department, but wasn't really an experimental child psychologist. He was really a developmentalist in a developmental tradition only in physical growth. And other people in the department at the time were Orvis Irwin, who was an early language development guy, but he was definitely in the developmental tradition, and very anti-theoretical, which was one of the things that the new group was trying to push, was much more theory and much more experimental research. I remember riding up in the elevator with Orvis Irwin one time after we'd had a class of his,

and I asked him how he accounted for some particular data he had collected that he'd been talking about in class, and he says, "Oh, I don't know how to account for it." He says, "I just collect the data and show you descriptively how it works." He says, "You can get somebody else to do the theorizing. I'm not going to do any of that."

**Waggoner:** Jeez, that's hard to believe.

Palermo: So it was a classic description of what McCandless, Spiker and Castaneda were arguing against, just a collection of data without any theoretical framework within which you were collecting it.

**Waggoner:** Did it involve manipulation or it was more naturalistic observation based?

Palermo: Yeah, he was describing the early sounds in infancy, for instance.

**Waggoner:** So you'd have the norms and everything for development, but you--

Palermo: Right.

**Waggoner:** --wouldn't have any manipulation based on a theory--

Palermo: No.

**Waggoner:** --to see what was causing--yeah, okay.

Palermo: No. You'd have interpretations in terms of age. That is, you'd say as they get older these changes occur, and these differences between boys and girls and the--

**Waggoner:** Yeah.

Palermo: --things of that sort, but--

**Waggoner:** But not *why* boys are different than girls?

Palermo: --no.

**Waggoner:** Okay. Well, that's interesting.

Palermo: Other people at the institute at the time included Ruth Updegraff, who was in charge of the nursery schools, and Ralph Ojeman, who was--did a lot of parent/child kind of things, but most of it, he--most of what Ralph Ojeman did was to go out and talk to parent groups and this sort about child development and--

**Waggoner:** Yeah, like how to raise the child properly or--

Palermo: Right. And everybody used to run that portion of the department down, or Ojeman had money for this, and he used to hire people. He hired Gene Lovett and Seymour Zelen, who were there as employees of Ojeman within the framework of the institute, or the station. So those were the main people who were in the department at the time. Later on Charlie Smock was hired as another experimental child psychologist.

**Waggoner:** But that was all in the institute, right?

Palermo: All on the--not the institute. The institute was at Minnesota. It was called the Iowa Child Welfare Research Station.

**Waggoner:** Right, sorry, that's right, yeah, the Child--that was all the Child Welfare.

Palermo: Right.

**Waggoner:** And the psych department proper there was nobody?

Palermo: Oh no, in the psych department proper there were a--nobody in child--

**Waggoner:** That's what I'm--yeah, that's what I'm getting at--

Palermo: --or developmental. Right. But the people--the main people in the psychological department including Spence who was head of the department--i.e. Farber, who was the clinical--head of clinical, Harold Bechtold, who was a statistician and tested measurement type, Don Louis, who was another methodology type, and who did motor psychology of motor activities, motor learning, motor learning, and Brown, Judson Brown, who was the motivation to--he was also a student of Hull's, or actually a student of Miller's at Yale before coming to Iowa.

**Waggoner:** I guess I wanted to--well, I guess I wanted to ask is did he get in trouble for spending so much time in the child research station? In other words, they were separate--

Palermo: Well, they were separate, but when McCandless, Castaneda, and Spiker took over we actually took more courses in the psychology department than we did in the station.

**Waggoner:** Okay. Well see, I wasn't sure about the connection there, because--

Palermo: One of the things about Iowa at the time was that it had a pretty straight jacket program, that is, when you entered at the beginning of your first year you knew what courses you were going to be taking almost every semester thereafter, and when you would graduate, assuming all went well and all usually had to go well or you left. So that was true of both in the institute and in the psych department.

**Waggoner:** So it was more like the relationship between psych and human development, which was, you know, you can jump back and forth with no--

Palermo: Yes.

**Waggoner:** --complications?

Palermo: Right, they're not--in fact, I became the president of the psychology colloquium much to Spence's disgust that, you know--I must admit, he was very upset to discover that somebody outside the psychology department, well, a student outside the psychology department had become the president of the colloquium, which was run by the students in those days.

**Waggoner:** You were officially a psych major, though, right?

Palermo: No, no.

**Waggoner:** Oh, okay. I have--alright.

Palermo: I was the Institute of Child Development, I mean the Child Welfare Research Station.

**Waggoner:** Officially that's-- officially that's where you were then? Okay. I always thought officially you were in the psych department.

Palermo: No, no. I took more courses in the psych department than I did in the station, but I--

**Waggoner: Now I got it.**

Palermo: --uh-huh, but that was because I was required to more or less.

**Waggoner: No, I got it now.**

Palermo: So when I was president of the colloquium the students got uppity, that is, the other members of the--the other officers got a little uppity, and Harold Bechtold was our advisor, and they decided that the faculty had been turning down people that we wanted to invite, and it was our organization, and we shouldn't have to get the faculty's approval to invite people that we thought we'd like to hear. So it was my job to go and talk to Spence about this particular problem. So I made an appointment with him, and I went into his office at the appointed time, and presented this problem to him. I said the students would like to be able to invite without faculty rejection of our candidates. And he exploded. He got up through the ceiling. He didn't see any reason for that, and he didn't see why we knew--they knew these people, and we didn't know them, how in the devil could we know who to invite and who not to invite. So I sat and let him blow off his steam, and when he came down to--I said, "Well, perhaps it might be appropriate for us to find out why these people that have good names, big names in the field aren't so good. Maybe we should be able to see--listen to some of them and find out what you know about them," and made a number of other arguments along similar lines for our being able to invite who we wanted without their rejecting them. And interestingly enough, he took me seriously and thought maybe the arguments I was making were good arguments, and that we should be allowed to have more freedom in inviting the people we wanted. And in fact, from that point on Spence and I were very good friends, and he always made sure that I was invited to the luncheons with the speakers who came in. I, of course, had to go and pick them up, and in those days going to Iowa to give a colloquium was a very scary thing for a lot of professional psychologists.

**Waggoner: Worse than coming here?**

Palermo: Oh yes--

**Waggoner: Would it matter if it was at the Black Moshannon?**

Palermo: What?

**Waggoner: I said would it matter if it was at the Black Moshannon?**

Palermo: Oh, no, no. They were scared of the faculty, of talking to them. They weren't afraid of coming to--

**Waggoner: Oh okay. I see. I thought you meant the travel--**

Palermo: --no--

**Waggoner: --oh. I understand, yeah.**

Palermo: --no, they were just--I was holding the hands of people like Brady, and the executive monkey guy, Carl Duncan at Northwestern, Charlie Osgood, who came in to speak--

**Waggoner: Really? Osgood was afraid--**

Palermo: --and they were all--

**Waggoner: --he was a little nervous?**



Palermo: --yeah, they were all nervous, and they--asking me, you know, “What’s it like to talk to this group? Who’s going to ask the worst questions?” and things of this sort. And so I felt very strange as a graduate student--

**Waggoner: Yeah.**

Palermo: --holding the hands of these so distinguished psychologists.

**Waggoner: Well, that makes sense. It’s be like going to MIT or something now and, you know, wondering what Chomsky was going to ask you or something.**

Palermo: Right. That’s right. But I always got to go to luncheon with Spence and whoever went to lunch with these speakers, and Spence was very--went out of his way to be nice to me. In fact, later on when it came to job hunting time, unbeknownst to me, kind of had been out looking for jobs for me, and in fact, I guess it was in May sometime, sometime after MPA, and it must--maybe in June or July, Spence saw me in the hall and asked me what I was going to do for a job next year, and I was--very proudly announced that I had been offered and accepted a job at Southern Illinois University. And he got very upset and he says, “Oh, you students, you take the first job that’s offered to you,” and he said, “That wasn’t a very smart move.” And I still didn’t know anything about what he’d been doing until I got down to Southern Illinois and I--my officemate was Izzy Goldiamond and he said, “Oh, this is where you ended up.” He says, “Spencer was working on getting you a job at Chicago.” And I said, “I didn’t know that,” and he said, “Well, it wasn’t--they were trying to set it up, but they hadn’t gotten it set up soon enough.” And then Hal Page came down from Wisconsin, and he said, “Oh, this is where you ended up.” And he says, “I thought you were coming to Wisconsin,” you know, “we had--we were trying to get some soft money together to hire you.” And then somebody from somewhere else, I’ve forgotten where now, but had also been approached by Spence trying to get me a job. He never told me anything about that.

**Waggoner: Oh jeez.**

Palermo: But--so I knew I was on good terms with him, and he was very good.

**Waggoner: You didn’t do your dissertation with him then or under him?**

Palermo: No, I did my dissertation with Al Castaneda. I started off--my first thought was to do it--I was interested in language at that point, children’s language at that point, and I wanted to do a dissertation on having to do with language development. And I approached Charlie Spiker first about doing that, and Spiker said, well, he didn’t see how I could do a dissertation on language, because I certainly didn’t want to do a developmental study of language like Dorothy and McCarthy, for instance, and there were no memory drums for children, that is language was paired--associate learning as far--or serial learning as far as he was concerned. So that precipitated--I said, “Well, we should get some equipment so we can do that.” Well, I went and talked to Hunter, who was the shop man in the department of psychology, and he developed what came to be known as the Hunter Card Master, which is a machine that--into which you could put plastic cards, on which you get a mount material like pictures that you could use with children on paired associate learning or serial learning tasks, and it turned out that the machine wasn’t all that great, but I used it in a number of studies and so did a lot of other people. But since I couldn’t do a study on language and that apparatus wouldn’t be ready in time for my dissertation, why, then I approached Al Castaneda about doing some studies on the H times D hypothesis, and I eventually did an experiment on the effects of interaction between motivation or drive and habit strength.

**Waggoner: Okay. So that’s why I thought you--that’s what I thought you did under Spence, but that sounds more like plastic, you know, all Spence type question.**

Palermo: Yeah.

**Waggoner: So, that something would be done in the Center for the Study of Child Development but--**

Palermo: Well, in fact, the dissertation I did on the H times D hypothesis is one of the clearest demonstrations of the interaction of habit and drive so when habit strength is--when the habit strength of a particular response is strong and the motivation is high, then the probability of making that response is extraordinarily high whereas if the habit strength is low and the drive is high then you get very low probability of occurrence, and if the habit strength is low, and the drive is low, then the probability of the response is higher than if the habit strength is--well, it's a crossover between drive and habit strength. And my dissertation got the crossover perfectly, and appeared in the *Journal of Experimental Psychology*. So, so what I did is one of the better supports for that particular hypothesis.

**Waggoner: Well, I didn't know it was published in the *Journal of Experimental Psych*.**

Palermo: Yup, in 1957, I think it came out. Now, Spence actually wasn't officially on my committee at all. He agreed to be on it, and in fact, the funny story, when my dissertation orals came we had them in the senate building at Iowa, one of the rooms in the senate building, the old senate building. And Spiker, and Castaneda, and Don Louis, and--I've forgotten whether--who the other person was on my committee. That's interesting. And, and Spence had agreed to come and so we were--all got there at the appropriate time at three o'clock in the afternoon or something like that, and Don Louis was drunk, as was not uncommon in those days. And he got a little impatient with waiting around for Spence, and he said, "I think we ought to get this thing started." He says, "What I want to do is if you had this to do over again how would you do it?" Well, I asked Al Castaneda and my advisors said to him, "Well, let's wait a few more minutes before we get started, wait and see if Spence is going to come." So we waited until five minutes after 4:00, or 3:00 or whenever it was. Don says, "Well, let's get this thing started. What I want to know is if you had this to do over again how would you do it?" And so the rest of the committee members looked at each other and finally decided, after the third effort I guess on Don's part, that maybe we should start and I should answer the question. So I gave them about a 20-minute response to the question if I had to do it over again how I would do it, and when I got through Don said, "Yeah. Well, but what I want to know is if you had to do it over again how would you do it?" Well, from that point on the committee proceeded to draw their wagons around in a circle to protect me in the middle and I hardly had to answer another question in my orals after that until the two hours was up, at which point Don said, "Come on, let's go down to the airliner. I want to buy you a drink," which was his way of saying I'd passed. And--

**Waggoner: So Spence never showed up?**

Palermo: No, he didn't. But--and Don Louis, who had a heart of gold, even though he did have an alcohol problem, was the first one to write me for a reprint when it came out my docturnal experimental.

**Waggoner: Too bad all Spences aren't like that. When did you do stuff with the manifest and anxiety skill?**

Palermo: Well, that came out of--Boyd McCandless was the clinician amongst the three of these people, and Boyd actually was the least wedded to the experimental child psychology orientation, although he was the head of the department. But Boyd got interested in motivation and anxiety, and he wanted to study that, and Al Castaneda was interested in the same thing. I mean, Al was also trained as a clinician, as well as an experimental psychologist. So they decided that it would be a good idea to take the Taylor manifest anxiety scale and create a forum that was appropriate to children, and then we could do the same kinds of experiments that were being done with adults. So Castaneda got--McCandless got the idea and talked to Castaneda about it, and Castaneda talked to me about it, and I got to do it. That is to say, I was assigned a job as a research assistant--

**Waggoner:** So that was while you were in graduate school?

Palermo: --yeah, making the children's manifest anxiety scale. And that's how that came about to be made and--

**Waggoner:** Yeah. I wasn't sure if it was--if you were in grad school or if you were already at--

Palermo: --no, no. I was in graduate school, and we standardized it when I was in graduate school and did some of the first experiments on the relationship between anxiety and other things, and at that time social behavior we didn't--I think we correlated the anxiety scale with sociometric scale of children in the classroom.

**Waggoner:** Yeah, he told me about that, yeah.

Palermo: And we also did some learning experiments. And then my dissertation was in experimental manipulation of motivation rather than using the anxiety scale as a measure of motivation or drive in the framework of the Hullian theory.

**Waggoner:** Okay. Well, that did all kind of tie together, because I was trying to--I always thought it was kind of disconnected the way you were going there, but it actually does all kind of tie in.

Palermo: Oh yeah.

**Waggoner:** So--

Palermo: Yeah, among the three of them, Spiker, McCandless, and Castaneda, Spiker was the intellectual driver of the group. Castaneda probably was as bright as--well, they were--I mean, they were all bright people, but clearly Spiker was the intellectual driver behind the experimental child psychology movement, even though McCandless turned out to be the spokesman most of the time, and Castaneda was--had some, I guess you would call personal identity problems. He was--grew up in a poor Mexican American section of San Francisco, and at the time he was my dissertation advisor I never even knew that his name was Alfredo, not Alfred. He signed all of his--everything was "Alfred Castaneda," and even when he got down to the University of Texas where there was a distinguished historian named Alfredo Castaneda on--he was named Alfredo--on the faculty there, Al still kept his Alfred and wouldn't identify with his Mexican American background. And of course, he had alcohol problems, and he became a worse and worse alcoholic until one time I saw him and he moved from Texas to Hunter College in New York, and I saw him in the Roosevelt Hospital when I had cirrhosis of the liver, and he looked as if he were pregnant as he lay in the bed.

**Waggoner:** Ooo boy, well, that's pretty--

Palermo: He was really swollen up.

**Waggoner:** --that's pretty bad, a pretty bad case.

Palermo: Yeah. He eventually recovered and became a member of the Alcoholics Anonymous but--and subsequently went to Stanford and became a member of the College of Education there and became very much identified with Mexican American heritage. And so it was--it came full circle eventually. But--

**Waggoner:** So then you went to Southern Illinois, just to sort of move things along?

Palermo: Right. Well, my first job was at Southern Illinois University.

**Waggoner: Right. You were there for what, five years?**

Palermo: I was there for three years.

**Waggoner: Three years? Okay. Because I was wondering on the way up whether you were there long enough to apply for tenure or you just said--**

Palermo: No.

**Waggoner: --to hell with it, I'm getting out of here.**

Palermo: No, I--yeah, I was there when Noble Kelley was the chairman of the department and Southern Illinois University was just becoming a real university. It was growing out of the teacher's college tradition, which it had before, and it still had a lot of the teacher's college people in administrative positions and it was very difficult for them to adjust to becoming a real university. And Dwight Morris, who was the president trying to bring about this conversion, was successful in doing so. So I was there when Noble was--Noble Kelley was the chairman. He was very much a clinical psychologist and thought that every member of the department was a clinical psychologist. And so we got treated as if we were clinical psychologists regardless of our background, and so--I remember one time bringing up a case of a student in faculty meeting that I thought needed some psychological help and would anybody else on the faculty be willing to take them on. And Noble said to me, "Well, why don't you do it? You ought to be able to do that." I said, "Noble, I'm not a clinical psychologist. I can't do this. Let's have somebody who's competent to do it do it."

**Waggoner: So you should have done that work on anxiety. That may have--**

Palermo: Right, yes.

**Waggoner: --have been more proof that you were a clinical psychologist.**

Palermo: Well, that's true.

**Waggoner: That was a teaching college, wasn't it? I mean--**

Palermo: Yeah, before--

**Waggoner: --primary was teaching rather than research--**

Palermo: --well, no. They were converting the two. I mean, we weren't supposed to do research.

**Waggoner: But still, you still had to teach a lot.**

Palermo: Oh, I just had to teach 16 hours a week, that was all.

**Waggoner: Yeah, so that's--**

Palermo: That was my teaching load. Right, I had a 16-hour teaching load my first year, and then I managed to do enough research to convince the graduate school to give me one course off, so I only had to teach 12 hours a semester after that--

**Waggoner: Yeah, that's about--that's what we do.**

Palermo: --the next two years. And I managed to do that and do a fair amount of research at the same time. How, I'm not sure at this point, but I did.

**Waggoner:** No, I'm not sure either if they're teaching 12 hours a week now. I don't know how you did it either.

Palermo: Well, I did, and--

**Waggoner:** Were you doing language stuff then?

Palermo: No. I was doing mode drive type experiments and H times D, and continuation anxiety scale kinds of things, and other experiments involving directly Hullian theory.

**Waggoner:** Okay. So it wasn't with kids then?

Palermo: Yeah, oh yeah, it was working the public schools.

**Waggoner:** Oh, okay.

Palermo: I did a few things with adults as well. I did--also did a paper with Les Malpass on mentally retarded children.

**Waggoner:** Oh, is that when you made up that thing you put the candy down in the thing to see if the kid would take the candy out?

Palermo: No, no--

**Waggoner:** Like a box or something.

Palermo: No, I don't think I ever did that. I don't remember doing anything like that.

**Waggoner:** Jeez, I thought that was how you manipulated what they were doing.

Palermo: Don't remember doing anything of that sort.

**Waggoner:** No, I don't--I must be mixing you up with someone else.

Palermo: And at that time a lot of the students who--or a number of students who came out of the East St. Louis slums were enrolled at Southern Illinois University and one of those students--or actually there were three that I became quite friendly with. One of whom was Ernie Washington, who then went on. He actually moved with me to the University of Minnesota, and got his master's degree there, and had a great deal of difficulty adjusting to the difference in culture in Minneapolis relative to East St. Louis, and went into the Navy where he met a woman who married--a nurse, and then went to the University of Illinois and got his PhD in ed psych and today is a professor of education at the University of Massachusetts. And we've kept in close contact throughout our careers. The other two individuals, one--both went back to East St. Louis. One of them became a principal in a school there, but I believe died of an overdose of drugs. He became a dealer. The other one became an alcoholic and died of cirrhosis of the liver. So Ernie was the only one who basically got out and continued a successful career in academia. The other two were just as bright as Ernie. It's really a sad kind of thing.

**Waggoner:** So you were there for three years. Then you went to Minnesota.

Palermo: Right. I went--yes, I went to an MPA the--my third year. Everybody went to MPA in those days--

**Waggoner:** Yeah, well that's--

Palermo: --Midwest--

**Waggoner: --a local conference--**

Palermo: --right. And I went in--University of Minnesota had a job in--well, the description--one little portion of the description was to teach experimental child psychology, a course in experimental child psychology. And they also wanted them to teach psychonomics—I mean psychometrics, and a bunch of other things for which I was not qualified. But Mildred Templin was interviewing at MPA for this job, and I'd met Mildred one time when she came to Iowa as a speaker. So I decided I'd go talk to Mildred. I wasn't going to really apply for this job, but she was interviewing people, and I stood in line, and the guy in front of me took an extraordinarily long time, because he really wanted this job. And it was clear to me from listening to the conversation that he didn't have a prayer of getting the job. He was even less qualified than I, and at the same time Delos Wickens was president of MPA that year and I wanted to go hear his presidential talk. But I finally stayed, and he left, and I sat down and talked to Mildred, and him--told her that I just wanted to say hello. And so she--we got talking about the job, and my qualifications, and I told her I wasn't qualified for most of it. I didn't think I should apply for that job. But we talked for 15, 20 minutes, and I went back to Southern Illinois and didn't think any more about it, but a couple of days later I got a call from Dale Harris saying would I be interested in coming for an interview. And I, of course, was delighted, and went for an interview, and was basically offered the job at the interview, and I said I couldn't accept without going back to Southern Illinois and telling the chairman of my department. I felt that was the right thing to do. So I got back to the department, and the next day I got a call from Dale saying, "Aren't you interested in this job?" or from somebody in the department saying, "Aren't you interested in this job?" And I said, "Yes, of course I'm interested." They said, "Well, you didn't--you didn't accept it while you were here." So that was sort of interesting. But at any rate, I informed Merrill--Noble Kelley that I was going to leave, accept this job, and I took the job at Minnesota.

**Waggoner: It was certainly a step up.**

Palermo: Yeah, it certainly was. It was a whole different world --

**Waggoner: I mean, yeah--**

Palermo: --I mean, going from a university which is just coming out of the teacher's college stage to a university that considers itself a university and acts like a university, and everybody in it believes it's a university, and furthermore all the people in the state believe that it's their university and it was a great university is an entirely different atmosphere. So I went to Minnesota and--

**Waggoner: When was that, what year was that?**

Palermo: 1958.

**Waggoner: Okay. I figured it was '58 or '59.**

Palermo: I got my degree from Iowa in '55 and then went to Southern Illinois for three years, and then to Minnesota. And at that time Dale Harris was the head of the department, the director of the Institute of Child Development as it was known in Minnesota. And Dale, of course, had been the person, who had been arguing with Boyd McCandless in public, and they had these--they were--I think Boyd was president in 19--of Division Seven in 1955, and Dale was president in 1956. And Boyd gave an argument for experimental child psychology, and against "developmental psychology" as it was described by the Iowa group. And the following year Dale was president, or it may have been--I may have them reversed, but at any rate, Dale was president and gave a rebuttal to McCandless's talk. And of course, those of us from Iowa thought Boyd had much the better of it, and subsequently I came to agree more with Dale.

**Waggoner: I was going to say—take with Minnesota.**

Palermo: But Dale had not--had hired me because I was to be the token experimental child psychologist, the person that they thought that they could get along with, and at the same time, who could represent that different position. So at the time I went there Mildred Templin and Dale Harris, John Anderson was still there. I think that was his last year, I think. And Merrill Roth, and--I can't remember offhand who else was there in the department. But it was a very nice arrangement, and I went from, you know, this three courses a quarter, which we had the quarter system--

**Waggoner: Oh, okay.**

Palermo: --at Southern Illinois two courses a quarter, which is what we had at Minnesota--

**Waggoner: Well, that's still pretty much for--**

Palermo: --well, the classes at Southern Illinois not only were quarter classes, but they met four times a week. Each class met four times a week, and you had four classes whereas at Minnesota I had two classes a semester and they met three times a week.

**Waggoner: Oh, so it's less than half then?**

Palermo: And there, of course, I got to meet Jim Jenkins. My first year there Jim was at Stanford at the think tank, at Palo Alto, not the Center for Advanced Studies. And so I didn't get to meet him my first year, and I did--

**Waggoner: Okay. Anyhow now ex--I don't know exactly where we were. You just got to this Jim Jenkins--**

Palermo: I was just talking about Jim Jenkins, yeah, who wasn't there my first year. But at the beginning of my second year, as soon as he arrived on campus I was at his door. In fact, I remember being at his door before he got there waiting for him to show up because I was eager to get to know him and to start a collaboration if possible. And in fact, we sat down and talked together for a long time, and hit it off right from the beginning, and subsequent from that point on we were--became good friends and colleagues, and discussed what was going on in terms of his research, and my viewpoint on that research. And I was a much--of course, a much stronger learning theorist than Jim, who was very much more an eclectic, and certainly not a Hullian, and I was a dyed in the wool Hullian in those days. So we used to argue about that. And then I would--I went to his seminars regularly and got to know his graduate students, as well, and began to bring my students with me over to his seminars. And pretty soon I convinced Jim that--and I--the argument that you can convince most any adult psychologist that children are much simpler to work with than adults are--

**Waggoner: Until they start doing it--**

Palermo: --right. Well, they never understand that until you get them caught into the argument. But--and they were having trouble interpreting some of their experimental results, and my argument was, well, in some--for instance, one of the problems they were having was that they couldn't demonstrate in a learning task that degrees of associative strength as measured by the word association norms that--Russell/Jenkins word association norms, they couldn't demonstrate the influence of word association strength on learning of those word pairs, even though they took words very low in the hierarchy. My argument was, well, they're all so well learned even though they aren't very high in the associative strength according to the norms that you need to do is work with children, because then you'll have much greater degree of difference in associative strength, and it'll be easier to demonstrate some of these phenomenon. Well, he bought that, and that, of course, led us to decide to collect word association norms from children.

**Waggoner: Right. So that's really when you started doing the experimental stuff with children's language was--**

Palermo: Well, it started with the—I mean even Ernie Washington, who came with me my first year at Minnesota, did a paired associate learning study with the card master, and we--the card master clinked and clanked in such a loud noises that we put the card master in one room and showed the opening through to another room so that cut down on the amount of noise that it was making. But we did studies on mediation actually with the card master, and I think Ernie actually did the first one of those studies, ran the subjects. I ran one of the studies myself. And to tell you a little side story while I'm thinking about it in terms of the mediation study, we did a study in which we showed mediation, mediated facilitation and mediated interference in a paired associate learning task, and sent it to the *Journal of Experimental Psychology*, which Art Melton was the editor of at the time, and we pointed out in that manuscript that nobody'd ever actually run the control. That is, you had mediated facilitation and mediated interference, but there was no control of no mediation at all, which should fall in between those two. So it was difficult to interpret your results as being facilitation or interference, or just relative interference, relative differences in interference or relative differences in facilitation, because there was no control to know what the non-mediation performance would be. So Melton sent the manuscript--I said, none of the--there've been--a lot of studies of mediation have been done and been published in such places as the *Journal of Experimental Psychology*, and as a part of the discussion I mentioned this. Melton said to Beck, he said, "Well, do the study right then." And so--

**Waggoner: That sounds like an interviewer's comment: If you mention it you better damn well do it.**

Palermo: So he said, "Do one more experiment demonstrating that with the control and submit all three experiments, and we'll publish them." So it turned out that it took three experiments in order to do this properly and demonstrate the phenomenon, and I didn't--I don't think I wrote up all three of them. I only wrote up one of them, which--or two of them. I don't remember exactly now, but I wrote the whole thing up, and sent it to Melton, who said, "Oh, that's very good. Now, cut out the first two experiments and we'll publish the last one."

**Waggoner: Yeah.**

Palermo: Which I found very frustrating. I remember going to the gym after the first turn down, and I showed Melton's letter to Jim Jenkins, and Jim knew Melton quite well. And he dug out an article by a physicist out of his file, which he said that he had submitted an article to a journal, and it had gotten turned down, which made him very depressed, and thought very little of himself, and wondered if he was ever going to make a contribution to the field until somebody recommended that he send the same article to another journal, and it was accepted immediately, which made him feel--revised his whole estimate of himself. Jim said, "Don't feel so bad. It's happened to other people a lot," and so I did feel a little better about it all. In fact, subsequently I had the same thing happen to me. I submitted an article to a journal and it was turned down, and I submitted the same article to another journal and it was accepted without revision.

**Waggoner: You didn't revise it in the meantime?**

Palermo: No, there was nothing I felt needed revision and neither did the editor of the second journal feel it needed revision. That's the only time that's ever happened to me.

**Waggoner: Yeah.**

Palermo: I never had another article accepted without any revision.

**Waggoner: Yeah, that's--**



Palermo: So Jim and I were getting along very well, and we decided that we ought to collect these data for children, and so we applied for a National Institutes of Health grant and--

**Waggoner: So you got the grant.**

Palermo: So we got the grant from NIMH or NICHD probably as I remember, and we started the basically four-year project of collecting the word association norms, about three years I guess. We had a young woman who worked her way through college supported by our word association norms, an undergraduate student. And we collected norms from--written norms from children from fourth grade through college, 500 of each sex, or maybe it was 250 of each sex in each grade, and 500 college students of each sex.

**Waggoner: Where'd you store the data? Did you keep it all separate or--**

Palermo: Well, we had some very large disks that we stored the data on, and we first put them on cards, and they were--we had boxes and boxes of cards, IBM cards, that eventually had to be moved from a building because they were too much--they were too heavy for the building--

**Waggoner: That's what I was wondering about. Oh, so it was computerized then?**

Palermo: Yeah, that was right at the beginning of--

**Waggoner: I was going to say, that would have had to been at the beginning of the use of computers in academic type things.**

Palermo: Yeah, but it was--but all of this was done by sorting cards, and so we had to sort cards by stimulus, the 200--we had 200 stimulus words rather than the 100 that had been the Woodrow and Lowell list that Russell and Jenkins had done their first norming on. And the norms that were done on--obtained from children in 1913, which we subsequently compared with our children; there are some interesting results from that.

**Waggoner: And that's where the book, that-- that's where the book was all about?**

Palermo: No. The book was just the norms themselves--

**Waggoner: Oh, okay.**

Palermo: --and the description of how the norms were collected, a point of Jenkins' word association norms. And then we--I mean, once we got the word association, why, then we could do the experimental research that we were interested in doing using associations as mediators and those--perception and word association learning and recall, and we were cranking out studies at a great rate, me and my students. And Jim and I would meet every week with our students and discuss the progress we'd been making, and we had a great little factory going, and it was--we were having a great time, as were the students, until--well, I should back up a little bit. Because after my first year, at the end of my first year at Minnesota Dale Harris decided to resign as director of the Institute and go to Penn State.

**Waggoner: Yeah, I figured that must have been about when he came here.**

Palermo: So, much to my surprise and delight I was put on a committee to look for a new director. And even more to my surprise, the committee and the college decided to appoint Harold Stevenson as the head of the--director of the Institute, who was, of course, also an experimental child psychologist.

**Waggoner: So that was a major shift then in--**

Palermo: So yes, it was a major shift in the whole orientation of the Institute, and that meant that I spent an enormous amount of time with Harold planning the--revising the whole curriculum, planning the way in which we would recruit students, and how we would--rewriting brochures, and all the stuff that went into basically starting a brand new institute, a brand new graduate training program, brand new series of courses at the undergraduate level. And at the end of Harold's first year there he said that he wasn't going to put me up for tenure, because it was too soon. Minnesota had a five year up or out policy at that time, and he wasn't going to put me up for tenure that first year because it was too soon, and it was his first year and so on, he didn't feel that he should be doing that right away. And at the end of his second year there he said he wasn't going to put me up because he thought I needed to get some more publications out before he put me up for tenure. All this time we were working together building this new program, and at the end of his third year he said he wasn't going to put me--in the meantime--so I put out five more publications that following year, and then at the end of his third year there he said he wasn't going to put me up for tenure at all. And so that meant that I had to leave Minnesota.

**Waggoner: Yeah. Well.**

Palermo: So I--and the interesting part about that was that he, to me at any rate, was that he blamed it on my letters of recommendation. He said that the people who wrote and supported me didn't--didn't support me.

**Waggoner: The outside reviewers.**

Palermo: Yeah. The thing that he didn't know was that all the outside reviewers had sent me the letters and there was absolutely nothing in those letters which could even conceivably indicate that I shouldn't get promoted. And it made Harold Stevenson a number of enemies in the field, because they knew that basically I was not being treated fairly by him. So I--

**Waggoner: Jeez.**

Palermo: So then I had, you know, a grace year the following year, my fifth year at Minnesota, and I looked for a job after he told me that, but it was late in the season, and I didn't have much. I had an offer, but I didn't want to accept that offer. The next year I got another offer at--plus Dale Harris called me up and asked me if I'd be interested in coming to Penn State. Dale had become head of the department here then, and subsequently--that was the year after SRCD had had a meeting here at Penn State at the Nittany Lion Inn, the last meeting that SRCD had at a--on a university campus. And I fell in love with this area at that time. I thought it was just a beautiful area. I liked it very much so when he called up and asked I didn't negotiate or dicker, I said yes, I'd be interested.

**Waggoner: So that's how you ended up here, huh? I always wondered.**

Palermo: That's how I ended up here. So Dale Harris made the same mistake twice, hired me at Minnesota and he hired me here.

**Waggoner: Well.**

Palermo: So--

**Waggoner: You--didn't you wanted to--**

Palermo: Yeah, yeah, I think we'd better stop for now and come back.

**Waggoner: Alright.**

**So anyhow, I think--it's 1962. You just came to Penn State.**

Palermo: 1963 I think.

**Waggoner: '63, okay.**

Palermo: I came in the fall of 1963. And actually, Hirsch Lee Woods came the year before, and Charlie Cofer came at the same time I did. He was supposed to come the year that Hirsch came, but he was on a sabbatical at Berkley, and he wanted to take that sabbatical from Maryland. So he stayed--he went to Berkley and then came the same year I did. Both Hirsch and Charlie were brought in as so called "star professorships," and Eric Walker, the president at that time, had decided to establish a number of these special professorships throughout the university and psychology happened to get two of them, and Hirsch and Charlie were the two.

**Waggoner: Oh, Cofer was in psychology? I thought he was in ed psych.**

Palermo: No, no.

**Waggoner: Oh, I thought he was in the ed psych department. Okay.**

Palermo: No. His office was where Frank Landy's office--

**Waggoner: Yeah, down on the fourth floor.**

Palermo: --yeah, right around the corner from mine. And of course, Charlie and I were--had known each other before he got here. I had met him through Jim Jenkins, and he was a part of the verbal learning and memory sort of invisible college, as everybody traded reprints. So Jim Jenkins had gotten me into this group, and then I gotten to meet Charlie and Jim Deese and Leo Postman, and a variety of other--Ben Underwood, a variety of others who were in the verbal learning tradition of the time. And then, to follow up on that, the *Journal of Verbal Learning and Verbal Behavior* was started very shortly thereafter, and Charlie was on the editorial board, and I think right from the start, about two years later--let's see--no, 1964 I was appointed to the board of the *JVLVB*, and I remained on the board until 1977. So Charlie and I were very excited about being able to be here together and, and we started a verbal learning major in psychology.

**Waggoner: Really?**

Palermo: We applied for a training grant and we got a training grant, and so we had support for our students, and we also had support for some postdocs.

**Waggoner: So that would have been specializations within experimental or separate?**

Palermo: Right, it was a specialization within experimental. Although in those days there was much more cross disciplinary, that is, within psychology, crossing the lines in terms of research and the particular area the student was working in.

**Waggoner: Right.**

Palermo: For instance, a number of students in clinical actually did their dissertations with Charlie and with me--

**Waggoner: Boy, that would never happen today I don't think.**

Palermo: It doesn't happen. I can't remember who it was. I'm not sure--I'm pretty sure--any rate, he is now at the University of Nebraska. Oh, Howe, Herbert Howe.

**Waggoner:** Oh, yes. Okay. Now you remember his name.

Palermo: Yeah. He is now at the University of Nebraska. But he was—he was a student in clinical in another college and then did his dissertation with me, in verbal learning--

**Waggoner:** --an applied sort of thing, like verbal learning in people with--

Palermo: No, no--

**Waggoner:** --aphasia or something or just basic research.

Palermo: --basic research.

**Waggoner:** Basic experimental research--yeah.

Palermo: Yeah. And there were quite a few students who did that. It's not an unusual phenomenon particularly a lot of clinical students. They of course were, as they still are as a rule, the brightest group of students at the college. It was always fun to have--when I came from Minnesota here I brought two students with me who immediately--David Wickham and he did his dissertation here although he got his PhD in Minnesota--at the University of Connecticut. Well, he wasn't here on postdoc. Then a second person who came was Nancy Nikkel, who came a year later and she got her master's degree at Minnesota with me and then had dropped out for a year and was continuing in Minnesota and then decided to come here and--for a change which she eventually did. And married a student in clinical at Berkley and she subsequently divorced, but any rate, she did her PhD with me, and got her PhD--got her degree from Penn State as opposed to Minnesota where--now, we continued the verbal learning research, of course, that I'd started with Jim Jenkins--

**Waggoner:** Yeah, I thought you did something with Jenkins.

Palermo: In fact, the word association norms came out the same year I left Minnesota in 1963 and I had another book come out that year as well, research in child psychology Lou Lipsett and I had published. And that particular book was a--sort of a propaganda kind of volume. We were pushing experimental child psychology and what we were trying to do with that book was to demonstrate that you could do good experimental research with children and that it had some meaning in terms of the larger theoretical issues in the field. So we took a slightly historical perspective--and published Dale Harris's and Boyd McCandless's--well, we published Dale Harris's presidential address to Division Seven, which had never been published before--

**Waggoner:** But that was arguing against experimental child psychology?

Palermo: No, we were printing the Spiker and McCandless side as well. So we presented both sides of the case. We presented a paper by Kurt Lewin with historical perspectives since Kurt Lewin was also arguing for an experimental and theoretical approach to child psychology. So those two books came out the year I left Minnesota and I arrived here.

**Waggoner:** This might seem like a dumb question, but where, if anywhere, was Piaget?

Palermo: Piaget wasn't anywhere. The interesting thing is that when I was in graduate school I read one thing by Piaget in all my years of graduate school, and that was a piece that had something to do with motor behavior. But that was, of course, from '53 to '55--'52 to '55 at Iowa. Now, there were some people doing things with Piagetian theory at that time. Dan Berlyne was very interested in Piaget and a fellow at Rochester who--by now was doing some research on Piaget. But it didn't really catch on until John Flavell wrote his book which came out in 1963--

**Waggoner:** I think it was '63, yeah, if I remember right.

Palermo: That was a great stimulus, because in effect what John was able to do with that book was to make Piaget understandable to the general population of psychologists. And of course, the times had changed a little bit and Piaget was publishing his own material, and people were interpreting it inappropriately.

**Waggoner:** Yeah, in behavioristic type terms.

Palermo: Yes, very much so. So the people who were doing Piagetian research much earlier than this really weren't doing Piagetian research they were doing what they thought were Piagetian but interpreted within behavioristic--and so it didn't have much of a contribution. But once Flavell's book came out, then people were stimulated to do research which was much closer to what Piaget was interested in, although still there was a behavioristic influence. It was difficult for people yet even in those days to have a good understanding, and I certainly didn't at that time. I didn't do much of Piaget. I was interested in Piaget much later.

**Waggoner:** Difficult to understand Piaget now.

Palermo: Well, that's true. He's not the easiest person in the world to read. But any rate, Piaget really didn't catch on until the '60s--Berlyne and Flavell and--although--I'm having a terrible name problem today--shouldn't be but--Hans Firth who was also a very strong supporter of Piaget was another one of the people who took a trip to Geneva when Berlyne started publishing--symposia about Piaget and took a trip to spend a year in Geneva. And, well, Flavell actually published his book and then went to Geneva. But Hans Firth went to Geneva also. He was pushing Piagetian.

**Waggoner:** Well, that was a little off--kind of off track from the mainstream there but--I was curious about that. I didn't think Piagetian theory got--until the mid '60s. I was wondering if it had any place at all--

Palermo: No. I don't think there was any effort to teach Piagetian theory, at a graduate level anyway, until I would guess early '70s. It might have been earlier than that. And certainly it would get presented in developmental--but no serious effort to think deeper about Piaget. Good Iowa boy like me didn't get very involved in Piagetian theory. I think that my own interests in Piaget came after people like Chomsky--now that actually started--that must have been--

**Waggoner:** That was--that was the early '60s, wasn't it? '62, '63--

Palermo: '62 I believe when Jim and I went to the conference with Roger Browning--

**Waggoner:** No, no. Is that the one that the book came out of?

Palermo: Yes.

**Waggoner:** --your article with Jenkins on association--

Palermo: Yes. Well, we wrote that--what actually happened was that Brown asked Jim Jenkins to attend the conference and Jim came running over to me and said, "Why don't we develop a theory of language activation based upon the mediation theory that we had been working on," and had been working on for some time. We hadn't actually worked on a theory of language activation but this was a stimulus to do so. But we did, in fact, develop that theory of which we were very proud at the time. And we were able to take it much further than either of us thought we were going to be able to when we started out. And when we got to the conference, of course Brown had invited, in addition to Stevens, Frasier, Chomsky, and Lees and some other--what other--oh, Holly--

**Waggoner: Holly?**

Palermo: Holly. So Chomsky, Holly, and Lees were all as discussants. And Sue Ervin-Tripp and Eric Lenberg and Moore and Jim Deese were discussants. Jerry Frieman was there--one more--oh and, Margaret Fullner and Tom Bever--so that conference involved--started off with Brown and Sue Ervin-Tripp and--on language acquisition that they had been collecting from children as well as Margaret Fullner who--from children younger than Sue Ervin-Tripp and Tom had been doing. And after each one of these papers, why, Chomsky and Lees and Holly would jump in and say, "How could you do this? This is ridiculous. How can you know what the confidence of the child is? They can't give you their reaction to the language. There's no way to get at the confidence of a child."

**Waggoner: So you can't assess the grammatical intuitions?**

Palermo: Right, right. Assess the grammatical intuitions of the children therefore you can't know what they know about language. And--

**Waggoner: --actually. So anyhow, psychologists were being pounded on by Chomsky and--**

Palermo: Yes. The phrase at the time was "trivial but--" "trivial and uninteresting" was the basic comment that was made on any contribution that psychologists made. And when it came time for Jim and me to present our paper that was--it was the last paper in the conference.

**Waggoner: Yeah, it's the last one in the book, too, if I remember right.**

Palermo: Right. And Chomsky and Lees and Holly didn't bother to come to that. They'd had enough by that time. So I remember saying that I wasn't terribly disappointed about that, because I thought they would probably not find it interesting and be very disruptive during the presentation.

**Waggoner: Even more trivial and uninteresting than the rest.**

Palermo: Right. And Jim Deese on the other hand, who was one of the commentators on the paper, loved the paper and thought--

**Waggoner: He was from the psychological background--he was from the verbal learning perspective.**

Palermo: Yeah, he was also from the verbal learning perspective. So Jim was very supportive at that time, although he, like the rest of us, changed his position--

**Waggoner: Yeah.**

Palermo: --later on. So that was a very interesting conference, and we were not unaware of Chomsky at that time. In fact, we cited Chomsky in our paper. But we recognized at the end of the conference that we didn't really understand Chomsky, and we would go home and make a serious effort to understand Chomsky. So we read him and we had seminars on him at Minnesota with our students, and we tried to--we slowly began to understand what he was getting at and why we were missing the boat with, you know, with our super mediation theory at the time, which had gone further than any other learning theory to try and account for language development within the behavioristic framework. At the same time that we were doing that, we were also communicating with Premack. Now we're going back to Minnesota, but what the hell. We were communicating with Premack, who was at that time developing his procedures for teaching Sarah language. He was at the University of Missouri at that time, and he was a graduate of Minnesota and knew Jim well so. He and his linguist friend, Schwartz I think his name was, at Missouri were trying to figure out a way in which they could teach language to a chimpanzee without having the chimp use his vocal cords. So they were working on--they started out with the notion that they could do it with a joystick and have the chimp move the stick in various

directions and have various meanings, and so--and before they got to the magnetic board, which they ended up with on a symbol system, but what would happen is that Premack would--his linguist friend would develop some ideas, and they'd send them up to us at Minnesota, and then we would sit around in a seminar and discuss those ideas and give--have reactions to them, and we'd send those back to Premack, and we had an exchange going with a seminar group for at least a semester, which was a lot of fun. At any rate--where were we?

**Waggoner:** I think we were, like, up to 1965 when you're at Penn State because Hirsch Lee Woods was here and Cofer was here--

Palermo: And we by that time had gotten a training grant for our graduate students and that helped us to recruit graduate students. I also got a career development award from NICHD at about that time, 1965, I think it had it from 1965-1970. And during that period of time I was spending more time doing research because accord--the grant was--the purpose of the grant was to allow me to do research 100% of the time, although I never could do it 100% of the time, because I didn't want to stop being on some committees in the department and teaching some graduate seminars so I never did totally what the grant allowed me to do. Although it took me about two and a half years to be able to learn to say, no, that I didn't have to serve on committees that I didn't want to serve on. And then the last year of my career development grant I went to the University of Edinburgh in 1969 and '70 and spent the year there getting my salary from Penn State via the career development award.

**Waggoner:** When did--

Palermo: And that's when I met Roger Wales and Margaret Donaldson and their students at Edinburgh, and Eve Clark had gotten her degree there, so I met her--

**Waggoner:** Oh, she got her degree at Edinburgh? Oh I didn't--

Palermo: Yeah, in linguistics.

**Waggoner:** Oh.

Palermo: So I met her, and she came back once or twice. And it's only by happenstance that she ended up where she was, because Herb Clark was offered a job here when he got his PhD from Johns Hopkins, and he had a job offer at Carnegie Mellon at the same time, and he took the Carnegie Mellon job instead of coming here. So I might have been a colleague of Eve Clark's if Herb had come here instead of going to Carnegie Mellon.

**Waggoner:** When did Walt Weimer come here?

Palermo: I'm not sure exactly when he started here.

**Waggoner:** I didn't know if I was jumping ahead or not. Chronologically, I just thought that that was about the time you came here. It was, like, late '60s, maybe '70 or something like that.

Palermo: Well, when I was at--he came here in the late '60s, because he was here before I left for Edinburgh. And then, while I was in Edinburgh I wrote the paper on a scientific revolution taking place in psychology, and the editor of the journal was in Edinburgh, and I made--presented the paper at a colloquium at Edinburgh and he was there and heard the paper, and asked me if I was interested in publishing it in *Science Studies*, and I said, "No, I'd rather publish it in a journal with a wider view." And I submitted it to *Psych Bulletin* or *Psych Review*--oh dear, I can't remember which journal, but it was--George Mandler was editor, whichever journal it was he was editor at the time. And I think he sent it to Tulving to review, and neither of them thought that there was a revolution going on in psychology, and basically they turned it down because they didn't believe the thesis of the paper. So then I approached the fellow who was editor of *Science Studies* and asked him if he still wanted to

publish it, which he did, and he did publish it. And then there were quite a few responses submitted to the journal to that paper, and a number of them were from philosophers, and I didn't feel that I was competent enough to respond to this group of philosophers who had responded to the paper, so I asked Walter if he would collaborate with me in developing a response.

**Waggoner:** Yeah. Wouldn't that--the reason I asked is because I always kind of thought that, you know--the question's asking who your significant colleagues and whatever were, and I just guess I always thought that Walter really sort of influenced your thinking a lot at a certain time. I mean, it seemed to me that way anyhow. Maybe it was the other way around, but it seemed to me that--

Palermo: Well, no question, but--that Walter influenced my thinking, and he was very--from my perspective, a very good colleague to have around, because he had no hesitancy about telling you when he thought you were wrong--

**Waggoner:** Yeah, yeah, I know that.

Palermo: --and you could argue with him and expect to get argued back. I didn't always agree with him even after we'd finish arguing and he thought that he had won the argument or thought he'd had the last word. But any rate, he was very important to my own thinking, and we got along reasonably well I thought and--which is not true of a lot of people who couldn't get along with Walter. Walter and I were three offices away and frequently talked and enjoyed each other. I went to his third marriage, third wedding at his home in Washington, Pennsylvania. He was, as you may know, married to a woman when he arrived here and shortly thereafter divorced, and married a graduate student, whom he shortly thereafter divorced, and then married an undergraduate student, and finally married a second undergraduate student. But Walter was a very significant influence I think on the whole department. He affected--Charlie Cofer was--I don't think Charlie was as willing to accept some of Walter's arguments as I was, but he certainly had an influence on him, and Mike Mahoney and Walter were very good friends toward the end of the--in the latter years of Walter's tenure here.

**Waggoner:** Yeah. Well, Mahoney was here, like, a year or so when I first got here. He left the year after I came or two years or something like that. So--

Palermo: Well, he and Walter were fairly--as close friends as Walter could have. Walter didn't have very many close friends.

**Waggoner:** Well, I think we finally got to the early '70s.

Palermo: Well, when I--well, as I say, when I returned from Edinburgh the--Walter and I collaborated on a paper called "Paradigms and Normal Science and Psychology," which was basically a response to the critics of my earlier paper, and he subsequently published a couple more papers in *Science Studies* following up on the general Coonian argument. And of course, I think--I'm not sure whether Walter introduced me to Coon or whether it was just in the air at the time, but--no, it was before Walter read Coon. But my effort was to attempt to describe a Coonian revolution as I saw it within the framework of psychology. And in fact, I sent a copy of my paper to Coon before I published it and got a very nice response from him, a very supportive response, in fact, to publishing that paper. Now, when I was in Edinburgh I spent a lot of time with Margaret Donaldson and Donald Wale--Roger Wales rather, and there was a lot going on at Edinburgh at that time. There were some good linguists there, there were good perception people there, there were good philosophers there and so it was a very exciting place to be in 1970, '69, '70. But I worked most closely with Roger and Margaret, and they were doing some research on children's acquisition of meaning, and the thing that I got hooked on was a paper that Margaret and Belfour did on children's acquisition of the terms--words more and less. And I sort of, I guess, was a little doubtful about the results and assumed that I wouldn't be able to replicate them here, but I came back and made the effort and expanded their base a little bit. They were using apples on apple trees, and I used apples on apple trees plus water in a glass, so you had a continuous substance as well as discontinuous substances, and got the same results with both tasks. So then the



question became, why do you get this result, why do children have this problem, and when do they solve it and when do they learn the difference between more and less and stop treating more as if--less as if it were a synonym of more.

**Waggoner:** So that was--up to that time you must have been doing syntax research in language?

Palermo: Well, I was doing some of this modeling of basically--based on the past tense inflection rules, so I was doing rule learning using paired associate learning task--kind of task, which there was a rule that related the letter responses to the number of stimuli. So we'd have a pair of numbers like 64, which would be paired with H and D, so any time a 6 occurred with H, response, and every time a 4 occurred it was a D response, and there was a different letter paired with each number.

**Waggoner:** Like an artificial grammar type--

Palermo: Yeah, artificial languages.

**Waggoner:** Yeah.

Palermo: And I was able to demonstrate to my satisfaction in this artificial task that I could get the same kind of results that have been published about past tense inflection in real language. That is to say that the regular forms were learned first and the irregulars were learned second, and that the rule then over-generalized to the irregulars before they got them straightened out again, which was the finding that had been obtained up to that point, at least in past tense inflection. And then there was the famous exchange between Dan Slobin and me in a volume that Slobin edited in which he soundly attempted to criticize this work, and Lynn Eberhart and I responded to Slobin in that volume arguing that we felt that it was a good analogy, and that it was useful to use these artificial languages in an effort to understand some things about rule learning, and rule learning that was relevant to language acquisition. But I did give up that research at a later time when I tried to--one of the criticisms that Slobin had raised was that there was no meaning involved in the items that we were using, that it was all artificial from a meaning standpoint. So I tried to put some meaning in it by creating some stimuli, which were moon men I called them for the children's benefit. I had four different heads that could go on four different bodies, and then they had different names, and so I could call anybody with one particular head John, and with a different head Fred, and their last names would differ depending upon the body that they had. But I could never get enough of the children to learn the task, and I got--I finally gave up on it, because I spent---I could get about 50% of the children to learn the task, basically a paired associate task again, but about 50% of them weren't learning, and it didn't seem to make any difference what I did. I couldn't get that much more than 50% of the children to learn the task, and with that kind of percentage learning it, I was never going to be able to go anywhere with this. So I gave that up, and at the same time, I got interested in the kinds of things that Donaldson and Wales were doing, and that led me to do the work on more and less. Now when--where are we in this thing?

**Waggoner:** Still, like, roughly mid '70s.

Palermo: No, I mean where are we in terms of the tape?

**Waggoner:** Oh, actually right to the very end.

Palermo: Alright.

**Waggoner:** Alright.

Palermo: So we started a series of studies, the first one of which was entitled "More About Less: A study in Language Comprehension" that appeared in the *JVLVB*. And subsequently we published several studies on this topic, and the next one was basically to find out when children did learn this

distinction. And it turns out that even in second grade kids were making this kind of confusion, and so we were--thought this was really interesting, because at second grade is when you start teaching subtraction, and if the children don't know the difference between more and less how can they ever learn to subtract or the mathematics involved, the arithmetic. So we then worked out an experiment which Melissa Holland really did in which she very carefully examined all the different meanings of less and more, and developed a task, a series of tasks in which the children could be taught the difference between more and less, and then she got a bunch of nursery school aged children, and started--the first task she had to present to them was I think a pile of blocks. And so she had two piles of blocks, one that had a lot of blocks in it, and one had not so many blocks in it. And she said, "Look at these piles of blocks." She said, "This one's got more blocks, and this one's got less blocks." And that was all she had to do. The rest of the training was superfluous, because once she had told them that this big pile had more blocks and the little pile had less blocks the kids knew the difference between more and less, and that was all it took.

**Waggoner:** They'd never been told that before? I mean--

Palermo: Well, I don't know exactly why it took so little to teach them that, but once they had that bit of information they performed perfectly on everything after that, almost perfectly. And so then, it appeared to me that it wasn't that children didn't know the meaning of more and less. It's just that they didn't know that more meant more and less meant less. And they had the concept, they just didn't have the language that--

**Waggoner:** Right.

Palermo: --they could utilize in discussing the concepts that they already had. So that study really changed my whole thinking about language and, for me, made it pretty clear that the concepts come first and the language comes second, not that language doesn't help you to learn--

**Waggoner:** No, yeah--

Palermo: --concepts at a later time, but you can't use language for that purpose. But when children are acquiring a language it seemed to me that most of the acquisition was learning what language went with what concepts, the concepts already exist--having existed. We also, incidentally in connection with that Holland study did the first Piagetian task that I ever did, an experiment. We did a little Piagetian task on the assumption that if children didn't know the more and less task, then they probably wouldn't be able to conserve, and if they did know it then they would be able to conserve. And so we anticipated that the children who are identified in these two camps would be correlated. Well, it turned out not to work. There was no relationship between the two insofar as these tasks were concerned. So--but that was my first--

**Waggoner:** Yeah. Well, that would be a reasonable hypothesis then--

Palermo: --at the time it seemed reasonable. I'm not sure whether--if I'd thought more deeply about it I would have done a different experiment. But at any rate, that's what we did. And at the time that this research was going on we started--Walter and I started a couple of conferences, the first of two conferences, which resulted in two books called *Cognition* and *The Symbolic Processes*, and those were very exciting conferences, because the people who were at them really discussed different perspectives and--

**Waggoner:** Real Gibsonian.

Palermo: --yeah, it was a strong Gibsonian influence. Judy Gibson was at one of them, and of course, Turvey and Shaw were influenced strongly by Gibson. And so they were arguing with everyone else, lots of people who disagreed with them, and it was--those were two extraordinarily exciting conferences, and I think the books that resulted for them--from them, the two books *Cognition* and

*Symbolic Processes*, which Erlbaum published, were important books at the time. And I myself turned to some more theoretical kinds of papers at that time beginning with a general descriptive kind of paper that Dennis Molfese and I first presented at Michigan at a conference, which Slobin, and O'Neil, and Sue Ervin-Tripp and--well, I'm not sure that Sue Ervin-Tripp was at that conference, but at any rate. Bever was at Michigan. And that paper subsequently appeared in the *Psych Bulletin* and had an influence in the sense that it tried to sort out what was going on between the onset of language and when children presumably had the language pretty well in hand, that is, language acquisition from age--I'm sorry--what we dealt with was language acquisition after age five, because it--the argument was that language acquisition was all over by age five at that time. And we were able to show that in the literature there was quite a bit of evidence that there was still a lot to be learned about language after age five. And I also got involved in a series of papers talking about semantic development, because the Holland and Palermo paper led me to change my general view on that. So I have a number of papers that I wrote on semantics and language acquisition. And then in 1975 I went to Sydney, Australia for my sabbatical, and I went there because a friend of mine that I went to graduate school with, Dick Champion, was professor at Sydney, and every time he came to the United States, and they have pretty liberal policy for travel in Australian universities because they're pretty isolated, but every time he came to this country I would invite him to wherever I was, and he'd come and give a talk, and he kept saying, you know, someday I've got to go to Australia. And so in 1975 was the time, and I got a Fulbright, and went to Australia, and my main job that I set for myself at that time was to write the book *The Psychology of Language*, the textbook which--on psycholinguistics basically, which was one of the early books to come out on the topic. Let's see--well, I think the rest of my career basically focused on semantic development, and initially what I was doing was criticizing the Clark and Clark theories of feature acquisition as being an account of semantic development in children. And I didn't see how that could possibly be the case; it seemed to me that children--if children already knew the concepts and were just learning the words to be able to communicate those concepts, and they certainly weren't learning the words a feature at a time. And one of the strongest arguments from my perspective about that was that children could understand metaphor, and if they can understand metaphor then they certainly aren't acquiring the meanings of the metaphor a feature at a time. At least that seemed to me the strongest argument against feature theory that you could make. And so I got involved in the question of do children in fact--what age do children understand metaphor. And at that time based on Piagetian theory, primarily, people were arguing that you couldn't--children couldn't understand metaphor until formal operations level.

**Waggoner: Yeah, Billow's studies.**

Palermo: Right, Billow's studies among others. And so we started doing research and trying to find out when children acquire--are capable of dealing with metaphor, and then we kept moving the age level down further and further, and of course, you became involved in that research eventually and played a significant role on pushing the--discovering that--or demonstrating that children even at preschool age understand metaphor, and I'm still convinced that metaphor starts with the cross modal transfer in infancy.

**Waggoner: Yeah, the Wagner Winner.**

Palermo: Yeah.

**Waggoner: It's a Kedian/Gardner.**

Palermo: Right, although I don't think that that's a particularly potent demonstration of it.

**Waggoner: Well--**

Palermo: If you look at that they didn't--they only had less than the majority of the stimuli used actually demonstrated in transfer, but--

**Waggoner:** Hmm. I guess I never actually read that study to tell you the truth. I hate to admit that.

Palermo: Well, then most of my research and writings, theoretical writings, dealt with conceptual development with metaphors being the primary focus until we got involved with Phil Baldi with some work on prefix and legation of English adjectives.

**Waggoner:** Yeah. Well, I remember you doing that study. What ever happened with that?

Palermo: It came out in a book that was published I think in Berlin, *Historical Semantics Word Asso-- Word--Historical Word Formation*, and we've done another half dozen studies which I need to write up, rewrite, or revise and resubmit. That's basically it. Now--

**Waggoner:** Well, that pretty much deals with question number three, I think, significant colleagues and all that. The next question is supposed to be what political and social events have influenced your research and writing.

Palermo: Well, I grew up in college through the McCarthy era.

**Waggoner:** Oh, well, that's true, yeah.

Palermo: And one had to be careful about what they said. I don't know of any direct political influence on the kinds of things I did from a psychological point of view. I think most of the things that I got involved in were basic research and theoretical research, theoretical ideas, and I would guess-- I've never done a count of this, but I would guess that those who were more interested in applied kinds of ventures who'd been affected by the social/political situation in ways that I don't feel that I was particularly.

**Waggoner:** Yeah. So the content of your research and the way that your research went didn't really reflect politics or social changes or--

Palermo: I mean, I don't see how it could have been totally uninfluenced, but I don't know of any--I can't think of any particular way in which my work was affected by social/political issues--

**Waggoner:** Right. Well, it's not like it's--

Palermo: --not that I wasn't involved in some of them, and rather strongly in a few of them. But I managed to keep those separate from the things I was doing at the laboratory.

**Waggoner:** Yeah. Well, studying language development--

Palermo: And certainly Chomsky was very involved in social/political issues.

**Waggoner:** But it wouldn't be like somebody who studied sexuality in adolescence and, you know, having the '60s affect everything--

Palermo: Yeah.

**Waggoner:** --you do then. But--

Palermo: Well, I don't think I had any particular influence.

**Waggoner:** Yeah, that would be one I'd have a hard time answering, too. Hmm. Personal research contributions. We've pretty much covered this already, like, what were your primary interests in child development at the beginning of your career?

Palermo: Yeah, I think we have talked about that pretty much.

**Waggoner: It was more the, you know, applying the Hullian theory, and the motivation and the--**

Palermo: Yeah, I mean, my earliest interests, like most other people, were in clinical, and even when I moved from Massachusetts to Iowa I still had an interest in clinical and told Charlie Spiker that I was interested in clinical child psychology, to which he responded with less enthusiasm than I'd anticipated. But the more I got involved in research, and it wasn't a matter of giving up something I really loved, it was a matter of being converted to other things which I liked even better. And I really did get very involved in research. I think the fact that Charlie Spiker and Al Castaneda and Boyd McCandless were all interested in them, even though Boyd had clinical interests I didn't--I shifted away from clinical and never felt I was doing anything that I didn't like.

**Waggoner: Actually we've pretty much summarized all these things, like what continuities in your work are most significant, what shifts occurred, what events were responsible, but--**

Palermo: Which, which item are you on?

**Waggoner: --number two--**

Palermo: Under personal research? Oh.

**Waggoner: --personal research contributions. Yeah.**

Palermo: Well, the continuities are I've always been with language, although I didn't start out there. I've always had that interest, and the shifts clearly were from an Iowa/Hullian behavioristic orientation to a much more Chomskyan/Piagetian perspective on children's development. And as I was saying earlier, that I got interested in Chomsky first, and eventually that allowed me to understand Piaget better, and subsequently I became very much involved in Piaget's ideas and trying to understand them, and gave a number of seminars on them, as was my habit to give seminars on things I didn't understand so my students could help me understand them. And of course, at this stage of the game I've been a member of the board of the Jean Piaget Society and now edit the Jean Piaget Society newsletter. So--

**Waggoner: Quite a switch from never having basically heard of Piaget--**

Palermo: Right. That's true.

**Waggoner: --but I guess the other shift I was thinking about was from primarily studying syntax to moving more primarily towards semantics assuming the past tense is more of a syntactic sort of phenomenon than a semantic one.**

Palermo: Yeah, it's a morphological one, but--

**Waggoner: Yeah. Well, that's actually true, yeah.**

Palermo: --but--and I never really did anything specifically on syntax I don't think. But I was interested in the rule-learning part, which was what everybody at the time was talking about with respect to syntax as learning rules.

**Waggoner: The verbal learning stuff with the mediation theory?**

Palermo: Well, we tried--

**Waggoner: That wouldn't necessarily be syntax--oh, that'd be closer to syntax than--**

Palermo: --oh yeah, I suppose the--yeah--

**Waggoner: Since it deals with--**

Palermo: The mediation theory was--did deal with syntax--

**Waggoner: I mean, more so than semantics or--**

Palermo: Well, we tried to account for semantics as well in that paper. You'll have to go back and read it sometime.

**Waggoner: Yeah, I have the book, I have the book. I probably have read it, but--**

Palermo: Oh, we had big ideas. Just because most of them are wrong doesn't--

**Waggoner: It doesn't mean they weren't big. So what events are responsible for the shifts--well, we just kind of summarized that, like, going to the conference with Chomsky, and going to--**

Palermo: Right, I mean we--

**Waggoner: --to Scotland and--**

Palermo: --yes, those are the primary, I think, factors, and Walter Weimer's presence here, Charlie Cofer being here, as well. Those were the primary influences on my research and my theoretical contributions. There's no question about that.

**Waggoner: The third one here I'm not sure we have actually done. Please reflect on the strengths and weaknesses of your research and theoretical contributions, the impact of your work. To some extent, we've touched on that and its current status, so I mean, so far we've sort of just covered what you have done, not necessarily said, well, good, bad, other, what would you do different, or whatever.**

Palermo: Well, I don't know what I would do that was different other than perhaps I would like to have gotten enrolled in theoretical linguistics insofar as a psychologist can do that. I went to one summer linguistics institute at the University of Illinois, and at that time became acquainted with Lakoff, and with Hadge Ross, and with the fellow who wrote the semantics book, John Lyons at Edinburgh. In fact, John Lyons was one that did help me get to spend my sabbatical in Edinburgh. And they were particularly effervescent individuals in their various ways. John was a little--John Lyons was a little older and a little more settled than, say, Hadge Ross, and George Lakoff, who at the time were young Turks making their mark.

**Waggoner: The time when Lakoff was unsettled?**

Palermo: Well--but that was a very interesting summer, and I learned a lot about linguistics so I also learned that at the time linguists weren't much interested in what psychologists had to say or contribute. They've become much more so in the period of time since. But at that time they were very much involved in their new venture and felt that particularly anybody who'd been a behaviorist wouldn't have anything to say of interest.

**Waggoner: Yeah, that's true.**

Palermo: Although Hadge Ross was always open to talking to anybody about anything, and he was a particularly warm and friendly individual, and very smart, not to say any of the others weren't, but I really enjoyed Hadge. He was a very likeable person, as well as a smart guy.

**Waggoner:** Actually, questions three and four you were kind of asking the same sort of thing it seems to me. Like, asking which contributions were the most wrong-headed is sort of a way of asking the weaknesses of your research and theory or contributions so. So what was the most wrong thing you ever did, Dave?

Palermo: The most wrong thing?

**Waggoner:** The most wrong-headed research you ever did.

Palermo: Well, I mean, at the time I was doing them I never thought any of them was particularly wrong in the context in which I was doing them they seemed right. Looking back it's easy to pick holes and to argue against a lot of the things that I did. Certainly there were the work—the behavioristic work, the mediation theory is something that doesn't play any part in my thinking anymore. Interestingly enough it still plays a part in some thinking of my colleagues, but--some of my colleagues and still people in the field. But I think those are weaknesses, certainly not strengths. I've felt that I've contributed theoretically in terms of discussions of semantics and the relationship between cognitive development and language development.

**Waggoner:** Yeah. Well, I guess, again, we've already covered this, but the *Cognition and Symbolic Processes* books.

Palermo: Yeah, they're--

**Waggoner:** You--just from my evaluation, from what little I know about the history of the field, it would seem to be pretty significant as well.

Palermo: Yeah, I think they were. In fact, those books still sell and so there are people who are still looking at them for various reasons. I think they are historical markers, which may last a while and maybe looked from a historical perspective as being important. But things like that eventually get lost.

**Waggoner:** Yeah. Well, in time everything does. But--

Palermo: Right.

**Waggoner:** Anyhow, where we were was at discuss--or please reflect on your experiences with the research funding apparatus over the years. Comment upon your participation, the shaping of research funding policy, etc., etc., etc.

Palermo: Well, I don't think I had any direct influence on the research funding apparatus over the years. I had grants from the National Institutes of Health and the National Science Foundation is--was when Jim and I started our research on words, and there was sort of--we applied for NIH grants in support of the word association data collection project, and we supported a grad--an undergraduate student throughout her graduate career along with a bunch of graduate students who collected data. And then, when we got to doing the experimental research we applied for National Science Foundation money. And one of the things that we did at the beginning was that because he was better known than I was and old--a little older than me, he put his name on the first grant as project director, and then subsequent grants I put my name on.

**Waggoner:** So at the time grant reviews aren't blind?

Palermo: No.

**Waggoner:** Oh, really?

Palermo: No, grant reviews are quite open.

**Waggoner:** Then that would explain why an older more established person would probably have an advantage compared to somebody who hasn't done anything, and doesn't have a name or anything like that.

Palermo: Yes. And of course, there's strategies for applying. When you're younger you--it's a good idea to apply for a smaller grant. One of the big jokes amongst those who are on research panels is a young PhD applies for a couple million dollars to extend his or her dissertation. But I think that the review process is extraordinarily fair, and I always tell the story of the time that Lou Lipsitt applied for a fellowship, and I was on one of these committees for NICHD I think it was, and I was the primary reviewer. And in order to be as honest as possible I bent over backwards to point out all the weaknesses in the application, and when I got through with my report everybody else at the table looked at me strangely and said, "We didn't see anything wrong with that application. We thought it was terrific." So I had bent over too far in the other direction to be critical. But I think that on the whole the process is as good as you can get it. I've seen so many people on panels who disagreed with the applicant in terms of theory and perspective strongly support a grant application, because it's a good researcher who just has a different perspective. Occasionally you get the opposite kind of thing, but the other members of the panel are generally there to correct that kind of thing. There was a time when I was on a panel that was half pediatricians and half psychologists and so some--what typically happened in this case was as a psychologist I'd lean over and ask my pediatrician friend to my right "Is this really a good application or have I misread it?" And they'd generally be pretty honest about it.

**Waggoner:** Yeah. So there's no MD PhD--

Palermo: No, there wasn't in that particular group. They worked very well together.

**Waggoner:** Oh. I always kind of thought it was a blind review--

Palermo: No, it would be really difficult to do a blind review in a grant application. But I think the same kind of thing happens in the journal-reviewing situation. I know when I was editor of the *Journal of Experimental Child Psychology* I spent the most time with the youngest least experienced submitters. Frequently, somebody who was submitting their dissertation, I would spend a lot of time helping them rewrite it to get it into a form that was publishable assuming that I thought the research itself was publishable eventually. And there are a lot of editors who do that sort of thing, whereas older more established people you can be harsher on if they send in a paper, and occasionally they do. Whereas, some people in the field who think that because they're well known and well established that it doesn't make any difference what they submit, they ought to get it published, and I was typically harder on them than on those who were younger and less experienced. I don't like to follow that on the--my hardest task was to turn down a paper of Charlie Spiker's once. It was the most difficult thing I ever did as editor I think, and it wasn't because it was sloppily written up either. I just didn't think it made a contribution, that particular paper.

**Waggoner:** Well, you did about the same thing when you were dean, right? What was the--when you were dean for vice president and--

Palermo: Yeah, the biggest kick I got out of being dean since I had a little money to distribute was to give it to the younger faculty members. In fact, I told the older faculty members if they didn't know where to get money other than inside that they shouldn't be applying at all.

**Waggoner:** Yeah. That's probably true. So--

Palermo: Once in a while I gave them some money to--sort of seed money, but the idea was to get the younger faculty members started so that they could apply for outside money.



**Waggoner:** So really that position was mostly--you were mostly handing out internal money, you weren't really acting like a--I don't know what you'd say--like a go-between between Penn State or people here and other sources like NSF or, you know--

Palermo: Well, some of that was involved, but basically the idea was for me to get faculty members started as best I could, and to make it possible for them to get a research program started, and then they could--once they'd established themselves they could apply for outside monies. And that worked pretty effectively. Some people never did go outside, and then the money from my office would dry up.

**Waggoner:** Yeah. Well, kind of have--

Palermo: And some of the older faculty members, when they were changing directions or something of that sort, needed some transition money and that--I would provide that. But I know there was one faculty member in economics who, you know, from whom I received an application my first month in office I think, and basically sent in the same application he'd sent in the previous year for which he had gotten funds, and he hadn't done anything that I could tell, or at least he didn't say he'd done anything. So I turned him down, and I heard that there were cheers in the department about that. But--

**Waggoner:** Well, I know what that's like being on the professional development committee about the first thing we check is if somebody's put in a grant request before and if they've actually done anything with it, and if they haven't they go to the bottom of the pile. So--

Palermo: Mmhmm, sure.

**Waggoner:** --yeah, you can't keep rewarding nothing, even though there are some faculty members who would like to think you should.

Palermo: Well, we were--the only other occasion we had to affect policy was in the study sections. They always had--were proposing things, or asking our opinions about things. So we did, in fact, respond to various new initiatives that were being started. But I never felt that we had any great impact even then, because I know occasions on which we gave advice not to do things, and they went ahead and did them anyway and vice versa. So I wasn't personally directly involved in high-level decision making of all funding. I got--I received funding, I acted on committees that decided on who got funded, but that was about it.

**Waggoner:** Yeah. So that was for the National Institute for Child Health and--

Palermo: Well--

**Waggoner:** --Development or--

Palermo: --NIH was the money from word association, and NSF was the money for the research, and then I had an NICHD career development award in 1966 through '70 I guess it was. And that was a very nice honor for me, and gave me the opportunity to spend full time on research, or at least it was supposed to have given me the opportunity to spend full time on research. It didn't do as well as it should have since I couldn't say no to people who requested things of me.

**Waggoner:** Yeah, I know what that's like.

Palermo: And I always liked to teach at least one graduate seminar a year, so I did that during that period. And then I went to Edinburgh for the final year of that grant, so then I could get entirely away and not--for the last year.

**Waggoner:** The Boyd McCandless Award didn't have any research funding associated with it, it was just an honorary.

Palermo: Well, yeah. That was--

**Waggoner:** I didn't know if--

Palermo: --an award.

**Waggoner:** --it had some stipend or something that was supposed to be for research or--

Palermo: No. That was an award that Division Seven asked me to chair a committee to set up, and so I asked a few people here on campus, Dale Harris, and Frank Devesta, and a couple of others, I've forgotten who now, to decide the kinds of criteria we would use for that award, and then we decided a person who had not been--who had gotten his degree not more than seven years previously, and who was making a significant impact in terms of their research. And Boyd, of course, was a professor of mine, so it made it particularly interesting to me to make this award, and to develop this award, and to make the first--I think I made the first two presentations at Division Seven, plus one not too long ago to Jude Cassidy, and it's--she is not in our own department, so--

**Waggoner:** Yeah.

Palermo: --I was asked to make the presentation, which was fun.

**Waggoner:** Yeah, as I said, I wasn't sure if that had some sort of, you know, like the Mark Macarthur grants, whatever they are, you get so much money--

Palermo: No, no--

**Waggoner:** --to go and do things or just--

Palermo: --this is just an honor that you can put on your vita.

**Waggoner:** Yeah. Hey, it looks good, right?

Palermo: No, no money, I don't even think they pay your way to APA--

**Waggoner:** Oh boy.

Palermo: --if you receive it.

**Waggoner:** Alright. Alright, I guess we can jump ahead to the next one, personal and institutional contributions. We've already dealt with, like, where you were, but maybe if just real quickly say that you were here from--you were in a certain place at this period to this period.

Palermo: After I got my degree at Iowa in 1955. I went to Southern Illinois University for three years where I was an assistant professor. And I went to Minnesota, where I was for five years and--from '58 to '63, and then in '63 I moved to Penn State, where I've been ever since until I retired December 31<sup>st</sup>, 1991.

**Waggoner:** Thirty years at Penn State, huh?

Palermo: Well, not quite.

**Waggoner:** Twenty-nine officially before you retired. Oh boy. Boy, that's a long--you were here longer than Joe Paterno. Jeez.

Palermo: That's true. Well actually, I haven't been here longer than Joe Paterno.

**Waggoner:** Well, as head coach--that's right. He came in the 1950s-- late 1950s I think.

Palermo: Yeah, he came when he finished his undergraduate degree at Brown. He was going to come here for one year before he went to law school.

**Waggoner:** Yeah, I read his book. Umm, let's see. For persons connected with well-known research sites it said describe your role with whatever the site was, changes in the unit--

Palermo: I didn't have--I wasn't enrolled on any well-known research sites--

**Waggoner:** Yeah, I guess just the universities.

Palermo: --other than universities.

**Waggoner:** I think most of this stuff--we've already sort of talked about how, when you were at Iowa, you know, it was sort a movement away from descriptive child development to experimental child psychology, and the same about going to Minnesota. About the only thing that we didn't cover I think, actually, from the previous time you were talking about your--what Penn State was like when you came here--

Palermo: Yeah, it was sort of interesting to go from Southern Illinois to Minnesota to Penn State, because they were in--each of them was in different stages of development when I was there. Southern Illinois was just shifting from a teacher's college to a university, and Dwight Morris, who was the president there, wanted it to become a university, and the new faculty that he hired, and he hired a whole bunch of young PhDs about 1955 when I started there, we at the bottom wanted it to become a university and tried to act as if it were a university. But all the people in between--not all of them, but most of them were not used to a university, didn't know what a university was like. All they knew was the teacher's college atmosphere. So in many ways those people stood in the way of it--of the university making this transition, in part because of ignorance, and in part because they didn't like change and didn't know what to do. So while a lot of people came into Southern Illinois at that time, also a lot of them left.

**Waggoner:** I was going to say, not many stayed very long.

Palermo: So I have friends all over the country who were--who started out at Southern Illinois in those early years and have spread out all over the country. And then I went to Minnesota and that was one of those--I don't know whether we talked about this before--one of those things where I went to MPA at my third year at Southern Illinois--

**Waggoner:** Yeah, I think that's earlier on the tape about how you just sat down to talk to the person--

Palermo: --yeah, right--

**Waggoner:** --and they said, "Oh, we'll give you the job." Gee, must be nice.

Palermo: --well, those days are--things weren't quite as, well, legislated as they are now.

**Waggoner:** So it was pretty much the old boy's network?

Palermo: Yup. Although, you know, I didn't know any of the--I knew Mildred Templin, but I didn't know any people in Minnesota really. But at any rate, it was a nice break for me. And Minnesota was totally different from Southern Illinois, because it was a university, and everybody at the university thought it was a university, and even the people of Minnesota were proud of their university. So it was a totally different atmosphere, a very nice atmosphere, and I really enjoyed that aspect of my time at Minnesota. Then--

**Waggoner: You just came to Penn State.**

Palermo: Alright. Well, when I came to Penn State I came as associate professor, became full professor. And then, in my last three years I--three plus years, I was Associate Dean for Research and Graduate Studies, College of Liberal Arts. So I had really enjoyed my years at Penn State. But it was trans--when I first came here Penn State was halfway between Southern Illinois and Minnesota, because they had--they had been Penn State College a few years before, and they had already started their movement into becoming a university, but even then, when I went down to take a research grant down--to submit a research grant here they had a research office in Old Main, and I'd take it down there, and they'd say, "Well, we're not sure we can get this in by the deadline," even though I gave them a week which was pretty unusual. And so I carried it around. I always carried my grants from one person to the next; one person did the job that they had to do with respect to the grant, and then I would carry it to the next person to be sure that it didn't sit on somebody's desk for a week or two.

**Waggoner: Ah jeez, sounds like Bloomsburg.**

Palermo: But now they have a wonderful system and they bend over backwards to facilitate any research--any faculty member's efforts to get outside money.

**Waggoner: Yeah.**

Palermo: And Penn State, of course, has risen to the top ten in external funding.

**Waggoner: Yeah, and number two in some things just behind Johns Hopkins and MIT. Well, I guess it was different though when you first got here, huh?**

Palermo: Yeah, quite. But it was a nice environment, and unfortunately they let it grow. It continued to grow and the community and the surrounding areas continued to grow, and we didn't invest in property around here.

**Waggoner: Yeah, just buy your house, sure. You should have bought half the mountain over here. Right now you wouldn't--**

Palermo: Right.

**Waggoner: --you could have retired at--jeez, 45 instead of 60. Actually, I was looking at that question. Do you think the department here has played any unique or speci--the department as a whole has played any unique or special role in the history of child development research, as opposed to, say, you know, Iowa or Minnesota or--**

Palermo: Well, it never had the institute, for instance, such as Iowa and Minnesota had, although Rich Lerner and I, about ten years ago I guess it was--it been that long?

**Waggoner: Yeah, it was about that long.**

Palermo: --started the Center for the Study of Child and Adolescent Development. Actually, it was Evan Pattishall, who was dean of health and human development at the time, it was his idea to start such a center. And he asked Rich, who was in his college, if he'd like to be the head, and he did, and

Rich then asked me if I'd like to develop--work with him to develop it. So Rich was the head and I was the, you know, associate director. And we built I think a nice facilitating organization within the university, which brought together the people who were interested in developmental from all colleges and all departments. We have at the moment probably representatives from six colleges on campus, including engineering--

**Waggoner: Engineering?**

Palermo: That was surprising. Yes. Who, you know, we have a small amount of funding, again, seed money for grants, for individuals and--within the center, and we have monies to bring in outside speakers, and some monies for conferences, and we started a book series growing out of the conferences, and it's--I think it's been very good for the university since we probably had more developmental psychologists on this campus than most any place in the country at this point, maybe in the world.

**Waggoner: You know, see that's--I was just thinking that. I mean, this interview's pretty much oriented towards the psych department, but then you have all of human development down there--**

Palermo: Sure.

**Waggoner: --and, you know, I don't even know if there's anyone being interviewed from down there for SRCD. Then who it would be Warner Schaie gerontology and--**

Palermo: I don't know whether--

**Waggoner: --of course, that's always had the more adolescent, adulthood, aging, gerontology orientation, at least it was--**

Palermo: They also don't have enough old goats over there yet. [Laughter] Human development hasn't existed as long as psychology has.

**Waggoner: Yeah, that's true.**

Palermo: So they don't have the same opportunity for people to have aged, mellowed or whatever.

**Waggoner: Oh, is that what it is? Eh, it's just like wine; the older you get the better, huh?**

Palermo: Right.

**Waggoner: Oh, let's see. The next one is experiences as a teacher of child development; what courses have you taught, comment on the tension between teaching and research in the field of child development.**

Palermo: Well, for me--to answer the latter question first, for me personally there's never been any tension between teaching and research. I've enjoyed both of them and find that they--the two are complimentary. I see it as a part of the university professor's job to do both--

**Waggoner: Well, you're--**

Palermo: --and as far as I'm concerned it's a 50/50 job. That's--

**Waggoner: Well, you're probably the exception at a major research university then.**

Palermo: I don't think so. I don't think so.

**Waggoner: Really?**

Palermo: You know, I think there are some people who would much prefer to spend 100% of their time on research, although not many that I know, and some people who would just like to teach graduate seminars--

**Waggoner: Mhmm, instead of undergraduate.**

Palermo: --and not undergraduates. But I've always taught one graduate, one undergraduate course, at least, and that was true at Southern Illinois University, where the teaching load was much heavier, taught 16 hours a term.

**Waggoner: Yeah, I know. I'm complaining about 12. Yeah, but those are the days you had to walk to work through the snow up to your waist and that too. So--**

Palermo: Right. That's true. Everything was tough those days. But we did research as well as teach. And then, when I went to Minnesota, of course, I only taught two courses. But at Southern Illinois I taught introductory psychology, child psychology, adolescent psychology. Those were the--and then a graduate seminar, although the graduate seminars you didn't get to teach every term. There were three terms a year, so maybe one term a year you'd get to teach a graduate seminar. The rest of the time you taught undergraduate courses. And one of Dwight Morris's techniques for getting the university funded was to take in as many undergraduate students as he could, therefore increase the size of classes and the need for more classes. Therefore he'd have--could go to the legislature and say, "We need more teachers, more faculty to serve all these students," and that was the way to build--that was his technique for building the university.

**Waggoner: That sounds familiar except that with us we don't get any more faculty members. We just get more students and everything else. So--**

Palermo: At any rate, his method worked at the time. We complained about it regularly, but it was--it got the job done.

**Waggoner: Yeah.**

Palermo: And when I went to Minnesota the teaching load went down to two courses, and generally there was one undergraduate and one graduate course, and there I taught introductory psy--no introductory psychology, the institute didn't have a responsibility for that, but introductory child psychology, and sometimes two sections of it--undergraduate course. So--because there was always a graduate course and an undergraduate course there. And then, when I came here to Penn State, the teaching load was pretty much the same, and again, it was one graduate course and one undergraduate course. Occasionally I'd teach two undergraduate courses, but generally it was one graduate and one undergraduate course. And then the research went along with it, and when you're teaching the graduate students, and seminars and courses at the graduate level, and they'd become interested in your research and begin to participate in it, and the whole intermesh of teaching and research worked in a supportive fashion.

**Waggoner: Yeah. It really is different, though, undergraduate versus graduate teaching. I mean, one is more of the apprenticeship model, and the other is more of the standard lecture or whatever type model.**

Palermo: Well, it depends upon the undergraduate. Sometimes I had undergraduates who sort of worked into being halfway decent research assistants. They generally didn't have the background, but frequently the undergraduates will work harder than the graduate students. It's a privilege for them to be able to work on research projects, whereas for graduate students it's a job--

**Waggoner: Yeah. It's an obligation.**

Palermo: --but they're not always interested. One of the hardest things as far as I was concerned with respect to training students for research was that not everybody who comes to graduate school necessarily wants to be a researcher, and for me, doing research was, you know, part of life. I mean, you did it not because you had to do it, but because you enjoyed doing it.

**Waggoner: Yeah.**

Palermo: And the graduate students frequently get the idea, well, I've got to do just what my professor is doing. If I don't do that I'm a failure. So I was constantly--once I became aware of this, and I don't think I became aware of it immediately, I was constantly telling my students that just because I like to do research, I like to work in the university setting, there's no reason why you have to like it. You do the kinds of things that provide the rewards that you're looking for. If that was to be teaching, service, or whatever, that's nothing to be ashamed of, you don't have to do research just because I love it.

**Waggoner: Yeah. Well, but still, a PhD's a research degree.**

Palermo: Well, that's certainly true, but that doesn't--I mean, your life and your needs and your goals and the things for which you get rewards change over time, and that doesn't require that you have the same goals and rewards at 30 as you had at 20, or at 50 as you had at 30 and so on.

**Waggoner: Yeah.**

Palermo: Any good developmental psychologist should know that.

**Waggoner: Yeah, gee. Theory of adult development--I just finished covering that today in life span. I wonder why they put on there the tension between teaching and research, because they always--I always--maybe they meant the fact that taking time--putting time towards one takes it away from the other or--I mean, I'm not exactly sure what--**

Palermo: Well, I think in today's world the emphasis is on doing research, and research products are nicely countable.

**Waggoner: That's true.**

Palermo: You can count the number of publications that a person has and you get ratings on the quality of the journals that they've been published in, whereas teaching is much more difficult to evaluate. You can turn on some students, but--with all different kinds of teaching techniques. Somebody who is boring to you may be interesting to someone else, it turns someone else on. And so it's very--it's extraordinarily difficult it seems to me to evaluate teaching. Not only that but the evaluations that you get immediately may not be the same as the evaluations you get a year later.

**Waggoner: Yeah, I know. Well, once they get out of here and get out in the real world they understand what things are like, or if they're freshman or sophomores and they haven't had many professors yet it's like, jeez. You know, we're going through this. I just got on a committee to redo our student evaluation form, so we're thinking--**

Palermo: Good luck.

**Waggoner: --about a lot of these things. Yeah, I know. It's been done a million times, and you always come back with the same questions. But--describe your experiences in so-called applied child development research.**

Palermo: Well, I haven't had a lot of experience in applied child development research.

**Waggoner: I thought that would be a quick one.**

Palermo: My few efforts in the applied area have all been flops. I've tried once or twice to do things that were more applied, and haven't been very successful in getting the kinds of results that I would like to get, and I think it's probably because I was brought up in an environment which taught me to do rather precise research, and it's not as easy to achieve that control in applied research or in a lot of applied research, at any rate. And you have to be able to make up for what you--the conclusions you can draw by using different techniques than the strictly experimental kinds of techniques.

**Waggoner: Well, just--I mean, I never knew you did--you never really--I never knew you did anything that was really applied. I mean, just what--for example, what would that be? I always thought you were pure theoretical research.**

Palermo: Well, I tried--let's see. What have I tried? They've all been failures, so it's hard to remember. I never published anything that I thought was particularly applied. Well, I was going to do some--make some efforts to introduce metaphor in the classroom and--

**Waggoner: Yeah, that's--I remember that project. Yeah, now I remember that. I guess I never really thought of that as particularly applied, but I guess that would be about as close to applied as you can get. Yeah, I remember that project. I still--the data are still laying down in my office drawer. Yeah. Well, I can't say I do very many applied things either. Basic research is--I don't know, it seems more interesting to me. So I take it that's the answer to that question.**

Palermo: I'm afraid so. I'm just looking up to see when I had joined--

**Waggoner: Yeah, that's experiences with SRCD. When did you join? What were your earliest contacts with the society and with whom? Describe the first meeting you attended.**

Palermo: I think the first meeting I attended was at Berkley and that was very interesting. Me, I don't remember anything outstanding about it except Berkley. I'd never been to Berkley before, which was fun. The one I do remember was not too long after that, was here at Penn State, which--

**Waggoner: That was an on-campus meeting?**

Palermo: --that was the last on campus meeting. It was our--SRCD was never held on--

**Waggoner: Well, SRCD must have still been pretty small then to be to be able to hold it on--I mean the membership--**

Palermo: Yeah. Oh, it was much smaller and very, very nice, small congenial meetings as--as smaller groups like the Piaget Society is today for instance.

**Waggoner: Yeah, so--**

Palermo: --in which you had a small group, a few sessions, you could go to--very few overlapping sessions.

**Waggoner: Oh jeez.**

Palermo: It was held in Nittany Lyon Inn and meeting rooms, as you know, over there are fairly small--

**Waggoner: Yeah, they're small.**



Palermo: Although they've expanded them now that they've redone the Inn. And one of the things that happened at a conference was that the weather was very bad on flights coming into town, and people got very sick flying into Altoona.

**Waggoner: Then you have an hour drive to get here.**

Palermo: Right. Some people didn't recover for a long time. But it was a very interesting meeting, and I certainly liked Penn State, as I've told you. I fell in love with the place at the time.

**Waggoner: Oh, so you weren't here when you came then?**

Palermo: No, no, I was still at Minnesota.

**Waggoner: Oh, I thought that was the meeting when you were here. Oh, okay. Alright.**

Palermo: No, I was still at Minnesota.

**Waggoner: But Dale Harris was here at that time, or was he still--**

Palermo: Yes.

**Waggoner: --at Minnesota?**

Palermo: No, he was here.

**Waggoner: Okay.**

Palermo: It was about 1963. I don't remember exactly--or '62 rather. '62, because I came here in '63. I think it was '62. At any rate, I probably joined SRCD as soon as I graduated. Oh, yes. Here it is, in 1958 I joined.

**Waggoner: Oh, okay, 1958.**

Palermo: There's a letter from Bill Martin, or--yeah, saying, "Oh behalf of the governing council I'm writing to notify of your election to membership."

**Waggoner: Oh, so you had to be elected or is that just a formality?**

Palermo: Probably. Pretty much a formality, assuming that you'd done the minimum--

**Waggoner: Ooo boy. I don't know if I should tell you this or not, Dave, but you joined SRCD before I was born. [Laughter]**

Palermo: Sorry about that.

**Waggoner: Yeah, that's my fault. I should have been born sooner.**

Palermo: Well, this is supposed to be older people reflecting on their lives.

**Waggoner: Man, now that's just hard to believe. It must be because I don't think of you as an older person. So--**

Palermo: So my earliest contacts with the Society and with whom? Well, Bill Martin was--and Dorothy Eichorn were two of my earlier contacts within SRCD. With Bill Martin I'd actually become acquainted

with him earlier, because I remember reading an article in *Child Development*, which I thought was particularly bad, and I wrote a letter to Bill Martin asking him--or pointing out a whole lot of the deficiencies of this particular article and asking why it had been published. Talk about the gall of a young assistant professor. And he wrote me back a tolerant letter saying that occasionally mistakes were made, and there were contributions that this particular paper had made, and I don't remember the exact details. All I remember is that--

**Waggoner: Yeah. At least he wrote you back. I mean, jeez.**

Palermo: --it was sort of like another stupid thing I did when I was young in that I went to a meeting in New York City, I think it was the Eastern Psychological Association meeting. At that time I was a graduate student, and I was still interested in clinical, and I went to hear a paper session on--that had to do with clinical work. And somebody reported a piece of research that was just awful, and I waited for when it was finished, a couple of questions were asked, and finally I asked a question, and it had to do with the methodology of the paper, which was abominable. And I pointed this out along with the inappropriate statistical analyses that had been made and a hush fell over the crowd, and I realized that I had asked too difficult a question for a paper that was so obviously poor, and I shouldn't have asked it. The person didn't have much of a response, and the crowd was obviously embarrassed that I had asked the question, so I didn't do that again. But as far as the Society is concerned, I went to most of the SRCD meetings--

**Waggoner: So this is describe the history of your participation, the scientific activities.**

Palermo: --well, I went regularly and presented papers regularly, participated in occasional seminars--symposia rather. It was such an enjoyable meeting that it was hard to not go.

**Waggoner: Yeah.**

Palermo: You got to know all your colleagues in the field, you also kept up on the research activities, you kept up on social activities, and there were always good parties.

**Waggoner: Well, that's the important--**

Palermo: One of the reasons that Dorothy Eichorn and I'd became good friends in part was that at one of these--I think it was an SRCD meeting where there was a fair amount of drinking done, and she had to go back to her room, which was in another hotel I think, and I don't think she particularly wanted to go by herself, so I walked her back to her room, for which she was grateful, very grateful and was, as always, and reminded me for several years thereafter about how kind I was to do that. Any rate, she's a very nice woman that I've always liked, and we've always enjoyed each other. As far as the scientific--so far as the governance of SRCD, I've never really played any part in the governance other than the time that I was editor of the *Child Development Abstracts and Bibliography*. I don't remember who preceded me as editor offhand, but I remember that I took over as editor and as a result then went to the board meetings. And--but that didn't last terribly long, because shortly thereafter I was--within three years I think I was appointed editor of the *Journal of Experimental Child Psychology*, and then I dropped--I didn't feel I could do both of those jobs, so I dropped the *Child Development Abstracts and Bibliography* and Hovind Thomas then from the department picked it up. And I thought that would be a good arrangement, because I'd be here to help in the transition.

**Waggoner: Yeah. So you were more involved with Division Seven than--**

Palermo: Yes.

**Waggoner: --with SRCD.**

Palermo: Yeah. Division Seven, I was--ran for president, didn't make president, but I ran for president, and I was on the board as a member at large I think, and I was also a representative of Division Seven to the council of APA for a few years. And so I was more, more active in the governance of Division Seven than SRCD.

**Waggoner:** Yeah, just--most important--

Palermo: Comment on--

**Waggoner:** --changes? Or--is that where you are, number four now? Oh, we didn't do this one. What do you believe are the most important changes to occur in SRCD and its activities?

Palermo: I don't know. It's grown a lot, I know that. It's not the intimate little conference that it used to be. And they've, of course, expanded their activities through--in connection with the legislature in Washington, tried to influence applied legislative activities, that is, legislation at the national level that would support children in this country, which I think is valuable, although I've never participated particularly in that activity. They have a fellowship program where young PhDs can go and work with congressional representatives.

**Waggoner:** Do you think there's been a change in the makeup of SRCD? I mean, was it initially almost all psychologists and then more pediatricians and whatever--

Palermo: No, it's always been multidisciplinary. Now, at one point the psychologists sort of took it over, and that was probably in the '60s where--'60s and the '50s, and '60s and '70s when it became almost exclusively psychologists, but it's always been a multidisciplinary organization, and a specific effort was made--I don't remember exactly when, but probably in the '70s, the latter part of the '70s to return to a more multidisciplinary organization. So the fact that developmental psychologists and educational psychologists and pediatricians and psychiatrists and anybody is welcome within SRCD that has any interest in child development. And I think--

**Waggoner:** Oh, you can tell that just by looking at the affiliations in the journals, and the articles where they show up.

Palermo: --yeah, that and it makes for a much more interesting meeting, because you have lots of people with different perspectives who are involved there, and the same is true with the journal. The journal is--I don't think that SRCD will ever give up its multidisciplinary orientation, and I don't think it should. But I don't know other than growing and the kinds of things that happen with growth and some of the kinds of things I've mentioned, I don't know what--

**Waggoner:** Yeah. Did it always have all the journals? Did it always--

Palermo: Yes.

**Waggoner:** --put out the *Monographs*, did it always put out the--

Palermo: Mmhmm, yes--

**Waggoner:** --the *Abstracts and Bibliography* and always *Child Development*?

Palermo: As long as I've been associated with the--

**Waggoner:** Okay.

Palermo: --organization.

**Waggoner:** It seemed to be a pretty well organized thing from the start then.

Palermo: Yeah.

**Waggoner:** Because a lot of them start out, you'll have, like, one journal, like APS, it had one for a while, and then it stuck another one on, and then whatever, but they have everything--

Palermo: Well, that may be the way SRCD started for all I know, but I don't really know. As long as I've been involved since 1955 they've had three journals. As my collection goes back they had all three. There's piles of boxes around here--

**Waggoner:** Yeah, I know. I see them all. Who are you going to hire to move these out?

Palermo: I don't have any idea at this point.

**Waggoner:** Oy. Comment on the history of the field, the continuities and discontinuities and events related to these.

Palermo: Well, we've talked a little bit about these things.

**Waggoner:** Yes, sort of the general introductory conversation.

Palermo: I mean, I participated and was one of the advocates of the beginnings of the experimental child psychology program within the field, and--led by McCandless, Spiker, and Castaneda, which really spread well beyond Iowa in the sense that people before that time had been calling for more theory and more experimental research, more statistical sophistication. There were papers by Noah Liss in the annual review and--well, I've forgotten the others now. I've forgotten my history of St. John well-known psychology. But people had argued that basically developmental psychology was dying on the vine largely because it didn't have any theory, it was collecting isolated facts, and not doing the kind of research that was being done in the rest of the field. So the field was in some sense prepared for the experimental child psychology movement and experimental research then took off. Now, the research was, of course, the particular approach advanced at Iowa was a strongly behaviorist approach, but I think the notion that--and the people at Iowa basically taught this, although they didn't practice it, was that any kind of theory was appropriate. It didn't have to be Hullian theory, was the argument. Any kind of theory that organized the kind of research you were doing and led to systematic programs of research was okay and should be fostered. It was the fact that prior to that time there didn't seem to be very much theory with the exception of Kurt Levin perhaps who tried to propose a theory, but at that time--at the time he was trying to advance theory within developmental psychology it wasn't accepted.

**Waggoner:** Hmm, it's just--given my training and everything with two and three seminars on Piagetian theory it's just hard to believe that there was a time--well, first of all, when there was no Piaget, and second, when theory wasn't that important.

Palermo: Well, it was a long time ago when there was no Piaget. Nobody in this country paid any attention to him, of course.

**Waggoner:** Yeah.

Palermo: But then the kinds of theories, of course, expanded from the behavioristic kinds. You got Piagetian theory. I think we talked a little bit about that already in this--

**Waggoner:** Yeah. Did, did you--

Palermo: --series of tapes.

**Waggoner:** --did you think that developmental psychology became cognitively oriented because of Piaget, or that cognitive orientation was already coming in and Piaget happened to be there to grab a hold of, or do you think it was--that you can't neatly categorize it as one or the other? That's always a nice answer, the middle ground, but--

Palermo: Well, I suspect that Piaget was something that was caught onto. I mean, in a sense this was a--people were looking I think for alternatives to behaviorism, and some of them went to Geneva and found at least one. And there was Vernor at Clark, who never really caught on with people, many people outside of Clark--

**Waggoner:** Yeah--

Palermo: --although--

**Waggoner:** --Capland

Palermo: --his general perspective isn't all that different from Piaget's. It's painting with a broad brush, of course. And then there was social learning theory, which entered particularly for people who were interested in social behaviors, but--and then Skinnerian theory, and Skinner himself argued against theory.

**Waggoner:** Yeah. So when you were being trained was Skinnerian theory the dominant version of behaviorism, or was it simply a--

Palermo: No--

**Waggoner:** --less important alternative to Hull/Spence theory?

Palermo: --well, it was just--when I was getting--doing my work, Tollman was still active, Guthrie was not so active anymore, but Tollman, Hull, and Skinner were all active researchers and contributors. Hull, of course, died and was--the torch was carried by Spence thereafter. Spence among others, but Spence was his most distinguished student. So--and of course, at Iowa the major theory was Hullian theory. We went to Indiana and then you'd be more likely to have a Skinnerian orientation or a stronger Skinnerian orientation. So there was a lot of arguments amongst the theorists, even in the '50s. And then of course, well, naturally Spence died, and he'd left Iowa before he died and the people that were left at Iowa after that were unable to carry on the strength that Spence had. And the field was changing and moving away from behaviorism and looking for something that was--really said something about the mind, that is, people became convinced that there was in fact a mind, and--

**Waggoner:** And actually had something to do with behavior and development, huh--

Palermo: Well--

**Waggoner:** --instead of just being excess baggage.

Palermo: Right. And so I think the field was looking for something different, and certainly Piaget was something that you could grab onto, and developmental psychologists could grab onto, but the rest of the field didn't have that. And then, of course, Chomsky was the other strong influence, and his criticism of Skinner had a big impact on psychology. It wasn't even answered by a Skinnerian until McCorkadale answered him ten or so years later. So, an entirely different approach to an understanding of behavior and the mind.

**Waggoner:** Yeah. Umm, actually, I'm kind of running out of tape and that's my last one.

Palermo: Oh, alright.

**Waggoner: So what are your hopes and fears for the future of the field?**

Palermo: Hopes and fears? Well, I hope that--one of my big hopes is that we won't retreat again to behaviorism, and I see there's still a lot of behaviorism around disguised with computers, and I certainly hope that the movement, which Piaget and Chomsky both are advocating, will have success, and that we'll move--the pendulum will actually swing in that direction.

**Waggoner: Well, I'm not exactly sure what you mean. Do you mean--well, it seems to me that the neonativists are not behaviorists. If anything, they're more sort of radically biological Chomskian than they are Piagetian. I mean, is that sort of what you're getting at or do you still--**

Palermo: No. I think a lot of the people who are working on problems of what they call "mind," having to do with mind, are really behaviorism in disguise. They use different terminology. They use a computer as the inner model instead of habit, strength, and drive--

**Waggoner: Oh yeah. Well associate strength--**

Palermo: And I--

**Waggoner: The models have to do with associate memory. Yeah.**

Palermo: And connectionism about which I probably shouldn't talk, because I don't know enough about it, seems like a complex form of associationism. And there is no--there seems to be, amongst that group, little concern, or little thought about the fact that one can change their values or change their mind about something, and it isn't--it doesn't seem to me that accounting for it with an associative model can work. I've been wrong before, once.

[Laughter]

**Waggoner: Well, for somebody to start out with an associative model by chaining words together it's a pretty strong statement.**

Palermo: I think we've talked about personal notes--

**Waggoner: Yeah, that was--**

Palermo: --at the very beginning talking about my family and my grandfather so that may cover the waterfront.

**Waggoner: Yeah, it seems like it.**

**End of Interview**