Wendell Jeffrey

- Born 3/4/1923 in Des Moines, Iowa
- Spouse: Bernice Wenzel
- B.A. (1948), M.A. (1950), Ph.D. (1951) all from the State University of Iowa

Major Employment:

- University of California, Los Angeles: 1955-90

Major Areas of Work:

- Cognitive Development

SRCD Affiliation:

- Governing Council (1965-71), Editor of Child Development (1972-78), Ethics Committee (1978-82)

SRCD Oral History Interview

Wendell Jeffrey
University of California, Los Angeles

Interviewed by Rachel Gelman
August 5, 1994

Jeffrey: Well, the first question is on family background--

Gelman: That's right.

Jeffrey: --and fortunately it was only about a year ago that I was at my sister's house and happened to find that she had some records that I didn't have of the data on which my mother and father were born. My father was born in 1886 and my mother in 1887 in eastern Iowa, both lived on farms all of their lives. My mother graduated from high school, and then taught in a small country elementary school for, as far as I know, just one year. My father only completed the eighth grade, and I have no knowledge of really what he did during that time until he got married, but I assume he continued to work on the farm. But what is even more startling, and I knew this but I have never known quite how it came about, that after they got married, which I assumed was around 1906 or 1907. She would have been 19 or 20 then having been out of high school and taught a year, and I think she quit teaching because she got married. But they headed for Des Moines, which was the capital city of Iowa, and set up an automobile dealership there. Now, the question was when that was done, but I don't know of anything else he did there, and I also -- I mean, before he'd set that up, and I don't know how he got the money to do this or how he decided to get into this business to begin with. But anyway, that's what he did, and someplace along the line -- and I don't know when that happened either, but it must have been four or five years later -- he decided he'd picked the wrong car, or maybe it was decided for him, actually all sorts of cars were being built at that time. Almost any carriage shop was trying to get into the automobile business and any machine shop was building engines. And so anyway, the car he picked wasn't selling terribly well and he decided the real business to be in was the tire business, because tires then were very poor because they did not know much about making tires. They were rather fragile, and wore out quickly or were punctured easily, and they were going over old roads, of course, so they had to take a fair amount of abuse. So anyway, he started the tire business then, both tire sales and repair. And that's the business I know about, and I remember being in that building downtown and knew what went on there.

And at that time, well, by the time I had joined the family, which was in 1923 -- I should add my brother, one of my brothers, was born in 1914, and the other one in 1917 then, and I was the third boy

born 1923. But, so anyway, the scene I came upon was in living at the edge of the city on an acreage, a rather pretty 13 acres of land. It had probably been a small farm or maybe a bigger farm that had been broken up. The house wasn't anything fancy. It was a comfortable sort of farmhouse with a small barn behind it, and my father really liked farming, or liked the idea of the farm at least. And on this farm, or this acreage, we had two or three cows, and chickens, and garden; it was a rather large garden, and both flower gardens and vegetable gardens, so we had lots of food handy, and milk and eggs, and vegetables in the summertime, and he loved to work in the garden after he'd come home from business. And I remember that well. I think he did well financially during that time, and I judge it primarily from the fact that by the time I remember we had a nice car, and I remember when we changed cars for another new car, and our house was well furnished and there was sterling silver and nice dishes around the house so that everything seemed like we were -- when I reflect on it seems that we were probably doing well at that time. However, with--

Gelman: That time is now, when?

Jeffrey: --well, that's '25, '26 was when I would really remember things, that if I was born in '23 then I was around to remember by '26, '27 school then, and finally in a rather nice school, which was in town, a standard city elementary school, which I was in until the third grade. Anyway, with the Depression, stock market crash, and what followed, things didn't go well and I don't know the details of that, although I know that finally, and it hurt him mightily, but he had to declare bankruptcy in probably 1931. And again, I don't know the details of how that was all managed, but out of that we moved to a farm, a larger farm, an 80 acre farm, about four or five miles outside the city. And I should add that along the way more cows had been added on the acreage; they were born there and we just kept them. So we had, I don't know, four or five cars at that time.

Gelman: [Laughter] you could have had four or five cars.

Jeffrey: No, only one car. And I think we were distributing milk even at that time to some of the neighbors adjacent to us. So he moved to the farm, or we moved to the farm in either late '31 or early '32, a farm that he bought, and started our retail dairy business.

Gelman: --business?

Jeffrey: --pardon?

Gelman: Another business?

Jeffrey: Oh yes, yes. It was rather interesting in that, although he liked farming, he was a businessman at heart in many respects, and so he wouldn't have been satisfied not to have contact with business and with the public. So that was where I spent the rest of my days before leaving for college is on that farm. And those were hard times; they were hard times for everybody almost. Not only the Depression, but there were droughts that just were terribly severe, and to have 80 acres that wasn't growing anything, and cows to feed or you didn't have any milk. And how do you buy hay from wherever you do buy it? Some way we did manage to buy hay at times from various people, but anyway, it was a very difficult time for everybody.

Gelman: What did they have their sons doing?

Jeffrey: Well, they--

Gelman: Your parents.

Jeffrey: You mean my brothers?

Gelman: Yeah--

Jeffrey: Yes, well--

Gelman: --I know this is a nine-year difference--

Jeffrey: --yes, from the first one. They were in high school and they were going to school in high school, and they did do some chores around the farm, but you see my brother, the elder brother, graduated probably in '32 from high school, so he didn't do all that much around the farm. And the younger of the two did some things, but in general we didn't get terribly involved with the big chores of the farm, only small chores like milking cows a few times. But there was always a hired hand as I recall around to do an awful lot of that, and my father. The emphasis for the children was to get an education. And I should comment that -- well, two things, finishing with my father -- he was a very competent individual, and there was sort of nothing he couldn't do of a practical nature. It didn't matter whether it was plumbing, or carpentry, or overhauling an engine, or anything. I grew up thinking that one did absolutely everything, painting, and it was done at our house, and of course there wasn't any money to hire anybody in during much of that time, but I think he did that anyway. I think he'd grown up in a similar situation. We stood out on the farm from our neighbors a bit. Sometimes they referred to us as "city slickers." But I think it was partly because our house was decorated, it wasn't a fancy house at all, but it was clearly decorated differently in terms of the pictures on the walls, and the things that were out. But more than that, my father took farm magazines and got bulletins from the state and county agricultural association or whatever they're called, and he did a lot of different things than the other farmers were doing. For one thing, he was rotating crops which was being advised at that time, and others didn't know about that or weren't doing it, and that seemed like a strange thing to do. But he also -- we planted soybeans and, interestingly enough, you wouldn't guess it now, but I think that was a rare thing to do in those days. And it was a good rotation crop, because it put nitrogen back in the soil fast, it produced hay, and it produced protein of the beans for feed. That's a very common crop in Iowa now, but it wasn't then. And there were some other things that -- there were some drought resistant things that we started planting first before others would. The others were sort of standard wheat, and alfalfa, and oats, and corn and that was it.

Gelman: So he was the research farmer?

Jeffrey: Well, in a sense, yes. So--

Gelman: But also, Jeff, he sound -- I can see now some background that's relevant to skills of yours--

Jeffrey: Yes.

Gelman: --and in particular, your ability to take any piece of equipment apart and make it work, find whatever it does, walk into my office and make my air conditioner work, build things to--

Jeffrey: Well, we learned never to be daunted by any problem of that sort. I think sometimes--

Gelman: Yeah.

Jeffrey: --we were in the end, but we started out as though we weren't daunted by it. As for my mother, she was really an intellectual in many ways. We had a considerable library, at least for middle class, or certainly for a farm family, but even for a middle class family in town. And I don't know, well, I'm sure she accumulated it, and she had read a lot and she knew a lot about literature, and it certainly encouraged, probably more than my father, our intellectual pursuits as children.

One other thing about the family and about my father I should say is that he was a good musician, too. And I really don't know anything about how he got started with that, but he could play violin, at least

at the level of playing for barn dances, that kind of music, and he did that well. He then played trumpet as a youth, and I take it was in the town band as a kid and young adult. And after he got into business he found it sort of hard to keep his lip up for playing trumpet, and I think it wasn't until then that he took up clarinet and played clarinet very well, or reasonably well. And my oldest brother played clarinet, and the younger one of the two elder brothers played violin and he was a superb violinist. He really played very well, and indeed, was invited to play for all sorts of things all through his life. It got to be rather bad, because it took up an awful lot of time, the PTA or someone would want him to come and play violin for them.

Gelman: And when did you have music lessons?

Jeffrey: Well, let me -- I'll come to that--

Gelman: Okay, fine.

Jeffrey: --in a minute. So anyway, he was a good violinist. As I say, he got unhappy about having to do that, and partly when he wanted to play football my parents were very upset, because he shouldn't play football. And at that time, too, this would have been in the mid '30s, there was more and more jazz music and he wanted to be involved with that, and he didn't get involved with that with the violin. So he took up the trombone as well as the violin, and he learned that very well fairly quickly. And my mother then played piano, and was a competent -- nothing seemed to daunt her, but I don't know how well she really played. She didn't practice as far as I knew in the time that I knew her. But she did get a lot of playing in, because of the way we would -- well, Sunday evening musicale, the whole family would get together and do something or other. Well, I should -- and yes, I've forgotten about that, too. But before I was old enough to play anything my father had a church orchestra and my two brothers and he played in the orchestra. He organized it and there were three or four other people. It was, I don't know, eight or ten persons from the church congregation who would play each Sunday morning in church. And I never got involved with that. But at any rate, there was a lot of music in our house. There was the practicing during the week of my brothers that I was exposed to, but I was also exposed to the fact that when they had some solo piece they wanted to play they would call for my mother to accompany them. And on Sunday nights when they'd try to do something together she always had to be there on the piano. So when I was probably four or five I was asked what I would like to play, and I said the piano because that was obviously a very basic sort of instrument, and that seemed like a great thing to do. So I started taking piano lessons, and I have a feeling -- I don't know exactly what happened, but I suppose those got disrupted a bit when we moved. And the other thing that happened is, when we moved there was, at the school I went to there was a junior band that they were starting, and they had lots of trumpets as usual, at least in those times, and no clarinets or very few clarinets, and the music director there knew there were clarinets in our house, so they got me started playing clarinet. And I kept up the two for a bit, but finally playing clarinet took over and I dropped piano, which I've always been sorry for, and I sort of knew I would be. But on the other hand there were these opportunities to play clarinet along the way. So--

Gelman: Can I ask a question?

Jeffrey: Yes, yes.

Gelman: What do you know about your grandmother or grandfather?

Jeffrey: Nothing. They were all farmers. My--

Gelman: You don't know whether your grandmother, for example, was also a schoolteacher?

Jeffrey: No. No, I really know nothing about them. I--

Gelman: So nothing about your mother and father's interests in what are considered intellectual matters like music, books--

Jeffrey: --well no, I -- see, I never -- the only grandparent I knew was my mother's father who was a very dignified and seemed urbane, at least for a farmer, or you'd -- he looked like a Kentucky colonel in some way, but without a flamboyant mustache. It was neatly trimmed, and he was a very handsome man, and I think he was just a very successful farmer is my impression. And his wife died fairly early as I remember, although my mother was into maybe adulthood at least by the time her mother died. But he's the only -- my father's father and mother both were dead long before I ever knew them.

Gelman: Do you know anything about when your family first came to America--

Jeffrey: Well--

Gelman: --how they ended up in the Midwest?

Jeffrey: --no, I really don't. I do know that my father's, I think it probably was his grandfather that came from England to America and ended up in Bangor, Maine. And my father always wanted to go Bangor, Maine, and I don't know whether he ever did or not. I don't think he did. So he'd heard about Bangor, Maine, but I have no idea how they got out. They would have been out early, I mean, it's amazing to think back and to that far. Now we talk about what things have happened since 1940, say, all the marvelous things that have happened since 1940. When you go back to 1900's, then--

Gelman: Exactly. That's why I'm asking. I'm very taken by the implications that you've drawn, and I think correctly so, with your parents' situation, kind of obviously very talented, your mother had musical and intellectual abilities, was a schoolteacher, probably would be today--

Jeffrey: Yeah, the interesting thing is she become a schoolteacher immediately after completing high school.

Gelman: That's right, yeah. Well, so could Ronnie's grandmother. And your father obviously managed to succeed and do all kinds of things himself to help that happen. You can't help but wonder their families were like and where they came from. And--

Jeffrey: Well I don't think there was any loyalty in the past or anything like that. I think it was fairly simple stock, but possibly a little better than some simple stock, but I have no evidence that it was all that different or that there were any other, well, anyone else in the family that did any better. Let's put it that way.

Gelman: So good native American--

Jeffrey: That's right, yeah.

Gelman: --Midwest stock, the kind that eagerly went to land grant universities?

Jeffrey: --couldn't afford to do anything else.

Gelman: Yeah, but they also supported them intellectually.

Jeffrey: Yes.

Gelman: Interesting.

Jeffrey: No, and my mother did that, particularly, and my father oftentimes would have liked me at least to have done some more work around, but I, as long as I could practice it was all right to practice

on clarinet. And when there was schoolwork to be done at the time, and so, but I did things. No, I've milked a lot of cows in the evening in my life, but I didn't get up in the morning to do it, or very seldom did I ever get up in the morning to do it, whereas my oldest brother did; he used to be out morning and night. But anyway, in general our intellectual pursuits and musical pursuits were encouraged.

Gelman: And so were tastes?

Jeffrey: Yes, and--

Gelman: And you mentioned that at least you're not -- it sounds like both your parents had inclinations towards having good, having attractive things, quality things.

Jeffrey: --yes, and indeed, my father always dressed well, and insisted that it didn't matter how little money you had, you could dress almost as well as anybody and look good when you went out and people wouldn't necessarily know how much or how little money you had. So--

Gelman: You were very fond of them?

Jeffrey: Yes, yeah, it was a good family I had no complaints about. My father was sort of a standard unemotional, or didn't express emotions. He didn't take the same sort of interest in what we did or didn't express the same sort of interest in what we did as my mother did. I always -- I remember being sort of shocked when I went off to college and they took me to the train, and my father shook my hand, and his voice broke as he said goodbye to me, and I couldn't believe that, I just couldn't imagine, because I'd never seen that kind of expression or reflection from him. Not that, you know, it wasn't that we weren't comfortable with each other, but that sort of affection, which is I take it rather standard, particularly for that time, but even now for many men. And so--

Gelman: We're at schooling.

Jeffrey: --schooling moved from, into the farm in the third grade, entered third grade in a consolidated school about five or six miles outside of Des Moines. It was partly a farm community, and there were a few houses around there. I think it was county road maintenance headquarters was someplace around there, and there were some people who ran trucks to carry gravel for the county, and other people like that around. There was Camp Dodge, which was a World War II camp and was used somewhat by the National Guard. It used to be at least in the summertime, but it kept a small group of people there year round. And there were three or four children that I went to school with who were the children of Army officers who lived nearby -- National Guard officers actually. And then there were a number of coalmines around in that area as you probably may have remembered, but the Middle West had a lot of coal. It had soft coal. And it was rather interesting. You could see farmland, and then there would be a coalmine, typically rather small. And so there were two coalmines, several miles on either side of our farm, and they were three or four miles away on either side of the school. But many of the miners lived adjacent to the school in this little community around the school.

Gelman: Did it have a town name?

Jeffrey: Well, it was called Johnston Station, and it was -- that was the other thing I -- it was a stop in jitney inner urban transit between Des Moines and a little town. I don't understand why that -- how it was put in, but a little town 25 miles to the north. It wasn't a big town, and there weren't jitneys out in other directions. Anyway, it was a stop there, and that I suppose was helpful in the community, too. But the miners were all Serbian and Croatian and so it was an interesting mix of students in the school. The school was small; there was about 20-25 students in each grade kindergarten through 12th grade. And so the children came from these assorted families. It was not a very good school, partly because of the Depression I think. I don't know why else. They couldn't offer a lot of courses. The high school, there was a physics course offered, but it was thoroughly hokey, and no laboratory, and no

biology course, no chemistry course. And I can't say that there was ever an outstanding teacher I had there with a possible one exception, which may be interesting, was a high school teacher who just came in while I was in high school and taught a very good course in grammar. And I really liked that, and I don't know to what extent that it was the way she taught it. My mother was a good grammarian, too, and helped us in whatever writing we did. Indeed, she was a very good writer. The letters she wrote to me were gems time after time. I couldn't imagine how she did it at a very busy time and a time when she wasn't particularly well. While I was in the Army I got these beautiful letters day after day, after day from her and they were always, I know, written late at night before she went to bed. But she took the time. They didn't come every day, but every three or four days at least.

So, I don't know what else to say about the school except I did well in school, and probably wasn't as challenged as I could have been, but I did get involved with lots of musical things. And that was interesting that during part of that time, the early period, the music program was weird in the sense that there wasn't a music person on the faculty of the school, but there were WPA provided itinerant musicians coming around. One of them that was there for a while was, his name was Noah Turney. I think he'd been with NBC or something like that, a violinist. He, you know, I didn't mean he could handle a group full of elementary or junior high school and high school kids in an orchestra. But anyway, we had the opportunity to get together to continue playing. And one of the problems in my music education is the lack of music education. I don't think I ever played anything but first chair, which were always things that you could whistle, and I was never a good sight-reader for that reason. I probably knew it to begin with, or if I didn't know it I could sort of look at it and start whistling it, and then once I'd do that I could play it by ear. I knew what note to start on. I never had anything on music theory, which I regret. But anyway, I was very good at doing what I did. And so on through high school, well, even intellectual things. But I was put in charge of a pep band, which was about five or six of us who were the better musicians in the high school band, and we went to all the basketball games and played, and we could make more noise than any five people you ever -- we were really pretty good and everyone was very happy to have us. So--

Gelman: So you were organizing then, too?

Jeffrey: Well, I'm not sure how it got organized. No, I'm not sure that I organized it. I think the -- we had by then a regular music faculty person, and he was relatively young and I think it was his idea. And he put me in charge of it because he didn't want to go traveling around to these things at night--

Gelman: --okay. We'll just call it administrative experience.

Jeffrey: Yes.

Gelman: I have one question. How did you learn any science or math in this high school if you said it didn't have any?

Jeffrey: Well, it did have math: algebra and advanced algebra.

Gelman: Oh, it did? Okay.

Jeffrey: Yeah. And I didn't take geometry there, because I think -- I forget what interfered with that, but it may well have been a course in typing that I insisted I wanted to take. I just thought that would be very important.

Gelman: That was smart.

Jeffrey: Yes, and it wasn't in the academic lines, though, you know, and that was sort of a problem to even get to be able to do that from a commercial track.

Gelman: I never learned to type for some reason.

Jeffrey: Yeah, I know. Most people don't or haven't, but I did. I didn't learn to type well. Indeed, that was probably the lowest grade I ever got was in typing, because the girls in the course would stay after. It was always 3:00 I think or something, the class was 2:00-3:00 or something like that, and they would stay and redo all their exercises until they were perfect and then hand them in. I would type whatever exercise I had to do, and do the erasures and hand it in, and go to either band practice or I was out for track one year, and even football another year. So anyway that was -- otherwise there weren't many challenges in high school. And I was reinforced a great deal for instrumental music, and we had a good -- the last two years of high school -- a good vocal teacher. And we had a very good mixed chorus and boy's glee club, and in some respects she's the most impressive teacher I had. She was really good at organizing, disciplining, but again, that wasn't much of intellectual experience.

Gelman: What was the school's attitude about intellectual matters? I mean, were they of a mind that their kids would go to college or--

Jeffrey: No.

Gelman: --were you the exception?

Jeffrey: No. I can't say that exactly except that not a lot of kids were going to college at that time--

Gelman: Yeah, I know.

Jeffrey: --you know, it just wasn't possible. And so very few of the kids were, even though they were taking an academic track, it wasn't clear how many of them were really thinking about college in the long run. Some of them were. Some of them were thinking about business schools and things like that. I guess some did think that, well, possibly they'd go to Drake University, which was the local university, and that they might be able to pull that off. So I wouldn't say they weren't thinking about college, but--

Gelman: Or at least another level of it.

Jeffrey: --not quite in the way that people would think about it today.

Gelman: Oh, I understand that. I'm just trying to get a sense of what the expectation of was of the school itself.

Jeffrey: Well, I think that set the tone for the school. This was Depression all the way through there, and people just didn't have money, and so it wasn't expected that you could do very much. To dream of going to college was just sort of an impossible dream for most of the kids there.

Gelman: What happened to that school after the Depression?

Jeffrey: Oh, I don't know. Well, that -- I left--

Gelman: Yeah, and never went back.

Jeffrey: --well, I did, and then that's another story. I went back for the 50th reunion.

Gelman: Oh.

Jeffrey: And the interesting thing is a number of -- it's amazing how many of class -- it was combined reunion for three classes actually, and at least two of those classes were the kids I knew best did go to college and do very well, but it was due to the GI Bill. They went to the Army and came out and, although one of them had gone to West Point and is a retired colonel now. Oh, I think -- no, that was

just one of them. And another one had stayed in the Army; he was an Army officer's son, but he'd gone into the Air Force, and he just stayed in the Army afterwards in the Air Force, and then quit, retired and was called back to do some interesting things. He was my best friend, and he was a very bright kid, and indeed, the sort of interesting things he did later involved intelligence stuff for flying things into Turkey and things like that that he had to supervise. I guess he was put in some sort of diplomatic position as a liaison officer for some special activities. I didn't get the whole story from him and we haven't gotten together since then. Anyway, the school was -- well, I got an education, a rather minimal education I'd say.

Gelman: What about friends, you know, among your school and high school? You've just mentioned your best friend--

Jeffrey: Well, yeah. It was essentially him. There were others that, well, mainly other kids that were in the band that I knew well and were friendly with, but I wouldn't go out of my -- I was glad to see them and see what they had done, and have seen that several of them have done well. My sister went through school six years after I did--

Gelman: You're not the youngest? What a wide range.

Jeffrey: --yeah. And her generation did better, but she would have been getting out of school soon--

Gelman: And after the Depression.

Jeffrey: Yeah, and after the war.

Gelman: Yeah, as it starts up.

Jeffrey: So things were different then, and by then Des Moines had moved out some, too, around there, and so I think -- and the school I went to was torn down, and there's a new school there now. So that's changed quite a bit. But--

Gelman: What does she do?

Jeffrey: What does she do?

Gelman: Yeah.

Jeffrey: Well, she went, she had a music scholarship and went to Simpson College, which was a school south of Des Moines about 20 miles, and she stayed there I think maybe a year and a half, and then her friend or husband to be got out of the Navy at that point, and they got married. And so that was the end of her schooling. But she ran after having two daughters and getting them -- well, maybe she worked a while before that. She worked all the time part time I guess while the children were growing up. But she worked for Bankers Life Insurance Company, and actually did very well, particularly when she ended up working full time, because she became a certified underwriter and could write and approve insurance at $500,000 or a million dollars, a fairly large amount. And at the same time -- that was an interesting thing, too. I'm not sure that's all, but part of her job along the way over the years at Bankers Life had been sort of troubleshooting. That's if someone didn't get their royalty check -- that isn't what they're called, but anyway, they didn't get their money, and something didn't happen--

Gelman: Dividend I think, right?

Jeffrey: --no, the commission, it's their commission--

Gelman: Oh--

Jeffrey: --see, the money goes in and they were supposed to get the commission. Well, the check didn't come. And so where did -- what went wrong, why didn't it come? And also, then they sent, I take it, an application in for something and nothing had happened, and why hadn't this guy heard, where's the policy, what's happened to it, and well, the policy's been sent out. Well, he hasn't gotten it. Well, what's happened? Anyway, she seemed to be very good at managing to figure out how things could go wrong, and looking to find out what. And the fact is, after she'd become an underwriter, and they think of themselves as real professionals, and that's a position for an underwriter. They asked her finally and I think it paid her handsomely to go back to doing this, because--

Gelman: --as recommended--

Jeffrey: Well, we can start, but--

Gelman: --yeah, we can start now. I guess the next thing's--

Jeffrey: --after high school -- yeah, college. Well, I couldn't afford to go to college immediately after high school, and so I knew I should work for a year at least. And that didn't seem like an unreasonable thing to do anyway. I was only just 17 when I graduated, and so I started looking for a job, which wasn't easy in 1940. And so the job I got first was as an usher at the Des Moines Theatre, which was one of the two first run theatres, and that doesn't sound like much today, but in those days it was an interesting job. In the first place, well, these theatres were the days of palatial theatres, all the marble staircases, and ceiling paintings, and organs in the theatre. And there were I think on the floor at any time five ushers. There were five aisles I think plus a head usher, and when you came into the theatre the head usher would tell you which aisle to go. Well, I should say that we had marvelous uniforms, nice military uniforms, and you really had to pass inspection when you came out on the floor to report to the head usher, and you had to have your tie straight and so forth, and your shoes shined. But then -- oh, I was going to say that when a person came to your aisle you stepped in front and greeted him with a slight bow and flashlight to the chin, and turned smartly, and took them down and showed them to their seat. And you took them to the seat you wanted them to go to and not where they wanted to go. You could ask them whether they wanted back or well down depending on the space. But there was no talking on the floor. We used hand signals to tell the head usher how much room we had in our aisles; he could query us. We could also signal if we had to leave the floor for any reason. And it was interesting to -- well, it clashes with what you see today in theatres. I might add theatres ran -- they were dark the whole, from the time they started. There weren't intermissions between shows. The intermissions were filled with newsreels, and cartoons, and educational film, or short subjects some of them, comedy things. And the other thing I'd really sort of forgotten about in thinking about this, people sort of poured in at any time. They didn't think much about when the movie started; they just came in. And if they got in in the middle of the movie they watched it around until they could put the thing together and they left. And of course, their chances were fairly good of getting in during newsreels and so forth. But some people obviously knew when a show was starting, but a lot of people didn't. They'd just come into the theatre. So people were coming in and out all the time, and they did need to be shown their seat because they needed to have a flashlight to show them to their seat. Anyway, that was an interesting experience, but that was split shifts, and it being downtown, and pay was $10.00 a week. I found that there was a job at a glass station and garage that was much closer to home, and they would pay me $12.00 a week, so I decided that I had enough of the entertainment business. Well, I didn't mention the fact that the manager of the theatre always wore a tuxedo, and indeed, it was owned by the Tri-State Theatre Company, and the executives of that company, there were two or three of them that would drop in almost weekly to see how everything was run. They always dressed in tuxedos, the whole group. It was an interesting time. But you know, the theatre, that was big entertainment in those days. There was nothing else, that's what you did on a date was go to the theatre, and it was nice to go 2nd run theatre. It cost more to go to a first run theatre, probably all of 75 cents.

Gelman: Well, it's still the case that the only place you see people in tuxedos and cocktail dresses is in the industry.

Jeffrey: Yeah, yeah, that's right.

Gelman: It's guaranteed that they're going to be dressed up.

Jeffrey: So anyway, then I worked the rest of that time at a garage and went to school, went to college in the fall of '41.

Gelman: Where?

Jeffrey: University of Iowa.

Gelman: Oh. So you did build your undergraduate/graduate work at Iowa?

Jeffrey: Yes, I did all of it there. And of course, that was interrupted by the war December 7th. And that disturbed me a great deal in the sense of what to do. And I might add that college, although one of the greatest experiences in my life -- and I always enjoyed the first few days of school here, the excitement of kids coming back to school. And I always thought that was just the greatest time, and I remember the days so fondly of going to college. And on the one hand I enjoyed the courses I was taking there, but part of it I think was the shock that many kids get that go on to college, that wow, this is a whole different world, and your expectations are a lot different, and I wasn't prepared for all of that. But anyway, I wasn't -- I don't know how to describe it. What with the war, I decided in spite of what they were saying that we should stay on. It just seemed so futile to stay on for another semester. What would that do for me? I think if I'd been in the year before and completed a year and this was the first half of the second year I would have convinced myself that I could stay on, or that I should stay on and finish two years at least.

Gelman: It takes -- you're describing a common experience of freshmen who go away from home--

Jeffrey: Yeah.

Gelman: --but then it's on top of a very serious national and personal--

Jeffrey: Yes, it--

Gelman: --on a young man who--

Jeffrey: --well, it was--

Gelman: --thinking about the challenge of defending the country.

Jeffrey: So anyway, I quit and went home, and started looking for a job in the defense industry. I'm not sure, I suppose I might have enlisted, but I was still too young for the draft at that point.

Gelman: You were still 17?

Jeffrey: Well, I would have been -- no, I was 17. Anyway, I'm not sure what the draft was. Early it was high, it was maybe 21, and it got moved down rather quickly. But I'm just not sure about that. But I don't think I was concerned about the draft immediately, but I think I knew it was imminent within a year or so or less than a year. But then an opportunity to take a course, a special defense course that was taught at Iowa State College, which is the engineering school now, Iowa State University--

Gelman: Where is that?

Jeffrey: --that's at Ames, Iowa, closer to Des Moines. There was a six-weeks course to take there, and I decided to take that. And that was a very interesting course. It gave a lot of basic engineering stuff, descriptive geometry, and engineering reports -- I've forgot now what that was about, but gave us experience with various sorts of -- oh, drafting. That's right. So it was basically a drafting course, and the other things were a supplement to the drafting. So we learned to use machine tools and welding and things like that, so that you got some idea what they were asking people to do when you designed something, or do a plan and what that'd indicate. And then a friend of mine from high school, or an acquaintance, another clarinetist in the band, had gone to California and was working at Lockheed Aircraft, and his brother was going out to join him, and so I was invited to go out with them or -- good idea if I went out. So I said yes, I would do that. And so I came to Burbank and got a job at Lockheed Aircraft.

Gelman: I didn't know that.

Jeffrey: Yeah. And so I worked in their drafting department at -- actually Vega Aircraft, which was a subsidiary of Lockheed, but their building was right at Lockheed air terminal. And so I worked there until -- from May or June, something like that -- until November, and at that point I was subject to the draft then, and it seemed a real surprise it hadn't already happened, and other people were sort of being drafted around me at that point. And I decided, well, it's going to happen soon. Oh, and they were moving my unit to another building, and it was then inconvenient for me to get there. So I said, "Well, that's it. I'll go home and spend a little time and get in the Army." So I did leave in November or something like that, and actually was not drafted for a while after that. There was some difficulty. I'd registered in California and whatever happened, it took them a while to get things rearranged, and I finally had to -- I didn't enlist, but you applied for induction or something like that to get things done and did go in. It was going to happen sooner or later, and the fact was, if you hadn't been -- I hadn't been classified. That was the problem--

Gelman: Yeah, so you didn't know what was going to happen.

Jeffrey: --classified, then you couldn't -- well, there was, a law was passed that you were subject to a $10,000 fine and so many years in jail if you weren't classified. And actually, I called the office of a father of one my friends, another one of them, and said -- oh, he was head of the induction, Iowa induction -- and told him what was happening, and he said, "Well, this is what you do, and I'll take care of it, and don't worry about it," and they did very fast. So anyway, then I went into the Army and took basic training at an Air Force basic training camp outside of Salt Lake City, and as I completed basic training the Army had set up a program called the Army Specialized Training Program, which was a college training program, and it was -- at that time they actually were getting a lot of draftees, and they were also recognizing that the war was going on and on, and that they needed to have people trained. They couldn't just stop all training of, college training of men so that they would -- well, the Navy had done this first with their B-7 Program I guess, and so the Army set this up, and I applied for that and was accepted into that program. I might add that I've also heard that another reason for setting up those programs was the fact that the colleges were having trouble existing. They wanted to keep colleges going and universities going, too.

Gelman: So they used the college campuses for these--

Jeffrey: Yeah, yeah. So they sent people to the campus for the program. And it was true that there were just -- no one had been -- and even women weren't going to college at that point. They were getting jobs, and some of them weren't going to go to a college if there weren't any men there, which is reasonable enough. And so--

Gelman: This is 1942 now?

Jeffrey: '43.

Gelman: '43? I was just--

Jeffrey: Yes, '43. So in the fall -- well, first we were sent to the University of Utah where we went through a series of exams, and then if we passed those exams we were sent to a college someplace. It was supposed to be close to home because we didn't get any standard furloughs, there would be just the week off between terms, and so they would send you to a college close to home. Well, it turns out we ended up, or I ended up going to Brigham Young University, which was south of Salt Lake City about 30 miles. Well, that turned out to be a marvelous experience.

Gelman: But quite an experience too, I would imagine!

Jeffrey: Yes. And that wasn't a great intellectual experience in itself. The training was fairly intense in, well, physics, and chemistry, and biology, history, and English. So it was a standard first year course. But there were some interesting kids there and I enjoyed being there a lot. It was maybe easier going to coll -- well, it was very intense there also, though, because we were on a sort of summer school schedule, and indeed, we were also very regimented. We marched to classes; we marched out of classes--

Gelman: This is the Army.

Jeffrey: --we had one hour off after dinner. Dinner may have been from 5:30-6:30 or something like that, and at 7:30 we had to march to the library and spend two hours there, and then march back and be in bed by 10:00. They'd check the next morning. And then there was -- they threw in some other general exams besides the classroom exams. They had three or four days of tests, national tests that were being given.

Gelman: Oh yeah.

Jeffrey: So that wasn't fun. But anyway, I had some good friends there. But again, music probably was a more important thing there than my intellectual education. And from there, then they broke that up in the spring of '94, because--

Gelman: '44.

Jeffrey: --'44, yes. And because the war was clearly -- could be brought to a close, and now they needed everybody they could get back into the war. And so we were shipped off, the group I was with all went to Infantry Division in Camp Adair, Oregon, outside of Corvallis. And we took basic training there, and then I was put in the artillery, actually in the instrument section of that survey, and fire controls, so some of my engineering background got me into a good job there.

Gelman: It would keep you off the front, wouldn't it?

Jeffrey: Yeah. Well, no, not quite. A little back of the front.

Gelman: But not -- yeah, okay, dangerous.

Jeffrey: Yeah, no, it was dangerous. If we could shoot them they could shoot us--

Gelman: Yeah.

Jeffrey: --shoot at us. But there was long delays. I think we started through basic training, which, I don't know whether it was a two-month program or so. Then you started day one, day two, then we were shipped to Fort Leonard Wood, Missouri, and went through basic training there, and then we were shipped to Camp Miles Standish in Massachusetts and put on a ship and landed in Marseilles. And Marseilles had been taken by that time, so that was -- but then we went into combat on up around just

about Strasburg and on into Germany from there. So I did get into combat for about the last three months of the war until the end of the war. But the experience, oddly enough, in the infantry and subsequent -- after the war was over I still spent another almost year in Europe before coming home. And that was as intellectually formative as almost anything that I had done previously, and that's maybe the wrong thing to--

Gelman: But say why. It was--

Jeffrey: --well, there were these other kids from college, indeed, there were not only the Brigham Young University group, but there was a group that had been at Montana State University, and there was a group who came from, oh, some smaller college on the west coast. So sprinkled throughout this division were a lot of college kids who weren't appreciated in the division by particularly the cadre of old infantry officers. You know? "These college kids! Who do they think they are?" And--

Gelman: But they constitute your cohort?

Jeffrey: Yes.

Gelman: And give you opportunities to find people you really like?

Jeffrey: Well, yes, and that. What happened was that there were a couple of people in my unit who were very interesting to talk to. One of them, I was trying to think of his name yesterday, and it wouldn't come. It was Hamilton Hess; he had a little more college than I had, and was a very thoughtful individual and a true intellectual, and had a lot of background in philosophy. And we would spend hours talking, and we'd stay in the latrine at night and talk until 11:00, and the guys would come in drunk from the service club, "What the h** are you guys doing?" You know?

Gelman: Logical positivism?

Jeffrey: Well, not exactly, more moral philosophy then. But actually, at Camp Adair, Oregon, in the service club they had an amazing library. I don't know, I never saw one like that any place else. They really had a rather large library in that club. And I managed to get some books there and read them. I remember one of them, it happened to pop into my mind; I'd like to look at it again. Indeed, I looked it up on our university catalog, and we had it in our library, but it's in the non-accessible part of the library, but I'll ask for it one of these days. It's Edman's *Philosopher's Quest*, and also his *Philosopher's Holiday*, but then there was Walter Lipman's *Preface to Morals*, and I'm not sure some of the other things I read. But it was that level of -- but we talked then politics, and social conditions, and fascism, and communism, and all those things, and we were together for, oh, almost a year I suppose, or at least a year, he and then some others at various times, and then there were other kids that I interacted with, too, who we didn't have quite the same intense intellectual relationship. But anyway, I just -- there was time to be thoughtful and think about these things, and time to do some reading. And I picked up books. I remember once at an inspection when officers came by, and my footlocker was open, and everything was neat in there, and there was also a stack of books, paperback books in there. And one was Snow's book on China, on Red China, and I forget what one of the other ones was, but they weren't standard murder mysteries or girly books. And one of their officers picked them up, and looked at them, and turned to me and said, "Do you read these?" And I said, "Yes, sir." And he passed them over, and they sort of thumbed through them, I mean, there was some mumbling, and put back and walked on. But they were concerned about that I was reading communist material.

Gelman: And it was *Red Star Over China*?

Jeffrey: Yes, probably. Yes, that's what it was.

Gelman: It's a great book.

Jeffrey: I think I had -- well, I'm not sure, because I managed to pick up other books. I was thinking of the French -- I can't think of his name right now (Malraux). But anyway, that -- of course, the year after the war then too was time to, oh, a lot of dumb stuff being done, but there was time to talk and read, and I got transferred from the infantry to a topographic battalion, which was a very interesting group, and some interesting fellows there that were, they were all college educated -- well, they were -- that was a very interesting group, because there were all of these sort of semi-professionals who were the artists, and photographers, and cartographers, surveyors, and then there were truck drivers.

Gelman: To get to places, to--

Jeffrey: Well we had -- see, they had this tremendous equipment, printing presses in trucks, printing, and dark rooms, camera equipment in huge semi-trailer trucks. So there was probably ten big trucks that carried all of this stuff. And the commander -- it was a colonel, a West Point man, and they were always -- the commanders obviously -- these topographic battalions were West Point trained. I don't know why that had to be, but anyway--

Gelman: It's sounds very high level skill--

Jeffrey: Yeah. Yeah. Well, it's -- I think--

Gelman: Being able to do these things is a really rarified ability.

Jeffrey: --yeah. So that was a very interesting group and such a different world than the infantry was in terms of how it was run. And there were not a lot of regulations, because they didn't need regulations. And, well, indeed, in part of a warehouse that we were set up in -- we lived in some nice homes right around a warehouse -- the fellows had a professional and artistic group had set up a nightclub sort of arrangement, and they'd take and drape the ceiling and walls with camouflage netting, and even painted some things, spray painted some things and some of that, set up a bar in there and coffee and things like that. And we had great evenings in there. I forget what recordings or what, but I assume there was music of that sort, and they'd sit around and talk, and drink coffee. I think, I don't know whether we had liquor or not there. I don't think we did.

Gelman: Where was this?

Jeffrey: Well, that was down by -- just outside of base. And we were supposed to be going back to the States and then to Japan. And then the war was over in Japan, and so then I, oh, then I got shifted to another outfit. It was just a matter of juggling people around and not much to do for anybody at that point, and that was back in Paris. But then they started again, to keep the troops occupied, they set up a college-level training program, which was a very interesting operation. It was established as Biaritz American University in the French resort town by that name in southern France.

Gelman: Yeah, I know it is. It's at the--

Jeffrey: --a lot of nice hotels--

Gelman: --from southwest tip of France.

Jeffrey: Yeah.

Gelman: It's at the border of Spain.

Jeffrey: --yeah. And they set up or they commandeered a number of hotels there. Unfortunately, I didn't get stationed in that palace hotel there, but in a much smaller one. Indeed, I got in two days late for reasons that I won't go into here. But they brought some people over, faculty from the United States. They took some out of the Army. I had an English course there. I didn't have a lot of choices,

but I don't think there were a lot of choices to begin with, but an English course from a fellow who was, I think he was a first lieutenant in the infantry or something like that. But he was working on his PhD at Princeton before going in the Army. But he wasn't a great teacher of English, and as he said, he hadn't read anything but early English for years.

Gelman: He was a PhD.

Jeffrey: But we had a -- it was a good course; it was a nice relaxing course. And then the star course that I took there was from a philosopher with a PhD was the captain in a tank corp. Paul Wienpahl was his name, and he ended up after the war teaching at U.C. Santa Barbara.

Gelman: You should make a point of writing some of these names down with spelling.

Jeffrey: Yeah. Well, I'll spell that right now, P-A-U-L, W-E-I-N-P-A-H-L. And he was -- again, it was a rather informal course, but he was a very brilliant fellow, and just very, very stimulating. But that was also stimulating and another highly intellectual climate with a lot of kids there. We had actually time there; the only requirement was we took three courses, we had to attend class every day, and whether you took or passed an exam or not didn't matter. That was it; that was your only requirement was that you just attend class. And so there was a lot of time to do -- well, you worked, too. But I always liked the class every day notion, because you then knew exactly what you had to do that night. You had three different courses to prepare for, and you sat down that afternoon or night depending on what was going on. But they had a theatre going there, too. They had some people from Hollywood running a theatre, and there were actresses and actors who'd been sent over to entertain troops, and they had some of them to put on shows and to take lead parts, and some of the soldiers then took other parts, so one could have signed up for courses in dramatic arts or set design and so forth. Anyway, that was only three months, but it was a lovely, lovely three months, and again, it was a very -- it added a lot to my sophistication, intellectual sophistication, which I certainly didn't get in the earlier life. So sooner or later I got sent home early in '46 or April of '46, and went back to college in the fall.

Gelman: And you're now how old?

Jeffrey: In '46, I'd be 23 years old. No, wait a minute -- yeah, 23 -- '43 -- yeah, 23 years old. So back to college and that was great. I really felt prepared for it now. But my ideas of what I wanted to do had changed, and I took a course in introductory philosophy then, because that -- it was a terrible course.

Gelman: Who taught it, do you remember?

Jeffrey: Oh no, I--

Gelman: Okay.

Jeffrey: --don't remember. It wasn't Gustav Bergman.

Gelman: Iowa, you know, had a number of great philosophers available--

Jeffrey: Well yes, but it was--

Gelman: --yeah.

Jeffrey: --it was not one of the greats. But--

Gelman: Okay.

Jeffrey: --and so I guess the other formative thing is another woman. I was taking French, spoken French, and the TA was actually a Belgian young lady, a Belgian who was a graduate student in psychology. And--

Gelman: --a TA in French?

Jeffrey: --yes, yes. And so -- and you know, there weren't a lot of TA positions in those days except in languages.

Gelman: In language, yeah--

Jeffrey: --in those days, yeah, languages.

Gelman: It's still true in the humanities.

Jeffrey: Right. So I had learned a lot from her, too, first about psychology in terms of what it was, and so I decided to take a psychology course, and that did it. It stood out in great contrast to the philosophy course which was soft, and I think I'd been through the soft side of philosophy. And I was a scientist at heart and an empiricist at heart anyway, and here was psychology potentially taking up a kind of life problems one wants to worry about, maybe not -- well, possibly could take up some of those--

Gelman: Sure.

Jeffrey: --and do empirical research about them. So that interested me; that did it. I knew exactly what I wanted to do from then on.

Gelman: Do you remember who taught you that course?

Jeffrey: Yes, Don Lewis. But actually, a couple of -- he taught part of the course, and Art Irion, who was a graduate student at the time, taught another part of it, and a third person -- oh, that's -- there were four, Al Shepherd, who was a -- you know Al Shepherd, right?

Gelman: He taught me as an undergraduate. I was in his lab in Toronto.

Jeffrey: Okay. Yes. Al Shepherd; he taught a good share of the course, and the other fellow was in Florida, and he only taught a short portion of it.

Gelman: Al was a great teacher--

Jeffrey: Yes. No, that was--

Gelman: --and a wonderful human being.

Jeffrey: Yes. So anyway--

Gelman: That's funny.

Jeffrey: --I then went full steam ahead. By going to summer school and taking extra courses I got my BA in '48, and even though I'd only had one quarter of school there before, I had picked up some credits at Biaritz which were discounted in the University and also from Brigham Young University, and that helped a bit. And by going to summer school the last year I finished a major in psychology in '48. And even though I did think about going someplace else to graduate school, it did not really seem to be the thing to do at Iowa with the great graduate school that was there, and not having had real contact

with that. I guess I had had a course with Bergman by that time, but certainly not with Spence or Brown or any of the others that were there.

Gelman: Did you do any research as an undergraduate?

Jeffrey: Yes, but just as part of an experimental course, not anything to speak of.

Gelman: Then why were you -- how did you know to go -- you wanted to go to graduate school?

Jeffrey: How did I know I wanted -- well, there wasn't anything else to do--

Gelman: Okay.

Jeffrey: --but go to graduate school, but also part of my education then came from this French teacher, who -- I'm not sure I really knew about graduate school until talking to her about it. And I certainly never knew how anyone became a professor, but then I never dreamed that I might do that sometime, and I didn't for a long time. But -- and this woman was in clinical, but that didn't mean non-experimental at Iowa, so in orientation -- but it seemed to me from what I knew from her and what I was learning in class that it was reasonable to go ahead in clinical. And there was an end point in a job presumably out there someplace, and so I started out in clinical. And at the end of the first year I had taken mainly the experimental courses, but I had taken a course in testing, and I guess that was my only clinical experience was a series of testing courses that even included Rorschach in the final phase. And it wasn't that I didn't -- actually, I thought those courses were good. I didn't mind taking those. But when I started looking ahead and saw practicums in counseling and, practicums in this, and all of the sort of things that my friends were doing were in clinical I said, "Well, that's not for me." And at the same time, I was in contact with people in, well, all sorts of people in experimental psychology, verbal learners, and rat learners, and eyelid conditioners and those types of people, and that was all interesting, and I found that in some respects I did want to work with humans, and indeed, there was some sort of experimental clinical research just being started at that time, and that looked pretty interesting. But then I found out -- I had contact with the people in the Child Welfare Research Station--

Gelman: Oh, how did that happen?

Jeffrey: --well, they were in the same building, and they took courses in psychology, and there was one of them in particular, a fellow by the name of Chandler Screven, who I think is still at the University of Wisconsin, Milwaukee, who was a great missionary for experimental child psychology. And he was really the first one I know of to talk about that, and he would talk about the possibility of setting up mazes to run children in and, oh, all sorts of things, application of learning principles to children, not very imaginative in some way except for the fact they were being applied to children. He is a very interesting character. I would like to see him again. He doesn't seem to go to meetings; he's never published very much. He left to go -- he went from Iowa to the University of Mississippi and taught there a few years and I lost track of him. He was a better talker than he was a doer, and what he did mainly while he was in graduate school was build hi-fi equipment himself, and he did some research, too. But anyway--

Gelman: I knew a graduate student who did that.

Jeffrey: Yeah.

Gelman: I mean, that was one way of looking like you were doing research--

Jeffrey: Right, right.

Gelman: --because you were building equipment. Right.

Jeffrey: Right. But he was very enthusiastic about experimental child psychology. And I then took a reading course, maybe that was in the summer, with Vincent Nowlis in the Child Welfare Research Station, and I told him what I wanted to know. I just wanted to know a little more about what research was possible. Well, I was very impressed to find that there had been some very interesting experimental research that had gone on there over the years. Of course, their purpose for being there was research. But there was just some other things, and I think I gave whatever library I have, in fact now--

Gelman: Well, tell me some.

Jeffrey: --well, there was a study by Jack. There were things that came out in that Iowa monograph series, and I don't remember the other names, but they were simple studies, not very theoretical. But one of them that I remember that I thought was particularly cute was they rated children's social skills in a nursery school. Then they paired them up with a high/low sort of thing, and then put them back into some situation, and would see whether having been in a situation of solving problems and interacting with a child who wasn't dominant -- a non-dominant child where it gave them a chance to be more dominant would affect their subsequent social skills--

Gelman: --Saturday, August 6th. It is the second recording session with Wendell Jeffrey, and this is Saturday. Let me just repeat what you said, which is we ended yesterday with an introduction to the part of your career that's best considered the move to development psychology.

Jeffrey: Well, I was speaking of this course, summer course I took under the supervision of Vincent Nowlis, and read a number of the Iowa monographs, and also I recall now having read some of the articles out of Barker, Kounan, and Wright which were very interesting, and again, an example of the fact that some very good experimental work had gone on that I hadn't really known about. But on the other hand, it wasn't done with any learning theory context, so it was just seeing a good background for the revolution, which we thought was to come. I then requested to transfer into child welfare, and I'm not sure that was done until the winter semester, but on the other hand, whether it was or not, I nevertheless started taking courses, and changing my curriculum immediately in the fall.

Now, it was a very interesting time when you realize that developmental psychology wasn't very old. Trace back the early studies at the turn of the century, but things didn't, serious things weren't happening until the late teens, early '20s, and so that was only 30 years, less than 30 years, before. And so it was actually possible to familiarize oneself pretty well with all the literature extant those days, not that I read everything, but the fact is, you know, you knew who was doing what, and who were the significant figures in all the field at that time. And that was interesting and is impressive to me now, because obviously it's not even possible to know all the significant research in one's own field, so things have changed a great deal. Now, the people that were influential from then on -- I might mention the faculty at the Child Welfare Research Station, Robert Sears was then head of it, John Whiting was there a cultural anthropologist, Vincent Nowlis, another Yale person who was actually in social development, or that was what he came there to teach. He, interestingly enough, had worked with Kinsey on the Kinsey Report. He did many of the interviews and set up the interview schedule. He was an expert in that area. There was Orvis Erwin, who worked on language and really did some very interesting pioneering research in that area. Howard Meredith was sort of a premiere physical anthropologist and probably presented the best in research physical development that ever existed. And Beth Welman, who was a pioneer researcher on intelligence, but the people at Columbia refer her as the promoter of the Iowa idea of a galloping IQ, because they looked down their long noses at her. And Ruth Updegraff, who I'm not sure if she was sort of social/educational/development concerning preschool education.

Gelman: Was Kurt Levine there?

Jeffrey: No, he'd left by the time I got there; he'd been there, I don't know, just a couple years before I got in. But his effect on the department was very minor and he -- although there weren't many supporters of him it on one hand, a lot of people regretted his leaving. The arguments between Spence and Levine were very rich and intellectual. They respected each other, but did not agree on a number of things. So that had been a very good era that proceeded my getting involved even with the department. I'm not sure when he left. It must have been, let's see, '47 when he went to Michigan.

Gelman: Did you read anybody in particular at the time whom you were really taken with?

Jeffrey: Well, what I became taken -- once in there and getting involved with things generally, I spoke of Chandler Screven, who had interested me in getting into developmental psychology. Along with him the book by Miller and Dollard, *Social Learning Imitation* was sort of a bible, at least for him and for some others. Well, indeed, it was for Sears and Whiting also, and Nowlis too for that matter, because it represented Yale, well, Yale outlook on life. And that we all found very exciting, and so that really focused our attention as far as research was concerned. Otherwise, I took various courses, covered the field, and none of which focused on that text particularly, but the other students and I, Screven, George Greer, who was a fellow student who didn't continue in development psychology merely because he didn't get a job in a university. Jobs weren't easy to get in 1951 and so a lot of people that graduated in those -- well, the other thing that was happening is that a number of people coming back from the war were getting PhD's, so suddenly they were turning out a lot of PhD's, and indeed, although the schools were burgeoning with students they didn't have the money to hire faculty. Anyway, it was in Washington that Charles Spiker was a co-student and both of these, or all three of these people, students, were very informed, so we spent a lot of time talking about the future of child psychology and experimental child psychology. Then John Whiting was influential in sort of a funny way. He was a very enthusiastic person, and he had his cadre of students who were very close and had a lot of parties. And he was -- had a great group to teaching, and a great theoretical facility, too. He could make an awful lot out of very little. Anyway, I respected him and learned a lot from him along the way. Vincent Nowlis was not really my mentor, but it was primarily a matter of convenience. There was no one else who was likely to understand what I was doing, but it wasn't really his area of expertise, but he liked me, and respected what I was doing, and some of the others also, so he was there to help us through. Probably a stronger influence was Farber from the department of psychology, and I continued to talk to him off and on about things when he was on my committee. And of course, Spence was very influential, and he was also on my committee. So that was the intellectual context I guess.

Gelman: How many of the people were there at the same time?

Jeffrey: Well, Glen Terrell, who you may not know, he ended up as the president of Washington State University, overlapped with me. He came in a year later. People like Lipsett, and Palermo, and White, and Reese all came in after I'd left. And what happened was that the year I graduated Spiker also graduated, but also Boyd McCandlis had been hired as director. Sears moved out in '50 to Harvard, and I don't know who was chair that interim year, because it was the year I was finishing up. But Boyd McCandlis was hired during that year. He came in that fall, so I didn't overlap with him. Well, he had taught at San Jose State College out here. Anyways, he'd been a teacher of Charlie Spiker's at San Jose State College but graduated from Iowa some years earlier. Anyway, he hired Spiker as an assistant professor position. So he stayed on. And then also, Al Castaneda was hired a year later, and it was those two who were powerfully influencing on the subsequent flow of the students. And what got them there to begin with I'm not sure. I think it was just the fact that they knew that Spiker and Castaneda were good people to work with. But on the other hand, it was -- there were always -- the Station that had such a good reputation. They were a wide -- there were a lot of people came there not quite knowing what it was about, but just on the basis of its reputation, and indeed, I think at times, well, many times it got students who really belonged in psychology, and as soon as they got there and were taking a course or two in psychology, transferred into psychology. So students, as in all places, students often turn up for their own reasons, and indeed, some of them came there on the basis of the reputations of various the people in the Child Welfare Station, but there

were only two or three of them that were respected. And yet many of the people came by the psychology department faculty and staff on the station were clearly "old school" and found much more excitement in the course they were required to take in the department of psychology which was in the same building. So now, the place was ripe for a revolution. In many respects it had been revolutionary from the beginning in the sense that they had fought the nature/nurture battle on the side of nature -- I'm sorry -- nurturing. And that was really a battle, and it was, in a sense, Iowa against the world at that point. If you look back over those studies on intelligence and the vituperation--

Gelman: In development?

Jeffrey: --in developmental, yes, the vituperation that came out in the *National Handbook of Education.* The intemperance of the argument regarding the Iowa research that showed that IQ varied with pre-schooling experience, which was demonstrated by comparing the IQ's of orphans placed home compared with those staying in orphanages. The children placed in homes had higher IQ's some years later than those that remained in the orphanages. Anyway, there was that tradition there already, so it wasn't hard to take another step and bring child development into child psychology. I think that's the point I'd like to make, that a lot of the courses in child development at that time presented normative studies. They were normative because they assumed behavioral development was all a matter of maturation, so if you established when some behavior occurred you just knew what the maturational course was.

Anyway, so there was this young group at Iowa in the psychology department, and we were all, I think most were on the GI Bill, and certainly for me that was one reason to work very hard to get out fast, and so I was only in graduate school three years. That was also encouraged at the time and there was nothing to keep me from doing it. I think it would have been good had I had another year. There were several courses I would like to have taken that I couldn't take. Anyway, I did get out in three years. And so as I think I've said before, we were a rather cocky group at that point thinking that we had started this new field of experimental child psychology. And I went to Barnard College at Columbia then, at the end of the summer of '51. In my first summer of Barnard after being a year there I was preparing my dissertation for publication and was getting ready to send it off to the *Journal of Experimental Psychology*, and was struck dumb by seeing Harold Stevenson's dissertation published there of all places. I had thought mine was going to be the first experimental research on children to be published in the *Journal of Experimental Psychology*. But Stevenson's study was a perfect example of the kind of things we were doing, and what's more, I had this response that I think every student at the time of publishing his dissertation has when he sees something and it's like, they've done my research. Well, I'll never be able to publish this. But after looking it over I realized it wasn't the same as my research, and indeed, they did publish the dissertation. But the significance of this was that at Stanford somebody had been the thinking along experimental child psychology lines also, and it's obvious that it was inevitable. It was time for this to happen. Because in some respects child psychology was fairly bankrupt at that point, it had worn itself out. There were a lot of these significant names, very respected people who sort of didn't have anything new to do. So I suppose Iowa was a more likely place for this new movement to grow out of than a lot of other places, but obviously it did. And I don't know what -- come to think of it now, I don't have any idea what kind of a program that was at Stanford beyond some classical physical growth studies and Terman's work on the Binet Test. I'm not sure how many developmental people there were there, and I think that's probably why Harold also chose to study within the framework of experimental and comparative psychology. I should add that the shift in the Child Welfare Station was taking form fairly fast with the appointment of Robert Sears, John Whiting, and Vincent Nowlis in what was probably my senior year, 1948 and thus provide my experience with what was going on there when I took my first graduate year in psychology.

Gelman: You haven't said anything about what you did as a graduate student.

Jeffrey: Well, I didn't do a lot in terms of research. I had to do some study along the way. I was expected to do something in the way of master's research or just a general research project lending to the dissertation. And, oh, I do remember what that was now. There was a rather neat little study. I

never tried to publish it. I don't think now it probably was publishable, although today somebody might have worked it up and published it.

Gelman: What was it?

Jeffrey: Well, to tell the truth, it would take me a while to reconstruct it. Oh, it was -- well, it had to do with specifically Farber's notion that conflict would increase motivation. And I made a primitive pinball machine. You didn't shoot the ball. The ball was dropped down through a round chute, and as it came onto the board it tripped a switch, which would turn on a light either on the left or on the right, and that was a signal to turn the lever to the left or right in order to make the marble go in the correct hole. When they would learn to do that, which they did easily, I'd turn on both lights. And now that we could question not only the change in reaction time but the pressure with which they squeezed the response handle or some other physiological measure, but the equipment for taking such measure was not available. I was not able to devise anything that would do it. I get some credit for the research design. I did work for Don Lewis for about three months on a Navy project that had to do with transfer and interference in gunnery tasks and that was good experience. But otherwise, I didn't have to work. And as there were not many jobs around then, and I was eager to finish fast. I did just that.

Then with respect to other research it was the thesis, or the other research was my thesis, and that grew directly out of the Miller, Dollard, and Doob *Social Learning and Imitation* book. Well, by then the new Dollard and Miller was out, the *Personality and Social Behavior*, which was even a better book than the Miller and Dollard, and the business of response mediation and secondary generalization acquired equivalence, acquired discrimination terms were all and there. And so that was the basis for my dissertation research. And the motivation behind it was the thought, which indeed I still have today, is that language isn't as magical as a lot of people try to make it out to be. It has some real virtues in terms of having rich stimulus characteristics, highly discriminable responses are made verbally, and so that is an important factor. But a lot of the literature then, as now to some extent, a little less now, sort of sees it as really being a different system and having different characteristics. And I might add that I think a lot of our attention in graduate school, if not on courses and our conversations among students, had to do with the idea that the child research within the context of learning theory was the child was perfect for linking all we knew about animal behavior and simple conditioned things to accounting for human behavior, and so that distinguishes the human from other mammals. And so this chapter on that approach in Dollard and Miller was a feast to work on, but in any event, I thought even there they were making more of it than they should, particularly within the acquired equivalency and acquired distinctiveness notion, that by labeling so much was added to the stimulus. And I felt any other distinctive response would do the same thing, so I set up an experiment that used for one condition a verbal response, and in another condition a motor response, and within these two different conditions there was a group who only got it in terms of the motor response -- or the verbal response or motor response that was given in the acquired distinctiveness paradigm, which meant you labeled these cues, but then you went ahead and learned something else without making another connection. That was one condition. The other condition was that they not only labeled those cues, but then those labels were associated finally with another response such that you had SR verbal connection, and then R verbal stimulus to another connection, so that it should play out that way, but it acquired distinctiveness, just the SR verbal and then the R and then the S motor response, then that would be the acquired equivalence. And I was essentially saying that that wouldn't help much. So it was really a rather neat little experiment. And it worked out almost perfectly, the mediation conditions worked out fine. The acquired distinctiveness conditions were sort of falling between the experimental and control group or the group who used the motor response or a label alone. So that was that.

And well, they wanted me to talk about personal research contributions in this context I guess. And I think it's still appropriate to say that when I had to teach four courses and one of them was a laboratory course that took two afternoons a week, so that was a rather heavy teaching load, and although I did some research there, it was not easy getting the subjects in New York City. Columbia

had a nursery school that was close by, but the teacher wasn't terribly friendly or cooperative, and I probably should have been more insistent. But what I did do finally, well, through some odd connections was being encouraged to do some research at a school for retarded children. I thought, Well, that will be very interesting, because I can compare -- well, what was it -- oh, my thought was that they would be interesting borderline children, not as verbal as kids we often run into, and so you could have older children with more motor proficiency and so forth, and still have a less developed mind, assuming that there were -- or that I was assuming that the so called "garden variety" retardation was really just underdeveloped intelligence, something like that.

Gelman: --the normal distribution--

Jeffrey: Yes, and--

Gelman: --there often special reason for being retarded--

Jeffrey: --but just delayed in development, not that they could never catch up, but that it was that sort of organic condition. I wasn't disabused of that right away, but it didn't take too long to see that there were all sorts of problems.

Gelman: Something else was going on?

Jeffrey: Something else was going on. These kids were just very different. I did find something I found interesting, but I didn't publish that because I didn't really have enough subjects or didn't, well, there weren't enough subjects.

Gelman: Or at least on the curve, the normal distribution--

Jeffrey: The teachers were spending a lot of time trying to teach these kids red and green discrimination, because they would sometimes be walking into school, presumably on their own, and they were having no success, so I said, "Okay. I can teach them that. I can teach a kid to discriminate red from green." And the fact is, I took them down -- I could take them down to the basement, and I set up a routine for training red/green discriminations, and I could get red/green discriminations, but when you take them up in the classroom, they didn't have the slightest idea--

Gelman: Couldn't do it at all?

Jeffrey: --couldn't do it at all.

Gelman: No transfer.

Jeffrey: No transfer at all. Now, I think I wanted to follow that up during the summer. I'm not sure whether that was the first year or the second year there, but anyway, that summer I thought, Well, I will go where there's a large group of retarded children, I can't remember the name of the school. It was in New York but on the west side of the Hudson, a famous school--

Gelman: Oh, I know.

Jeffrey: I haven't thought about it in years and haven't heard about it in years.

Gelman: They still do work there and they do have a decent director. I'm trying -- I don't remember right now.

Jeffrey: Well, anyway, I went over there, and I had an in in a sense that there was a former Yale man there who was -- I don't remember his name -- was a person who was familiar with Hullian theory, and I went over to talk to him. He was a very nice fellow, but the talk was sort of devastating in that I told

him what I had done, and he said, "Yes, of course. That's what happens." And anything I told him that I wanted to do he essentially said, "Well, I have done all of that." And he had done a lot of research, and I think he published some, but not a lot. And it was, well, it was very discouraging, because he was essentially telling me that, you know, it's just hopeless, and yet he was director of research there. I don't know what he really did, but he wasn't encouraging in the least, and so I didn't feel I could do anything there. So that was the end of that. I did some other things with students there, too, which didn't go anywhere. I really didn't have much time. And I taught the summer school, too, after that, which was what people needed to do in those days.

Gelman: No salaries, right, or almost no salary?

Jeffrey: Well, $4,000.

Gelman: Yeah, I was going to say maybe five.

Jeffrey: In the tradition of the old boy network, Dick Youtz, head of the department at Barnard College called Vincent Nowlis, asked if he had anyone to recommend for a teaching position. He said it would pay $3,600, and Vincent said, "Well, that isn't enough. You gotta pay him more than that," so they raised it to $4,000.

Gelman: So maybe that wasn't even about you--

Jeffrey: Yeah, oh yes. And as far as I know they never considered anyone else. As long as Vincent recommended me, then that was all right. So I did not get a lot of research done there because of the four courses plus laboratory load. I should talk about moving to UCLA then. It again was partially the old boy network.

Gelman: When does this happen? How long were you at Barnard?

Jeffrey: Four years. And, well, this never mattered, but I think it's interesting and I should put in here Bernice Wenzel was already on the faculty at Barnard and had been there two years and after two years had just been promoted to, or given a raise to $4,000. So the first offer was not unusual.

Gelman: No.

Jeffrey: That's sort of what happens. So anyway, we were married at the end of my first year.

Gelman: Did you meet there?

Jeffrey: Yes. And from that time on Millicent McIntosh, the president of Barnard, who as far as I know was an ardent feminist and wife of a physician, became concerned with the fact that we had married. Dick Youtz, who was on the dean's council, or the president's council, would say to me every so often that, "Well, we worried again about what to do about the Jeffreys," and he, at that time he'd say sort of, "Well, it's just that, you know, two people married." Although there was no official anti-nepotism rule at Columbia or Barnard, they kept worrying about it. And finally, it was in the third year, yeah, I guess the third year, the end of the third year that Youtz called me in and -- not both of us, but called me in -- and said, well, they'd had another meeting about us, and they had decided, the council had decided that only one of us could get tenure. And I said, "Well okay, I'll have to look for another -- I guess I'll have to look for another job." And he said, "Oh no, that isn't necessary." And it was clearly just assumed that Bernice who was coming up for tenure at that point would either retire to have babies, or remain an assistant professor. They'd have two more years before having to consider me for tenure, and presumably she could stay assistant professor the rest of her life, as did a couple of other women at Barnard whose husbands were on the faculty at Columbia. And one of them was a fairly distinguished economist. There was a sociologist, a woman, who was a full professor and had tenure. Well, there were several, but those weren't nepotism problems. Well, I said I would look for another

job. Youtz indicated very clearly that was not what was intended. Fortunately, within a month or so I got a call from UCLA essentially offering me a job. They had called the Iowa Child Development Institute to see if they had a candidate and the director recommended me. There were certainly other people considered when I was hired here. You know that. But anyway, I got a call from UCLA asking would I be interested and if so to send my vita. I talked to Bernice about it at some length, because people didn't in those days negotiate for two jobs before they would move.

Gelman: You either do that or be the first commuting couple?

Jeffrey: Yes, and we weren't going to do that either, not in those days particularly. And so she decided it was worth moving and we should do it. And she assumed there would be opportunities out here and was sort of excited by some of the potential other opportunities out here. So finally I received an offer and accepted it, and we moved out here in 1955, summer of '55. To get back to--

Gelman: Just one short--

Jeffrey: --yes--

Gelman: She got her job in the medical school when?

Jeffrey: Well--

Gelman: Is that a long story?

Jeffrey: It's somewhat a long story.

Gelman: Okay. That's enough.

Jeffrey: Let me--I can say something about it. She did some volunteer work for a while in the medical school, in the Brain Research Institute. She first attended some seminars and they had a weekly Wednesday colloquium, which was very exciting because people interested in the brain were coming from all over the world to visit the institute. And she knew Don Lindsley who was one of the originators of the institute as well several other people, the physiological people. It was an area that clearly interested her. And what happened there was she finally volunteered to work with one of the young physiologists in the institute. She was a physiological psychologist who could profit from some additional training, particularly in physiology department which had only recently applied for a training grant. This was after our first year at UCLA. This came after a first year of volunteer work. The physiology department recognized that psychologists had experience with regard to training animals that they have or know. Their attitude had been after all, Pavlov was a physiologist, and they knew Pavlov, so they knew all there was to know about conditioning and so forth.

Well, it turns out that she kept telling them certain things they might do differently, that really worked and they decided they needed someone to teach that sort of course and was finally given a position. I found out later it was clearly another thing for which it's time had come, because other schools about that same time were beginning to hire physiological psychologists to provide behavioral training methodology to their PhD students. So that's how she finally got hired. Now, coming here represented new challenges, well, with regard to research. The teaching load was at least three courses a semester. And there was almost no equipment available. I don't know how I got the money to get a little memory drum, but I did. And it was clear that I had to do some research fairly fast, and I was the only developmental psychologist here, replacing someone who did not get tenure because he didn't publish enough research. And, oh, more than that when I was called and offered the job I was told -- this was Gingerelli who called me and offered me a job. And I don't know. Did you know him at all? Yes?

Gelman: I sure did.

Jeffrey: He was a character. Well, he sort of mumbled along the way that of course I would be in charge of the child clinic, and I said, "Now, wait a minute." I said, "I'm not qualified to do that." And he said, "Oh well, you know, it doesn't matter." And I said, "Well, you know, I did not have an internship," and he noted that I'd had courses in testing, which was true. And I said yes, but I hadn't had an internship, but he then indicated that they had a training grant that supplied the person who was in charge of supervising the clinical trainees, and of course, part of the training grant was a child clinical psychologist, who had all the credentials but they needed a regular faculty member to oversee the operation. I think he suggested they would try to hire someone in the next year. And I said, "Well, okay." I thought it was worth risking it, and it turned out not to be a problem, because indeed, I think that one of my course requirements was met by that sort of supervision. And it didn't take all that much time. It took time in that I sat in on a lot of things, staff meetings and so forth, but it didn't take time in that I didn't have to be directly involved with the training. And in some respects, the person who was in charge of the program was very well qualified. She was very good. And so I felt that it was a decent situation, but I never let up in the staff meetings about the need to hire a qualified clinician, which we did eventually at the end of my first year.

Gelman: Was that Nakamura?

Jeffrey: Yes, Chuck Nakamura.

Gelman: So that was a good move, too?

Jeffrey: Yeah. But anyway, there was still the problem, research was clearly important to get a promotion. So the only thing to do to get publications fast clearly were to do some things in verbal learning research, on acquired equivalence, acquired distinctiveness, stimulus familiarity and things like that. So I did several studies of that nature, and indeed, I did get some child stuff started. But as you know, it takes time to get that done. You can't sign up 40 elementary school kids or preschool kids in the same way that one is able to get 60 introductory psychology students coming in on the hour. I actually liked doing research on college students, because it's possible to do an almost perfect study in terms of design. You can--

Gelman: Without the children this--

Jeffrey: --you can counterbalance everything, and you can set up a fair level of control, not as much I don't think now as much as I thought we were doing at the time, but anyway--

Gelman: You run out of subjects.

Jeffrey: --yeah, you run out of subjects. So -- no, that's it, you can't. And so anyway, that is what I did. Well, I did get promoted, but I also think that I probably had some child stuff going along the way, I'm sure I did have. I didn't really check the dates of subsequent publications. But that research then followed along from my dissertation in some respects, but not entirely. By that time stuff on acquired distinctiveness and acquired equivalence was pouring out of Iowa at a great clip, and I was a little disappointed in some of that. See, well, there's a difference in the way I look at things and other people look at things, and I didn't ever look at research as wisely as some people do. I didn't care to do one little study after another. I would look at things and say, "Well, this just isn't that interesting even if you did get significant results. I'm not sure what it meant." And so then I got involved with -- I guess -- let's stop for a minute.

And so with regard to my research here, I think partly I was beginning to think that possibly there was more to discrimination learning and concept formation than learning theory was accounting for or the kind of studies we had previously done had accounted for. And then beyond that, there were such things as infancy research that came out, and whereas I had done this research on discrimination learning, and was finding that you had a hard time getting some of these kids to learn left/right

discriminations or various, even tonal discriminations I worked on, and I guess also color discrimination. And yet there were some papers coming out showing discrimination in infants that I couldn't get in a three year old or a four year old. So that led to some research on performance variables, as a result I published a series of three articles on performance variables in discrimination learning -- and I did together that had to be learned in order to perform at the level we wanted to learn to make certain discriminations. And then that led to attention as an important variable. And I can't tell you now -- well, maybe I can now -- just when that came possible for me as a Hullian to use attention, because that was not in Iowa's vocabulary. You could talk about observing responses or orienting responses, and I was trying to recollect whether I dismissed Broadbent's book on attention as not worth reading because of the title, or whether I really just didn't know about it. But I must say when I finally read it and I think I was already sort of feeling that maybe I'd seen other things than attention, but when I read Broadbent's book that opened up a whole new world and aided me to shift the focus of my research. And I must say that in a sense now I realize that my Iowa training, although very rigorous, was very restrictive, and it didn't help to have Irving Maltzman, another Iowan, looking over my shoulder. I would talk to him from time to time about some of these revolutionary ideas, and he would have none of it. He dismissed out of hand. On the other hand, he was doing some things that were breaking with tradition too. It was all right for him to do it, but he wasn't going to trust me to drift away. At least that is sort of the way I felt about it. Anyway, that was sort of a funny situation and I did feel that somewhat constrained by him, because he wasn't loathe to speak up at any time--

Gelman: At any time. He still isn't bashful.

Jeffrey: --particularly when it came to promotions and so forth. And I respected his opinion, too, except that it was clear that a lot of other people were doing their own different things and talking at different levels. Of course, the word "cognitive" was another to him at that time. I'm not sure whether he will use it today or not, but he probably does. So anyway, there was a clear shift in what I was doing at that time, but then I was invited to write a chapter on transfer on the Reese and Lipsitt series on Experimental Child Psychology, which was a good step along the way. And I wouldn't say that was the most important thing I wrote, but I think it was good. Then, by that time I also had been working with infants to some extent and their attention was a very important variable, and I wrote for Lipsitt and Reese series the chapter on attention, and I'm almost as proud of that I think as anything. And that has -- at the time I think was referred to quite often--

Gelman: Oh yes.

Jeffrey: --and got the attention it deserved. I think the best of my theoretical work was the "Orienting Reflex and Attention in Cognitive Development," which was published in *Psych Review* in 1968. Then the paper I like even better is the version of that paper that I presented at the Minnesota symposium. That was probably 1969, published in the symposium series. Hill was editor of that series. And that's the most readable, and then that again was revised and presented more technically with some modifications at a conference on attention in New Hampshire arranged by Tom Tighe, and there I called it Habituation Perspectives from Child Develop -- no, that was the title of the symposium--

Gelman: --say.

Jeffrey: --Habituation Perspectives from Child Development and/or Behavior Neurophysiology, 1976. I don't remember what the title of my paper was there. Anyway, I think those were the papers, interestingly enough, nobody's ever paid any attention to, or very little attention to. And I think it's particularly interesting in the sense that it put together some of the Dollard/Miller work, and all that is done within that framework, and in Kendler's work with a reversal shift, and a lot of work that was already done. And with Gibson's notion of stimulus pre-differentiation and that sort of idea, and not only stimulus pre-differentiation, but her notions of perceptual learning--

Gelman: Learning, yeah.

Jeffrey: --which she talks about how it occurs, and what I provided is that an interesting notion about how percepts and concepts develop through the simple process of serial habituation component stimuli leading to the development of a concept. Unfortunately, it isn't a readily testable notion. There are three or four studies out that did try. I tried in a sense, but I sort of gave up. It was more complicated than I wanted to do, but nevertheless I still think it's an interesting idea, and is worth considering in a broader context to explain some of what goes on in -- well, how learning takes place without reinforcement, without forced experience, without training. And nobody else has ever presented more plausible mechanism. Well anyway, as I say, it's been generally ignored by the Gibsons, and many other as far as I know. I've never really looked to see how many times it's been cited in subsequent literature, but many times I wanted to say, "Look, this is how you can explain concept acquisition." So I think the other maybe question that comes up in here was about the applied research.

Gelman: There is another line of research that you've done that is still current, and it's still cited.

Jeffrey: Well, that's what I was -- I considered that applied research in a sense, because that's how I got involved with it, although it came up -- but I really -- in discussions with her and she phoned me later with a real problem which was would I speak -- it was a time when more and more people were talking about applied research, and indeed, it was a time when I think I probably had a number of good students, but I also had been given a number committee responsibilities. I was on a personnel committee during the winter and the spring, two times a week all day, and I got a course reduction for that, but I was also editor of *Child Development* at that time, too. So in the end I had very little time to get involved with research except to attend some supervisory meetings. But I really liked that idea of studying individual differences or looking for individual differences, which I do in learning styles, which I do believe exist, and I do think in the long run it would be great if you could only identify them more reliably. And that's the trouble there. I'm not sure how reliable they are, actually. I don't know whether they're context specific or just what it is. It isn't easy to get a reliable measure of. But if we did have a reliable measure, it would also suggest some different teaching styles, and maybe that would be the better thing to work on is to -- of a child, but obviously some children blossom under different -- will work well in one teaching style rather than another. So that we did, Zelnik and I did some of that work together. She continued to with some of that in Israel, and then Marilyn Welch was interested in that, and Joan Lawry who was a post-doc with me did some fine things using a variation of Raven's Progressive Matrices that they developed to obtain a measure of cognitive style, that is whether children tended to respond to large or small features of a stimulus pattern. So that was the extent of the applied research. I should add my comments about reliability, the difficulty of getting reliable measures. We did an awful lot of studies, well, I shouldn't say a lot, but a number of studies where we found something good or sort of first cut, and so then I'd say, "Oh, now, given this, let's replicate this condition and go on from there, and build on that," and indeed, that to some extent I think has been an important story of my life is that in looking at the performance of variables as they did, I often saw studies that showed something couldn't happen, and I would say, "I believe I can show that all you have to do is make the child more familiar with the response, or the stimuli, and then they can do it." But first you need to replicate the original finding.

Gelman: Right.

Jeffrey: Time and time again I couldn't replicate some research, either our own or somebody else's research. And I'm not critical of the people who did the research. I think that's a problem with a lot of the developmental literature, maybe a lot of the psychological literature. There are a lot of subtle conditions, but particularly when you're working with children where you can't mechanize everything, and where you do have to, some of it depends on the rapport you establish with them, the familiarity of the laboratory, how convenient or how congenial the experimental situation is. Are they thinking about how they can get out of here, which I've had a few children do at times? They may start fidgeting and the saying, "I think they're having juice now. I'd better go back."

Gelman: They don't want to be in the study.

Jeffrey: Yes, they're suddenly getting anxious, and those are things that you can't specify very well. Anyway, I found that there are an awful lot of fragile conditions that sort of, in child research, and that was certainly true of, particularly true of the learning styles literature. We would -- we think we have a really hot point, and then we couldn't replicate it ourselves, and that's in the same laboratory. I also, and I'm sure you've had that experience, too, of going to one school, nursery school, and get behavior you expect very neatly, and then go to another one and you don't get that result at all. There are clear different conditions on how they're taught, when they're taught, what kind of experiences they have had outside the classroom, what social/economic level they're at, all sorts of things like that can play a role.

Gelman: Yeah. That's one of the reasons I got interested in conversational readiness, the whole business of knowing the rules of an experiment as a young kid, but as I sit here listening to you I can't help but be impressed by how much you influenced what we're doing.

Jeffrey: Well, and that's fine. I would like to take credit for that.

Gelman: Well, we'll come back to it if you'd like to talk about it. I know it's not part of the planned things--

Jeffrey: Well--

Gelman: --made a couple of notes that--

Jeffrey: --no, I think, I think there is an area to talk about everything with training the students.

Gelman: Yeah, but this would be a very private conversation.

Jeffrey: Okay.

Gelman: That is, we can have it on tape if you want; it's just that I think we should hold it.

Jeffrey: All right. They did want to know what of my research was wrong headed, and I think a lot of it was, and I can't name any particular piece that -- I suppose the applied research may have been as wrong headed as anything, although--

Gelman: You mean the retardation work that you mentioned or the--

Jeffrey: --well, no, no. I meant the -- no, I didn't think of that as applied research actually. No. I meant the cognitive styles research, that we thought it might be simpler than it really is.

Gelman: Ah, okay.

Jeffrey: And Jerry Kagen, who originated that research, always makes everything look very simple, and it wasn't that simple. He has the good sense to move on fast to something else. Okay.

I think in many respects my role became one of just trying to clarify issues when I thought that maybe people were going too far with their interpretation, and to them to address these issues in new ways. Many have forgotten some of the good, logical positivist models that have made psychology great. And so that conviction made it very hard for me to move another time and say the direction of, well, let's say to theory of mind, which I think is weird. I don't mean to say there isn't something there, and I wouldn't say all the research is terribly loose, but I also wouldn't say that it's terribly tight either. And I must say that that was one of two things that led me to finally say that I had enough of editing *Cognitive Development*. And one of these was that in retirement but I thought it would be fine just to keep that as something to do. I found that with great freedom to travel, I also hated to come back and find journal articles waiting to be reviewed. I hated to work hard to get everything caught up before I

left. And one of my greatest concerns in editing was to get things through fast, partly because I had suffered severely in early days when the *Journal of Experimental Psychology* had lags of at least a year from the time something was finally accepted until published, and I had things that took very little rewriting that were almost two years before they got out. And that's very tough for someone who's trying to get promoted to tenure, and so I've always worked hard to get things done as quickly as possible in the editing field. Well anyway, the other thing was I was finding it harder and harder to get excited about some of the work that was being done, and knowing quite how to handle it. And yet it was obviously what was happening today, and so I thought it was time for someone who might be able to see this in a different light than I did--

As an addendum to the section on teaching, I was reminded that one of my virtues as a teacher, at least with graduate students, was that my seminars typically took up relatively new issues and books were assigned that had only recently came out, which we could review and evaluate. But the other thing that lies behind that that is appropriate was my own eclecticism and interests, broad interests in the field so that whenever anything looked like it might be hot or was seen as a new and stimulating area, whether I believed it or not, I thought we should take a careful look at it. And indeed, the seminars were part of my education as well as the students' education, because when you read the book through and say, "Yes, this is an interesting book," you don't see all of the issues as well as you do when you finally evaluate chapter by chapter and have student input on that, too. I think the other thing that students thought was a strength or at least helped get a number of students through and into different areas was the fact that I didn't think research had to be done only in the area I was interested in. I was willing to consider other areas. The only critical variable was could decent research be done in that area. And there again, by having -- well, a virtue of the UCLA department was nobody, almost nobody -- I can't think of anybody particularly in the early years -- that wouldn't see a student no matter whose student they were. If you wanted to go in and talk to somebody about a problem, the students were free to do that and were treated well. And that was a great advantage. One could say, "Go see Harold Kelly, or go talk to Cy Feshbach," who one might think was a developmental psychologist, and is a developmental psychologist but always kept himself isolated from developmental psychologists, again, partly as a clinician. And his real perception of himself was as a personality theorist in spite of all of his research on aggression, which seems very much like developmental psychology to me. But at any rate, there was always help on those issues, and there was always help on statistics of all sorts, and so that it was easy enough. Well, indeed, many of the students who came from UCLA, and in some cases I think claim me as a mentor did their dissertations with other people.

Gelman: Or had co-advisors.

Jeffrey: Or had co-advisors, yes.

Gelman: A high grade of co-advisors.

Jeffrey: Well, that was right. Typically that was what was done that -- maybe it was a rule that someone from developmental should at least be on the committee or a co-advisor, and so that worked out very well. Bernie Weiner was another person who I sent a lot of students to for his graduate course or seminars when they were interested in personality theory. And indeed, I don't think, particularly in the early years, he was interested in any way in development psychology problems, but he was always impressed with the students that I sent to his course would start translating his stuff into developmental problems. And he was always interested enough then that he would go ahead and help them in carrying out their research. So that was an example of the sort of thing that went on, and so I had a lot of students coming out who did work in a lot of different areas.

Gelman: Yeah. Tom Trabasso played a similar role.

Jeffrey: Yes, he was very good at that. Yes. Too bad he had to move on, because he was a very good generalist I think also.

Gelman: And led to all sorts of people working with you and himself in cognitive development.

Jeffrey: Mm-hmm, that's right. So anyway, then, as you said, probably my one virtue as an editor of *Child Development* was I could accept that a lot of different things could be very interesting, even if I wasn't equipped to evaluate them. I also knew by then, because of my broad interests, I knew generally who was doing work here or there that I could trust to give me a good evaluation of their research.

--and there was not a lot available in the early years, and I'm not sure when NSF actually started its program, but my impression is that it wasn't long before I applied for one the year I came to Los Angeles, which was 1955, and I was fortunate enough to get it. Indeed, there were two people in our department who got grants. I'm not sure how many applied or whether anyone else did, but John Seward and I both got rather small grants. But it was great to have any money, and I don't remember exactly how much I got, but it was probably $11,000 or $12,000 for two years, and that doesn't sound like anything now, but if you will remember that our salaries were, well, my salary was around $6,000 at the time. And there wasn't a lot of equipment to be bought for more than $70 or $80. It was -- it seemed quite generous at the time and certainly made life simpler for me because there wasn't a lot of institutional support at that time. I should correct that. The University did have a research fund, which we could apply for and generally got, but that was a very small amount, too, and primarily to hire research assistants. You could petition to use it for equipment, but the rule was that it was to be used primarily for research assistants.

The next grant I applied for was a training grant, and I did that the very year that they became available for something other than clinical psychology. And there again, I was very fortunate to get a small training grant, which seemed very generous at the time. I can't tell you how much it was, but it allowed us to at least have two traineeships and some extra support money. I say I felt fortunate to get that because at the time I applied we still didn't really have a program of any sort. We had only two young people in developmental psychology. But I worked hard on the notion which I firmly believed, and I still believe, that it was good for developmental psychologists to also get a good background in their general areas of interest, and that the best background probably came from going to where the best teachers were and the best areas were. And I think at the time it isn't -- it's easy enough to say that in the child institutes that existed at the time some of the training in, say, social development and personality was not up to par with what was being then taught in the general social/psychology/personality areas. So that was a notion I sold both to the Institute of Mental Health and also to students along the way. And the program continued and expanded over the years to, I think we never had more than 4 trainees, but that served us very well, or served us, me, very well over those years until we finally lost the training grant after it went through the throes of supporting only postdoctoral research, and finally was phased out entirely.

Gelman: When did that happen?

Jeffrey: Well, I'm sorry, I didn't look up those dates. I don't know why I didn't. I didn't even think about it. But that must have been 7 or 8 -- oh, more than that -- I suspect that was around '85, '84 or '85, sometime in there.

Gelman: Before you start--

Jeffrey: One interesting thing about the training grants and their renewals was as long as I can remember Stan Schneider was the grant officer, and every time when he would come out on a site visit for each two or three year renewal, and there were a couple of two year renewals because they, for some reason, weren't sure they should extend it for three years. And the complaint, even after we had five or six people who were all developmental psychologists, was that he wasn't sure that we had a program. He didn't understand what kind of a program we had. I thought I spelled it out in great detail, and we certainly had a lot of developmental courses by then, but we still encouraged students

to take minors in other departments and get a complete background if they were interested in, whether it was learning, or cognitive, or social, or personality, that they get the background from the best people in those areas, too. In any event, I was particularly amused one year when the person who accompanied him to UCLA for our site visit was John Flavell, and while Stan was giving his usual speech about he just didn't see that we had a program here, I won't try to quote Flavell, but in rather strong terms he said something like to hell with the program, look at the people he's turned out over the years. Look at the contribution of the department to developmental psychology in the country. And I don't think that had much of an effect on Stan, although we always did get our grant renewed until things got tough all over.

Now, with regard to institutional contributions, I've mentioned that I taught at Barnard College from 1951-1955, and came to UCLA in 1955, and retired in 1990. In my earliest years here it was easy to -- let me back up -- at Barnard I had mentioned I described problems I had getting research done there, particularly with children, and when I came here it still took a little time to get tooled up, because the teaching load was fairly heavy, and contacts had to be made, and there weren't eager developmental students around at that point. The person I replaced had been a developmental clinician, and so I didn't even see any students that may have been in the process of being trained by him. They just went on to work with other clinical people. So the students that I picked up right away tended to be students interested in learning, in some cases, paired associate learning, which is what I decided I ought to do to get some research done fast, and I could do that in the context of my dissertation work. Jenkins, Jim Jenkins at Minnesota was doing some very interesting similar work on adults, and so I worked on that. I was teaching introductory child development two semesters a year, and it was a class that ranged from 125-150 people. And in those years it was particularly easy I think to be very enthusiastic about child psychology, particularly experimental child psychology, and I think I taught, well, a fairly unusual course. It wasn't -- it didn't come out of the standard developmental textbooks of the day. And obviously I was enthusiastic enough about what I was doing that several students -- well, three that I can think of -- decided that they wanted to go to Iowa for graduate school. One of them particularly I tried to discourage. I thought there might be better places for him to go given his interests, but no, he wanted to go to graduate school wherever I'd gone. So he went, and they all got PhD's there, and have gone on to teaching positions. I might add that at Barnard there were three or four girls there who went on to graduate school that had been in my classes, and I felt reasonably close to for one reason or another. One of them went to Yale, and did her dissertation with Miller, and was kind enough to send me a copy of her dissertation, which thanked me in the preface for having gotten her interested in going to graduate school and getting her into Yale. So that was rewarding, too. One of the problems with teaching at Barnard was trying to convince the girls they could leave the East Coast at all. They really would prefer to stay in New York. If not New York, then they could go to Yale or Harvard. And interestingly, enough Rochester was all right, because I guess it was still in New York. But the thought of going to a Big Ten school anyplace was just -- they couldn't imagine it. It was--

Now, as for finally getting a program truly started, a graduate program at UCLA, although some of the first students did get sent off, I sent to do post-doctoral work that would make it easier for them to get a good job, there were others who for one reason or another couldn't leave or wanted to continue, and by then I had begun to establish more of a program and support for future appointments of developmental psychologists. So there were a number of the students in the early years who stayed on to work in developmental psychology here. After a few of them graduate, and after a network got set up so to speak, we started getting more applicants from outside, and then students who felt they should go elsewhere for graduate training, by that time I knew enough people in the field at other universities that it was easier to get them into other graduate departments. So the program did get moving. Oh, and of course, the training grant helped, because we did have applicants, and if it was someone I really wanted it was an easy way to get someone, and you could be pretty sure they would take that offer then because there weren't a lot of training grants or a lot of other money or fellowships at that time, so that helped get the program started.

As time went on, we were getting lots of good applications and were looking for money wherever we could get it for them, and by then there were more teaching assistantships available. And so between

assistantships and research assistantships, then people had research funds, and the training grant, and also scholarship funds in the graduate school, which got increased along the way, we really did very well at attracting students here. But I also managed to get many of the students into developmental -- or a number of students that went into developmental psychology here didn't come here to study developmental psychology to begin with. I think I was particularly lucky one year to be directing the pro seminar, and that was a good way of finding the very best students. And when I found one whose interests weren't well formed I often could persuade them that developmental psychology was the place to go, and give them the money, or give them the support if they would go in that direction. I think Marshall Haith was one I persuaded to go into developmental psychology that way, and also Patricia Goldman. She came here because her husband came to zoology. And all I do remember is at that point graduate students that hadn't had developmental psychology had to take a course in developmental psychology, which I always thought was a rather stupid arrangement. But at any rate, it then worked to my advantage in this case, because Patricia Goldman had to take that course, and it was clear that she was very bright and a very good student. And so I don't know whether I had to persuade her or not, I think she did have a fellowship. But I'm not quite sure what her interests were.

Gelman: --remember that?

Jeffrey: No, I don't think so. I'm not sure whether they even came in the same year or not. But they probably did. If not, they were only one year apart. And I don't remember -- that was an interesting thing about the area at that time, this would have been early '60s, late -- yeah, probably early '60s, and we still didn't have a lot of room. I don't remember now when the second building was being built. But by then I had great relations with the University elementary school and preschool, and there was these old temporary buildings next to them, which I managed to get one of as a research laboratory. They were being used, or one of them was only being used for storage or a couple of them were used only for storage, and I persuaded the Dean of Education that if they could consolidate the storage I had the money to refurbish one into a laboratory with some office space and some cubicles for research, which could then be available to the School of Education, as well as to developmental psychology. And that worked out very well, but the thing that also worked out well about that it provided, in addition to research cubicles there was room for four or five desks and it provided a very nice area for students to work, do research, and have offices. And there weren't a lot of offices available for students at that time, so the students, Marshall, and Patricia, and Les Cohen -- Les Cohen probably was still an undergraduate at that time. He did his undergraduate work, or took development psychology and started doing research with me as an undergraduate--

Gelman: Here--

Jeffrey: --here--

Gelman: --as a UCLA undergraduate?

Jeffrey: --yeah, and he stayed on, and it seemed to some people that he had taken a long time getting his PhD. That was not true. He just worked a long time for me, had a publication as an undergraduate with me. But at any rate, I forget who else was down there. There were four or five people, and they became very, very close, and it felt great to have this nice space, and they worked out deals so that they would do their own running to the library to get books for each other. And that worked out fine until our new building opened, and suddenly there was more room, and potential, although limited, office space for students, and now they felt isolated from the rest of the other graduate students. But in any event, I think that helped develop a great deal of closeness in that group, good spirit de corps. But that seemed to continue over several years there. And, well, indeed, I think it's tended to continue very well until finally in more recent years, I don't know what happened. There were more students, more faculty, and more subgroups being set up rather than a total group.

Gelman: In different places?

Jeffrey: Yes, that's true, too. They were spread out a little more in the department because then we had three buildings. I tried to keep people fairly well together, but I think more than that it was other people doing their own thing. I don't know, 1975 or even later, I was always really the head of things no matter who anybody was working with, the students were all fairly close to me and knew me very well, and by the, well, mid '70s I suppose Danny Kaye was here -- well, various others who had their own groups sort of set up. And I had been so busy partly editing, and partly on committees, and working with just one or two students, so they were having their own parties then. I didn't have to be the central figure, and that was part of it. It was clear students were hanging together but in different ways. I was no longer the main person or the main driving force in the area.

One thing more about the program as it existed, particularly when I was sort of the main person involved, and that remained true partly because Chuck Nakamura had a big foot in clinical psychology, and so his students who were not clinically oriented, although he supported them and was doing sort of social relevant research that was appropriate, those students were more likely to hang together with my students and therefore were involved with me, such as Diane Ruble, for example, as well as several other students. In any event--

The thing I'd like to insert here, or it seems appropriate here, more appropriate here than later, is comments about the field of psychology and where I think it's going, or where I think it's been, and comment on those early years, I split it up in many respects into decades, and I think in developmental the '50s to '60 decade was clearly one of lots of the classic experimental child research on the effect of language on discrimination learning, that sort of thing that we referred to earlier. That was wearing out at the end of the '60s, or the end of the '50s by, and in the beginning of the '60s or around that time some of these people were looking for other things to do, and, well, many were looking for other things to do. I think they'd gone as far as they could in many respects and spent maybe some more time than they should have at the end of that period. And there were two directions to go; one of them which I got very interested in, was the -- well, first I should say at Columbia I learned a lot about Skinnerian psychology from Fred Keller and Nat Schoenfeld, and that was--

Gelman: That was Columbia, all right.

Jeffrey: --and that was an interesting experience.

Gelman: Yeah.

Jeffrey: And Fred Keller was a marvelous fellow. I learned from the research approach popular there at the time. But the other thing -- there were two other things that I got interested in, in the back of my head at least. The first Harlow learning set study came out in probably '50, '51, and I remember puzzling about that a great deal, and sort of mulling that over in the years, and I kept that in the back of my mind as one of the problems to be concerned with sometime or other. And it certainly forced a different view of psychology than the classical Hullian view. So I felt one did need to come to terms with that, but I didn't do anything directly with that for a long time. The other thing that I found interesting was the theological work Lorenz and then, in this country, more specifically, Gig Levine at Stanford, and Victor Denenburg, who was then at Purdue, and also then, although this wasn't so relevant to some of the human issues, the -- oh, what is his name, the biologist at Rutgers, the--

Gelman: Lehrman , Danny Lehrman.

Jeffrey: --Lehrman, yes, Danny Lehrman's work--

Gelman: Instinct.

Jeffrey: --work on the ring dove and the inadequacy of instinct as an explanation for some of its behavior.

Gelman: Right.

Jeffrey: This forced all of us to reconsider some of our early dismissal of instincts and start looking at instincts at a different level than they'd been looked at before. And I got particularly interested then in ethology. That would have been at the time Marshall and Patricia, and there were a couple of other students who were very interested or got interested in that, indeed, you too, but--

Gelman: It took -- one of my exams was on ethology and early experience.

Jeffrey: --okay. Well, that was later, because we were just feeling our way, and I was feeling my way as much as anybody. But I remember an anecdote of some interest is that I announced that my seminar that year, whatever year it was, which would have been maybe '62, '63, was going to be on ethology. We'd looked at some of the work of Denenburg and Levine and others, and see what we could make of it. And Patricia Goldman, now Patricia Goldman Kie, came to me in my office one day very upset, and she wondered if I really had to teach this seminar, what could she do, she didn't want to take a seminar. She wasn't interested in animals. She particularly didn't like rats. And I suppose it should be mentioned she's now in the anatomy department at Yale, a member of the National Academy of Sciences, and was well known for her great work on the neuropsychology of the frontal lobes and their role in delayed reulat discrimination learning

Gelman: And its development.

Jeffrey: --and its development. Right. So, now that wasn't only -- well, I should add her dissertation was on rats, too. But of course, a lot of her training came later, so it was by accident that she had to go to New York with her then husband and find a job while he was at Columbia I think, maybe the College of Physicians and Surgeons, and so there she worked in the Museum of Natural History with Ethyl Tobach.

Gelman: Yeah.

Jeffrey: --I won't dredge up now. We can fill that in later. And then I don't know quite how she got to Washington and worked in the laboratory of a famous neurologist and neuroanatomist there whose name I also don't remember. So that's where her real life's work finally developed. But she had a good background for that. Now, that covers then that period where I went primarily in the direction of ecological studies and directing studies that way. The other thing that was happening, and where a lot of other people started looking for things, was in sort of a rebirth of interest in Piaget. I suppose there were a lot of people who had been interested in Piaget and had been teaching Piaget, but there were an awful lot of people, and particularly the experimental psychologists, who rejected his work out of hand. My only familiarity with Piaget was the critique of his work on children's the concept of causality by Deutsch, Jean Marquis Deutsch. It appeared to be a devastating critique, and that was enough to settle it for me that I didn't want to hear about Piaget again.

Gelman: That wasn't the best-prepared book he wrote.

Jeffrey: Well, I wouldn't say any of his books were terribly well prepared.

Gelman: --well written at any point, but the causality book has the problem of a kid getting assigned to a stage that they once passed precausal or whatever, and that doesn't happen in the number book, so in the sense of convincing it's a phenomenon, much easier to take on the causality book.

Jeffrey: Well, clearly. Anyway, I think a lot of people then, and mainly a lot of these experimental people, suddenly heard of Piaget, and that's how -- I don't quite know who was pushing it at that point, but suddenly -- so when people like me heard that his name coming up again, and maybe it was in respected circles that it was coming up. Oh! How I do remember hearing Bill Kesser, he was at Yale,

a development psychologist, talking about the work on conservation, and I hardly knew about that I'm sorry to confess. But like everybody else I suppose who was hearing about this at this point with my background, it just sounded ridiculous. We didn't like the idea of stages to begin with, and we didn't particularly like the way he presented his research on stages. So a lot of other people at that time were beginning to try to do training studies and do various sorts of things to disprove--

Gelman: What time is this in your memory?

Jeffrey: --well, see, I think that was starting around '62, '63, '64--

Gelman: Okay. So just about when I was starting graduate school?

Jeffrey: Yes.

Gelman: Yeah.

Jeffrey: And so there were these two directions that I think things were going at that time, which were very exciting in some ways. I found them daunting in the sense that I wasn't prepared. Well, I was better prepared for the biological direction, although critical of the old notions. There were clearly things here that had to be answered, and there was always potential that better controls needed to be done than a well-trained learning theorist or logical positivist might have a better idea of how to sort out some of these things than some of the biologists who had done the work, the early work. But in any event, there were these two directions that I think were becoming popular. I mentioned earlier that once one got into cognitive psychology and, looking at things more broadly, attention became a concept, and so part of then my interest still followed along that middle road of cognition, although we still weren't calling it cognition, but early perceptual learning indeed is what fit in best with biological stuff. And at that point I did get involved with Arthur Parmalee in pediatrics. I'm -- our earliest contacts -- well, I had been urged to talk to him by Don Lindsley, who knew him because of his affiliations in the medical school, and I did go down and have a chat with him on a couple of different occasions. And I'm not sure how that developed exactly, but he was doing some research that he wanted to talk to me about on the Moro reflex once, and I then got some students down to both observe that research and look at the movies he made and try to rate some aspects of the Moro response. And out of that developed some long discussions of research and observer reliability and things like that, which led to a much closer association, and finally it led to me lending to him - -that may not be quite the right word -- well, it was lending to him to begin with, the -- I had hired someone who hadn't quite received a PhD at Minnesota to help supervise student research here. Her name was Evelyn Stern. She was very good and very sophisticated in research methodology and statistics, and I lent her to help him with his project in the afternoons, but also to help us get some things started that he was a little leery about our working on in the nursery, which he had a right to be. Indeed, I spent quite a bit of time myself first learning what had to be done when you were working with neonates and learning the whole procedure of scrubbing, and putting on gowns, and what you could do and what you couldn't do, and again, there was a matter of also wooing nurses or convincing them you were a good guy, which I'd spent a lot of time doing that with nursery school teachers and--

Gelman: Right, getting cooperation?

Jeffrey: Yes. And so I had done a fair amount of that, I suppose, just observing what he did, and I don't remember that that was leading up to any specific research at that time, or else it was just helping him with some of his research. Then we did start, with Evelyn there helping him, then we could -- he was willing to let her help students and go along with them and do some of this. But at the same time I think I was always there for some of that early research we did. Both Les Cohen and Tamar Zelniker had just come in graduate school here. We were doing some perceptual -- well, not perceptual learning, I don't know what -- habituation studies on perception and perceptual discrimination studies, that's right, in a habituation framework. And I was always there during that

time, as was Stern. But finally, after six months or a year of that sort of thing I think we went down one day a week when it was clinic day. Yes, because we weren't using neonates then, we were using two, three, and four, and five month olds, so you had to go down on clinic day. So anyway, with that contact set up then Marshall Haith did his dissertation down there, and with Evelyn's help as well as Parmelee's. Parmelee was interested in our research, and by that time he was applying for a rather large research grant, which he got. He then took over part of Evelyn's salary, and finally wanted to take her whole salary, because he couldn't do without her, and I decided it was good for her. He could finally pay her more and give her a regular position, which I couldn't give her, and give her something more permanent than a training grant which has to be applied for every three years, and it seems like you're applying for it every year, because you're writing an annual report.

Gelman: With limits on how much you can put into it for assistants.

Jeffrey: Yes, right.

Gelman: --getting smaller and smaller.

Jeffrey: Mm-hmm. Well, at that time we were doing pretty well at expanding it, although I wasn't -- I think I did well, because I started out so low, and I was rather startled when I was on site visits for a couple of other plans at how much they got to start out with. In any event, that's how that work got started, and Les Cohen went on to do his dissertation down there, too. And now, as for changes that happened after that, '60s period was really a period of considerable ferment I think. And then what followed sort of was, well, the increased interest in language. Now, that period was starting during the '60s--

Gelman: Yes.

Jeffrey: --and the Chomsky influence on--

Gelman: Yeah, in fact, I had two minors; one was on ethology and one was in language acquisition. In the case of language acquisition, I could read everything.

Jeffrey: Yes.

Gelman: It had just started.

Jeffrey: And that would have been what year?

Gelman: --late spring of '65?

Jeffrey: Yeah. But anyway, it sort of started two or three years before that, so that was another part of the ferment at that point. And it was a real evolution I think, which allowed us to just really cut ties with the past almost completely, at least for those of us who could make that step; there are those who never did. And then as we got into the '70s -- again, using decades as easy markers to remember and remembering also that things are always going on for years. You could trace things back for a long, long time. But as far as general influence on me certainly, and I think reflecting what was going on in the field generally, that could really be called the new cognitive era where by then people had sort of made their peace with Piaget to some extent, and many of them willing to go on from there without trying to either criticize or defend him any longer, and write a new, or take a fundamentally new approach to the whole business of human development that I think could be truly be called cognitive now and with roots traced in a number of different directions.

I do want to add to that there's maybe one other possible shift that's happened along the way in the '70s-'80s. The shift in the '80s I would say is to -- well, an even broader consideration of an integration and attempts to integrate all of these different views, some of them wilder than others.

My own opinion is some of this is misguided, but I think that may reflect my age and the fact that one can only make a limited number of paradigm shifts in one's career, and that becomes a serious problem, how far you can really get from your original roots. And as I think I mentioned earlier, that had always been a problem for me, I could see that things weren't quite the way I thought they were, but was not very good at saying with conviction, "This is the way they really are," and I left that to other people, and when other people said it I often said, "Well, now wait a minute," and would try to show that the problems were different than they thought, not that they were necessarily wrong, but that it was more complicated.

I joined SRCD in 1956, and that would have been the year or the year after I came to UCLA. And the first meeting I went to was in Washington in 1959, and that was celebrating I think the 25th anniversary of the founding of SRCD. And I had that figured out once, and there's some notes here, but I don't know where it is right now. Anyway, it was rather interesting that -- actually, it wasn't in Washington, it was in Bethesda, Maryland. And they were a relatively small group there, because it was a relatively small society at that time. But I think one thing I found particularly interesting was that the founders of the Society were essentially -- Jerome Frank I take it was the big power on one hand, but the in group that did it was Robert Woodworth and Poffenberger from Columbia. Poffenberger was with NRC in Washington for a year or so at that time, and I don't know quite how this all came about, but of course, it was interesting to me since I'd been at Columbia and Bernice, my wife, and Woodworth's last teaching assistant, also did her thesis research with Poffenberger. And neither of them--

Gelman: --were developmental.

Jeffrey: No, they were both experimental and neither of them seemed like people that would be involved with the Society for Research on Child Development. But nevertheless, they were, and more than that, Robert Woodworth may have been the first or second president of SRCD. I happened to look -- the second president, and in 1933 Stoddard was first chair they're called, and that was temporary. That was a temporary group then. And the board is interesting, John Anderson, Frank Freeman, Arnold Gessel, Robert Lund Sapir, and Harold Stewart. Stewart is the only one I don't know. The meeting was interesting, partly because a lot of historical material was reviewed. But also there were only two or three young people there, it was interesting to see some of the older people, the names of whom I knew but hadn't ever met. But more than that, one of the youngest people was Gig Levine who was a new professor at Stanford or relatively new, and I knew of some of his work, because he was doing some of this early experience stuff, and we had a good time talking about what we thought was developmental psychology really should be. We also thought really that SRCD was more or less more moribund and something could be done about it, and probably should be done about it, not that we did anything right away. But the interesting thing is that, as we all know, a lot of things are going on of this nature. And let's see, the next meeting I went to was '61 at Penn State, and it was still small enough to be held at a place like Penn State, even though you could hardly get there, and there I met other people, people I knew, but there were other people now going at least to meetings, Fran Graham for one, Betty Caldwell, Dorothy Eichorn, whom I had met years before at a Western Psychological Association meetings, and I think I met Bill Kessen and George Mandler for the first time. I knew their names, but I hadn't met them before. In '63 the meeting was at Berkeley. Nancy Bailey was president, and Harold Stevenson had gone on the Governing Council that year, so he had clearly then -- I don't know when I first knew -- well, I do know, because I knew him certainly before I came to California, and I arranged a symposium at the WPA and invited him to come to present some of his research when he was at the University of Texas. Anyway, he was on the council that year. It was his first year on the council. And in looking at the makeup of the council, I'm sure he's the one who forced the reexamination of how the Society was being run. At that time it was being run by a Bill Martin, who was at Purdue, a developmental psychologist who'd been editor of the journal for I'm not sure how long now. I should have checked. But he was also business manager of the Society, and indeed, he and his wife were the Society. It ran out of their garage so to speak. And Harold thought this was not appropriate, things should be changed, and Martin should be replaced as editor at the very least, and more than that, I think, wanted more a professional management or a more organized, better organization of the Society. I was only hearing this from the outside, but I know there was all of

this upheaval and I did see Bill Martin was very upset by all of this. And anyway, by the next meeting, in 1965 which was in Minneapolis--

Gelman: Right, I went to that.

Jeffrey: --Martin had been replaced in the meantime with Robert Hess at the University of Chicago as editor of the Journal -- no, I'm not sure now. Maybe he was the business manager. I don't think he was editing it. I think Alberta Siegel may have started editing it at that time, had taken over for Bill Martin. In any event, presumably things were going forward on a new basis. I joined the board at that meeting and my first board meeting was in 1967. I should have added that Baldwin was chairman of the Society in '65. David Levy, a Chicago pediatrician was president in 1967 when I joined the board, and it was a terrible meeting in many respects. I didn't quite realize what I'd gotten into, because things were in a terrible fix at that point. The Society wasn't being run well; there were lots of problems, economic problems and so forth. I might add that it was an interesting group that was on the board at that time. Stevenson, myself, I think Fran Graham, and Eleanor Maccoby, Paul Mussen, Harold Stevenson, and Julius Richmond, who was a pediatrician, and things clearly had to be straightened out. We interviewed Hess and his secretary, who was doing most of the running of things, and the board was clearly not happy with how things were running, and so we arranged then -- and I'm not sure how and when this happened, but Margaret Kinney Harlow then was set up as business manager, and Alberta Siegel, who I have down here, was editor then. Maybe she took over as editor at that point and Hess had been editor. I'd have to check back on that. I'm not sure about that. But at any rate, the next meeting was in Chicago, or that meeting was in Chicago in '67 when this happened. At that point nobody even had a notion about where we might go for our next meeting, and that came up late in the meeting, because we'd spent almost all of the time of the Society meeting -- it wasn't just one day. We were in there three days I think meeting--

Gelman: I've had the impression that council met all the time through meetings.

Jeffrey: Well, it did then. It did then, at least. I thought that it was doing better recently, however.

Gelman: I think so.

Jeffrey: And, of course, now it meets yearly if I remember correctly, but then we only met biannually and started the date of the general meeting and hoped to finish before the end of the general meeting, which, as I remember, was three days. Given in the previous year there'd been a very successful meeting of the Western Psychological Association at the Miramar Hotel in Santa Monica, I suggested that maybe something could be arranged there, but I didn't think it was appropriate for me to do it, and I didn't want to do it, but I proposed that I could arrange for some Southern California people to do it. And tentatively I would say that it would get done, but I'd need to check with who I knew at Systems Development Corporation in the education area there. I thought I could persuade them to do it. And indeed, I did persuade them to do it. The only problem was that they didn't know quite what they needed. So I had to spend much more time in the management of that year's meeting than I intended or wanted to, but it worked all right--

Gelman: --because it was a great meeting.

Jeffrey: --except for the fact -- yes, it was a great meeting. But the fact was we had to guarantee the hotel 250 people or something like that, and I thought I wasn't sure we'd get 250 people out here. As a matter of fact, I remember Bill Kessen saying, "Santa Monica? Where is Santa Monica?" when it was proposed that SRCD to be held there. And so, yeah, I was not sure that we could assure them of 250 people. But we did guarantee that and got things going, and we had to have all the meeting rooms in the hotel. Well, as I say, it took finally more work from me than I had expected or anticipated, but it was a roaring success except for the fact that about 500 people came instead of 250, and many of them were very upset to be stuck in hotels other than the Miramar Hotel. And they were quite vociferous in expressing their displeasure at the business meeting at the end of the annual meeting. At that time

there wasn't another nice hotel around really. There were nothing but motels nearby, but that hotel was really quite nice. Well, at that meeting Richmond, Julius Richmond, was president and Stevenson was taking over as the next president. After that the next meeting was in '71. I think that would have been my last time on the board.

Gelman: Was that the Philadelphia meeting?

Jeffrey: No, no. I'm sorry. The Minneapolis meeting was my last time on the board.

Gelman: Right, okay.

Jeffrey: Right.

Gelman: And Philadelphia came after Santa Monica.

Jeffrey: Yes, and Margaret Harlow had died in the meantime, or at least was desperately ill. I'm not sure. And Dorothy Eichorn became executive officer at that point. And, oh boy, my notes are someplace else -- yes. Now, I have that straight. It was after the Chicago meeting in '67 that Alberta Siegel was replaced by Betty Caldwell as editor, and she was editor from '68-'71, which seems like a much shorter term than normal. I was surprised at that. I was appointed editor then in 1971 after that Minneapolis meeting, and from then on my only association with the administration of the Society was being on the editorial board through the 1977 meeting in New Orleans, I had finished being editor by then. And so that's the end of my association with any respect of SRCD's administration.

Gelman: There's an awful lot of people I know and trust who feel you were, if not the, at least one of the couple of best editors *Child Development* has had. Can you say something about how you got such a reputation? That is, what did you do to make it visible in a way that people really wanted to read it?

Jeffrey: Well, I think it's -- I'm glad to have some of the credit, but on the other hand, I think some of the growth was just the growth in the industry. There was more good research going on, and I was very concerned with the journal and making it a good journal, and still keeping its breadth in spite of my own specific interests, to keep it as broad as possible and recognize the need to make it more than just a psychology journal, and that wasn't always easy to do. But I think one of the things I certainly did was watch what was happening very carefully in terms of what was coming, how many were coming in, and what we could accept, and accept them accordingly, so that I could keep backlog down, and that made people fairly happy to first get a fairly quick response, which I also was very eager to provide after spending some years of waiting six, seven, even eight months once as I remember, to get a reply on a manuscript I'd submitted, and then after revision, having it take another year to publish. That was deadly. And it was getting deadlier because it was getting even harder by that time to get promotions without publications. So there were a lot of people in serious trouble if they didn't get their research published in a timely fashion. At the same time, I wasn't interested in publishing anything that didn't meet fairly high standards, although there again, I think that I made some compromises that I don't know that anyone ever complained about in terms of looser standards I did set for something that I thought was new and interesting and might lead to something else, and lead people to get started in that area if only to show this result was wrong. And I don't -- oh, the other -- I guess the thing was is that I did involve myself I think more than a lot of other editors have. I spent an awful lot of time and at first there really wasn't an article that went in the journal that I didn't read. And generally, when I'd increased the size of the board, when they came back to me with recommendations for publication I still looked them over, although not always real carefully. But some of them it was very clear that these were going to be fine, and there were others that I would worry about a bit that I don't know that I ever took any action except to make some editorial suggestions. I didn't ever turn anything down that somebody else had accepted. And I rode herd fairly carefully on the associate editors in terms of telling them if they were getting too much, or couldn't handle it with a fairly fast turnaround to let me know, and we'd get it done some other way. And I don't know, I

thought it was a good journal, and I hoped it would be a good journal, and certainly a lot of people have been very complimentary over the years, so I don't know what I can say other than that. But as you must remember, I spent a lot of time in that editorial office that I had.

Gelman: Oh yes.

Jeffrey: Now, I think we need to talk about my view of psychology, of developmental psychology, and to comment on the history of the field. Well, I have possibly a unique perspective on history of the field on one hand, but I do have one in mind, which comes from a lecture I gave and I developed over the years for the graduate core course. And when I went back to pull out some notes just to see what I had said and get some things fixed up, I noticed all the notes I had was the first time I did it history and nothing else, so I had it all in mind better then than I have now, but I will make a few comments about at least one man's view of the history of developmental psychology. And certainly one has to note that in talking about history it has to be filtered considerably. You can't give credit to everyone, and there's always someone who started or was doing something 10 or 15 years ahead of his time that didn't get recognition for it, or was little recognized, and others plugging along before something really became important and took over. So I, for the most part, have divided history into decade segments, and I know it isn't appropriate to apply Kuhn's word "paradigms" to what goes on in psychology, but I think it's still a decent word as long as people understand that I'm just talking about sort of general shifts in orientation in the field. And I'd also note that I am not and never have been in any position to say much about what's gone on in the social development or in personality development, although I think some of that parallels the scheme that I have of things. And for me, in most respects, developmental psychology starts in the '20s, and obviously there are those before the turn of the century who did some interesting things, and by the way, I've never thought G. Stanley Hall was one of those, but he was a great organizer and he did focus attention on child study and so forth. But he didn't seem to me to be a really critical figure otherwise in the history of developmental psychology. But in many respects it seems to me developmental psychology got its biggest boost with the development of the Stanford Binet, and the Binet Test first, of course, and then the Stanford Binet, which gave people an instrument to use. And the issue, there was an issue already extant, heredity, environment, nature, nurture, there were people obtaining norms which were worthwhile. On the other hand, there were those people who thought that intelligence was all hereditarily determined, so that's all you had to do to describe development. And there were others that thought otherwise, and of course, Watson was one of the most prominent of those. So the combination of the Stanford Binet and Watson and others challenging or pushing the nurture side of things, that really captured a lot of attention over the next two decades in a sense -- for '40, or late '30s, and there really came to a knockdown shootout at the end in the *Journal of Educational*--or the *National Yearbook of Educational Research*.

Gelman: Yeah, I remember that.

Jeffrey: --anyway, there were other things certainly on the fringe of that which were very interesting. Well, Watson himself did some classic experimental work with children, and as I mentioned earlier, that Iowa Child Welfare Research Station there was some very interesting work going on in concept formation and experimental work of various sorts. But what dominated the field at that point, it seems to me, was the nature/nurture issue. And with that, though, that was essentially moribund in the '40s. The war didn't help, of course, but there weren't a lot of new people going into developmental psychology. Levine is a good example. Well, he didn't come until after '40 in a sense, didn't start doing that work until in the '40s, so he was maybe part of the start of a new era.

Gelman: Where's Harlow, about the same time?

Jeffrey: Well no, Harlow's monkey research wasn't until the '50s, early '50s, yes. Well, mid-'50s possibly. So anyway, Levine was doing some experimental work. As I said, there were some things off in other directions, but there was the influence of Dollard and Miller, and there were a lot of younger people who sort of saw children as an interesting thing to work on, but from the standpoint of learning

theory for the most part. So that decade then of, well, the '40s was sort of a lost decade in many respects. The '50s then were taken up with experimental child psychology, and indeed, that's when that word first got used, and indeed, we didn't want to use the word child development or developmental psychology because all of its implications, and avoided that as best we could. Now, that decade was fizzling out in the late '50s as people -- the acquired equivalence paradigm had sort of run its course, and people didn't know quite where to go, and that's when they started looking -- oh, and I should add that there was the things like Harlow's research, the European ethological research, Levine and Denenberg doing their work on early experience here in the footsteps of Tinbergen and Hinde. And then, there was Piaget who, of course, had been around for a long, long time, but no one had paid any attention to him, I shouldn't say no one, but very few of us had. And we mentioned earlier the article, the very critical article by Deutsche in, I think it was in Barker, Kounin and Wright that devastating critique of Piaget's article on the concept of causality.

Gelman: Oh God, yes.

Jeffrey: Well, anyway, other people did start thinking about Piaget again, and I think it was mainly because of the popularization of the conservation studies. And--

Gelman: And also Flavell's book.

Jeffrey: --well, but -- no, it followed -- people were already getting into that, and that was sort of the impetus for Flavell's book. And as a matter of fact, he had done acquired equivalence stuff before that, verbal learning, discrimination sort of stuff. I really don't know the conservation stuff was first done in the states. In 1962 or '63 at a NSF summer conference I was surprised when Bill Kessen discussed Piaget's research more broadly than I knew of it from the Deutcshe account in B, K, & W.

Gelman: Well -- and others. It sort of straddles the beginning of the '60s.

Jeffrey: But I mean, when did Piaget first--

Gelman: Oh, ages ago. I mean, the number book is -- in 1941 was--

Jeffrey: --yeah. Well, that's my point. He had been floating along and doing these things. I don't know then who picked the -- who got this circulated in the United States. But it was circulating a long before researchers took up and subsequently refined some of his examples.

Gelman: --the runner at all book--

Jeffrey: That's true.

Gelman: --because that was '61, and had all those conservation studies.

Jeffrey: Yeah. So those -- right, and those were starting then back in the late '50s that that work was getting done. So then there was this sort of big shift, and I'm using the decades loosely, but by the '60s it was going full swing. But the focus was very broad then. This was laying out the basis for a really new approach to developmental psychology I think, and the first time we might start using developmental psychology again instead of experimental child psychology. Because we started looking at biological factors, and the approach to Piaget was, for most of us, to say, "This can't happen," and then to be slowly convinced that, well, although it can't happen, it's -- things are a little different than we wanted them to be from our own limited perspective or original perspective. So there was a reorganization of our view of children and of research in the '60s, which mirrored to some extent what was going on in psychology, too, as the classic learning theory stuff was wearying, and people were being more cognitive in general, using that term more frequently and, well, clearly, and that's interesting, too, because the influence there, the information processing stuff, which really came out of World War II to a considerable extent, but that was there in the '50s, but it was in the '60s that it

really influenced -- well, the mathematical modeling in the late '50s, and information theory attention, it all got its roots. Tanner and Swets and others did this work during following similar research during World War II.

Gelman: There -- it seems to me there was some -- there's a convergence that produced a broader interest in the work by Newell and Simon and others concerned with communication.

Jeffrey: Yes.

Gelman: --if a machine can think, well, then we can at least talk about thinking--

Jeffrey: Right.

Gelman: --a theory of structure instead of just like the Gestalts picking on examples of what didn't work.

Jeffrey: Right, right, right. Yes. And I didn't, I meant to include Chomsky in that--

Gelman: But you point that--

Jeffrey: --as part of that whole--

Gelman: There was something that was going on.

Jeffrey: --there was a real melting pot of ideas at that time. And that really forced a restructuring of our conceptualization of human development--

Gelman: That's a nice way to put it.

Jeffrey: --human behavior. So then we're into the '70s, or that's the time, I think that took the '70s to get it restructured to now we have a fairly sophisticated and much better integrated approach to all of these problems, which now recognize the biological predispositions that may be there, and the hereditary components of certain of these, the environmental possibilities, and in general an approach, well, a truly cognitive approach I think. And here, interestingly enough, I said that social development had followed to some extent, and obviously social cognition with the ultimate change in social development and social psychology. So that brings us -- am I '60s to '70s, '70s to '80s, and yes, that gets to be close enough to home that you see too many things and you can't pull a theme out, a new theme out now, but I think it's even greater maturity in what people are tackling. And moving on, again, I suspect that the '70s to '80s was a particularly rich time, and I think this may be a more confusing time, part -- and maybe that's only because we're in the midst of it and can't pull out a theme as easily. But I -- now restructuring how do we approach some of these problems. There are a lot of them that have sort of run out of gas I think, although we -- the language acquisition studies have, well, the focus there has changed. There are new arguments, some -- I was going to mention someplace along the line that that would have been in the '70s, '80s, the categorization literature, which I think was particularly interesting, and some of that I suppose is still going on.

Gelman: Oh yes.

Jeffrey: But--

Gelman: I think it's going to get -- what's happened is that it's getting, I think, especially focused in the sense that there's kind of a recognition of accumulative database that people have to deal with, and so it's harder to make the theory. You've got to be--

Jeffrey: Well--

Gelman: --it places much more precision--

Jeffrey: --that's right.

Gelman: --and I think it therefore is a little bit more chaotic, because it's much harder, especially given that universities are harder to be at. I mean, grants are harder to get, young people have all kinds of difficulties coping with the many demands of additional teaching or whatever.

Jeffrey: And children are harder to do research with, right, or to get to do research with.

Gelman: Right, with the increased difficulties.

Jeffrey: So anyway, I think I wouldn't say it isn't an exciting time yet, but it's certainly a difficult time to -- although we've come through difficult times before and managed, but it's certainly -- but we at least think much more money is required now to do research, but we might need to rethink just how we approach some of our research and, I think, still research can be done without necessarily having it all down on computers and so forth. Anyway, the final question is what are your hopes and fears for the future of the field, and my hopes are
-- be used to progress, but -- and I am not fearful that it won't. I think that the one thing I'm sorry we haven't done better and wish we could do better is to apply more of what we know to education and the practical in a practical way. And that's a serious problem. And indeed, I think that's where people working in social development also need to focus a lot of attention, and I'm sure some of them are on the problems of delinquency and the problems of society, which are pretty chaotic right now and the time when we're talking about a crime bill and whether we should have preventative measures in -- and when you don't really know how well many of these social measures will work, because we don't have data on them. We don't know what's best to do, except we feel fairly sure, or some of us feel at least that better family structure would help, but how do you promote that? So anyway, I think that sort of ends the story, and we did it--

Gelman: Well, at least for now.

Jeffrey: --yeah, for now. Okay.

Gelman: Thank you so much.