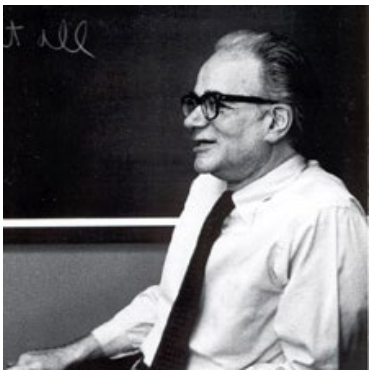
Harry Beilin



- Born 6/4/1921 in New York City; Died 1/11/2007
- B.A. (1946) Brooklyn College, M.A. (1949) and Ph.D. (1952), both from Columbia University
- Married to Iris Fodor

Major Employment:

- CUNY, 1968 – 1995, Professor of Developmental Psychology
- CUNY, 1995 – 2007, Professor Emeritus of Developmental Psychology

Major Areas of Work:

- Language learning, cognitive development, and the acquisition of mathematical understanding

SRCD ORAL HISTORY INTERVIEW

Harry Beilin

Interviewed by David Bearison
At The Graduate Center, City University of New York
April 27, May 16, May 30, June 15, July 11, and July 18, 2000

Bearison: It's April 27th, 2000 and I'm sitting with Harry Beilin, and we're going to begin, an Oral History for SRCD. So being a good developmental psychologist, I want to start at the beginning, so I wonder if you could tell us, what kind of a family were you born into? What was it like for you?

Beilin: Right. Well, first of all I was born in 1921. June 4th, 1921. My parents were both from Minsk, Russia, at least it was Russia then, it's now Belarus. They were both working-class people. They both came to this country, at least my father came first, and then my mother afterwards, they were not married at the time. They knew each other, they were friends, but they actually hooked up after she arrived here.

Politically they were both socialists, -- at least in Russia. I guess one might call my mother a social Democrat more precisely, that reflected her politics. They were rabidly – at least my mother was, rabidly anti-Communist, at least that was the flavor within my family. The family (my uncles) more or less divided between those who were sympathetic with the Bolshevik Regime and those who were hostile to it, and they had –

Bearison: So what year are we talking about, the revolution was in 1917, right?

Beilin: Right.

Bearison: What year are we talking about now when they decide to immigrate?

Beilin: Well, I don't remember exactly the year my father came here, but I think he came before the revolution. My mother came, I think, before as well, although I can't be precise about that. I know that they were very much harassed back in those days, at least in Czarist Russia. I know my mother went to some demonstrations and she, in fact, got nicked by a bullet from, I guess it was from the Czarist troops, whatever. In any case, they decided to get out both independently.

Bearison: The one thing I don't understand is, I mean if she's anti-Communist you'd think she would have been like, you know, the white Russian, you know, so why was she harassed, was it because she was Jewish or --?

Beilin: Yes. Basically it was a matter of pogroms. I don't think it was because of political decisions, although they could have been. From her description of it, they had to be very surreptitious about their political activities going to meetings late at night and all of that.

In any case, in this country my father worked in a shoe factory. I'm not quite sure what he did; it was probably some semi-skilled job.

Bearison: And where was it?

Beilin: I guess somewhere in New York. My mother had a job most of her working life as a finisher, so called, in a clothing factory; worked most on women's dresses, I think, basically doing sewing. That was at least up until the time of the Depression. The Depression hit us very hard. Oh, incidentally, and not so incidentally, my father died when I was five years old.

Bearison: Wow!

Beilin: And my mother was then on her own, very much on her own and she had a very hard time of it.

Bearison: But when you were born were there siblings that you had in the house?

Beilin: There was actually an infant that preceded me that died, I think, in his first year, so – and from then on it was just my mother and myself, so I had no brothers and no sisters.

Bearison: And what caused your father's death?

Beilin: From what I read now in his Death Certificate (the cause is slipping my mind for the moment), it's a disease that's now capable of being controlled. It was Hepatitis, that's what it was. I'm not sure what type, but I don't think it specified that.

Bearison: Do you have any memories of that time and what that was like?

Beilin: Yes. Actually, since I was about five years of age I do have some vivid visual memories of my father, who by that time was somewhat of an invalid. It was very clear that he was not well at all, so I do retain some memory of him, definitely.

Bearison: And the memory of the loss, do you remember him dying and what that was like?

Beilin: It's hard for me to access my reaction at that time, but there's no question that over my lifespan the loss of my father was a very important influence on my life.

Bearison: Well, I want to come back to that as we go through your lifespan, so let's just stay now at you're a five year old, you've lost your father, your mother is struggling, the Depression had not come yet, but it's about to come. Is that where we're at?

Beilin: Yes. Because after all I was born in '21, the Depression came '29-'30, so I had a chance to grow up, but we were still very poor. My mother had bought, I don't know where she got the money, but she bought a newsstand. And we lived in East Harlem at the time, on 105th Street between Madison Avenue and Fifth Avenue, and it was a tenement district of lower class, definitely lower-class neighborhood.

Bearison: Was it a Jewish ethnic neighborhood?

Beilin: It was a mixed neighborhood. It was Jewish, Italian, some Irish, I think at that time.

Bearison: Did your folks have an extended family here that they could turn to? Did you mother have sisters or brothers that could help her out?

Beilin: Yes. Well, it was mostly my father's family. My grandmother actually lived in the next building to us, but then unfortunately she died a year after my father did. Afterwards we moved to the Bronx, and then there was quite a bit of family around. My uncles lived in the Bronx, I had two uncles and I had cousins, so there was a family context in which I grew up. And it turns out the most important influence of my life was really one of my uncles. He was very fortunate. In Russia he was one of the rare Jews who obtained a university education as an agricultural engineer. He married –

Bearison: What was his name?

Beilin: His name was Elias Beilin. He married a Russian woman that was Christian, big scandal and all of that, and he participated in the revolution. I don't think he was a Bolshevik, in fact, I think he was – I'm not quite sure what his politics were but he definitely was not a Bolshevik, that is, a Communist. He was recruited by the Bolshevik's because of his engineering background and his professional competence, so he became the local commissar of public works or something of this sort in Saratov, which is where he got his education and where he married and where he was living. He saw the writing on the wall after a few years and got out with his wife and came to the States, and he did very well in this country for a few years until the Depression came. He was the only highly educated member of the family, and he took an interest in me, not a great interest, but sufficient to develop a really close relationship with him. And I think that he played an important role in my life in showing me that there was a place where one could go and achieve and the like.

Bearison: Well, when you say 'took an interest,' could you tell me more about it, such as, did he talk to you about your school work, telling you the importance of an education, how to make a buck in this city. What did he do? How did he show an interest in you?

Beilin: Well, it's hard for me to recall the details. I know he was keenly aware, or at least he tried to access what my intellectual capacities were, because I know he gave me puzzles to work out. And I think he was very disappointed in me because I really wasn't very good at those puzzles.

Bearison: What were the puzzles like? Do you remember them?

Beilin: They were like these little cubes that you had to take apart and then put back together again, little wooden cubes with various cutout parts.

Bearison: Sure.

Beilin: I didn't do very well at that, so I think he wrote me off as being potential for an engineering career. And I know when the Depression came and my mother lost her job, we had to go on Welfare for a while and I think the Welfare agencies wanted also to access my capacity, so I had to go up to City College Testing Bureau and they gave me an intelligence test.

Bearison: And how old were you then?

Beilin: I can't remember exactly.

Bearison: Well, roughly.

Beilin: I must have been about maybe ten, twelve years old, so – I remember the results came out that it showed that I wasn't exactly brilliant, but I wasn't exactly a failure either, so I was in a dubious category where one couldn't know where I was going to go, how much I was likely to achieve in life. But nevertheless, strangely those memories stick out in my mind, those experiences at least had a profound affect in making me aware that I had to be conscious of my intellectual capacities, and that they had to be taken into account somehow in my future life.

There was another set of circumstances in my youth that were very important, and they relate to school. My mother went to work; I was really left on my own. I was a little kid, but I was very adaptable. We grew up in a pretty tough neighborhood, especially in the Bronx. It wasn't tough in the ways that it's tough these days, but there was still plenty of tension, especially between the Jewish kids and the Irish kids and the Italian kids, although I had friends

among all of those groups. But what happened was I identified very much with school. I had a crush on my kindergarten teacher. I used to wait for her after school and I'd walk her to her train station. She would invite me to stop and have an ice cream soda with her, and the like.

Bearison: What was her name, do you remember?

Beilin: Yes, I do, interestingly enough. Her name was Ms. Koppner. And by the time I was eight years old I went on a school trip to the Metropolitan Museum of Art. That really hooked me. We went to some kind of lecture, I don't know exactly what it was about, but they had a series for school kids every Saturday morning, and I attended religiously, I attended for the whole semester. I would travel down from the Bronx on the subway by myself at eight years old, and I explored the museum at that time, and museum going got to be part of my habitual way of entertaining myself.
The museum was also located in a very, what to me at that time was an unbelievably posh area.

Bearison: It still is.

Beilin: It still is, but from the prospective of Depression years it was even more so. And I got to see what the other side of the social scale was like, and of course, again it had a profound affect on me. It was an inaccessible world at that time, but it must have generated some kind of desire to be socially mobile, for sure. In any case –

Bearison: So the affect was sort of like, 'that's where I want to be,' you know, it's like, 'I want that.'

Beilin: I never enunciated it in those terms. I mean it was so incomprehensible to me to even approach that kind of world. I never had the resources to even be able to move in those kinds of circumstances. And that was impressed upon me more and more as I became an adolescent and I realized the distance between them and us.

Bearison: But did you also realize at that time or into adolescence that some of those people were Jews, so it's not just that being a Jew that was totally inaccessible to you, but that even Jews could become, you know, live like that on Fifth Avenue. Were you aware of that?

Beilin: Well, I don't think I was conscious of that aspect of it, at least from my perspective now. I'm sure I realized there were plenty of wealthy Jews around. It wasn't a Jewish thing in those terms, except as I began to grow and get older and develop, I felt increasingly alienated from my Jewish background. In a way I got to feel ashamed of my mother who spoke English not very well. She was literate, she read, but there was a considerable distance that developed between us, and I more and more tried not to identify with my Jewish background. And that manifested itself increasingly as I got older, until I got to be an adult and then that whole situation reversed itself.

Bearison: Your mother never remarried while you were growing up?

Beilin: No, she didn't. She had the opportunity to do so, but she always said that she didn't do it because of me, didn't want me to have a stepfather. From some of the choices she had, I think it was a good decision. There was a Rabbi who wanted to marry her, and heaven help me if I ever had a stepfather that was a Rabbi. My life would have been very different. But my mother was, I wouldn't say reasonably social; she had boyfriends, so she had a life that she managed, even though it was an extraordinarily difficult one for her. Luckily she lived to be eighty-seven years old. Oh, she did remarry later, I almost forgot about that. After I got married, the first time, she got married. It was as though it was safe now for her to get married. She married a cousin, interestingly enough, and a complicated story, but she divorced him. They went on a trip down to Florida, he kept insisting that they go to Florida for the winter and she refused at first. And then finally she agreed and she discovered a whole community of people from Minsk from her old Russian background, a little colony down on south Miami Beach, what is now referred to as South Beach, a real trendy area in Miami Beach. In any case, she discovered this community. She told her husband, "You go back to New York. I'm staying here." Well, just imagine the repercussions from that. In any case, she got divorced and then after a couple of years, she wanted to get married again. I tried to discourage her from doing that.

Bearison: How old was she about now?

Beilin: She was in her seventies.

Bearison: Oh, really!

Beilin: Yes. She was very active. She went dancing almost every night. She was leading a good life from her point of view. I tried to discourage her from getting married again, but she said it was shameful to live with somebody and not get married, so she got married and that turned out to be a disaster. It was good for a while, but then he began to get senile and their married life really began to fall apart, and then she just left him.

Bearison: I want to bring you back to going to the Museum. Would you say, you know, you're eight years old and you're going there, would you say then the first sort of – the beginning of your intellect was centered around art and the things you see in museums? Is that how you first began to stir your intellect in areas of things that --?

Beilin: Well, no my interest was more general. I became an avid reader early on, and I read a great deal. And I think that my intellectual interests began to be pretty broad from a relatively early age. I can't –

Bearison: These are books that you would get from the libraries?

Beilin: Yes. I was a habitué of the library. We had a local library in the Bronx that was just wonderful.

Bearison: And were there any particular kinds of books that you favored during this time?

Beilin: Well, at first it was mostly fiction and then when I got to college then I started to read the serious classics.

Bearison: Well, what kind of a student were you in these days, I mean prior to college, in grade school and even in high school, what were you considered?

Beilin: I was a passable student, high passable. Not –

Bearison: C's and B's we're talking.

Beilin: Right. That's about it.

Bearison: Were there any particular subjects that you excelled in more than others?

Beilin: Yes. Oh, yes! I began to develop an interest, actually again at an early age, in science, particularly biological science. Not only was I going to the museums, but I began to go to the zoo, the Bronx Zoo. And I got interested in herpetology, I got interested in all of the biological divisions, and that became a more abiding kind of interest, almost then overriding the artistic.

Bearison: Were these sort of solitary pursuits, or did you share these with friends and other people that viewed that kind of intellectual activity?

Beilin: It was both. It was more solitary than social, although I had a pretty active social life as a kid. There were lots of kids on the block to play with. I wasn't a good athlete, but nevertheless I played handball and stickball with all the other kids, and I was never the first one to be chosen for the team, but nevertheless I participated. And I developed and I joined the Boys Scouts. I did a lot of hiking, and I had a pretty active social life through them, with them. And by the time I got to high school, I then developed a core group of friends, two of whom, in fact, are still my friends. We've been friends all these years.

The biological interest became more focused as I got into high school as well. I was really very shy, but I aggressively tried to overcome my shyness and a good measure of that is I became the president of the Biology Club. I spent – the solitary aspect of my activities too all through my life, reflected in the fact – and also the interesting commitment to school. I was very lucky. I got into Stuyvesant High School, the science high school.

Bearison: Yes. It was probably the finest high school in the city at the time.

Beilin: That's right. Especially at that time, it had no competition.

Bearison: So how were you able to do this? You said you were a mediocre student getting C's and B's, and you didn't test that great even when you were younger. How did you get into Stuyvesant?

Beilin: Well, because I was good enough, maybe I'm putting myself down a little bit, but I was always comparing myself to the best of the kids. So even among my own little group, the three of us – I had this one friend who was extraordinarily bright and gifted. I had the other friend and I compared myself to him, but I knew that I was better than he was, and we all went to Stuyvesant together. We had a morning session at Stuyvesant. I used to spend all my afternoons in the biology laboratory, and I began to – at first I was interested mostly in the animals that they had there, and I developed a personal collection of animals. I had, much to my mother's distain and aggravation and anxiety, I had snakes, I had rats, I had mice, and I had alligators, all at home.

Bearison: All at the same time, all of them?

Beilin: No, not all of them.

Bearison: And what was the interest with these animals, I mean was it just to observe them or was it -- did you try experiments with them?

Beilin: Well, I did actually. I tried dissecting frogs. I got the landlord of my building to allow me to work in the basement, he liked me, and I set up a little laboratory. So I would dissect, after the animals died, or for one reason or another, I would dissect them. Now whether I really learned anything from it, I don't know. I took a more increasingly sophisticated interest in some of the activities.

Also in high school, I went once with a group to visit a laboratory at one of the universities, and we went to one of the advanced laboratories in genetics, and part of the demonstration was showing how they were working with Drosophilae, the fruit flies at the time in this lab. I think it was Morgan's laboratory; I'm not sure any more. Thomas Hunt Morgan. And part of the demonstration was taking the salivary glands out of the larvae of the fruit fly and then processing them, fixing them, staining them and then showing the structure, essentially the cell structure, especially in the nucleus it showed all of the chromosomes laid out. I got really fired up by that.

And then in the biology laboratory at school I started to breed fruit flies, got the larvae, dissected out the salivary glands and went through the process of fixing them. I never succeeded in doing a proper job of –

Bearison: But they even had the equipment for you to try –

Beilin: No. I tried to develop the equipment, interestingly enough, because they had some really sophisticated equipment in their lab. One of the things they had was a micromanipulator where you could take a needle and inject it into the cell. Well, I couldn't afford a micromanipulator, so I tried to build one. One of the reasons that I thought I had the skills to do it was because at Stuyvesant I had a shop in metalworking and I could make screws and I could make knobs and all of that kind of stuff. And I also had a class in glass blowing, so I knew how to make these tubes, fine tubes, so I tried to put all that technology that I had acquired together in this particular project.

Bearison: But to me this sounds like, even in today's standards, were pretty exceptional kind of activity. But what was that like, was everybody at Stuyvesant doing something incredible like this or were you really beginning to shine amongst the students, that you would be engaging in this kind of activity?

Beilin: Stuyvesant had an incredible number of kids who were doing comparable things, so they were doing it in the physics lab; I was doing it in a biology lab.

Bearison: So you're just another one of the guy's there doing – or girls, doing --?

Beilin: Yes. I didn't consider myself exceptional.

Bearison: I want to go back, you know, and sort of looking at the chronology of your life, you know, you're now in high school, but I just want to review that aside from your father dying when you were five, were there any other sort of major transformative experiences or traumas that might have affected your growing up? Were you ever hospitalized for anything, or did you have any illnesses that are notable, any notable events that might have made your childhood somewhat out of the ordinary?

Beilin: Well, there were a few traumatic events because I got sick early on and I got separated from my mother. They sent me to a convalescent home for kids in Coney Island. That was a very traumatic experience for me being separated from my mother.

Bearison: And how old were you?

Beilin: You know I've tried to estimate when it was, but I probably was about seven.

Bearison: And do you remember, what was the illness that you had?

Beilin: It was respiratory, but I don't know what it was now. Interestingly enough, I've had recent problems with my lungs, and I don't doubt that there's been a long history of difficulties. Then my mother had a series of –

Bearison: How long were you separated then, do you remember?

Beilin: It must have been at least a month. I remember it was not an unpleasant place, lots of nice kids, although I don't remember any particular kids, and we were on the ocean in Coney Island, but the separation from my mother was very painful. And then my mother got ill in various times, she had to have some serious operations, and then I got separated from her again and I was taken up by my relatives, lived with one or another relative for some time until she recovered. So those were traumatic events, and I guess I have always suffered from the effects of separations and that kind of emotional deprivation. My mother was a very nurturing woman, making me somewhat over dependent, at least attempting to. She tried to control my life a lot, and I rebelled from a very early age, and it was easy because I was on my own a lot. She'd go off to work, I'd go to school; I'd come back. My uncle was very concerned, as well as my mother, that I would be alone after school hanging around in the neighborhood and it was a pretty rough neighborhood. So they enrolled me in a Jewish school, so after school at the regular school I would go for instruction in Jewish culture and Jewish language. It was run by a group of socialists called the Scholem Aleichim Schools.

Bearison: Was this also to prepare you for bar mitzvah also?

Beilin: Well, I refused to be bar mitzvahed, even early on. Our family was very non-religious. My mother was non-religious, and with all the socialists and communists in the family, certainly were almost anti-religious, so I refused to be bar mitzvahed, although there was never any real pressure for me to be bar mitzvahed.

So the effect of these – well, we're talking about my independence, I got to be really independent because I had to be on my own a lot until my mother got back from work really quite late at night, well, not really late at night, but seven o'clock or whatever. And I'd have to come home, go to this Jewish school and come home again, do my homework, go out to play, and I was on my own a lot. So I had to prepare, there was usually some food leftover, or left for me, so I would take my lunch myself and then wait until my mother got home and she would prepare dinner at night.

Bearison: But did you feel, I mean was this a good life for you or did you feel deprived that you were left alone?

Beilin: It was an incredibly painful life.

Bearison: Really!

Beilin: It was a very unhappy life. First we suffered from the effects of poverty. There was many a time when we did not have very much to eat.

Bearison: Really!

Beilin: I remember one very painful occasion, it was a Saturday, I wanted to go to the movies, and the movies then cost ten cents maybe, and my mother didn't have the money to give me, and I was totally irrational about it and we had an enormous fight about it. But my life at that time was spent, enormous periods of boredom, and the attempt, you know, fighting off the boredom, of being alone, not just the poverty but I guess the aloneness because up until the time of high school I never really had lots of friends. I mean there were local kids, you played in the afternoon and that was it, not any close friendships. When I got to high school then I began to develop some close friendships.

Bearison: We said also, I guess because it was Stuyvesant, there was a way to get out of the ghetto and begin mixing with different classes of people, so was that an influence on you?

Beilin: Well, I never felt school was a mixing of classes, because it came as a surprise to me actually at the time of graduation, to realize that there was a class structure.

Bearison: Really!

Beilin: I went up to some of my friends, the people I had become acquainted with at school, and I asked them "Oh, where are you going to college?" The replies kept coming back, "Oh, I'm going to Harvard. I'm going to Yale." And I thought to myself, most of us are going to the city colleges, and I realized that these kids were coming from a different world.

Bearison: I'm surprised though, if you never talked about it, they never looked, the way they appeared, the way they dressed, you never went to some of their homes and saw the different types of worlds they lived in?

Beilin: No. Never. It began to be impressed upon me the social class differences towards the end of high school when I started dating. I started going out with girls. I went on a trip, and the kids from Stuyvesant, which is an all boys' school, went with a group of girls from a girl's high school on some trip. And I got infatuated by one of the girls; she was a gorgeous, beautiful looking girl, personable, charming beyond belief. So I found out what her name was and got her telephone number, called her, took her out. It turned out that she was -- I guess it was called an Upper East Side girl. She was really upper middle-class, not only that, but she had all of the style of a person from that class. I felt totally devastated actually on this date, and the social class differences were apparent at every turn. And I had hardly any money in my pocket to even – I think I had just about enough so that we could both have an ice cream soda together, so the social differences were brought home to me very painfully to me in that time and that context.

Bearison: Where were some examples of these social class differences that struck you so dramatically?

Beilin: The dress, also the confidence, the self-confidence that a girl like this had, and also the --

Bearison: Did she sort of convey the message that, you know, you were beneath her, this was a mistake to ever go out with you, you'll never amount to rise to her class?

Beilin: Yes, you could say that. Yes. I knew well enough not even to try. I can't remember if I tried to call her again, but for sure there was no way that I could compete in that world. And it might work out well in some movies, but in the script that I was living by, the separation was pretty clear. But what it did, I guess what it did, the lesson I learned, however, from it was that despite the rejection which I felt, there was – I did not consider anything to be a barrier to my attempting to conquer it or approach it or whatever, so I never hesitated after that. I never generalized so that I, you know, backed off and said, "This is not my world. I'm sticking with my own kind," quite the contrary. What I did was, I increasingly began to sharpen up my social skills. When I went to college, was admitted to Brooklyn College I took very seriously the speech courses. One of the reasons was, another traumatic event of my life, one of our classes at Stuyvesant was invited by the city radio station to put on some kind of play or performance. And we went to rehearse at this radio station and I had some little part to speak, and after I did the director called me aside and he said something to the effect that, "I should try to change the tone of my voice or the

manner of my speaking," because he told me in effect that I sounded lower class. He didn't exactly say that, of course –

Bearison: But the message was clear.

Beilin: -- I knew exactly what he was saying. I sounded too much like the Bronx, too Jewish; God knows what, and that made as such, if I wasn't self-conscious up to that point, I certainly was from then on. So I very consciously, when I got to college and had the opportunity in my speech classes to alter my speech, or at least attempt to make it more like standard American speech.

Bearison: What was your choice to go to Brooklyn College? How did that come about? Did you consider other places or was it always, that's the city --?

Beilin: No, I considered other places. In one way, I made some irrational choices. I decided that I wanted to become – I had a number of options, at least I thought I had a number of options. I wanted to become a biologist, but I also as part of my experience in high school in the glass-blowing class, I got intrigued by glass, so I decided that maybe I would like to become a glass engineer. So I applied to the state school of glass technology, it's a part of Cornell University; I forget one of its divisions, although it was a state school. But I got turned down, so that cut off that avenue. I knew I could not afford to go to college, a paying college, so I ended up deciding to go to one of the city colleges, that was the only choice open to us.

Bearison: But even the idea of going to college, getting a college education, was that something that your mother and your uncle were supportive of, or saying, "Hey, Harry, it's time to go out and get a job, bring some money home." How did they face your ambition to get a college education?

Beilin: My mother was not very happy about it, especially when she heard what I wanted to do, what I was aspiring for. I told her I wanted to be a biologist, and she said, "Why don't you learn a trade?" And then she kept reiterating that.

Bearison: What does that mean, ' learn a trade,' means you don't go to college but you apprentice out somewhere and learn like be a garment tailor or something? What does she mean by learn a trade?

Beilin: I'm not quite sure how clear a picture my mother had in mind, and what – become a mechanic of some kind, a trade in the sense of vocational school trades.

Bearison: I see.

Beilin: But she never, even though it meant I could not become a wage earner immediately, she put up with it. Not only that, we were on Welfare at that time. Interesting thing is, the Welfare agencies, I don't know which agency it was, the City Welfare Department, did not put any strong pressure on me to quit. They more or less accepted the fact that I could go to college and the family still remain on Welfare. I'll always be eternally grateful to the City for that. It wasn't until later, however, when the War started that things got very tight, and then they forced me to quit.

Bearison: But even with all your poverty of high school and even the last years of middle school and high school and college, you never had to hold down jobs to bring money into the family?

Beilin: Oh, yes. I worked every occasion that I could.

Bearison: Oh, okay.

Beilin: I did desperate things. When I was young I tried shining shoes. I did it once, the guy told me, my first and last customer told me I knew nothing about shining shoes and that I better give up, which I did. I then tried some independent entrepreneurship by going into the newspaper business. I sold newspapers on Saturday night, mostly the Daily News. The Sunday – I would buy the papers on Saturday night, for the Sunday Edition, early Sunday Edition, and then try to sell them within a two-hour period before it got too late. Well, I did that for a few weeks and I made a pittance at it, so I gave that up. Then I got more traditional kinds of jobs. Actually, through the high school

employment office, I got a job delivering brooms and sanitary supplies, and I worked a half day, worked after school and then when it didn't interfere, I guess by that time, maybe I hadn't started my biology labs in the afternoon. But I would work in the afternoons and then on Saturdays, and in the summer I worked full time. I got four dollars a week in school time, eight dollars a week full-time, that was forty-four hours a week, and it was hard work. After, I quit that job to take on a job delivering paper. I worked for a paper supply house. That was another backbreaking job.

Bearison: How would you deliver paper, you weren't driving a truck or something were you?

Beilin: No, I had to carry it.

Bearison: Really! What do you mean, on your back?

Beilin: Go on a bus, heavy packages, heavy bags, sometimes cartons folded up. That was really hard work, but I was young, strong actually, and I was also adventurous. Through a friend I found out that I could get a job aboard ship, so during the Christmas vacation I got a job on the S.S. Kungsholm, working in the – the first job I had, I did it twice. In the first job I was a third-class pastry chef, so called. What it was was carrying sacks of flour or sugar from the hold up to the working area, chefs working area. I would also hand-whip cream and did all kinds of things ancillary to the job of baking. That was also very hard work. Then I found out what the good jobs were, and the second time I went was during the summer.

Bearison: This is still in high school?

Beilin: Yes. Well, the first was while I was still in high school, the second I was a freshman in college. I got a job as a night potato peeler, and I had to peel potatoes at night. I wasn't a big success at that because the chef discovered that I put the potatoes into this machine that would scrape off the jackets, and I would leave them in there long enough so that I didn't have to take out the eyes, so they were very unhappy with me. But it got me traveling, so I traveled – this was in 1939 before the War, or the War was just started, but we were not involved with it, so I traveled to the Caribbean and Central America, long before those kinds of activities became popular. So I had worked hard as a kid, didn't make much money, but nevertheless knew that that was not what I wanted to do the rest of my life for sure.

Bearison: I want to ask you to sort of take me through your Brooklyn College years, and then we're going to stop and take a break, you know, and resume some other day.

Beilin: At Brooklyn College I enrolled as a biologist, biology was going to be my major. Now I have no clear idea of what I would do as a biologist. My aspiration tended to be on a "realistic" level. I knew I wasn't going to be a biology professor, for sure, what could I do? Well, I knew that there were jobs in the Civil Service for biological lab technicians, so that became my aspiration. I would get a degree and compete in a Civil Service exam and get a job as a lab technician.

Bearison: But this is the same guy who at Stuyvesant was trying to replicate the genetic micro experiments on fruit flies. It sounded like you were out to get the Nobel Prize, now what you say is, "Hey, all I'll be is a technician. It doesn't match. What happened?

Beilin: Well, reality. I wasn't going to become a research professor. That had been, you know, a joyful activity, an interesting activity, but it didn't go anywhere. I didn't even end up successfully having a set of slides of chromosomes. So I became a science major and I took all of the tough courses. I took courses in physics and chemistry and the biology courses, all long laboratory classes. In retrospect I wasted enormous amount of my college years that turned out to be of almost no value to me. I wouldn't say no value, but not enough value to justify all the time that I put in, and it took me away from other things.

Bearison: You never thought of biology as a way into medicine, which would --?

Beilin: No, I never – from an early age I discounted the idea of medicine. I guess there were two reasons. One was the bias against Jews in going into medical schools, and also I wasn't sure that I would have the grades for the – So my Brooklyn College years were very hard working years. I also had to work in the afternoons or weekends, and we lived in the Bronx at first and I commuted to Brooklyn, so I would spend an inordinate amount of time traveling, and

then we moved to Brooklyn near the college and that made life a little easier. And I began to go out with a girl in my biology class and that went on for a while.

Towards the end, well I guess I was in my last year, or the third year going on to the fourth year, when the Welfare Department finally said – the War had started, we were in the War, they finally said, "You have to go into a training program, a trade of some kind," so finally my mother succeeded getting me into a trade. So I had to quit school –

Bearison: But Brooklyn College was free, so they basically said if you didn't do this we weren't going to give to your mother to live with, is that the --?

Beilin: No, they wanted me to go out and get a job, to work so that she would no longer be on Welfare.

Bearison: I see, so they wanted to basically take your mother off of Welfare and say, "Okay, let your son give you the money." Okay. Got you.

Beilin: They thought, first of all, see by that time there were many more jobs around, so one of the reasons I think that they let me stay on in school, what was the point in putting me out into the labor force unemployed, I might as well be getting an education.

Bearison: So this was like in the end of your third year, you know, they told you to drop out and get a trade, don't go back for your final year?

Beilin: And I went. It was unbelievably stupid, because what I did was go to a vocational high school at night, they gave me courses that I had taken at Stuyvesant, metal turning, wood working, whatever. At that point also, they arranged after I had taken the course, they arranged for me to have a job in the Board of Health as a laboratory technician, interestingly enough, so I worked in the Gonorrhea Control laboratory, sitting at a microscope diagnosing slides as to whether they showed the presence of Gonorrhea or the absence. And I did that for maybe a year, maybe a year and a half, and I was being paid a pittance. My girlfriend at the time, the one I had been in school with, the biology class with, she graduated the time I should have graduated, and she got a job working for the Navy inspecting electrical equipment at the Western Electrical Company, Kearny Division. She said, "Look, you can make twice as much money if not more by getting a job inspecting or something like that, why not do it? There's no point in working in a biology lab." By that time I realized my aspiration to become a biology laboratory technician was crazy because the idea of sitting behind a microscope for eight hours a day just was something that I couldn't tolerate.

I went to the Western Electric Company and I took a number of tests, and on the basis of those tests they offered me a job, and they assigned me to work – they gave me a fancy title, "Quality Control Engineer." It was really a quality control inspector, and in the wire division. I was inspecting wire, cable, and so-called factory cabling, and I began to work and I made a lot more money, especially for those times. At first I had a deferment from Service on the basis of my mothers dependence upon me, and I worked there for about a year and a half until I finally got called up, and by that point then I went into the Navy.

Bearison: Let's stop here then. This is a good place to stop.

[Continuation on May 16, 2000]

Bearison: This is Tuesday, May 16th, and this is preparing for the second meeting with Harry Beilin for the SRCD Oral History Project. As I recall, the last meeting we left off about you going into the Army or Navy?

Beilin: The Navy. Yes.

Bearison: So it looks like, we'll call this volume, *The War Years.*

Beilin: Right. I went into the Navy at first in the C.B.'s, so called the construction battalions, not by choice but by assignment.

Bearison: But you did choose the Navy as a service?

Beilin: Yes, I did.

Bearison: And what led to that? Why did you choose the Navy?

Beilin: Well, at the time of my induction the assigning officers simply asked, "Do you have any objections to the Navy," and the fact is I didn't. I had, in fact, earlier on inquired about going into the Naval Aviator Program, but I discovered that my eyesight wasn't good enough, so I gave up that idea. But the Navy always appealed to me for some reason.

Bearison: So what year are we speaking about?

Beilin: Well, that was I think, of 1942 or '43. I think I went in in '43 and came out in '46.

Bearison: And so what was the status of the War in '43?

Beilin: It was still going on everywhere. I didn't stay in the construction battalions very long, I discovered that I had no particular liking for wielding a twenty pound sledgehammer, which is what I was doing for a while, working on a ripp-rapp wall in Rhode Island.

Bearison: What's a ripp-rapp wall?

Beilin: It's a sea wall to keep the sea from eroding the land. I asked to be assigned to the Hospital Corp. and I, by a fluky kind of circumstances I got transferred to Hospital Corp school and then I got assigned to a base in Montauk, New York, and I was there for a short while. And then I got sent to the west coast onto a troop ship to Okinawa.

When the War ended in Europe, all the facilities were concentrated on the Pacific War. And I was part of a group that was to go in the invasion forces of Japan, and the staging area was in Okinawa. So I got on this troop transport, it took about a month to get to Okinawa. While aboard ship, pretty close to Okinawa, they announced the Atom bomb had been dropped, and then the plutonium bomb, then I landed on Okinawa the last night of hostilities; they were still dropping bombs around us in the harbor.

Bearison: Do you remember your thoughts about the War at that time? Did you see this as, you know, it's time for America to get involved, this is going to make it safe for democracy like World War I, and what was the reaction when people heard on ship that they dropped the atomic bomb?

Beilin: Well, first of all my attitude basically to the War was a positive one. I was naturally very exercised about Hitler, and I knew that it would be an enormous threat to people like myself, you know as a Jew, and that Hitler was definitely a menace to democracy and all that. Now what surprised me while I was in the Navy, was the extent to which among the troops themselves, the personnel, their attitudes toward the War were very negative, that they didn't really feel that it was their War, that they had a stake in it, and that surprised me a great deal. I also experienced some personal anti-Semitism in the Navy, and I barely got out of a fight with a guy, actually because the other sailors around pulled us off, but we were going to get into a bloody battle ourselves over some remarks that he had made.

Bearison: But at that time, I mean it wasn't just in Europe, but anti-Semitism was very prevalent in this country as well as in Europe, I mean it went to it's logical extreme in Germany, but I'm not surprised that there was some very anti-Semitic flavor in the country and in the world.

Beilin: Oh, yes. There was no question about it. I experienced it also when I was working at the Western Electric Company, got into a fight again with a guy over remarks he had made.

The attitude towards the dropping of the atom bomb was one of great relief, at least for myself and the other guys around on the ship when we heard it. We knew it was going to be a bloody battle in Japan. We had been given lots

of knowledge of the brutality of the Japanese, and I saw first hand the effects of it. When I came back I was assigned to a hospital ship and the –

Bearison: Came back from where?

Beilin: From Okinawa. My service priority came up for getting out and I was on the first ship, a hospital ship, I think, that took out the first group of Prisoners of War from camps in China, and the –

Bearison: These were U.S. Soldiers imprisoned?

Beilin: These were mostly U.S. Soldiers who had fought in the Battle of Bataan and the like and had been taken prisoner, and who had worked in the coalmines. The Japanese had put them to work in the coalmines, and these were the survivors and they were in unbelievably terrible shape. There were people dying on the ship every night. So we were not at all unhappy about seeing the bomb dropped.

When I got back to the States and was discharged, I went back to my job at Western Electric because the large corporations were required to rehire their personnel from before the War. At that point I began to think very seriously about my future and what I really wanted to do because I wasn't very happy working at Western Electric. I didn't see any future there for myself. At first I thought I might get some training and become a professional engineer, and I tried that, I took at course at Pratt Institute, some electrical engineering course or something like that, and I realized I was not cut out for that, so I went through an agonizing period of thinking about what to do. I stayed on at Western Electric for about a year and a half and then I quit.

Luckily, well there were two things that I decided I would do. First I thought about training, getting further education. My thought was that I would work for a master's degree. I had gotten the idea about going to graduate school actually from seeing one of my friends, a girl that I knew from before the War, who had gone to Colombia and taken a degree there.

Bearison: But you didn't have your bachelor's yet, right, because you left early?

Beilin: That's right. Good point. What I did when I got back is I reenrolled at Brooklyn College and I was able to complete my bachelor degree in half a year.

Bearison: While you were working, so it was like part time?

Beilin: Yes. I went at night. And I realized the thing I got a great deal of pleasure out of academically was philosophy courses; I took a course on Plato, which really inspired me.

Bearison: Do you remember what it was about Plato that was so inspiring for you in those days?

Beilin: Oh, I don't know. It was just the intellectual focus; I guess the thinking about ideas in the way that Plato explores them. We were reading the dialogs, some of them anyway. It was an undergraduate course, so how profound could it have been, but it really moved me.

Now I began to think, what kind of career could I pursue, and I thought, well I want to take advantage of my prior experience so I said, well I've had this industrial engineering kind of experience, hands on, and my biology and philosophy, so I thought, well, industrial psychology seems like a kind of compromise. So I thought about enrolling in a kind of place that would provide me with that kind of education.

Bearison: Correct me if I'm wrong, but I thought in those days this was just a brand new emerging field of psychology, industrial psychology, or what was the status of this field at that time?

Beilin: No, there was already a considerable tradition.

Bearison: Oh, really!

Beilin: Oh, yes. But I had to also work, because even with the G.I. Bill I knew I couldn't afford to go to school so I had to build up a fund for myself.

Bearison: What was the G.I. Bill, I mean how much would they pay, a lump sum of money or a percentage of money?

Beilin: They took care – you had a certain allocation of credits that you could take depending on your service experience, and then you could take courses up to the allowable amount. And it turned out –

Bearison: But you could take these anyplace, it doesn't matter how much they cost, like you could take so many credits and they'll pay for it whether it's at Columbia or Brooklyn even though Brooklyn's free at that time and Columbia --?

Beilin: I don't think there was any distinction.

Bearison: Okay.

Beilin: I could be wrong, but I don't think so. And then I took a Civil Service Exam thinking that I – I knew how to get a job so I checked out the Civil Service bulletins and discovered that there were jobs available in the Department of Health in the – actually as it turned out, in the Air Pollution Control Unit, or that and also Health Inspectors in general. So I took the Health Inspectors exam, I passed it. I got a high enough grade so that I got employed almost immediately, and then I got assigned to the Air Pollution Control Unit of the Department of Health.

Bearison: Where was that, in New York?

Beilin: Yes, in Manhattan. And I worked there for a couple of years while I enrolled at school, and I had a really good career there, I moved up very fast. I stood out among the other people who were in that job, and the director made me a kind of personal assistant to him.

Bearison: So what kind of work were you doing?

Beilin: Well, the day to day work was going out and inspecting and responding to complaints about air pollution, smoke was coming out of a smoke stack some place, so I would contact the superintendent of a building, either a small building or a big building, didn't matter. I learned about boilers and the production of heat and these byproducts, smoke and the like, and when if it was necessary I'd give them a warning, only occasionally did I have to give them a summons and appear in court. As the assistant to the director I ran a convention for them, the Smoke Prevention Association, or whatever they were called in New York.

At the same time I was working on a master's degree at Columbia's Teachers College in what was the personnel psychology program. I realized early on that I really wasn't interested in personnel psychology, industrial psychology and the like, and I moved over progressively within that program to counseling psychology. The work I did was pretty good, and I began to think beyond the master's degree because I saw other people working for doctorates and I thought that I was as good as they were, and I began to think that maybe this would be for me, and have a career in counseling psychology.

After I got the master's degree I decided I would quit my job and go to school full time. I was still on the G.I. Bill. They also provided a stipend for dependents, and that was important because I was still supporting my mother.

Bearison: Were you living with her, or were you living on your own?

Beilin: Yes, I was living at home. In those days it wasn't so unusual to do something like that. I more and more acquired skills in the counseling psychology area, and not only that but I began to take leadership roles, I became the President of the Teacher's College, what do they call it, well it was a club, the Teacher's College Counseling Psychology Association or something like that.

Bearison: Well, what was it about counseling psychology that interested you, why did you like it? And wasn't counseling psychology considered then, as it might be now, as a little nasty stepchild of clinical psychology?

Beilin: No, it's only since that time – counseling psychology's become more like clinical psychology. At that time it was oriented around career counseling, and the – I had interest in that domain, and that may have been because of all of the agony I went through in my own career that I took an interest in. Now I realized, however, that my interests in psychology were more theoretical and analytical than was provided by this applied program, so I kept choosing courses that were at the more intellectual end of the continuum. On that score, I took a course, for example, with Robert Woodworth who was still around at that time, it was on history and systems. They had visiting professors like Kenneth Spence, and I took a course with him, which was ostensibly called "Motivation," but it was through him that my eyes were opened about philosophy of science.

Bearison: And Spence was at Teacher's College, not Columbia?

Beilin: He was at Columbia. So those people at Teacher's College could take courses at Columbia, so I enrolled in a course of experimental psychology in the psychology department at Columbia. I had a course in experimental with Noterman, which was oriented to Skinnerian psychology. I was very eclectic in my choices and I got to be seen in the program, that is in the counseling psychology program as a bit of a maverick, and not really committed to the field in the way that people who were going to become practitioners and wanted to be practitioners were.

Bearison: So I get the sense that you really didn't have a mentor there, one person who was really guiding you though this, it was sort of you on your own?

Beilin: Well, I did. I had a mentor all right, and it was Donald Super. And Donald Super and I did not hit it off well at all. He saw that I was really pushing in these other areas and his commitment was to counseling psychology itself, although it was within this career framework. And I also stood out among the students. I wasn't the only one that stood out, but I stood out among them so that he valued my contribution, but he didn't push me the way he did some of the people that he favored. There was also tension between himself and the Jewish students. He admitted he had a strange background. He had been to Oxford, his father was an official in the YMCA, and he had grown up part of his life in Poland and there was an anti-Semitic streak that he admitted to on the basis of his experience, family experience, whatever, but he was unbelievably fair. He almost bent over backwards to be fair to his Jewish students. And at that time there was a revolution in academia because you had all of these ex-servicemen coming into the universities and they upset the sociology, social climate within these very traditional programs like the Columbia programs. But they made a pretty good accommodation to the students and the students –

Bearison: Was part of this upset that suddenly all these people who never would have gone to college were now going to college, where college at one time was strictly for the upper-class and aristocratic people, especially you didn't go to graduate school, so you have like all these sort of common-folks thinking they're good enough to get a college education. Is this what you mean when you say it's a revolution?

Beilin: Yes, it was very much a part of it. It was a massive upward mobility.

Bearison: Okay.

Beilin: And the interesting thing is that became the subject of my dissertation. My dissertation was on the upper mobility of lower-class youth, boys. It was part of a larger framework of an interest in occupational choice, on the nature of occupational choice. Now I antagonized my mentor, my advisor, by first taking a course with Eli Ginsberg in the Business School who had just written a book on vocational choice or vocational development, and I was inspired by that book and by him, and I chose a topic within the framework of his interests rather than those of my own professor. Only afterwards, actually after I had chosen my dissertation topic then Donald Super became interested in vocational development, both theoretically and practically, and he started a large research program of his own on vocational development. After I had finished the dissertation, when I finished my dissertation work, he invited me to join his group who were to study the vocational development of a group of adolescents in Middletown, New York. We were to live in Middletown for the academic year, and for one academic year. My job – I was –

Bearison: Where is Middletown, New York?

Beilin: It's about two and a half – about two hours from New York, up the Hudson.

Bearison: Really? Past Poughkeepsie?

Beilin: Just about the level of Poughkeepsie, on the other side of the river.

Bearison: I see.

Beilin: The other students who were working on the program did the core work of interviewing these kids in school, mostly boys, and in those days they were not interested in girls because girls didn't have careers. But they gave me the job of assessing the occupational opportunities in that community. I went around to various industries and interviewed them, found out what kind of jobs they had, that were available to the community and the like.

Bearison: Was this a government supported research project, you had a grant to study this?

Beilin: Super had grants, but they were from Teacher's College itself. It was one of the funds. And I shared a house with Donald Super and Martin Hamburger who's a fellow student. I learned a lot from Marty Hamburger. We became very good friends, the fact is we're still very good friends. I learned to appreciate classical music, chamber music by virtue of my contact with him. He was a very bright guy and we had constant discussions, it was a constant seminar that we were conducting, and almost independently of Donald Super.

Donald Super would live up in the house, we rented this house, he lived in the house as well during the week and he would go back to his family on the weekends, we stayed there all of the time. Well, they collected a lot of data and it became an ongoing project that I think lasted for thirty years, and they collected data periodically.

Bearison: The purpose of this project was what? What was its goal?

Beilin: It was to study vocational development.

Bearison: But does that mean that, how these young – how they come to decide --?

Beilin: How they made decisions about their careers.

Bearison: I see.

Beilin: And what the bases were. And he developed all kinds of measures and developed a theory of vocational development.

Bearison: So the data was both interviews and what, like questionnaire type measures or --?

Beilin: That's right. There were questionnaire measures; there were tests, standardized tests as well, and then the core interview.

Bearison: Were they using these vocational interests measures in those days, the Strong vocational interest battery and stuff like that?

Beilin: No. The Strong and the Kuder, and a number of those, and Super was a specialist on those kinds of things, but we did not use them.

Bearison: So these were – this was a good time for you then I would think? You were sort of happy to be doing this sort of research, you thought it was important?

Beilin: Yes. It was an exciting time intellectually all around. Living with Donald Super was an ordeal that was not a pleasant experience. He was a really uptight guy, very difficult personally.

Bearison: What do you mean, like sort of autocratic and not engaging or --?

Beilin: Yes. Well, he didn't act like an autocrat, he tried to be just the opposite of one, but he was a very severe task master, that is, he had very high expectations with respect to work, which was quite reasonable, but he was basically a kind of cold fish and he was difficult to relate to.

Bearison: Was he a lot older than you?

Beilin: Well, yes. He must have been, I'd say at that point, I think he must have been at least fifteen to twenty years older. He died some years back. Now after that, almost by a fluke, I was offered a job. It was at one of the affairs run by the Counseling Psychology Society at Teacher's College, an affair honoring the retirement of one of the men who ran the program for many years, his name was Harry Kitson, and I was involved in organizing that affair as well. While at the festivities, a guy came over to me and asked me if I wanted a job, if I was available for a job at the University of Connecticut. He was going on leave and would I be interested in taking over his job for a year. So I jumped at the chance and the following year, after the year that I put in on the Middleton project, I went to Connecticut. I rented his two hundred year old cape cod colonial and I lived in great style, and then I taught that year, running a one-man program in counseling psychology.

Bearison: So this was your first real contact with the profession of teaching a university instruction?

Beilin: My first academic job, real academic job.

Bearison: I was wondering, you know, last week you talked about your family and your influences in your life, and particularly your uncle who really questioned what you were made of intellectually. How did they react to you now getting a PhD, I mean a PhD was much, much less common than it is today, and now you're going to be an academic – Did they see this as, "Wow, we always expected this of Harry," or "I can't believe he's come this far," and what was the reaction to you getting into this type of academic achievement?

Beilin: Well, you see my mother for one always used to say, I don't know if I mentioned this before, she said, "Why don't you learn a trade," so I pointed out that becoming a biologist and a laboratory technician was a trade, but she accepted that, but she had no conception of what was going on in that world. And she saw that I kept going on with my education. She wasn't happy when I went into the Service, of course, and then when I came back and I had a job, both before the War and then afterwards, and then when I went on to the university and got the degree, I guess she was happy and proud. My mother was very non-demonstrative, so I never – she never said, never came out and said you have fulfilled my life by your achievements. And the rest of my family, I think they began to look at me with some awe, and I suddenly became a distant person, a distant person to them, because I was now inhabiting a world that was totally alien to them, and they could only look and observe it and see what was happening, and more and more my life was centered elsewhere.

I always had a very active social life. My mother always wanted me to get married, which I kept postponing. It was very interesting, I never really wanted to get married until after I had started my career and was working. Somehow I felt that if I got married it would interfere with making decisions about my career, as I wanted to make them independently of any other people influencing it.

The year in Connecticut was somewhat fulfilling, because I handled an enormous amount of responsibility, and the job was a demanding job, there was an enormous number of students, an enormous number of doctoral dissertations, way out of proportion of what was reasonable. And I was also full of piss and vinegar, I looked around at the situation at Connecticut and I realized that they were operating by relatively low standards, and I had this rather exalted idea of myself and my education at Columbia, because it was really high quality stuff.

I took a look, for example, at the requirements and statistics, and the doctoral students were required to have only one course, so I went to the head of the statistics programs, and I said, "You know, it's conventional to have two courses, one of descriptive statistics and one on inferential statistics, don't you think that's what we should be requiring of our students?" Well, he hemmed and hawed and nothing came of it, so I had a doctoral candidate, a well-known guy locally who was the commissioner of education, in one of the New England states, working on a

doctorate in this program, and his dissertation was pretty far along. When he submitted the actual dissertation to me I thought that it was awful, pure garbage, and I knew that I was in a real bind because I have taken over this other professors responsibilities, and yet I felt I could not in all conscience have my name endorsing his dissertation. So I went to the Dean, and I told this to the Dean. He said, "You have no choice, this is an important man, we can't just fail him." He said, "I can't believe the dissertation is so bad." I said, "I think it is."

Bearison: You had come in on the tail end of it right?

Beilin: Right. He said, "What do you suggest be done?" So I said, "You endorse it. You take it over." He was very unhappy about that, but he had no choice, that's what he did. But from that point on I knew that my goose was cooked. It so happened that the man who's place I was taking was on sabbatical in South America, I think in Brazil or whatever, applied for a second year, and they granted it to him but they didn't want to keep me on, so I was out on the market looking for a job and –

Bearison: Was it largely because of this sort of gutsy attitude you took about this student? Do you think that's why they didn't want to keep you on, largely because of that?

Beilin: Yes and no, because as far as the teaching goes, I think I did a very good job. However, I was not really good for some of the teachers who enrolled. For example, I had to travel out to one of the outlining towns where they had a satellite program, and I gave a course in basic, I don't know what they called it, counseling, interviewing or whatever. I don't think I served them very well, and it was largely – these were hands on people, school people, and the fact is that I did something that one should not do. I went into academic teaching without having any experience, actual experience in a counseling center, getting lots of counseling experience myself – the skills that one should have for that job. So on the one hand, I think I gave them value, and on the other hand I think that I shortchanged them.

In any case, that job ended, I went on the market looking for other counseling psychology jobs. There were none, there was just absolutely nothing out there.

Bearison: We're now talking about what, 1945, '46, '47, what?

Beilin: Yes, by that time it was 1953. I got my degree in 1952.

Bearison: Okay. So we're much later than.

Beilin: Because I got the doctorate degree in '52.

Bearison: So this was '54?

Beilin: This was probably '53. I didn't have – there were just no jobs to be had, or –

Bearison: You mean in academia or in general?

Beilin: In academia. I was only looking –

Bearison: The fifties I remember, or heard about as being a very prosperous time in America.

Beilin: Not in academia, it was pretty tight, at least in all the fields that I qualified for. I went to the APA that year looking for a job and I was interviewed for a job, not in counseling, but on a research program. I was interviewed by John Anderson, from the Institute of Child Welfare as it was called at that time, at the University of Minnesota. He was looking for somebody with interviewing skills, and I had those, so I was offered the job to come out to Minnesota and work on what was called the Nobles County Project. It was run by both John Anderson and Dale Harris on a NIMH grant. The idea was for me to locate down in Nobles County, specifically in the town of Worthington, and collect data on a follow-up of boys, my job was with the boys, they also studied girls, it was a project devoted to the prediction of mental health and adjustment. What they had done was they had worked in the schools collecting data on a population of students in one particular year, and they had given them unbelievable

numbers of tests. This was a test oriented program, and the factors – their thinking was largely organized around tests and measurements; standardized tests, non-standardized tests, all kinds of tests. So they had bombarded these kids with all kinds of tests. So the idea was a few years later, we were to go down and interview the students who had been in that class, either boys, and they were going to hire a woman later to interview the girls, and then make an assessment of their adjustment from an interview with them. I was to work and live down in the community for a year, and then come back to Minneapolis and analyze the data. Actually, analysis of the data began or was continuing and they had a staff that I supervised, I had one, two, three, I had four assistants including one Chinese man, a native Chinese would come to the States, had gotten a doctorate in statistics at Iowa, couldn't get a job, and then took this research job working for me analyzing data.

I went out to Minnesota thinking I was a big city boy coming out into the sticks, fortifying myself with a large record collection so that I wouldn't be stranded, and I settled down. And after a period of breaking in in Minneapolis, I went down to Worthington and I discovered things they had not told me. Number one, they had been thrown out of the schools and told never to come back. Why, because they had administered a questionnaire in which they asked the kids to reveal, in a sense to squeal about other kids about their – I don't know, habits or – I forget what the specific content was, but some kid came home and said, "This is what they're asking me in school," the parents began to talk to each other, they came running into the school and they enlisted the local judge and they all descended on the poor program and they were told to pull out, get all their stuff out and leave.

Bearison: You know you hear so much about that stuff going on today, you know, where the parents in schools have no so-called respect for the psychological studies that we do. I didn't think it would be in those days, "Oh, psychologists are coming to study us," they'll treat us like Gods then, but no it sounds like it's no different then than it is today.

Beilin: Well, you also have to remember, this was the Midwest, it's a rural community, combination rural, small town, they're very sensitive about their privacy, in those days for sure. Well, as it turned out it was my job to placate them. Now here comes New York Jewish boy coming in to telling them, you know, he represents the University whom they are suspicious of, and would like to start collecting data after having been thrown out. Well, I had my job cut out for me, but for some reason I must have been persuasive enough, and benign enough in their eyes that they accepted me, and they agreed to what we wanted to do.

Now we were not working in the schools as such, what we were doing – I had an office that was provided by the local probate judge and I interviewed people away from the school, but it still required some cooperation. And I began to collect data. I began to interview these guys, and I had to develop the interview, that was the first job. I stayed in Minneapolis and worked on that first. And we all agreed about the interview schedule and then I went out and started to collect data down there.

The interviewing went very well. The only problem was that I was a little bored with the lifestyle and the job and the aspect of living there, and I began to – I was also basically dissatisfied with the way that they were approaching the whole matter, the kind of data, that is, that they were interested in.

Bearison: You mean the way they were using this data to access adjustment or measure adjustment?

Beilin: Yes. See they had worked out predictors on the basis of the tests they had developed, all kinds of predictors as to who would be highly adjusted, who would be poorly adjusted. Then the interview data went to provide another set of measures of well adjusted/poorly adjusted.

Bearison: Predictor measures or outcome measures?

Beilin: Outcome measures.

Bearison: Okay. So the tests are the predictor measures, the interviews are the outcome measures.

Beilin: Right.

Bearison: Okay. Got you.

Beilin: And then the idea was to see how they matched up. Ultimately, on the day they came in they did not match up at all, you couldn't predict, and it was a real blow to them when it became evident that was the case. I wanted to include other kinds of measures as well. I wanted to introduce some projective tests. They went livid; they were hostile as all hell to projective tests.

John Anderson incidentally was a very well known figure in psychology. He was one of the great figures of that era in developmental psychology. Developmental psychology at that point was highly oriented to measurement, and descriptive statistics or descriptive data, largely in a – I'm grasping for the word (psychometric), but largely in the measurement tradition. And the measures that they were committed to were all standardized tests, or a test that they worked out themselves. You can just imagine they were hostile to Freudian psychology, and not only Freudian psychology but also to projective measures and the like.

Bearison: Which comes from Freudian psychology. Projective measures are really a derivative of Freudian psychology.

Beilin: Right. Well, John Anderson had been President of APA, he was very well known, had lots of power, was a gruff, difficult man, an autocrat if there ever was. If Donald Super was difficult, this man was three times as much and he and I did not get along terribly well, we were quite honest about that, but I did my job and I worked hard. I clearly had a different style from them. When I arrived in the Midwest, I showed up at nine o'clock in my office. John Anderson pulls me aside and very graciously says to me, "You know, we start here at eight o'clock in the morning." I said, "That's going to be difficult for me. Back east we're accustomed to starting at nine." But, I said, "I'll stay an hour later," and the fact is they saw how hard I worked and how dedicated I was to the job and they never raised a question from that point on.

My basic attitude was always to give good value for the money I was getting and the job that I was expected to do, so I never hesitated on that. Not only was I committed to the job; I was committed to the idea of what we were doing. Now my life in Worthington improved immeasurably when I got very ill. I'd gotten oak poisoning and I ended up in the hospital, and I was really in bed in terrible shape, but gradually I was taken care of, they had just discovered Cortisone at that time and they used it on an experimental basis with me, and that cured me. And in the hospital I became acquainted, one of the doctors who took care of me, his name was Robinett, he and I began to work up a kind of friendship, and one of the nurses, a head nurse began to take an interest in me. And when I left the hospital they invited me over, each of them invited me over to their houses and from them I discovered there was a whole community of these very sophisticated, knowledgeable people. Here I was very proud of my record collection and high-fi speaker and the like. Robinett had a system three times better than mine, although he didn't have as good a collection of records as I, you know, that took me down a peg or two. And I discovered, I was introduced to a man who owned the local telephone system, and his wife had connections with the State Department and they were like a local station for visitors to the United Stations that the State Department arranged to visit and see America and all that, and through them I got to meet all kinds of interesting people from abroad. And I met also through them the editor of the local newspaper and we got to be good friends. I went with him on flight breakfasts. We would fly down to Iowa, you know, have breakfast someplace, come back, so suddenly my life there was transformed, became terribly interesting. I lived one of the most interesting years of my life down there in Worthington at that time.

Then after that year I went back to Minneapolis and continued on to work, but becoming progressively more unhappy about the work. But something else important began to happen to me. At the Institute, there were a couple of young members of the staff, teaching members, who represented a whole new approach to developmental psychology. Now in particular, I began to be influenced, largely in my discussions, with Eugene Gollin, who was a graduate of Clark and had been one of Werner's students. And he began talking to me about developmental psychology, about developmental theory, about Wernerian ideas, and I really got fired up. I also got an introduction to Piaget, not my first introduction because I had taken a course at Columbia with Arthur Jersild, who is also one of the pioneers in the Child Development field. And I had my first instruction to the early Piaget work, you know, on language, on moral development and the like, so I knew about Piaget. But what I discovered when I was in Minnesota, there had been a Swiss who had taken a master's degree at the Institute of Child Welfare, which has changed it's name to the Institute of Child Development, as it's now known. And he had written a paper, a master's

thesis, it was kind of a monograph on the new Piagetian ideas that had become known after the War, and reading that stuff really hooked me. I began to think, this is the kind of theory that I've been looking for.

Bearison: Now this is before Flavell's book came out, right?

Beilin: Long before, yes. One of the first papers I wrote as a counseling psychologist was on a developmental approach to vocational theory. And what I had done was take ideas from Arthur Jersild's book and provided a kind of developmental orientation to vocational development.

Bearison: But what, is this sort of the Wernerian notion of a developmental orientation when you say, what was it of developmental orientation then for you?

Beilin: It was very eclectic. It was a number of – it wasn't Wernerian at all. It was really a conglomeration of ideas from various places. You couldn't – I don't know how to identify what – it was Jersild who had said, "Well, these are the developmental principles," and I took those developmental principles and I said, "Vocational development entails and incorporates these kinds of principles." It's become, incidentally, a classic paper in the vocational development field.

Bearison: Because it comes to define this perspective.

Beilin: Well, it's really laid out, it was one of the first, it wasn't the only application of developmental ideas to vocational development, because Donald Super in one of his books had adopted ideas from one of the Brunswick's, I think it was, and had applied them in a rather casual way. Mine wasn't the first ever, mine was the first more systematic to doing –

Bearison: And where did you publish this?

Beilin: I'd have to look at my bibliography. It was in the *Journal of Counseling Psychology*, it was 1955, and the title was <u>The Application of General Developmental Principles to the Vocational Area.</u>

Bearison: So this was like your first developmental paper, right?

Beilin: Right. My talks with Gollin, we also became good friends at that time. I socialized with his wife and family and two kids, and he had some other friends I got to know. One of them, in fact, became an assistant of mine, a research assistant, Rosenberg, who went into child language, development research, got a job at the University of Illinois, Chicago Circle for a large number of years, for many years.

My discussions with Gollin had a profound affect on my thinking. I didn't realize how much, in fact, at the time, all I knew was I couldn't accept a Wernerian position, it seemed too shallow or whatever at the time. Piaget, with the new formulation seemed to provide enormous rigor and depth, and that planted the seeds –

Bearison: I can see what you mean by depth, but what do you mean by rigor? When you use that word rigor, what does that mean? I mean you're coming from a place that really wants sort of hard-fast measurements and then Piaget is sort of like more projective in interviewing than rigor. Why do you use the word rigor, what does that mean?

Beilin: Because he was approaching things with logic, the logical system –

Bearison: Okay.

Beilin: -- that seemed pretty rigorous to me, even though there were a number of things that one might criticize about that.

Bearison: When you began to recognize Piaget, I mean did you see his work in the spirit of say what John Dewey had been doing in this country? You must have been familiar with Dewey, I mean did you see Piaget

as he had seen himself, I mean did you see – oh, he fits into a whole tradition that had already existed in America. Did you think that, or --?

Beilin: No, I couldn't put him into any intellectual framework that existed, quite the contrary.

Bearison: Okay.

Beilin: The prevailing theoretical positions, mainstream position at that time was very behavioristic, after all, you know, I'd been studying with Kenneth Spence, and you couldn't get more behavioristic than that. At the same time I was exposed at Teacher's College to a phenomenologist, so I also had a course with Donald Snygg, coauthored with Combs on a more phenomenological view of psychology. And in addition to that, I had been trained with a clinical orientation that was Rogerian, so Donald Super was a Rogerian in a kind of mild way, but we had some Rogerians on the staff at the clinic at Teacher's College, so I was exposed to a lot of that. But nevertheless the mainstream psychology was behavioristic, and it was beginning at that time also to melt into neobehaviorism.

Now, Gollin and a couple of the others represented something else that was very important. Coming out of the Clark tradition, what they were were the first of a new breed of experimentalists, that is, experimentalism applied within the developmental framework. They were very hardheaded and they represented something really new in developmental psychology. I didn't recognize that at first, although I knew there was something going on there.

After being in Minnesota for three years, I decided that I wanted to leave. John Anderson and I were not getting along very well. I don't say – my problems were not with Dale Harris so much, he was more or less a cipher in all this, he was doing what John Anderson basically wanted him to do, but it was really rather difficult working with John Anderson and after a while I just wanted out. So I began to look for a job, this was 1956. I'd been there from '53 to '56. Again, no jobs, nothing around. Now I had decided I wanted to go into developmental, made the decision I was ready for a transformation.

Bearison: And that was a common recognizable sub-discipline in psychology at this time, developmental psych?

Beilin: Oh, yes. Oh, yes.

Bearison: Okay.

Beilin: It was a lively field. There was a division, after all an APA Division 7. There was also SRCD had already started up, although I didn't know much about that. I looked around for jobs in counseling psychology as well. There was one job in Michigan, I don't know if I applied for it or not, but I certainly didn't get it. And I got interviewed for jobs at NIMH, just as well not mention the names of the people who interviewed me, but one of them, a well-known women said, "You know, we can't hire you." She said, "You have the qualifications for the job, but we could never hire anybody who John Anderson would not approve of." So that was that.

In desperation I decided that what I would do is go back home, come back to New York, and see what I could scrounge up in the way of teaching experience.

Bearison: You mean like adjunct courses?

Beilin: Right. Right off I went to Brooklyn College, my old alma mater, and went to the psychology department, went to the education department. The psychology department had nothing, the education had something, so they offered me, I think, one or two courses, and I also had a job in the continuing education program.

Bearison: Also at Brooklyn.

Beilin: Then I went to Queens College and I got a job at Queens College, adjunct job, I think, one or two courses. It ended up that first semester I had an enormous number of courses, I was teaching all over the place. The second semester, it could have been that it happened in the first semester, a woman who I was replacing partially got ill, and

then Brooklyn gave me her whole program, so I had at least, I think by the time I ended up, was teaching eight courses.

Bearison: In one semester?

Beilin: And they were all new preparations. Now in the education program I was teaching what was basically educational psychology, and –

Bearison Excuse me; we're going to stop in ten minutes because I have to prepare for another meeting, so –

Beilin: Okay. I decided at that point that I was going to seriously re-tool, I was going to make myself into a developmental psychologist. I also had ideas about my research program, although they were a little vague. I still had some, although I had no more education credits under the G.I. Bill, but there was a state program, you could take a test, and if you qualified you could get education credits that you could use in any educational institution in the state, so I got that money and I enrolled in the New School, and I took – at first I took some fun courses on chamber music.

Bearison: But what made you go to the New School, here you had such a fulfilling experience at Columbia and at Teacher's College, why didn't you go back there?

Beilin: Well, you couldn't take individual courses like that; you've got to be in the degree program. The New School was ideal, so I enrolled in a course in logic and epistemology with Sydney Morganbesser. I enrolled with a course with Solomon Asch in social psychology, and these were revelations to me, both courses. I discovered – oh, incidentally while I was in Minnesota, I also sat in a course on philosophy of science with Herbert Feigl, the great Herbert Feigl, and I sat through that, I religiously attended that one. In fact, at the end he said, "You've been here at every session," he didn't expect that I would last it out. And I participated in the classes, it was a wonderful experience, and I got really hooked on philosophy of science. And then Morganbesser, I went into Morganbesser's course; it was like being one of the first artic explorers into either the North or the South Pole. It was an unbelievably difficult experience, the stuff was opaque to me, but I recognized what I didn't know and I went on from there. Maybe we should stop at this point.

[Continuation on May 30th, 2000]

Bearison: This is the third meeting with Harry Beilin for the SRCD Oral History Project, and today is Tuesday, May 30th. So you want to backtrack a little bit.

Beilin: Well, I want to backtrack a little bit to talk about my first publication.

Bearison: Great. Good.

Beilin: It actually was when I was still a graduate student working in a doctoral program at Columbia, and the paper was published, it was a short paper, and it was published in *The Vocational Guidance Journal,* and it concerned changes that were taking place in guidance practices in elementary school, and it reflected a more mental hygiene/mental health type orientation than had existed ostensibly before that. In any case, I got a letter from Harry Kitson who was the retired or retiring professor who started the guidance program at Teacher's College, and it was a very nice letter congratulating me, and parenthetically pointing out to me that I had written the word 'loathe' when I really meant 'loath', which I've never forgotten and have always been sensitive to since.

The interesting thing is that my mentor Donald Super, never mentioned the article. I think that he felt or thought that maybe it was premature for me, I was still a graduate student, to be publishing on a kind of 'think' piece prior to having had much experience in the field.

Bearison: I also think in those days it was much less common than it is today for students to publish while they're still in graduate school.

Beilin: Oh, absolutely.

Bearison: So it was a much bigger deal then.

Beilin: Oh, yes. No question about it. But it started off my career, writing and publishing and kind of anticipated what was to come.

Bearison: But was it the thrill of seeing your work in print and your name in print that sort of gave you a thrill to say, "Yeah, this is me, this is what I want to do?" Did you get this sort of like emotional high from it, an inspirational high?

Beilin: You know the strange fact is I have never had that kind of experience from publishing. Seeing my name in print never gave me any jazzy feeling. It's hard to explain, and it's always been that way. It's almost always when the thing appears in a print it's an anti-climax for me. I'm beyond that point, I'm on to something else, and I never got much ego satisfaction in a sense out of that aspect of publishing. I've wondered about that, in fact, although not very much I must say.

Bearison: Well, let's come back to that later as you look back on your career about what does give you ego satisfaction out of your career. Let's come back to that.

Beilin: At the point at which we left off last time, I was talking about my experience taking courses at the New School. I mentioned – and this was part of my re-tooling, coming back to New York, changing fields from counseling psychology to developmental psychology. I had this clear idea that I wanted to be a developmental psychologist and a cognitively oriented developmental psychologist. I was also forming in my mind the idea that I would commit myself to Piaget's general theoretical approach, although that was not fully formed at that point.

Bearison: Let me ask you this, when you say, 'developmental psychologist,' did you understand it within this sort of Wernerian tradition of what a developmental psychologist is, or did you have some other idea of what a developmental psychologist is all about?

Beilin: I think it was the last year that I was in Minnesota at the Institute, Dale Harris had organized a conference on developmental theory and he had a lot of heavy hitters there, including Werner and Ernst Nagel and – interesting, the only one he didn't have was Piaget, but a number of other people who are talking about what developmental theory was all about –

Bearison: Was Werner there?

Beilin: Who?

Bearison: Was Werner there?

Beilin: Yes, Werner was there. It's hard for me to reconstruct what I thought developmental psychology was. The one thing that I knew that it was not, was this older tradition in child psychology that was descriptive, psychometric in its orientation, and I recognized that there was a change that was taking place, much more theoretically oriented, much more cognitively oriented and that development was concerned with changes that were taking place in the psychology of a organism that was evolving in particular ways.

Bearison: So at the time you were sort of re-tooling at the New Schools in those days. Who would you say were the major developmental psychologists, who were the stars of the field at that time?

Beilin: Well, of course, there was Werner.

Bearison: Okay. So he already would have been a star then, he was already known?

Beilin: Oh, yes. He was a stellar figure within developmental psychology, although at the same time somewhat marginal. He represented this European tradition and a kind of Gestaltish orientation, holistic, you know, and everything else that went with that. And, of course, the new Piaget was being suddenly aware of in the American scene.

Bearison: And what year was this, do you remember?

Beilin: We're talking about the 1950's.

Bearison: Early fifties, '50, '51, '52?

Beilin: Well, I graduated in '52. I went out to Minnesota from '53 to '56 –

Bearison: Oh, okay. So we're talking about '57, '58 already.

Beilin: Right. That's right. I knew that even though I had a pretty good grounding in psychology, I felt that I really didn't have enough of a grounding in certain areas. I was always pushing ahead to become more knowledgeable and more highly skilled and the like, which is why I took advantage of the opportunities of the New School, and I told you I had this course with Sol Asch, with Morganbesser on logic and epistemology, and I had a course in perception with Irv Rock. I had an advanced statistics course with Greenberg, and it affected me both generally; my general orientation, but it also gave me some rather specific foci. For example, at that time too, it was early on that I decided to really commit myself to developmental psychology, and I joined the Society for Research in Child Development when it was in its very young stages. And the first convention I went to was at Penn State University; I'll never forget that. An interesting event happened. I was taking a train to Penn State; one did that in those days, and when I was looking over the program, I realized that I had read a word correctly when the word was spelled incorrectly, and a psychoanalyst would have no trouble with this, the word that I read was mother. Now I thought, here's an interesting problem. How is it that we recognize things for what they are intended to be when in fact they're not represented that way? So I thought, well, I think I'll do a study around that notion. And I developed a research experimental technique for studying it, and I mentioned it to Sol Asch, and he said, "Oh, well, that's an anagram problem." It struck me like a bolt of lightening, "Oh, is that what it is?" Whereupon I went to the literature and discovered there was a controversy around anagrams and why. There were a number of theories; mostly neobehaviorist theories about the way anagrams are processed. I already had a kind of Gestalt and cognitive view of things, so I said, "Well, there must be an alternative way of thinking about this theoretically." I couldn't accept the behaviorist view, which was based on cumulative transitional probabilities of diphthongs and the like.

Bearison: It sounds like a Gestalt problem to me.

Beilin: Well, yes. That was the thesis that I was presenting, that words constitute Gestalts, and they're not simply the addition of individual letter units. So I did a study, in fact, an experimental study contrasting word anagrams with nonsense anagrams, and controls for all kinds of factors, and –

Bearison: And you were still at the New School when you did this study?

Beilin: No, no. I wasn't teaching. The new school was a sideline for me. It was extramural activity. I was teaching at Brooklyn College.

Bearison: Well, you never really told us how you came to be teaching at Brooklyn College, did you?

Beilin: Are you sure? I thought I had. Well, I'll come back to that in a minute. In any case, what I discovered was what I expected, was that there was a difference in processing time in changing word anagrams relative to nonsense anagrams. An example of a word anagram would be the word 'cat,' which can be transformed into the word 'act,' and then contrast that with nonsense anagrams (arcih) that could be transformed into a word like, chair. With controls for all kinds of things, it turned out that my prediction was correct. Incidentally, that was my first publication in an experimental journal, (*The Journal of Experimental Psychology)*. Now that I got a great thrill out of, in the exception to what I said before, because it established in my own mind, one, that I was a real psychologist, and that secondly, that I was an experimental psychologist. And to me this was very important at the time, both

status-wise, establishing my identity in contemporary terms, and the feeling of great satisfaction in having achieved that.

Well, in any case, getting back to Brooklyn College, I told you that when I came back to the city I couldn't get a fulltime job at first. What I did was I picked up courses at Brooklyn College, at Queens College, ended up with a very heavy schedule. Well, as it turned out, a woman at Brooklyn College had gotten very ill, she took a leave of absence and they offered me a full-time job and I took it and then began to teach. It was all problematic as to whether I would get reappointed, but right off I worked very hard, I was very conscientious.

Bearison: Your appointment was in the psychology department.

Beilin: No, it was in the education department.

Bearison: Was it a School of Education or just the education department.

Beilin: Well, it was a department at that time; they later developed a School of Education.

Bearison: Were there any other developmental psychologists there, or were you –?

Beilin: Yes. There were a couple of developmental psychologists actually. There was Marty Nass; I don't know if you know him, he was a psychoanalyst on the side, had a private practice, but he published mostly psychoanalytic oriented research. And I think there was one other person, but mostly people who didn't publish, I just thought it was a very teaching-oriented program. And most of the people, of course, in the education program were curriculum people with an instructional emphasis.

Bearison: And it was just an undergraduate program then?

Beilin: No. It was mostly an undergraduate program and a master's program as well.

Bearison: Okay.

Beilin: At first I only taught undergraduates. I taught almost every conceivable course that they could give me. I taught philosophy of education, sociology of education, educational psychology, God knows what. In any case, I used that experience also to retool. In the education psychology course that I was teaching, or child development, I basically was reading the textbooks, elementary textbooks, for the first time. I'd had one course at Teacher's College in child development, and it was all organized around Carmichael's handbook at the time, so this brought me back to learning the content of a developmental psychology, so I used that as a retooling exercise as well.

Bearison: And was Brooklyn College like a pleasant place to work at that time? Were the students good? Did you enjoy the experience, getting the position there, or --?

Beilin: The students were still pretty good; there was a range, and in fact, as I'll mention, they had a couple of honor students, but on a whole they were pretty good. The classes were very large. They weren't like two hundred and three hundred like they have in these days, but they were always between thirty-five and forty students in a class. And the workload was teaching five courses, so it was pretty heavy going.

What I began to do was think about working with students also. As I had no research assistants, I was thinking about working with students as research assistants, as collaborators. Well, I had two students on two projects. After I did the anagram study, which incidentally changed the whole discussion of that issue in the literature. It kind of totally undercut the behavioristic approach to anagram problem solving from that point on.

I then thought of doing a developmental study on anagrams, anagrams problem solving in children, and I enlisted one of the honor students that I had in my class, her name was Reba Horn, and we did a study together. It ultimately got published too, in the experimental literature, or maybe it was in the developmental journal, I don't remember any more. And then I got the idea of doing a Piagetian study; it was my first thoughts about really moving into the research domain, the Piagetian research domain, and it was a study in geometry on learning length concepts against area concepts. What I formulated was a training study, basically.

Bearison: If you go back then, you think of the whole spectrum, the whole span of Piaget's work at that time, because I know you followed this up later and the geometric studies have been a great interest of yours. What attracted you to that particular bit of Piaget as opposed to where else it was going? Do you remember that?

Beilin: Oh, yes. I told you that my first encounter with the new Piaget work was at Minnesota in this master's thesis that had been written by this Swiss psychologist. By the time I got back to New York and I was looking into the developmental literature a little more thoroughly, I became aware of other people who were writing about the new Piaget. In particular, there was Eric Lunzer in England, and Kenneth Lovell, those two in particular were doing studies on Piaget. And then I became aware of Jack Wohlwill, the American who had gone to Geneva, had done, I think, a learning study as well, and he published in the American literature. They became kind of models for me.

Now there was a controversy at that time over one of Piaget's ideas that was highly contested, or was contested anyway, at that point it was not highly contested, but it was contested. It was over the question of learning, the acquisition of knowledge. The Piagetian position at that time was that you basically could not train kids if they weren't "ready."

Bearison: That's right.

Beilin: I was not satisfied with that position. I was already well imbued with the American point of view about learning, because after all, most of American psychology at that time was based on learning theory of one kind or another, mostly, of course, Hull/Spencian and Skinnerian. In any case, I thought I would do a training study, and it seemed that a good vehicle would be geometric concepts. Why that of all of Piaget's work, I can't say for sure, except that one of the books that came out, the post-War books was on geometry and I was intrigued by it, so I developed a kind of very simple model. I was interested in showing the differential effects of training on length measurement as against area measurement.

Now I devised with this student, I say <u>with</u> this student charitably because (her name was Irene Franklin) it turned out that she was not as stellar of performer as I had expected by virtue of being an honor student. In any case, I worked out this other procedures of training and we went out to schools in Brooklyn. And she did some of the training, when I discovered that she was pretty terrible at instructing kids; I had to do most of it myself. In any case, what that study showed is that with kids at different ages, that is, different school grades, you could train kids in length measurement, but that with area measurement there was no success with the younger as you did with the older kids. This was my first paper on Piagetian theory and Piagetian type studies. I think that was published in *Child Development* at the time. And that established in my own mind that I was really a member of the developmental research community, I was now publishing in this domain, and then my ambitions had no limit at that time. I decided that I would apply for a NIMH grant and do a larger training study. It wasn't easy to get the people at Brooklyn in the frame of mind of encouraging research grants, interestingly enough. At that time, in fact, there was no one in the education department who had a grant.

Bearison: Did you have affiliations with people in the psych department at Brooklyn at that time, or were you pretty much separate from that?

Beilin: I had some contact with them, and when they discovered that my paper was published in *The Journal of Experimental Psychology*. When they discovered that I was a legitimate experimental psychologist, the attitudes in the psych department changed towards me, or at least I became an entity whereas I was a total unknown to them before that, and I began to make contact with individual people in the psych department. One of my colleagues, my friend from Minnesota had taken a job at Queens College, and he had been in the Brooklyn psych department earlier and he introduced me to some of his friends. The most important of whom was Harold Proshansky, who later became very active in the doctoral programs here at the Graduate School. In any case, I did have some contact, but it wasn't anything formal. I didn't do any teaching there; I confined all of my efforts to the education department. And it didn't take very long and I found myself with tenure and with a promotion. I went fairly quickly up the ladder to an associate professorship.

In any case, I got a grant from NIMH and I undertook a big program on training Piagetian concepts. The idea there was, again, with the Piagetian resistance the idea of training, a number of people got the thought that one way to

challenge the Piagetian system was to show that training was possible and the acquisition of especially logical concepts was possible. Most of the concepts that were focused on at that time were conservation, the various conservations. And the reason for that was that the conservations are so counter-intuitive that most people at that time especially were shocked to see demonstrations of the difficulty of young children conserving. So, it became a general vehicle, and as you know, after a while there were hundreds of studies on conservation.

Bearison: But the training studies, as I understood, fell into two camps. One camp was, you know, this is a mark of development, development of growth, how can we promote it, and that was sort of the American tradition that Piaget talked about. But the other part of it was that the weakest part of Piaget's explanatory system is to account for the transition between stages. What are necessary and sufficient conditions to get the child to make the transition from one stage to the next? And so then people engaged the training studies as sort of the experimental method in order to account for necessary transitional factors and they were less interested – they actually weren't interested, they could care less about anybody using this research in order to train children and to teach children to let it go. It was purely a method of trying to do an experiment on the transition. So you presented yet maybe yet another third approach to this. You say you want to use this as a way to disconfirm Piaget's ideas. How does that fit into these other two approaches, because I've not heard that as a rationale to engage in training studies?

Beilin: Well, you're right in capturing the differences in attitudes toward Piagetian theory and research. The way I conceived it was that there were two kinds of interests involved. One group of people, and they were exemplified by David Elkind, were interested in simply demonstrating that Piaget was correct in his characterization of development, so they would do all kinds of verification studies. I had absolutely no interest in that.

Bearison: Do you mean by replications when you say verification?

Beilin: They were replications, basically replications.

Bearison: Yes. Okay.

Beilin: That tradition, well that tradition in a sense evolved in a different way when they began to change the conditions of a presentation and use it as a way to undermine the Piagetian argument. Then there was another group of studies that were interested in testing theoretical claims, they're not interested in replicating Piaget, but in a sense, testing the theoretical assumptions underlying the theory, and that's how I saw my own work, that I was interested in testing the limits of the theory, it's characterization of change. Now the people like Rochelle Gellman, for example, in her studies on training, was essentially interested in undermining the Piagetian position and she was not the only one.

Bearison: But it was to account for the phenomenon in behavioristic terms, that's what she was out to do.

Beilin: Well, in terms that were alternatives to the Piagetian position, that were behavioristic, neobehavioristic, whatever. And what I was interested in was not undermining the theory, it was in a sense to change it, that is, to improve it, to show where it's limitations lie and then contribute to change.

Bearison: There's another phenomena in the history of psychology around these training studies, which I've always found quite incredible. It has to do with the first generation of American Piagetian's, say somebody like Jack Wohlwill, he spends time with Piaget, he comes back here, he too tries to do some training studies –

Beilin: Right.

Bearison: -- and he finds he can't train students, and he reports back that, "Yes, Piaget is right. These are structural changes, they can't be changed." But the next generation, and I include myself in that, I was a student of Wohlwill's, low and behold, I'm able to do a successful training on conservation. Wohlwill is shocked – I don't want to tell my story here, but he goes out and has another graduate student replicate my findings before he would let me publish it. And then from then on, all the second generation people, and I guess you're sort of in that group too, but my senior, there are a variety of ways that we can train children to advance, for at least at that time, from concrete operational to preoperational reasoning. But Wohlwill wasn't alone, that whole group of first generational people were unable to train. What happens is that even

people now who go back, somebody used the same procedure that Wohlwill used and it worked like a charm, so to me it reflects something in the messy, soft, biased side of scientific research. It had a lot to do with the expectations of the experimenter, and for Wohlwill, the training study, the lack of training was a way to confirm Piaget's position. And for you and I it was a way to add to the position and to help to account for mechanisms of change, and so we had different expectation from this, and sure enough you were able to do it, and it's always stood out to me as an anomaly in how we look at the science and how it develops.

Beilin: But there are some people who never gave up, for example, Mimi Sinclair did what was in effect a kind of training study and she came up with the standard Piagetian view.

Bearison: But you can't –

Beilin: Later, one of my training studies was designed to show that you could then instate a logical operation by linguistic means, and she was the language person --

Bearison: That's right.

Beilin: -- and she tried to do ostensibly the same thing, and couldn't affect any change.

Bearison: But after a while when Americans were doing so many of these, it's sort of like they finally turned to it, remember?

Beilin: Yes.

Bearison: So they did this book by Inhelder, Sinclair and Bovet, where they finally said, "Okay, we're going to do some of our own," and they actually were able to do it after saying that you can't do it, you can't to it, and that was a big change. And for them --

Beilin: And that came later.

Bearison: Yes. Much later. But it was really taking their cues from people like you, not only Americans –

Beilin: But they never acknowledged our work.

Bearison: That's right. Well, I also happen to know, and I don't want to get too much into it, but at the time these training studies were coming out, Jacque Voneche told me that Piaget, my study in particular, but other studies too, he had them go out, he had his assistants replicate those studies, and he was finding out that they were effective and they could be and that they were – yet he wouldn't publicly acknowledge it. He told me when he was at Clark he wanted to meet me because this -- because they had replicated my – but mine wasn't the only study, they were going out – he was sending assistants into the field to replicate these studies but he never acknowledged them in print that this is happening, so –

Beilin: I have my own drama with Piaget, and I'll tell you about it now. Just about the time, and the time I'm talking about when I began publishing, I got to be known and I was invited to give a paper at a conference in Monterey, California that Piaget was also going to present a paper on. It was a small conference. It was organized by a couple of people who worked for McGraw-Hill. A book was finally published out of that conference, it's called *Measurement and Piaget*, if you recall.

Bearison: Yes, I do.

Beilin: At that conference I presented a paper in which I pointed out that if Piaget's claim was true, that development occurs in a progressive fashion, a stage like progression that cannot be altered by way of experience, this is in a kind of fixed pattern --

Bearison: Fixed sequence. Yes.

Beilin: -- that the only implication that you could draw from it or the most likely implication was that there is some kind of genetic innate maturational factor that is controlling the system. Piaget was at this conference with Inhelder.

Bearison: They didn't like that, I'm sure.

Beilin: And Inhelder, while I was talking, Inhelder was translating for Piaget. Well, you can imagine Piaget's response. Right after my talk he got up and – oh, I think John Flavell was a discussant, and he took a very positive view of my thesis. Piaget, however, said, "Beilin doesn't understand my theory, and he implied that I've got it all wrong. He said that his theory was designed to explain novelty in development, and that was at its core, and therefore my thesis couldn't be correct." So I pointed out and replied to him that there are two kind of novelty. There is novelty in the development of specific ideas and then there's a species kind of novelty, that some characteristic develops within evolution you could say is novel to that species. And I was asserting that the kind of novelty that exists in this stage wide progression is a species controlled novelty, and it's different from the novelty of ideas or the other things that are more characteristic of the Piagetian position, or the kind of novelty that Piaget's talking about.

Well, Piaget never forgave me for this and he used me as a kind of model for criticism over and over again.

Bearison: How Americans misunderstand him, and he goes back to Beilin.

Beilin: Well, yes, and Canadians. He used me as an example of one kind of error. He used Dan Berlyne as an example of the other kind of error in trying to make him into a neobehaviorist. An interesting story about Dan Berlyne too. I was at a meeting that Hans Furth had organized on language, and Dan Berlyne, through an intermediary asked if my name was really Beilin. So when he learned that, in fact, it was an original name, he came over and he introduced himself. He said, "You know, my family's original name is also Beilin." He said, "Where is your family from?" So I said, "Well, we were originally from Minsk." He said, "My family was originally from Minsk." Then he tells me this story about how when his family came to England, the immigrations service had made an error, and they changed the name of Beilin to Berlin. Now when the first World War came, it was rather uncomfortable to have a name like Berlin in England, so they anglicized the name and made it Berlyne, and the family, part of the family anyways moved to Canada, and that's where he grew up. So I always had this image that Piaget was being bombarded from the left and the right from one form of Beilin or another.

Bearison: Yes. The same genetic links splitting that about.

Beilin: Well, in any case, my study and the NIMH study resulted in a paper, it was an enormously complicated study. It contrasted different forms of training, and I showed that the most successful form of training was by language instruction, that is, rule learning, linguistically based rule learning, and that the others were successful too but not as successful. And the most famous study, of course, was Rochelle Gellman's, who – it got the most attention anyway, which showed that attentional training was the most effective.

Well, in any case, I went on from there to the study of language. I got interested in language studies. At that time the two big revolutions in psychology were Piaget's and Chomsky's, and with this work on language construction and rule learning I became interested in the larger questions about language acquisition.

Bearison: But at the same time, I mean, Piaget's position at that time was rather incredible and unique, this idea that language doesn't form the core of thinking, that there's something more basic than language. I mean, how did you feel about that, was that something that attracted you or did you think this is crazy, it's all wrong here? How did you deal with his rather unique and creative take on language, especially after having studied logic and epistemology, I mean where were you in that regard?

Beilin: Well, my view on the Piagetian position was, again, two-fold. On the one hand I was ready to accept the claim that was made by himself and Inhelder, that the origins of language are cognitive, so that the precursors to language acquisition are cognitive in nature.

Bearison: The semiotic function.

Beilin: On the other hand, I was not sympathetic with the view, in fact, rejected it, that language played no role, or played only an ancillary role or secondary role in the acquisition of thought. So, I then was interested in testing these notions out and at that time, of course, the principal alternative was the Chomsky view. And the Chomsky view, as you know, dominated almost all language acquisition studies from that point on.

I went out for another grant and I got another substantial grant, this time from NICHD, and undertook a series of studies. Now by that time –

Bearison: Had the Chomsky/Piaget debate been published by this time?

Beilin: Had it been published?

Bearison: Yes. By the time that you were going into this arena?

Beilin: That came later.

Bearison: Okay.

Beilin: Now at that point an important transition took place in my academic life. I had been at Brooklyn College at that point by about ten years. I was now also a full professor. I had gotten a full professorship by a presidential appointment, it kind of bypassed the regular administrative procedures, and it happened easily, not so easily maybe. I had told the chairman of my department that I had an offer from another university, and that –

Bearison: Do you want to say where?

Beilin: Yes. It was the University of Rochester. It was after Flavell left, and they were looking for a replacement.

Bearison: Elkind was there, or was Elkind the replacement ultimately they got when you didn't take the position?

Beilin: That came in later. It wasn't an actual offer, it was an invitation to apply, one of those things, but I knew that they were interested in me. So I went to my chairman, I said, "Look, I got this feeler, I wanted you to know that if I get an offer from some institutions I will turn them down categorically. If I get an offer from another kind of institution, there's no question I will take it, no matter what." I said, "But then there's another kind of institution in between these two where I would have to weigh the alternatives and possibilities." I said, "The offer that I just got, or at least the feeler I just got is in that category, but I just want you to know that I have it." So he said, "Give me a copy of their letter." I said, "Oh, no, no." I said, "I don't want you to use it, this is just a conversation between us." "No," he said, "Let me have a copy please." He said, "I won't misuse it." So what he did, of course, is he went to the president, showed him the letter, you know, touted me up, whatever.

Bearison: Which is what the job of a Dean -- that's what all good Dean's ought to do.

Beilin: I knew the game, and before I knew it, I got promoted just like that. Now there were other big things happening in the university at the time. You know the way the city colleges were organized; each individual college was semi-autonomous. Just at that point, this is now in the middle of the sixties, the decision was made to organize the colleges into a university and in addition to that they would have a graduate school.

Bearison: Was there any particular reason why at that time that decision was made? What was happening that they finally said now is the time to do it, to combine all of these colleges and make them into a grand city university? What was happening in the times that led to this decision? I mean one of the things that I remember is that there were more and more people coming into colleges, the growth of university was incredible. The governmental funding for higher education was very strong then, but was that part of what was happening or --?

Beilin: I think all of those things were in the air, and I think also there must have been a political background to it as well.

Bearison: New York City politics.

Beilin: Yes. And the state legislature.

Bearison: Yes. Because Rockefeller was there then, right?

Beilin: Yes, that's right. And it was Rockefeller, I think, who was really pushing the idea, because there were also the medical schools that they were bringing into being and organizing them.

Bearison: And he was promoting the State University of New York, Rockefeller.

Beilin: That's right. You know, in situations like that, the teasing out all of the causal factors is a difficult thing. I wouldn't even hypothesize about that. But in any case, the graduate school was organized, and –

Bearison: So prior to this time there were no doctoral programs, there were just master's level programs at most, but no doctoral programs at all?

Beilin: No. And I taught in the master's program at Brooklyn, and that was about it. Now by that time I felt that my community, my basic intellectual community was not in Brooklyn, that it was dispersed all over, the people that I communicated with, went to meetings with and the like, was a kind of international community, and I felt I had my allegiances to the colleges, or Brooklyn College in particular. I worked very hard; I was on lots of committees. I was seen as a good citizen, and I tried to be one. I was also a bit of a goad; they had all kinds of distorted ideas of their productivity and their effect on the outside world.

Bearison: Whose they?

Beilin: The education department. There were some very good people on the faculty, excellent in fact, outstanding, and they sometimes gave them a hard time. The one that they thought was most obnoxious was Edgar Friedenberg. Edgar Friedenberg made a big name for himself in the 1960's. He was a southerner, a patrician. He, however, had found the common man, and he was extolling lower-class values in a middle-class world, and they just couldn't abide by his thinking. And he wrote a couple of very persuasive sixties-oriented books on adolescence. In any case, I was always very critical of them too, and there was always a little bit of tension, but not like with Edgar Friedenberg. Edgar Friedenberg ultimately left.

In any case, Frank Palmer, he was the psychologist on the Social Science Research Council, came to Al Bowker, who was the new chancellor of the university, came to him with the idea of setting up a developmental psychology doctoral program. And what he offered to Bowker was support for the first years operation, with money from, I think it was the Russell Sage Foundation, and support from the Social Science Research Council.

Now Frank Palmer was an interesting guy. He was originally a developmental psychologist and trained by Wayne Dennis at Lafayette. And he also played a very instrumental role in the transformation of developmental psychology. At that time the transition was being made from the normative, descriptive, psychometrically oriented developmental psychology into an experimentally oriented developmental psychology that was also very cognitive. Now I don't know if you remember, or maybe it was before your time, that there were a series of monographs, SRCD monographs that came out under the joint auspices with the Social Science Research Council, that had a number of people, Piaget and Inhelder, Bill Kessin and a number of others, make contributions that really signaled the change in the field. And from that point on the field was transformed, the journals were transformed, the general atmosphere was transformed, the annual meetings were transformed, all became very experimentally oriented. It went along with the philosophy at the time, that is, the predominant philosophy of science that infused all of the thinking, or lots of the thinking among developmentalist as well as the rest of the sciences, was logical empiricism, and logical positivism, and it all came together within that framework. So in any case, Frank Palmer could make a persuasive argument. He had the status, he had the money behind him, and they then put the developmental doctoral program into being.

This became important for me in that Frank Palmer was looking to identify people within the city colleges who had national or international reputations. That way he learned about me and he learned about a couple of other people like Alfred Castaneda who was a neobehaviorist, on the faculty at Queens, and a couple of others. In any case, we were brought together and we constituted a committee to design the program. And Wayne Dennis, who was the chairman of psychology at Brooklyn College at that time, was the senior figure among the group. And the program got constituted. There's a complicated history around my being transferred from Brooklyn College to the Graduate School, because I was also involved with the educational psychology doctoral program. In fact, I was the executive officer of that when it was first constituted, kind of acting executive officer.

In any case, Frank Palmer and I and the other people developed a doctoral program, and when I transferred over myself, I brought my grant with me, or maybe the grant came after I was here, I can't recall exactly. But this was about 1965 that we're talking about.

Bearison: Do you feel at the time that if there wasn't the Graduate School, eventually you would have left Brooklyn College simply because you didn't have the chance to work with doctoral students and this was a lack in your own career development?

Beilin: You know I can't be totally honest about that because I'm not sure.

Bearison: Had you ever thought about leaving Brooklyn College for that reason before that?

Beilin: Yes. On my – it was funny. When I got tenure, I think I got tenure on my fortieth birthday, and I remember going to a party at Marty Nass's house and I was very depressed so they asked me, "What's wrong? You don't seem to be yourself?" I said, "Well, I've just gotten tenure, it's my fortieth birthday and it looks like I might spend the rest of my life at Brooklyn College," and I said, "That's depressing." But the fact is, by that time I had spent ten years of my life outside of New York in the Navy, at Minnesota, in Connecticut. I came back to New York and I was happy being back in New York. And I realized I'm an inveterate New Yorker; I needed the city for it's cultural depth, it's wealth and the like, and whether I would have been tempted – I might have gone some place else if I had been offered a super position at a super university, but other than that I don't think I would have been tempted.

Bearison: Your transfer to the Graduate School of the City University is a good mark to sort of begin the next phase of your career, so we're going to stop soon, but I also want to know, I suspect that during this time you met your wife and you married?

Beilin: Oh, yes. Oh, yes.

Bearison: So tell us about that.

Beilin: When I was in Minnesota there was an undergraduate research assistant working on the same floor as I in the institute. She was working for Mildred Templin processing data, whatever, and I got to know her. I was a single guy. Her name was Lois Berkenpas; she was from a rural community in Holland, Minnesota. I got to know her, began to go out with her, although it was all done kind of surreptitiously because in those days it wasn't exactly kosher to be going out with an undergraduate, but my friends knew about it because we socialized with the other members of the faculty, my buddies.

When I came back to New York, when I left Minnesota, she and I retained our connection and she would come to New York periodically, and then we decided to get married, and we did with Gene Gollin as my best man. At that time we settled in Brooklyn, and then she went to work as a Librarian at first in an accounting firm, then she took a job with Gene Gollin as a research assistant with him at Queens College when he was there. And then she enrolled at NYU and got a master's degree in education, and then went on and got a doctoral degree at Teacher's College in social science education.

Bearison: And was this something you were very supportive of, that you liked the idea of her doing all this?

Beilin: Oh, yes. It was always important for me to have a girlfriend or wife who shared some kinds of interests, and with whom one could have an interesting conversation, though she had the feeling that I didn't encourage her

enough, actually, that I didn't point out avenues that she could take that would keep advancing her. And in some sense that was probably true, that I wasn't sure that she had the motivation to go on and do doctoral work, but she did. It turned out that she was very achievement oriented herself, and after Teacher's College she had a job teaching at first at Westchester and then got a job teaching at Hunter. She was on the Hunter College education faculty. We were married for twenty-seven years; we had two sons, both of whom have turned out to be historians.

Bearison: Academic historians?

Beilin: Yes.

Bearison: And when the oldest son was born, where were you at that point? You were an associate professor at Brooklyn College when he was born?

Beilin: Yes. We lived in Brooklyn Heights at that point. My oldest son was born in 1965, just at the time of the transition to the Graduate School, or it was just before the transition to Graduate School, the discussions were already going on and I was teaching here part time.

Bearison: And had Lois already decided that she was going to build a career for herself then?

Beilin: She was already on her way, or she was at that point in graduate school.

Bearison: So what was that like that you had a new baby come into the family and you're trying to build your career and suddenly your wife wants a career, I mean was this -- in those days that was sort of the cusp of the feminist movement, there were some women who thought this would be horrible and other women thought this was the way to go, they can have it all. How is it --?

Beilin: I was very supportive of that. I guess it was because I grew up in a family where women worked.

Bearison: That's right.

Beilin: My mother worked, and it was natural. Now my mother worked because it was economic necessity that pushed it, but –

Bearison: But, you know, given your own background and your mother and you being left alone so much, I could see you just going the other way and saying, "I want my children not to have to go through what I went through and I want their mother to be at home for them because my mother wasn't." And you said you were lonely, you'd come home and be alone and stuff, so it could just as easily go the other way because of your background, but it didn't.

Beilin: And then another important event occurred, in that I had a sabbatical coming up, and when my son was about one year old I also realized that we couldn't keep living in Brooklyn Heights because we had, what was in effect a one bedroom apartment. It happened to be the most magnificent apartment I've ever lived in, with eighteen-foot ceilings and mahogany paneled walls and two fireplaces, marble fireplaces, but we knew that we couldn't keep living there, so I decided to go on sabbatical. I arranged to go to England and I had an affiliation with the University of London Institute of Education through contacts. I had a kind of a sponsor there, it was Professor Vernon, and it turned out that was one of the most interesting years of my professional career.

Bearison: So you went with your baby and your wife to London?

Beilin: Yes. Friends arranged for us to have an apartment. We had an apartment on Clapham Common, facing the common. It was an unbelievably weird experience, but that's another story. But in any case, I got involved rather quickly with the life at the Institute, and almost by accident I got to know Richard Peters, who was a philosophy professor at the Institute. I had heard that he was giving a seminar on Piaget, so I approached him and I asked if I could sit in on it. He was very disdainful, really typical English off-putting professor, so he asked to know what my qualifications were, whatever. You know, I casually dropped the point that I was doing research and the like, and he grudgingly let me come in.

Well, the first session of this seminar he started to make all kinds of declarations about what Piaget's theories were about, that was his first mistake, so I began to correct him. You know, he looked at me like, "Who is this guy?" And the students were flabbergasted because nobody ever challenged the professor.

Bearison: And here's this typical American coming in to challenge him.

Beilin: Arrogant American in any case. But from that point on we got to be, I wouldn't say we got to be fast friends, he was kind of a remote kind of guy, but he became a real champion of me. And then one day he said, "I would like to arrange a little discussion between you and David Hamlin," David Hamlin was another well-known philosopher, anti-Piagetian. He said, "Let's have a gala event," you know, I didn't look forward to that, but I felt intimidated. So he arranged this debate between Hamlin and myself at his seminar, that's when I learned that you want to play the philosophers game, you're at a disadvantage to start with. They'll go from Plato and the presocratics, up to the present to undercut your argument, but they never –

Bearison: But they're a moving target, they never stay. I know that game. Okay. We're going to stop here.

[Continuation June 13, 2000]

Bearison: Today is Tuesday, June 13th, and this will be the fourth meeting with Harry Beilin in regard to the SRCD Oral History Project. So as I recall, the last time we sort of left off where you had come to the Graduate Center, right, and they were setting this place up. Is that where you sort of see yourself last?

Beilin: Yes. I'm trying to recall a number of events that seemed to happen all around the same time. I think I mentioned how I got into working on Piagetian matters.

Bearison: Yes.

Beilin: And that happened while I was still at Brooklyn College, and I had my first publication of a study, this training study with Irene Franklin. And concurrently, I worked on anagrams, I think as I mentioned.

Bearison: Yes.

Beilin: I worked with Reba Horn, and then I did a developmental study on anagram solution problem solving. I think it was also while I was still at Brooklyn College that I got involved with Martin Deutch and Cynthia Deutch on what later became The Institute for Developmental Studies, something like that. They were housed at first in Brooklyn at Downstate Medical Center.

Bearison: Who, the Deutch's?

Beilin: Yes. The Deutch's.

Bearison: But did they have a – where was their faculty appointment, was it at Brooklyn College?

Beilin: No. I think whatever it was they had some kind of adjunct status, I think, with the State University Medical School, and they worked with a man named Dr. Freedman who later went to the New York Medical College. I can't remember whether Freedman was originally involved in Brooklyn. In any case, what Martin Deutch was involved with was the beginning of what became to be known as the Compensatory Education Project or Program.

The idea at the time was that minority children, poverty children, basically black children, were by virtue of economic deprivation, unable to perform in school and that a program could be undertaken based on psychological principles, mainly cognitive principles that could overcome the defects of poverty by the institution of preschool programs in training, whatever.

Martin Deutch and Cynthia Deutch were really the true pioneers, at least among the first of the pioneers in this movement that ultimately became much better known as the Headstart Programs. At that time there were a few people around the country who were also instituting programs of one sort or another, Susan Gray, I think in North Carolina, Wykert in –

Bearison: Ypsilanti.

Beilin: Michigan.

Bearison: Ypsilanti, Michigan, wasn't it?

Beilin: Michigan, right. The Deutch's, I forget where they got their money mostly from; I think it may have been the Ford Foundation. In any case, they got bigger and bigger and then they moved to Manhattan to Harlem, East Harlem, at New York Medical College, that's where they developed their Institute for Developmental Studies. So I got involved with them as a consultant, and got very caught up in the whole compensatory education movement myself, and I worked with them for a number of years. And then, in a parallel fashion, I began to get involved with the mathematics education community, and I wrote a number of papers that reflected upon my notions about how cognitive developmental training, or cognitive training would help to overcome the defects or limitations in kids.

Now at that time it's not realized, I think people forget that the whole idea of the compensatory education movement was to, in compensating for the limitations of one's environment, compensate with basically cognitive developmental education programs, and it also involved language training too, Vera John was very much involved with the Deutch's as well. She was a kind of a psycholinguist or a linguist, anthropological linguist, actually, who was arguing at the time that it was the lack of language at home that was contributing to, in a major way to the deficits. This whole notion of deficit later was attacked by the Black Nationalist movement and that whole orientation was later rejected by lots of the early poverty workers.

Bearison: If you reflect back upon those times, it was a time, because the government had a lot of money, it was a very liberal attitude across the country, and part of that was this idea that psychology was going to cure the social ills, that psychology was the great white hope of the future.

Beilin: Right. Yes.

Bearison: Now how do you see that now looking back on those times, because, I mean, to what extent was it successful, to what extent was it totally misguided, to what extent was it just a political movement which had no real foundation in the science of psychology? How do you see all that now?

Beilin: There was no question, it really got caught up with political developments, and what was in the air was the conviction that psychology could help, and cognitive psychology in particular.

Bearison: That's right.

Beilin: And, of course, Piaget was among the leading lights providing theory and cognitive development and lots of people latched onto Piaget's ideas to provide the meat and the content of the early education programs. That's why the Deutch's were interested in me, for example. But the idea was to increase intellectual performance in children, that was the fatal flaw, that you could by way of training, reversing the affects of poverty, increase the functional intelligence of children so that they could get better test scores and the like. When the Headstart Program came in it had the same goal, and it didn't take long to discover that it wouldn't work and didn't work, whereupon what the Headstart program did was to change it's goals.

When you get a big movement like that going with lots of political muscle behind it, you can't let a program like that fail or go out of existence. The liberal black communities wouldn't stand for it, and especially if a lot of money was involved so that now the Headstart budget is in the billions annually.

So, looking back on this whole notion, the whole conception was flawed, and it never – there were all kinds of studies done to justify the programs and the movements, some of them were down right fraudulent, others were

simply oriented to changing, to looking for whatever evidence you could to the success of the programs, so they picked the most irrelevant kinds of ostensive gains.

So in the end, my position at that time was, and I wrote about it, was that of the total population of disadvantaged kids, in those days we used to refer to them as disadvantaged kids, you had about twenty percent who really could gain from these education programs. The problem was, how do you identify that twenty percent, and isolate them or encourage them, and neglect the others. It was a very unpopular kind of idea. But I felt that if you could identify the kids who could profit from the programs and you gave them – you put most of your resources with them, then over a series of generations you could really affect social change, and the idea never took hold with anybody, or hardly anybody. And I'm still convinced, in fact, of that.

In any case, I worked with the mathematics groups for a number of years until they became disenchanted with Piaget, and my own research in geometry was pursued parallel with that.

Around the same time, we're talking some time in the sixties, towards the end of the seventies, well things were happening in my own career and it's hard to try to keep track of things chronologically, because in retrospect if you look at the publication dates of some of the papers I wrote and the things that I did, it's difficult to sort out what really happened in terms of the time because lots of the events took place before the publication dates, obviously.

Now one of the things that happened that I got involved with, was with Piagetian affairs themselves. I was already publishing and getting known, and felt very much identified with the Piagetian movement. Around, I think it was 1962 or '65, between '62 and '65, Tom Bever and a colleague of his had a paper published in *Science* in which they reported undertaking an experiment on conservation which showed that Piaget's notions of conservation were basically incorrect, that with their method of testing they demonstrated children's understanding of conservation, and they reported a kind of u-shaped curve in which very young children showed conservation and then lost it and then regained it. I don't know if you recall that research?

Bearison: I remember it. Sure.

Beilin: Well, I read the paper in *Science*, I subscribed to *Science* at the time, and *Science*, of course, was a leading scientific journal in the country. Well, it represented a real blow from such a prestigious source coming in, an attack on Piaget just when Piaget was really getting off the ground. I wrote a note, a technical note pointing out the methodological deficiencies of the study that they had undertaken and suggested that the study been seen in that light and rejected. Well, the editors of *Science* sent my note out to review. The review that came back said, "Well, it's true that the study is flawed methodologically, but the results are so important that it justifies publishing the results," and the editors of *Science* rejected my note. Well, I immediately shot back a letter saying, "How was it possible that a journal that called itself *Science*, could admit that a study was methodologically flawed, but that the results somehow had significance?" I said, "The flies in the face of the canons of *Science* to hold a position like this," whereupon they wrote back and accepted my note.

In the meantime, while all of this toing and froing was going on, I undertook a study myself, in a sense, replicating their study without the methodological flaws, and by the time they accepted my technical note I had a report of the study, whereupon I sent it to them and I asked them to consider the publication of a longer article. They wrote back that they would accept the article, but they wouldn't publish the technical note. That was okay because of the fact that it pointed out the methodological difficulties and the like.

Now something else happened, which was rather important in my relations with Piaget. Piaget rarely reacted to studies that were critical of him. In this particular case, however, what I had heard from my friends, particular from Gilbert Voyat, who was his student teaching at City College, that Piaget sent a basically technical letter to *Science* arguing that Bever's study was worthless. The editors of *Science* turned it down. Now you don't just turn something down by Piaget, he was furious.

Interviewer: Who was furious with this?

Harry: Piaget was furious that they wouldn't give him the opportunity to respond to this devastating article, which he saw as flawed as well. So from what I understand, Piaget made contact with the Swiss Embassy, and the Swiss

Embassy got around through its contacts with *Science*, the journal, and persuaded them to give Piaget the opportunity to respond, and apparently it was effective because then Piaget was allowed to present a full-blown analysis of the study, which they then published as a feature article.

There was a question as to whose paper should be published first, Piaget's or my own, and in the conversations with the editors, telephone conversations, I pointed out that mine had been sent in first and it's a simple timeline consideration that mine should be published first, and that, in fact, is what happened.

Tom Bever would never acknowledge, he was wrong, but he conceded that what he was studying was not conservation, but it was something more important, so that was that.

Bearison: Where was Bever at that time?

Beilin: Bever at that time, was at Rockefeller University. Now Tom Bever, interestingly enough, was one of the first post-doc's of Jerry Fodor, and George Miller. And I always thought that there was a connection, that particularly George Miller had played a role behind the scenes in the publication of that article. But Tom Bever and I had good personal relations. We never got to be really good friends but we did socialize later, although there was always this difference between us intellectually.

Bearison: Where is he today, Bever?

Beilin: I think he went to the University of Rochester.
Bearison: But you don't hear much of him in terms of publications and stuff.

Beilin: No. Around the same time my own work was getting to be better and better known, and as a consequence of the grant that I had from NIMH on the training study that I was undertaking, the results of which got published in *The Journal of Experimental Child Psychology*, I got put on the Editorial Board of *The Journal of Experimental Child Psychology*, and I began to do a lot of reviewing of papers for them. And then out of the blue came a question as to whether I would agree to be editor of the journal. Now, I accepted, of course, but it's hard in retrospect to realize what a revolutionary thing it was for somebody like myself to be made the editor of that journal. The journal was largely the work of Skinnerian's.

Bearison: That's right.

Beilin: And the first editor was a Skinnerian, the founding editor, and when he decided to retire, I don't know whether it was he that suggested my name, but in any case, what I heard later was there was great anxiety in the community that ordinarily got published there about having a Piagetian at the helm. As it turned out, I don't know if I ever published a single Piagetian paper, but I published a great many Skinnerian papers.

What it reflected was, the experimental model I was using vis-à-vis, Piaget's work was succeeding in the larger mainstream community. My model in this originally was Jack Wohlwill's. I don't know if I mentioned this before, but Jack Wohlwill in doing his early Piagetian studies was approaching them experimentally and doing experiments, and this is what I did. My feeling was that Piaget would never be acceptable to mainstream psychology unless the results of research, done from an experimental point of view, would confirm or establish the viability of the theory. I guess to some extent it worked, otherwise they never would have thought of me in relation to a journal like *The Journal of Experimental Child Psychology.*

Bearison: But you know Wohlwill turned his back on Piaget, while you never did that. I mean, he really became very anti-Piagetian toward the end.

Beilin: Well, he changed.

Bearison: Yes.

Beilin: After a few years as editor of the journal, I decided I had enough of that. It was very demanding. Journal editing puts you on a treadmill that you'd never get off, even when you go on vacation, it's keeps going and then you just have to make up the deficit afterwards, and I found that it was interfering with my research, so I gave it up.

At that point, Larry Erlbaum, who was the psychology editor at Academic Press, asked if I would take over the *Developmental Psychology* series. I think it had been under the editorship of David Palermo, and it had another name. And I think that he wanted out from that, so I agreed and I took it over and I became the editor of *The Developmental Psychology Series*. I led that series for maybe eleven years, something like that, and during that period we published about forty books, some got to be very good sellers and drew a lot of attention, and others turned out to be pretty flat and uninteresting.

With a journal like JECP, you have really enormous power. When I was asked to join a review panel for one of the federal agencies, I refused to do it on the grounds that I didn't think that a single person should have that much power, being a gatekeeper in a journal as well as in a granting agency. I don't think there were too many people who shared my point of view, but in any case, that was my feeling.

After a while I gave up the editorship of the book series, although by that time there was a new psychology editor at Academic Press, whom I got along with actually very well, Larry Erlbaum, who had been the psychology editor at Academic, had gone off and created his own publishing house, and I got to be at that point, a consulting and acquisitions editor for him. I didn't bring as many books to Erlbaum as I had with Academic Press, but the fact is I still am at this date, doing the same kind of work with him.

Now my relations with Piaget always had this funny up and down character. Another event that occurred in which we had occasion to meet, and in which he got very exercised with me, was an occasion when he came to New York, actually to the Graduate Center to give a talk. It had been arranged by Gilbert Voyat, and in conjunction with the clinical training program at City College. They didn't want to hold the meeting up at City College for the obvious reasons, that being up at Harlem at that time was not the best place to be holding a meeting. So they asked me if I would be the local host at the Graduate Center, which I was happy to do. Then as the meeting approached, the idea was that Piaget was to have a public address that was open to everybody in the university community and outside the university community, and a private meeting with the clinical psychology students and faculty, which was to be closed to everyone else. As the time approached, I got a call from an editorial writer at the New York Times who had heard about the private meeting as well as the public one, actually through a cousin of his who was teaching at Yale University, and was a developmental psychologist. He asked me if he could attend the private meeting. He was very honest in telling me that he had been cultural affairs editor in Paris for the New York Times, and he had tried to interview Piaget many times, all unsuccessfully, but he thought that if he only could just be allowed to come to the meeting and listen, that that might be okay. I said that I could not allow that to occur without Piaget's explicit permission, so I got in touch with Gilbert Voyat and I asked – I put the question to him. The answer came back that Piaget would rather not have him there, so I conveyed this to the guy and I told him that he was welcome to attend the public meeting and that would be that.

Well, we came to the private meeting. There were a lot of people. There was a lot of energy there. A lot of excitement, and the New York Times guy comes up to me, identifies himself, and said he could see no reason why anybody would object at that point to his just going in. So I pointed out to him that Piaget had said, "No." Well, the guy wouldn't take no for an answer, and he more or less muscled his way into the meeting. Okay. Piaget gave his more or less informal talk, and then opened it up to questions, and there were lots of questions about Freud and psychoanalysis. And Piaget takes off and says that in effect he didn't think that psychoanalysis in the long run, that is, as a theory, would have a future, largely because of developments in neuroscience and biological psychology, that Freud's insights were already being undermined. He didn't say that the therapy would disappear, but I don't recall that anybody asked directly on that score.

After the meeting I got to talking with the reporter again, and from the questions he was asking me, I began to have some trepidations about what was going to appear in the New York Times. He kept asking me, "Why are people so upset about what Piaget said about Freud?" So I said something to the effect, "Well, if a theory is standing on quicksand and you see that it's going to go under and you happen to be involved, you might be a little concerned about this." So he kept asking questions of that sort, and I said, "Look, remember, I told you that Piaget did not want to have you here. If you report the content of what you're asking me about it can be a lot of trouble." I said, "I

would appreciate it if you would give me the privilege to at least reacting to what you write before you publish it." "Oh," he said, "Absolutely." He says, "I'll send you a copy immediately."

Bearison: I have to tell you that that goes against every rule of the New York Times, they do not do that for anybody, so he's just bull-shitting you, but go ahead.

Beilin: I won't say whether I believed him or not, but you know I hoped he would be an honorable guy about it. Well, then there was the public meeting in which nothing was said about Freud, but he was reporting on his own theoretical work and research. The next day The New York Times appears, and the headline in the second section says, "Piaget condemns psychoanalysis." As soon as I saw it, I knew there was going to be trouble. And sure enough, Piaget was livid, unbelievably livid. And half of his ire, of course, was directed to The New York Times, but the other half was directed at me, because he thought that I had engineered the whole thing.

Bearison: You also forget to mention that the entire clinical psych program at City College rests on, in those days, on psychoanalysis, so he's also putting down the very people who invited him to come and speak in the eyes of The New York Times, because – so they're livid too, I guess.

Beilin: Well, it's even worse than that. Piaget had very close ties to the psychoanalytic community and saw this as a total violation. I shot off a letter to The New York Times immediately. I pointed out that the writer had totally misrepresented Piaget's position on psychoanalysis, and that I more or less identified the difference between the theory and the practice, which the writer had totally disregarded and so forth. And not only that, I made some mention that the fellow had violated an agreement with me. Piaget shot off a letter to The New York Times too, which Gilbert Voyat translated into English. Gilbert showed me the letter, and as soon as I saw it I knew The New York Times would never publish it, because he was carrying on about, "If they only knew about his writing they would know, blah, blah, blah." It was a very belligerent letter, and it would never get published I was sure. My own letter was very calm, very measured, stuck to the facts and also the relations between the reporter and myself and the violation of a trust. The New York Times editors, the editorial page editors were very distressed. They called me and tried to verify the facts and all of that and said that they would publish my letter. In the meantime I wrote – Oh, that wasn't the end of it. The next day I get calls from Time Magazine, and Newsweek, asking me about this whole fiasco. Well, they didn't see this as fiasco, they asked in terms of "What's the significance of all of this?"

Bearison: Was this because your name had been cited in the article?

Beilin: I don't remember. It's quite possible, but he made reference to me, which may have been why Piaget thought that I was complicit in all of this. What I succeeded in doing was dampening all of the interest and enthusiasm of these national journals to report on this thing because I told them it was all inaccurate and Piaget wouldn't endorse it and all of that, so they backed off right away. Well, I got a guy on the telephone who had a talk show, it was one of the small stations, and he starts asking me about the whole thing too, I was on his program for between a half an hour and an hour, talking about Piaget and psychoanalysis and all of that. And I sent off a letter right away to Piaget, through Voyat. I explained what had happened, how it had happened, my own position on this, which was first of all sympathetic with Piaget, and then secondly, tried to protect him and things got out of hand, largely through no part of my own, and that kind of quieted Piaget, except for one thing, the fact The New York Times would not publish his letter infuriated him no end. And not only that, that they published my letter. After the *Science* fiasco, he didn't need this, so you can just imagine what his feelings were about me.

In any case, the anticlimax came when Piaget was going to Baltimore, I think he was going to get an honorary degree from someplace.

Bearison: At Johns Hopkins.

Beilin: Maybe Johns Hopkins. So the reporter called me and he asked me if he could go along in the car with Piaget. He wouldn't say a word, he wouldn't ask Piaget any questions, he would just listen, right? You know, I laughed, I thought the guy was crazy, so I told him "No way." I didn't have to go to Piaget to ask him if this was okay, I said, "No way, forget about it." Well, you think this was the end of it. It's not the end of it. About a year later, Piaget was given an award from a small foundation that had recently been set up, I forget the name of it. He was given an award of about twenty-five thousand dollars. And they had a dinner for Piaget at the Harmony Club, it

was a black-tie affair, the first time I had to put on a black-tie in a long time, and then the next day there was to be a lecture, more open kind of lecture. Well, at the dinner when the award was given, Piaget was very cordial to me. It was almost as though nothing had ever happened.

Bearison: Did you speak French to Piaget, or did you have a translator?

Beilin: No, always somebody translating, Gilbert in this particular case. Then the next day Piaget starts his public address. He said he was very happy to be in New York, very happy to get the award, except for one thing, he was in the same city as The New York Times, and The New York Times did not have the courtesy to allow him to comment on an article that had maligned him, and that it was all very well and good to publish Harry Beilin's letter, but they had an obligation to publish his own, which they had not honored. So it was clear to me that Piaget never forgets, but that more or less ended that episode. I'm trying to think of what I should comment on next.

Bearison: Well, you know, if we stay with – I like the idea of staying with this sort of thematic narrative rather than a chronological one, so we stay with the Piaget thing. I mean in the end, you've proven, at least in America, to be probably the primary foremost supporter of Piaget's ideas, long after a lot of people began to question if not the validity of his position, the relevance of his position. How do you see this, I mean is this a great disappointment for you or do you think, hey this is just science marching on and the cold Kuhnian paradigm shift? Where do you see yourself in this?

Beilin: Well, I had devoted, obviously, a great deal of my own research efforts in this direction, that is, within the Piagetian framework. My basic attitude towards Piaget and his theory was (I may have stated this before) I was interested in testing the theory in a way that would improve on the theory. I had another controversy, for example, with Piaget over language. Now this will be a long answer to your question.

After the grant on the training studies, I got interested in language, language acquisition. As I may have mentioned, the Chomsky revolution was taking place, and they made very strong claims about modularity and the like. The basic Piagetian position was that language did not appear sui generis, that when language did develop, it developed out of cognitive processes and there was a prelinguistic period during which the preparation in the sense of the cognitive resources of the child were being developed.

So I got interested in doing some language type research, and I got a grant from NICHD to study the cognitive basis of language development. I was very lucky – I was lucky and unlucky. I had one wonderful research assistant in Barbara Lust, who worked with me. And I also had some others who weren't so happy to have, and with whom I had some rather difficult times. In any case, one of my early studies was to show that you could train – In fact in the training studies I showed that you could train logical operations linguistically through verbal rule instruction, and it was in fact the most effective method for instating logical structures. The Piagetians basically denied that. The main Piagetian language development person was Mimi Sinclair, and she had ostensibly done a study of a parallel nature and showed that you couldn't instate the system, and they always insisted on that.

In any case, I eventually, with my students and assistants, published a book called *The Cognitive Basis of Language Development*. It came out just about the time that there was a wholesale rejection of Piagetian-type thinking about language. The book got two kinds of reviews. It got over-praised, you know, 'the greatest thing since gangbusters,' and also panned, terribly. Eventually, the book had very little influence on the field. And I came out with some proposals about language development, which I still think are valid, that the reason that you could instate a linguistically-based system for logical operations was that it provided a kind or created a kind of algorithm, which the child could use until he really understood what the cognitive system was really about. But in the meantime, he could solve problems of a similar nature. In any case, this was another idea that never flew, but I still kept at it. From the language studies I went into studies of geometry and the study of strategies and structures, largely because there was a new thrust in the development of Piagetian thinking, to emphasize strategies in addition to the structural features of development, and a more functional type of development. And, of course, some other people picked up on similar ideas, like Siegler who exploited the whole strategy notion.

Now the long-term effect of the increasing hostility to Piaget, of course, was to diminish interest progressively in Piagetian research and theory. It happened over a long period of time, it didn't happen suddenly because people were sniping at Piaget from the start, from the 1920's and '30's, and the post-War work just added a whole new

cadre of hostile workers. Some of the people working in this field seemingly sympathetic to Piaget, were in fact trying to undermine him, like Rochelle Gellman and, in fact, Flavell himself.

Now I stuck, in a sense, loyally to the Piagetian framework even though I was critical of certain features of it, its position on language, its position on training, which were serious issues in the theory. In any case, I was also one of those who tracked the developments in the theory very carefully over a long period of time, and I wrote about the philosophical and conceptual changes that were taking place. And the most important one came, of course, right after his death in 1980 with the publication of the last book when his work with Garcia practically undercut the foundations of what was the standard theory, the logic based theory; the extensional logic theory, basically. And at that point I began to write about the new Piaget.

[Continuing July 11, 2000]

Bearison: Today is Tuesday, July 11th, 2000 and this will be our fifth meeting with Harry Beilin for the SRCD Oral History Project. So how long has it been since we've talked last, three weeks?

Beilin: Yes, about three weeks. The question you asked related generally to my place within the Piagetian approach to developmental psychology and the changes that have taken place and what my own feelings are about the changes. It requires first understanding the nature of the changes that have taken place in Piagetian views and the whole literature. I, of course, got in on the ground floor at least in the American scene, and once I got into it I tracked it very carefully, precisely.

A number of important changes took place in Piaget's thinking and in the thinking of the people in Geneva. The changes were monumental in many senses. Of course the greatest change occurred during the war years or more precisely - before the war years. After the early work of Piaget, and the new work turned out to be known as the standard view, his structuralism, the logicist or logical orientation of the theory. That's the point at which I entered onto the scene myself.

Now the changes that began to take place were sometimes very gradual and other times they weren't so gradual. The studies on language, for example, were affected or occasioned by Mimi Sinclair's obtaining her doctorate in Geneva. And I got involved with their language studies just about the same or approximately the same time the Genevan work added a important dimension to discussions on the nature of the relationship between language and thought.

Later, there were the changes in the logics that Piaget was interested in, new developments in logical theory affected his own orientation and work like the developments in category theory, there were developments in catastrophe theory, and all of these added a new dimension to Piaget's thinking. And each one of these I more or less tracked, read about them, commented on them. Another big change that took place was on the thinking about strategies and the role of strategies in thought, and this was occasioned by the work of Barbel Inhelder mostly after she had spent a year in Pittsburgh with the Carnegie Mellon Group, which resulted in a book on learning and development. That added an important change that many people didn't recognize as occasioning a fundamental change in Piaget's theory.

Later on toward the end of his life the greatest thing that occurred and the most fundamental to the theory is what I characterize in my articles and papers as 'Piaget's New Theory,' and this was organized around, or came out of a series of books, the most important of which was the work that he did with Garcia on *The Logic of Meaning*. *The Logic of Meaning* book was so radical, and it came so late in the Piagetian corpus and was accompanied by the change in attitudes, or at least parallel to changes in attitudes towards Piaget's theory at that time, that the very radical nature and the change in the theory has more or less passed by most developmental psychologists and psychologists in general.

Bearison: I want to jump in here for two reasons. One is, I never really quite – I think one of the reasons why he is passed by is because I don't think it's very clearly stated what he really means by 'a logic of meanings.' And I read it, and it's more like a promise of things to come than what it is, and so there's not

much there. But the other more basic thing I want to bring up with you is that one could argue that what we have seen is a real Thomas Kuhn notion of a paradigmatic shift, that it didn't matter what Piaget had done at this point close to his death. It didn't matter whether he really explained the logic of meanings, what we had seen was a total change in what the field thought was relevant in accounting for psychology and cognitive developmental psychology, and the relevance shifted and it shifted toward issues of context and culture and away from the search for the universals, and epistemology became like a meaningless term. So when you respond to that, it's not that, yes Piaget changed and stuff, it's like he could have done anything at the end, that really we were in the midst of a Kuhnian revolution and it had nothing to do with Piaget, it was much bigger than Piaget and well, the relevancies changed for much more cosmic reasons than Piaget's work.

Beilin: Well, there are two answers to that. First is, I agree that a monumental change took place in developmental psychology and psychology in general. The shift towards an emphasis on culture and on context was long in coming actually, it wasn't so revolutionary in the Kuhnian sense, it didn't occur overnight.

Bearison: No.

Beilin: It actually built up as a consequence of changes that were taking place in the 1960's. And the whole shift in intellectual life and the fact that it has occurred as a consequence of the events of the 60's, 70's and 80's, finally caught up with psychology. And you're right, Piaget's work got swept aside because of its emphasis on universalism, it's commitment to enlightenment rationality, and a number of other features. So in a larger sense, sure, Piaget was swept aside by larger currents, but what I see as happening to Piaget's changes in thinking in the later work, the emphasis on meaning for example, is consistent with those changes. Piaget, through his discussions with Garcia, began to adopt subtly some of the views that are represented in post-modernist thought, at least that's the way I've been interpreting them.

Interviewer: Aside from Piaget, what about you? How did you see your place in this? We see this shift occurring, and it's not revolutionary, and I don't think it started – maybe the precursors were in the 60's, but I sort of see it in more of the mid 70's to the 80's, but how do you see yourself? I mean, you look back on your earlier work, does it seem irrelevant now because the whole Zeitgeist shifted, and do you feel that much of your career has sort of been left out on the edge because of this new shift and new interest, and do you see it maybe that the cycle coming back again in structuralism and universalism, and rationality will be relevant again, and this old stuff will come back? Where do you see yourself in all this shifting?

Beilin: Well, that's a good question of course. No, I experienced what happened to my colleagues who were central to behaviorism and neobehaviorism. What happened to those poor souls when cognitivism came in, and I had some very good friends who were in the older movement so to speak. Suddenly they were technologically unemployed, they were redundant, what choices did they have? Well, they could either change themselves, become cognitivists, and this is what happened to a number of them. Others persisted and changed very little. David Palermo is an example of one who changed. The Kendler's, for example, did not change and they have persisted in their views.

Now when things began to happen with Piaget they began to occur late in my career. What choice did I have? Should I have adopted the contextualist point of view, the cultural orientation? I did not change for a simple reason, for ideologically I was not favorable to the nature of the changes. And the reasons are complex, but largely because I viewed the changes that took place as having a general political undercurrent that I was not sympathetic with. You want to start the changes in the 70's and 80's. I have a really good reasons for doing it in the 60's because I think the political changes that took place then were of such momentous nature and they had a political bias to them that the changes that took place within psychology, the shift, for example, to Vygotskianism and the like, had in fact, again, an undercurrent of a political point of view. I got into a debate with Klaus Riegel, for example, who was openly Marxist in his orientation, and it was no secret about where they were coming from, some of these guys. Not all of them, everybody went into this cultural orientation for many different reasons.

Bearison: But their sense of Marxism, at least as I saw it, we're not talking about Communism here, we're talking about Marxism, and they never thought – because that goes back to the 40's and 30's, but they never saw themselves at Communist when you say political, right?

Beilin: Right. Yes.

Bearison: Okay. I wanted to make that clear.
Beilin: They were ideological Marxists.

Bearison: Yes. Okay. That's how I see it.

Beilin: Now what their real political thinking was, whether being Communist or not, is another matter that never became an issue within the intellectual circles that I was familiar with.

Bearison: That's right.

Beilin: In any case, I was not disposed to going along with it. I started off actually being a kind of quasi Marxist early in my career, but I got cured of that pretty quickly and I was never sympathetic to Marxists thinking, orientation, ideology, however you want to characterize it, so when the changes began to take place within psychology generally with the shift to political activism and the concerns with social issues, for example, more generally in the field and in particular in developmental psychology, and in its institutional forms, for example, the structure of SRCD and the like, I was not particular sympathetic with it, so that when the changes began to take place rejecting say the Piagetian view, I was not about to go along with the opposition. And maybe it's just the rigidities of an older man, though I don't think of myself as an older man even though I am. I believe in the particular point of view. I did not go along with Piaget on everything that he – on the changes that took place with him. You're absolutely right that the last works are really only programmatic. They don't lay out a well-defined theoretical position, vis-à-vis even the standard theory. He says that the logical, the structure of the logic, the truth-table logic, for example, has to undergo some kind of revision, but he doesn't say how. The *Logic of Meaning* is programmatic in its general form, although he makes some very specific suggestions. For example, he refers to the early influence of inferential processes in the sensory motor period, which is a radical rethinking of his early ideas, and a number of other proposals are very specific, but they don't really extend enough to represent a theory. And what's happened is that no one has really picked up on it in a really systematic way so that everything in the Piagetian corpus is in limbo at the moment for the most part.

Now what's my feeling about that? What's the future hold for Piagetian psychology?

Bearison: I want to get to that, but before, you were talking more about where your resistance to the change and stuff. Do you feel that it served to marginalize you either among your immediate colleagues or the broader set of colleagues, but also among students that you had been working with? Did you feel marginalized when this happened and you felt --? Can you talk a little about that?

Beilin: The answer is yes.

Bearison: Yes.

Beilin: Locally I felt increasingly marginalized; there was more and more resistance among the faculty here in the program to even the structure of the courses that I was teaching. It was more or less understood early on that my one-year sequence in cognitive development, for example, in which I focused one semester on Piaget and then the second semester on everything but Piaget, that is, in cognitive development. Well, more and more the point of view, I think, came across to the students from the faculty, not necessarily people like yourself, but others, whom I needn't mention, who more and more were telling the students that they didn't have to take my courses, so I felt that increasing alienation and the like locally.

In the broader picture, that is, in the field with Piaget's marginalization, naturally I became somewhat marginalized too, although because I was writing about the new Piaget and changes, people were still paying attention, some people at least were still paying attention, so that I never felt totally isolated from the field, and even now there are references to that work. Now my feeling is that the field goes in cycles, intellectual life goes in cycles –

Bearison: I want to get to the future and the cycles because I'm curious about that, but I also want to comment that I also felt that in some sense as people started making a shift away from Piaget, but you stayed with Piaget and struggled to reinterpret where the theory was evolving, that in some sense it gave you a level

of prominence that you didn't have before, so that it wasn't simply that you were being marginalized, you might have been on a local level, but I think on an international level you were gaining prominence because so many people were leaving the field and you stayed with it but just not in a static way but in a dynamic way, showing that this is a theory that's right for further evolution. So I think you –

Beilin: That's true, and in fact, suddenly I developed some cache' in Geneva, which I never had before. One of the – I guess the last time I was in Geneva, for example, Barbel Inhelder invited me out to lunch, just the two of us. She never said, you know, "We thank you very much for what you've been doing," although I did hear that through third parties. I ran into Jerry Bruner, for example, on the street one day and he told me, you know, "I was in Geneva and I was talking to Barbel Inhelder, and she was commenting on how important this work was that you were doing and all of that.

Bearison: That's good.

Beilin: So nobody had to say anything to me, but I knew that the fact it was true. Now, that didn't mean that there wasn't any opposition to my ideas in Geneva, there was from Jacques Voneche, for example, who was the, as I see him, the fox in the hen house who is ostensively in charge of the whole Piagetian operation, but he's not really a Piagetian he's an anti-Piagetian as I see it. So it's a mixed picture, but –

Bearison: I'd be curious to know about that, but not for this – about Voneche.

Beilin: But you're right. Increasingly as the field begins to get thinned out I stand out as one of the committed people. Now, I do have –

Bearison: Which, I think, again is something for you to feel good about. I mean it is a real –
Beilin: I do. And I really have a deep conviction that people will come back to Piaget. If for no other reason that the later books, not only the book on the meanings, but the last three or four are rich, unbelievably rich in experiments that no one has really exploited at all, and there's also the conceptual base which is pretty complicated. But the cleverness, the originality of many of the experiments just sit there waiting to be worked on, exploited, whatever.

Bearison: But what would it take for the cycle to bring Piaget back, I mean will it take a shift in the Zeitgeist --?

Beilin: It will never come back in its original form.

Bearison: Yes.

Beilin: It will always be just adapted in one way or another.

Bearison: But would it take a renewed interest in universals, in structures of development to bring back Piaget?

Beilin: It would be something like that. I'm loath to make predictions about the future.

Bearison: How do you see this, I mean right now this is a time of tension in development and it's been that way for a while, but I think it's growing when you think of the tension between technological advances in the biology and plus the social-cultural context, so that you know it's like they're competing in their struggles now to find some way to coordinate advances in these two fields so that one isn't against the other.

Beilin: Well, the way I see it the effect of context, on environment, on cultural, will never go away, I mean it's always an element in the picture. But increasingly, as I, in fact, pointed out a number of years ago, the direction in which the field is going to go and has gone is in the enrichment of the biological sciences, the incorporation of more and more of biology into developmental studies. And this happened with the whole neuroscience movement and the like.

Bearison: Well, I think it's happened, but has it happened in developmental psych so much as other fields, areas of psychology?

Beilin: Well, not as much.

Bearison: And certainly not in this program it's not.

Beilin: No, not in this program, but in the field, yes it's creeping in more and the discussions of evolutionary psychology and the like are – some people have the view that the field of developmental psychology will have been preempted by each of these independent movements.

Bearison: That's right.

Beilin: Whether that'll be the case or not, I don't know, but there's no question that increasingly biological thinking will enter into the mix. And I don't mean it in the sense of the naivitist thinking, for example, people like Susan Carey or Rochelle Gellman and the like, it'll take different forms, but there's a certain indefinitability about it I think. In a funny way I'm still an optimist about Piaget and I have no doubt that ultimately people will come back to examine the richness that lies there.

Now I want to bring up another interest of mine that we haven't talked about. Some time around the late 1970's, almost 1980, I had a next door neighbor at the Graduate Center, Stanley Milgram. We had adjoining offices, we saw each other casually, I didn't know him terribly well, he didn't know me terribly well, but he came into my office one day and he saw photographs on my wall and he said, "Who did these?" And I told him that I had. Well, he was somewhat overwhelmed by the particular photograph, so I have to backtrack a little bit.

I became a photographer sometime in the 1960's, actually while I was on sabbatical at the University of London. I began to do some photography, and that interest kept increasing. When I got back to the States I enrolled in some courses at the new school actually. I worked with – I had as my mentor, I call him that, Joseph Breitenbach, who was one of the old style photographers who recently had a revival, a bit of a revival of his work. And out of a large class, he picked me out and my work and asked me to join a small group that he was working with on his own when the course that I was taking finished. So I worked with him and this local group of students for six months, and more and more I began to do my own work photographing and I got better and better. I built a darkroom, did my own printing, enlarging and the like. I also began to read in the photographic literature, in the literature about photography itself, and I became increasingly aware that there were a number of people, philosophers, even psychologists like Arnheim, that's right, Rudolph Arnheim, and art historians like Gombrich and the like, who were talking about perceptual processes, cognitive processes and the like, and I became aware that there was something that you could call a psychology of photography that nobody was really exploring.

Well, Stanley Milgram comes in to my office and we discover that we have a common interest. He was very much interested in the film median, but in particular photography. So I don't know whether it was he or I, but one of us proposed doing a seminar together on the psychology of photography, and to my knowledge it was probably the first course and seminar ever devoted to that topic. Most of the students who showed up were from the personality program, personality and social program, two from the environmental program. I think there may have been one student from developmental. It turned out to be a wonderful, wonderful seminar. Milgram's interests and mine complemented each other. I was much more theoretical, he was much more practical, empirical with all kinds of clever ideas about little studies to do and the like, and all of the students undertook research of one kind or another. It was a wonderful experience. What I decided to do was to continue with a seminar of my own, but devoted to the developmental aspects of the new psychology as the topic, and I had a number of students who had enrolled in that, the word had gotten around. And from then on, about 1980 until the present, I've continued with doing research. I've had a number of students who developed doctoral thesis on photography, some of which were really very good. So I've gotten another, in a sense, area of visibility in the field as a person who is committed to a psychology of photography. It's never really taken off as a big thing, but it has established itself as a point of focus within a particular domain of visual representation. And I have been invited to write chapters, papers and the like, and that was a very satisfying and important part of my professional life in development. I don't do it anymore, but hopefully other people will continue.

Bearison: Are you still a photographer though?

Beilin: Well, since – yes and no. When I became ill a couple years ago I couldn't work in the darkroom any more, it actually became too dangerous for my lungs, and also I didn't have the stamina to do a lot of the work that's required, carrying heavy equipment and all of that, so I sort of sloughed off on that too.

Bearison: I'm going to ask you something because in any oral history, a professor, even you've got a group for this, so I'll just ask. How do you see the role of students in your work in your career? You know, some people say, "You know, I learnt the most I ever learned from my students," the others say, "Well, you know, it's a burden, it's a job you have to do here," so it's usually people fall somewhere in between. How do you see the role of students in your career? And not just the roles, but the practices of teaching?

Beilin: I understand the import of the question. Interestingly enough, students have been in an indirect sense important to my career or to my intellectual life, in that I always took my teaching very seriously. It was very important for me to be current in the field, and I always felt that there was an important responsibility of mine to ensure that students had a broad vision of the field as well as deep one. My courses changed constantly as a result of that, so I wouldn't say that students were important in a individual sense as much as they were important in a more general sense.

Bearison: Yes.

Beilin: One of the reasons is I was a very demanding taskmaster. I was very tough. My standards were very high, my expectations were very high of students, and many students couldn't take that, and I don't see myself as being the most popular of professors. They either loved me or hated me, or avoided me. It was more than one student, for example, that would end up crying in my office because I didn't praise her enough, didn't, after a half hour of criticizing a paper or dissertation, end up saying, "Well, you know, this is really wonderful work," or something of this sort. So I was tough in that sense. I developed a small group of loyal students who really found my teaching, mentoring and the like, gratifying, exciting.

Over the years I've encountered former students, students who really gave me a hard time. And they would tell me in retrospect how important my teaching was and how my views were important. I've always been astonished too, not just students, but colleagues. I go to meetings these days and someone will tell me of a remark that I made to them that seemed to influence them in one way or another. I've always been curious about that effect, because I always have this feeling about myself that I'm a somewhat shy, quiet kind of guy, that seems to be belied by what people tell me. So I know that I've influenced a lot of people and in a good way, and it's always given me a lot of satisfaction when I think about that.

Bearison: The other day I was reading an article in the New Yorker, and actually it's an article about this physician who works with the peasants in Haiti and its like he's a real Albert Sweitzer kind of character, so this is where this little quote comes from, but it reminded me of you, so I thought part of this, I would read you this quote. It says, "In my life among my friends and among my professional colleagues I enter fights that I know I can't win and I think, that's all right, I'm prepared for defeat. But I also know of my small victories, and yes, I love them." I mean, I thought of you but do you see yourself in that remark?

Beilin: Yes. That's right. I've always had the feeling myself as engaged in an uphill battle with life, with my relations, with the demands of the world, of the culture. And that I've never felt that anything came to me easily, that I had to work for it, fight for it even, and that in a way I've looked back and I say I've really been surprisingly successful in many ways. In individual battles it's never bothered me knowing that I would lose, for example, around here at the university, I was always in a way the odd man out. My views were always in the minority. I was fighting battles constantly and losing them, but it never stopped me because I've always had this feeling of being right. And in the end of having the fruits of my labors in thinking come back to and knowing in fact in many cases I have been right. So it never bothered me to lose a battle, because I always felt in some sense I would win the war. I've also lost battles, but they never discouraged me in a sense. I think that there's a certain toughness that I have that keeps me in there in the fight, and that I guess it's the one thing that's sustained me, otherwise I would have felt defeated because the world changes constantly. The professional world that I live in changes constantly but it's evident that I don't get totally discouraged by it.

Bearison: I certainly think that that's one of your more remarkable qualities that I admire, that is this ability to stay with it and to know when you're right, and to assert what you think is right and not to be defeated, and just to keep going on and that you're a strength to be reckoned with.

Beilin: Thank you.

Bearison: One of the other questions, we're sort of cutting across laterally now, topics, you know, to ask developmental psychologists. You have two grown sons now, is that it?

Beilin: Yes.

Bearison: Do you think raising these guys in any way, how did it contribute to your understanding of children's development and cognitive development, or did it not? It's like, hey that's my family, that's home, here's my office I really don't mix the two. What's your relationship with your thinking about children developing and having two children of your own?

Beilin: Well, firstly I never – before I got married, I never really thought about children. I really didn't like children, to be honest about it.

Bearison: Why not?

Beilin: Maybe it was because I had such a terrible childhood myself. It's the only way I can explain it, maybe a psychoanalyst could explain it better. But in any case I didn't want children. After I got married, my wife very much wanted children and I went along. It turned out that my children became the most important things in my life, and when they were very young, because I was heavily immersed in Piagetian thinking, what I didn't want to intrude was any of the technical aspects of the work with my kids. I didn't have them sit down and do all the Piagetian tasks. In several ways –

Bearison: But also at that time, you're living with their mother and their mothers a psychologist also wasn't she?

Beilin: No, she was an educator.

Bearison: Oh, okay. But she had some professional interests just as well though.

Beilin: What she actually did for her dissertation was a Piagetian dissertation, but that was largely through my influence on it. She did a study on object perception and whatever.

Bearison: Look, I don't want to take away from your relationship with your children so let's stay with that.

Beilin: I observed them very carefully without being, in a sense, obtrusive. Hopefully, not being heavy handed about it. Actually my wife and I had a great many differences about my attitudes, or our attitudes towards the kid's development, mostly in regard to the importance of school. To me getting an education was the most important thing in my life. I grew up as a lower class kid in a poor neighborhood with a poor mother, being brought up without a father. There was only one way I could get out of that world, and it was through school, through education. So education became primarily important in my long-range thinking.

It continued on with my own kids. Doing well in school became important and so the primary effect I had on my kids was largely through my interactions with them around intellectual things. First of all I read to them a lot from as early as they could understand about things. I used to read stories to them, I used to read books to them until they themselves began to pick up on books.

Bearison: How many years apart were they?

Beilin: Three years.

Bearison: Okay.

Beilin: The older one, Lewis, the younger one Ian; three years apart. They were very competitive brothers. The older oppressed the younger one in the usual fashion, and that always created problems. But in the long run I had a motto that I always reiterated with the kids, which reflects my own attitudes and values. I picked up on this slogan that NAACP used to advertise, that 'a mind is a terrible thing to waste,' and I kind of reiterated that often to my kids. But in the end, both of them turned out to be interested in intellectual history, and I think it's no accident that they developed that way.

Bearison: What do you mean, this is a professional interest that they have when you say --?

Beilin: Yes.

Bearison: And what do you mean by intellectual history?

Beilin: The history of ideas. The older one became a French intellectual historian, did his dissertation on Pascal. The other one just finished his dissertation, he started off in German intellectual history but shifted into German social history, interested in nationalism.

Bearison: That's incredible, so they're both getting their PhD's. Do they want to teach? Do they want to be professors?

Beilin: Well, they started off that way wanting to be professors. My oldest son has had a lot of trouble getting a job, a permanent job, so he's given up the idea of being a professor and is starting law school next year. The younger one is just graduated; he got his degree so he will face the realities of the real world soon.

We've been very lucky, my ex-wife and I, with our two boys. They've had difficulties in the way that normal kids have in growing up, but they have been free of problems, and we see all kinds of problems in other families all around us so we have been extraordinarily lucky.

So, I think the SRCD has asked all kinds of questions, or some questions that they would like covered, like what's been my participation in SRCD? What are my feelings about the SRCD?

Bearison: Yes, I'm less interested in that, but go ahead.

Beilin: I know that, but I feel a sense of obligation.

Bearison: Okay.

Beilin: The first SRCD meeting I attended was early on; I think it was in 1952 at Penn State. I went to this meeting at Penn State, and that hooked me onto professional organizations, or at least that professional organization. I've been much more loyal to SRCD than I have to APA and the SRCD has given me a great deal of nourishment, no question about it.

I haven't been happy about the changes in SRCD, in the same way that I haven't been happy about the changes in the field because the organization itself has become much more activist, it's engaged much more in social polity, has siphoned off a large part of it's resources into political activities and the like, or quasi political activities. I guess in some sense it was necessary as a part of the times to be seen as relevant or be seen as doing something important since one is concerned with children and the like, but that's not my bag, so I have not participated in any of that, I'm much more concerned about the intellectual content of the discipline.

One thing that gives me heart, it shows you how intellectual movements occur and the way in which they're resisted subtly and in some sense to good effect. There has been in the field, this is in psychology generally and in developmental psychology in particular, an anti-quantitative point of view, an attack on statistical methods as a basis for making inferences about studies, an attack on causal explanation, an attack on analysis of variance, for example,

as a basis for making inferences, and the shift toward qualitative methods and the like. Qualitative methods have been around for ages, now qualitative methods are the sine qua non of goodness and like. However, if you look at the journals and you look at Child Development in particular, the paragon of developmental journals and developmental psychology and you do a count of the kinds of studies that appear there, they're still predominantly experimental studies. In fact, I think the journal that's shifted most from a experimentalist's point of view is *The Journal of Experimental Child Psychology*, my old journal. Something has endured, something is continuing, and that gives me heart. In this way too the Kuhnian idea is not an accurate representation of the way change takes place.

Bearison: But I think you also have to think when you talk about rise and seeming ambiguity of interest in qualitative methods, I think that's more a function of this place and a few other places. I don't think by and large this is sweeping the field, and one of the ways – reasons that makes me think its not, is what you said, there's not that many journal outlets, scientific journals that are publishing this kind of stuff. It's coming out in books, yes, and it's coming out in some French journals, you know, Sage Publications has made an effort in this direction, but it doesn't seem to have taken over the mainstream, but if you hang around this place enough you begin to think that this is the Zeitgeist. And I told that to our students very often, that what's happening here is exciting and it's new and it's cutting edge, but don't mistake it for what's happening in the field as a whole. So I mean in some sense one could say that we're in a place that gives us a very biased view of what the field is and where the field is at, this is not the mainstream here.

Beilin: Well, yes, there is other evidence. For example, I hear from my wife, Iris, about research in the clinical scene. There's a shift there also, towards qualitative research, they're big on that kind of thing. The context –

Bearison: But you know, even there I think the little clinical work that I read, there's a whole push toward empirically based outcome research for clinical therapy, because if they can't show that they'll have no future with the HMO's and managed care practice. So it's all this push and the American Psychology Association makes this very clear, that you've got to push for empirically based outcome studies of psychotherapy. And why, because it has – you have to come up with some kind of a model, scientific model like medicine does, to justify spending big bucks on it, so I still think it's not the mainstream in clinical.

[Continuing July 18, 2000]

Bearison: Today is Tuesday, July 18th, 2000 and we're preparing for the final meeting with Harry Beilin for the SRCD project. So how are you doing?

Beilin: Okay. So, are we ready?

Bearison: Yes.

Beilin: I think a point at which we might start today is looking back on what I think were mistakes that I made.

Bearison: Good.

Beilin: In retrospect, at least my having decided to be a biology major in college was a mistake despite the fact that it also had very good consequences in that the work I did on Piaget, Piaget's theory of research and all of that was affected very much by my having a biological orientation. At the same time, the negative aspect of it was that I spent an enormous amount of time in chemistry laboratories, physics laboratories, biology laboratories, an enormous amount of time unfortunately, which I could have used much more effectively if I had concentrated on the humanities and things I was interested in like philosophy and the like.

Bearison: Do you think this gave you a very like positivist approach toward the social sciences because of all of the work you've done in the so called 'hard sciences?"

Beilin: Well, I don't know about that. I'm not clear as to whether that the relationship between one and the other might have been.

Bearison: Okay.

Beilin: The other thing that I did, and I would advise incoming students against, is thinking big. I always, in a sense, attempted to think big. I did studies of enormous scope, at least spent an enormous amounts of time, collected enormous amounts of data, reviewed enormous amounts of literature and I think I would have been more effective, actually, if I had done the smaller studies that I had also done, more limited studies. The work I did on anagram problem solving, for example, addressed a simple problem. The results were very clear-cut. It had an affect on that literature almost immediately. And then the studies I did on language resulted in a large book, spent years in a sense in that field, work on geometry, it was also a large scope and never led to anything that I felt was that worthwhile. So there were different styles that people have in the way they approach doing the work.

Bearison: I want to make it clear. Are you saying that stick to small manageable problems, or you can study big problems but do it in small manageable ways and methods, or are you saying both? You know what the difference I'm trying to get at?

Beilin: Yes. Well, you can deal with big problems, but one should deal with them in manageable ways, smaller more limited ways. In some sense I shouldn't regret what I did because I feel that I did good work and led to good results, important results. I don't think I'm in a position to evaluate what was the most important stuff that I ever did.

I think back, for example, the paper that I wrote on teachers and clinicians attitudes towards the behavior problems of children, one of my earliest works was also the most reprinted of my papers, at least in those days they were reprinting a lot. And until the intellectual climate changed, the educational climate changed and changed attitudes towards that particular subject matter; it was a very influentive paper. But which of the Piaget works are the most important? I'm not sure at this point, I would say. In any case, looking back on the negative aspects of my career, I don't want to magnify those negative aspects, but I think one should be knowledgeable or at least aware of those.

Bearison: Do you think this is a good time for graduate students in psychology to enter the field of developmental psychology or would you advise them not to do it today?

Beilin: It would depend on their interests and as to why they would be going into the field. You know there are many reasons, many motivations that move people to action. If a person were interested in, say cognitive neuroscience, developmental cognitive neuroscience, I would think that they could look forward to an interesting and productive and worthwhile career. There are other fields that I don't happen to be enthusiastic about. I would say that if people went into those that they would be effectively going into dead ends. I think a lot of students in our program, for example, who have committed a lot to social action and public policy and the like, will have short careers, at least in those, because the conditions that people think are important change almost every day and there are no enduring or basic questions that are really being addressed.

Bearison: If somebody wanted to go into a field where they can affect social policy involving children or adults or even the aging, but you know from some kind of social policies of human development, would developmental psych be the right way to go or would they be better off going into, I don't know, maybe even political science or a master's in public health or something like that, you know?

Beilin: Well, that might very well be the case unless one is interested in basic research questions. If one is interested in fundamental mechanisms of behavior or the nature of developmental change for example, then this is the ideal context in which to work.

Bearison: As you say, it seems like more and more students are coming into the field, or into study, graduates taking developmental not for basic research issues and questions, but because I don't know, activity research, I want to make a change, I want to make the world a better place for humans to develop.

Beilin: That's right.

Bearison: And you think, and I share this view with you, that that's sort of misguided to think you can do that kind through a doctoral program in developmental psych.

Beilin: I'm as skeptical as one can be about psychologists changing the world. From what I can tell, an enormous amount of energy and effort goes into thinking that psychology somehow has a special way of dealing with problems, issues, concerns of a general nature, when we see that the field itself changes constantly, that the things one believes in or believed in ten years ago they don't believe in now. What kind of contribution can a field make to big questions when it's in this particular state of constant change itself? Now –

Bearison: I want to stop here. I believe that psychology is not in a good position to make changes, but not for that reason, because every field is in constant change as the Society itself is.

Beilin: Well, true.

Bearison: And so, you know, even look at the field of technologies that the basic research behind technology changes all the time, but it's effective change.

Beilin: You're right about that. I'd have to qualify. The fact that the field is changing is irrelevant, but what is important is what's the content of the kinds of suggestions that psychologists can make? And here I think that they stand on shaky ground. I have not seen – if you look at the various controversies that psychologists have been involved in, the recovered memory syndrome, is a pretty good example, for example. Psychologists rushed in, felt they had the expertise, shown to be in many cases less than honest, fraudulent and self-deceptive, and in the end, of course, psychology is diminished in the eyes of the public, as well it should be. So, I'm very skeptical of, in a sense, psychology – at least institutional psychology's self-importance, exaggerated self-importance.

Bearison: Do you think part of it is because, I mean, here I may be moving you into the post-modernist prospective, but ultimately all psychology questions are evaluated so that they don't bring psychology as a pure positivistic science to fix and make better the human condition, but that it reflects the values of the human condition and therefore is no more privileged than any other aspect of studies to affect change, because the changes come from the values that we place in the psychological questions?

Beilin: No, I don't buy into that point of view. Some ideas are better than others. Some values are better than others, and one has to acknowledge that. If you don't acknowledge that then everything becomes totally irrelevant, or based on questions of relevance and relativity, and in the end nothing is valued. I think rather that it's important to call out what is most enduring, to try to face the nature of reality and discover what the reality is. Another position, for example, is that we create reality, that everything is a social construction. I'm totally skeptical of the social constructivist point of view, and in fact, there's an element of Piaget's philosophy and psychology that I'm skeptical about and it deals with the constructivist aspects of it, which is why I've insisted, kept insisting, that Piaget is not a true constructivist, at least not a radical constructivist, but a constructivism is based on a kind of realism, it's an attempt to construct a view of reality that is confirmed or disconfirmed by the nature of that reality. So on this I've had a debate with Leslie Smith, for example, who is skeptical of the realist point of view and orientation.

Bearison: Well, what do you mean – how do you feel when you talk about a reality as being a social-cultural reality, and that one might argue that with evolution we grow more dependent upon mastering socio-cultural realities than say physical realities, and what's the constructivist role in affecting socio-cultural realities? Think of the lives that people say in Russia, in this shift from Communism to so-called Democracy now, it came about in ways nobody predicted. You know, it's probably one of the most profound changes in my lifetime, in your lifetime. And so it didn't come with a big bang, it sort of came with a whimper, Communism died and they've – their lives are incredibly changed and maybe really in the most basic ways about what's a life about, you know? And that didn't come about through some kind of natural forces; it came about through social forces.

Beilin: Well, I agree with that. There is a constructivism in the creation of social facts and social conditions. Those are not – I wouldn't say they're not affected in some way by natural forces, but the social world is, and the world of values is a constructive world. There are people that believe in universal values, for example, I'm not one of those.

So, on that score, I want to look to the nature of the processes by which these worlds are constructed, no question about that.

Bearison: Okay.

Beilin: The only question I have is with regard to the child's conception to the world as a physical world, and the laws and the principles that operate with respect to time, space, geometry and the like.

Bearison: But even there I would assume that you would agree that the values that society places on categories of knowing are not natural. So for example, some cultures are very time-oriented, other cultures have a sense of time that's really, totally different than ours, it would drive us crazy to be among people who had such a looser sense of time. But our knowledge of time might not be a construction, but what we value about it, what use we make of that knowledge, whether it's something, spend your time learning about this, or spend your time learning about this, that too is a construction, so –

Beilin: Well, yes, but what is implicit in what you're saying is the distinction between the phenomenon that you're trying to understand against the way you value the information that you arrive from that investigation.

Bearison: Yes. Getting back to Piaget, I think Piaget was very sensitive to that distinction when he could talk about the Navaho kids coming up with an idea of conservation of matter before number and stuff like that because of what their social environment is like and stuff. I think it's of an important distinction and I think that Piaget was very sensitive to that kind of distinction.

Beilin: Right.

Bearison: And then maybe those who misinterpret Piaget don't see that distinction and make him seem like a constructivist in all ways, which he wasn't.

Let me ask you a little bit about a topic they call 'Styles of Scholarship." I once had an idea of just interviewing very accomplished people to just find out how they go about doing what they do professionally. And for us a lot of what we do is we put our thoughts in writing and we publish it and that way it gets to influence other people who read our works. How do you go about that? Is it difficult for you? Is it something that gives you great pleasure? Does it take place in stages and false starts, and how do you map out a project that's going to take you into publication of some kind?

Beilin: Well, I think that essentially ideas generate other ideas, so that if I'm working on one project, either in the combination of that or in the process of arriving at some conclusions, other questions arise. In the general picture of where I've gone I would say that has been the kind of general theme. At the same time, accidents occur, or contingencies arise that suggest an idea or a problem that should be solved.

Bearison: Could you give some examples or illustrate?

Beilin: Yes. I think I may have mentioned this once in our discussion. I was riding on a train to my first SRCD meeting at Penn State University, and I was looking over the program and I realized as I was reading it that I had seen a word that was misspelled, which I had never the less interpreted as the correct word, without even noting the misspelling. I think I mentioned too that the word was 'mother.' Now I suddenly became interested in the idea, why is it that we're able to do that? That is, why is it that we're able to process linguistic materials, lexical materials in such a way that violations of the linguistic order don't interfere with our extracting the meaning from it? That's how I got into studying the anagram problem, the anagram problem and searching the anagram literature and then undertaking a couple of studies and the like. Now that came out of the blue, but that's not, for example, how I got into studying language development. I saw it as an issue when I was researching – when I was doing the early Piaget work, and the question kept arising in my mind, is Piaget correct on his notions of language. Then I became aware of the Chomskyan position and I saw a tension between the two positions and that prompted me to pursue language studies. It's not a clear line of development, its in fits and starts and it's affected by all kinds of things that one reads or I read and it's a very pluralistic in a sense, world of generation and it's not linear and it's not unitary, it's influences come from various places. And the effect of intellectual movements affect the way I think about a

problem or have thought about a problem. I was affected by structuralism, philosophical structuralism. I was affected by semiotics. I was affected by other intellectual concerns of the time, so that to trace my own intellectual development and how I arrived at problems to deal with and studies to do would be a rather complex process of analysis and deconstruction to really sort out.

Bearison: Do you think being a New Yorker has affected your thinking in any way, or do you think it's pretty much would have been the same if you were anywhere?

Beilin: Being a New Yorker has affected my life in a major way, mostly social life and intellectual life, cultural life. When I was in Connecticut and Minnesota on large campuses the organization of social life was very different from the way it is if you work at a university in New York. There you tend to see your colleagues socially, you tend to go in a circle or through many circles. In New York –

Bearison: But it's provincial, I think in the end. It's a very provincial life.

Beilin: Yes. That's puts a kind of negative cast to it, but it has a great many virtues.

Bearison: Yes.

Beilin: Also some liabilities. Your life tends to be a little more transparent. You're under the gaze of your colleagues all of the time. But in New York, one sees ones colleagues occasionally, sometimes only rarely. Your main social contacts are with other people for the most part. How you connect with them, sometimes complicated in my case, lots of the social contacts I had actually developed through my wife or wives. But being in New York was in a sense, feeding in a trough filled with all kinds of intellectual and cultural goodies. This city is incredible, and unbelievably rich. Too rich in some sense, because you can't absorb all of it, not that one would even want to. So, whether all of this affected any of my intellectual work I can't say.

The sources of my intellectual nourishment have been more or less independent of my life in the big city so to speak. I've always been a voracious reader and early on I read the political journals like *The New Statesman*, the English publication, and I focused a lot on it's back pages, so called cultural sections. And when I got tired of the political position of *The New Statesman* I switched to other sources, and I discovered new ways in a sense of making contact with the intellectual world so that for years and years now I've been reading *The Times Literary Supplement*, which is a rich source of contact with almost every aspect of intellectual life and culture. This constantly nourishes me, keeps me in contact in a way and is a constant source of ideas and I'm able to keep up with where the world is going intellectually. And I've always done this in psychology as well. I've always been sensitive to where the field has been going, and I've always tracked this, for example, in developmental psychology, when I used to go to meetings and bring back the message, what is it that they're doing out there, and try to make our students and faculty for that matter, aware of at least what my view of developments were and have been.

Bearison: How do you want to be remembered by your colleagues?

Beilin: Well, I used to think of how I wanted to be remembered by my children.

Bearison: Which means that that's really more important to you, you really don't care that much about how your colleagues remember you, but it's how your children remember you that's really essential to your identity, is that what your saying?

Beilin: That might very well be true, although in a way what my colleagues, and I mean colleagues in a broad sense, not just in the local faculty –

Bearison: Yes, I mean that too.
Beilin: -- but of the field generally. With my kids, it was always important to me as to how their characters developed. I wanted them to be of good character, however one defines that. And in the main, I think they've turned out positively that way. For myself, I guess what I would have liked to be remembered as, is a man of integrity. A man who believes in his ideas, who's willing to fight for them in some ways. I don't consider myself to

have been much of a hero on issues, on some issues that were important to me, mainly political issues, I didn't go out there and fight the good political cause that I believed in.

Bearison: Do you regret not having done that?

Beilin: Well, I thought – you know that I think that I wasn't exactly a coward, but I wasn't exactly forthcoming either in fighting against the forces that I thought were having a negative effect on our universities, society, culture. I always made it clear, for example, there was never any doubt in anybody's mind that I was an anti-Communist for example, from way back, but I didn't make a big deal about it, at least on political ideas. And on the professional level my positions vis-à-vis, Piagetian psychology, for example, were always pretty clear too. I've always been known as a rather harsh critic, I think to the point where some people have used me as a kind of hatchet man to go after some papers that an editor might think questionable, because I was always very tough and didn't hesitate to make my opinions clear. So I think there's a mixed picture of me out there in the professional world, in any case, so that – but I think overall the feeling could very well be that I might have been tough but never unfair, never ad hominem to the extent that I was always interested in attacking ideas and not the individual. Without denying the fact that to the people you criticize they take it as a personal thing, I learned this early on as an editor.

Bearison: Don't you think you're as much of a person as everybody else, that when you've been criticized you've taken it personally also? It's hard to separate your ideas from who you are.

Beilin: Well it depends on the nature of the criticism. I have more than once written to an editor and asked them to thank the reviewer for what was essentially a really beat up review, because I recognized that it was done fairly, that it was done with the intent of pointing out what the really legitimate criticisms were, and I appreciated that and I valued that. At the same time when criticisms, and I have faced some criticisms that were unfair and mean as all hell, and naturally I was very angry at them. I still harbor some long-standing feelings, you know, about some criticisms; there's no question about that. But through the long run I think that the picture I would like to have retained as long as you know there is any memory after one leaves the scene, is of someone who did well.

Now I always had a feeling that I was always very honest with myself, that I didn't try to fool myself, and I've done a number of things in my life that I regret that I would wished I would have done differently and the like. And I know that that stands as a mark as well, but it's part of the picture and it's the reality.

Bearison: How do you feel about this idea of retirement and how the University – do you think they sort of not make use of people, good intellectual thinkers and by pushing them aside, or are there other people here that might be faculty that we both might think about that say, gee it's really time for them to retire but they just keep plugging away and plugging away and it's like they're just taking space away from other young people that may be able to come in with more creative ideas? How do you feel about this, you come into the University now, you have an office, but you don't have that level of involvement, that of as a faculty member does, just to say faculty emeritus, I mean, how do you feel about that? Do you feel, "Hey, I feel left out of all this, I should be a more central role in the University?" What are your thoughts about all this?

Beilin: As an emeritus professor I feel pretty lucky. First of all, this University has been generous to me. It gives me an office, I don't have a great office, but it's a good office.

Bearison: Yes.

Beilin: And it's serviceable. Now I don't feel that I need to engage in the policy-making activities of the department or of the university, that, I think, would be an intrusion. I've had my time, I've done my work that way and the new generations need to define life for themselves. At the same time, I feel that I'm a value to the University and to the program in particular by participating in it's intellectual activities, going to the brown-bag seminars, raising questions, not being silent, being my same old self, critical, evaluative and where possible, productive. So, I regret some aspects of being out of the scene, but not within the University community. It's mostly that I -- because of questions of health I'm not productive in the way I used to be. I'm not writing the way that I could be writing. I can't volunteer to give talks and make paper presentation the way I used to, and there's some regret.

Bearison: At one time you said that you were in the process of writing a book but you didn't want to talk about it. Are you still doing this and would you like to talk about it now? It could be an advertisement for your book.

Beilin: I have about three hundred pages of manuscript written for a book. I don't think it'll ever be written.

Bearison: What three hundred pages? What's that two-thirds of the way there already, half way there?

Beilin: Yes. Maybe even more.

Bearison: Yes.

Beilin: The book was supposed to be on the explanation on developmental change, on mechanisms that cause that change, and I did a chapter in a book I think in the event has captured some of what I wanted to say. I think there are two reasons why I think I'm not compelled to finish. One is, I'm not sure how much of a unique contribution I could make. And the other is the hard work of finishing. I just don't have the physical wherewithal to stick to it.

Bearison: What do you mean, its just that you get fatigued, or is it mental fatigue or physical fatigue or --?

Beilin: I live, to be honest with you; I live in a low state of misery. I cough a great deal. We haven't experienced it luckily in our discussions because it usually comes about in the morning and then in the evening and after I eat. I cough a lot, and it's productive kind of coughing and it may last – and this is from a combination of changes in my lungs and also a kind of chronic sinusitis and a reflux problem. So the coughing knocks me out physically, and I can't – and also I get out in the world I walk, walking is very difficult for me because of shortness of breath, I have about a thirty percent reduced functional lung capacity, which means that I walk and I have to stop and catch my breath and walk again. So, I don't have – it's not a life of high misery, I have friends who are in that kind of condition and luckily I'm not in that state.

Bearison: And how old are you now?

Beilin: I'm seventy-nine now. I'll be eighty next year. I feel incredibly lucky to have gotten this far, so I could go on a lot longer or not. I'm philosophical about that, whatever philosophical means in a sense.

Bearison: Since this is our last meeting, it's only inevitable that I've got to ask you, how do you feel about having talked to me about all this stuff and some day that it'll be transcribed and archived somewhere and --?

Beilin: It's helpful to review the events of ones professional life, as well as one's personal life and see it as – attempt to see it in some kind of prospective. What it also highlights is the difficulty in really understanding the forces that impel one to act in the ways that one has and to make the decisions that one makes. When I was in counseling psychology my specialty interest was in the nature of vocational choice and I was very interested and concerned in how, in fact, choices were made and how change occurred in the process of vocational development, and I never really felt that we had an adequate handle on all of that. So I look at my own career and I try to understand the processes by which I made choices and was affected by the nature of my experiences, the nature of changes in the world. I've become, you know, almost intimidated by the complexity of the process; reviewing events hasn't led to any great insights or enlightenment on my part, and maybe somebody else reading it will see things into it that may not be so obvious to us, but –

Bearison: Does the complexity of events in your own life as you review it now at this period in your life, does it bring you any closer to some kind of a spirituality, I mean I know you'd said that when you were growing up you weren't very religious. Do you feel that you've become more religious now?

Beilin: No. I lost – I don't remember ever being – having any religious sense. I do have what you call quasi-mystical feelings, I guess. I love to sit in churches and listen to organ music and feel transported. I go to concerts and I hear a Bach B Minor Mass or The Messiah, and I've been at times, you know, moved to cry. So I have –

maybe other people experience these as religious feelings, but I have no – I don't experience anything religious and that hasn't changed in my getting older. I don't feel any more a need to a belief in God than I ever have.

Bearison: Well, you know, I've come to have a sort of reawaking of my Judaism, not so much because a belief in God but as a sense of the continuity of life and that it connects me to a past and connects me to, I think, a future, so I have an interest in both seeing the culture of Judaism be maintained in the future, but also respectful of its past and begin to see myself as a part of that, so you know, it's brought me closer to religion if not closer to God in that sense. But then when I attend the religious services it becomes very meaningful in that regard that I'm part of a ritual that has a long and noble history, and what I'm listening to and the words I'm hearing spoken are words that people long before me spoke and hopefully will way into the future, so in that sense one might think about coming closer to a religion.

Beilin: Well, if you ask me what my feelings are about being a Jew, I have a strong sense of identification with being a Jew in a cultural sense. I feel that I'm part of a long culturally historical tradition that has believed in important things, that's committed to some important values and some ethical values, although I'm not sure that I was always able to live up to those myself. But religious ritual, even Jewish religious rituals leaves me basically cold. I have participated with the ceremonies for those who are getting married, for example. My first wife was non-Jewish and for many reasons, or particular reasons I told her I could not marry her unless she converted. Now she thought it was outrageous at the time, but I think it was the right move because –

Bearison: This was your wife Lois?

Beilin: Yes. My ex-wife Lois. She converted to a kind of reformed Judaism, but the interesting thing is it was mainly to ensure that our kids would be brought up as Jews, but it came back to me with a vengeance because she became more Jewish than I.

Bearison: Yes. I've heard that.

Beilin: She's joined a synagogue, she became a member of the board of the synagogue, she was – so that – which was a reflection, I think, of my own commitment. My second wife, my present wife is Jewish, so there's no problem on --

Bearison: Have you been to Israel?

Beilin: Yes. I was in Israel by virtue of the invitation of Sydney Straus to deliver a memorial lecture –

Bearison: In honor of who?

Beilin: Tamar Globerson. I thought that the experience of going to Israel would have a profound affect on me. It had no affect. It left me cold. I couldn't identify with the culture. It's not a religious culture, it's a culture like going to Italy or England, wherever, it just happened to be Jews. Not only that, I couldn't understand them because they were speaking Hebrew, and I couldn't understand the Hebrew. I kind of made a joke about it when I was introduced. I don't know how it went over, but I've never been among so many Jews and felt I was so alienated. But I thought that going there would somehow grab me, that it would get at something really deep in me and touch the roots of my Jewishness, but it had no such affect.

Bearison: I mean, I don't want to get into it because it's not my story, but that's exactly what it did to me. It did get into deep and got into my roots and the diversity and all that was amazing to me, that these are all Jews and they're so different, and it's so different and yet they all come from this common sense of identity. You know, this is incredible, so we had a very different experience.

Beilin: Well, that's good. I'm glad for you.

Bearison: Is there anything we left out? Is there anything we should talk about now before we stop?

Beilin: Oh, I'm sure we left a lot out, but that's inevitable because this can't go on forever.

Bearison: Okay. So should we stop here then?

Beilin: Yes. And in the process let me thank you very much for listening to whatever that I had to say and for being sympathetic and asking good questions.

Bearison: Thanks. I've enjoyed this very much too, so I thank you.