

Jack Block

- Born 04/28/1924 in Brooklyn, NY; Died 1/13/2010
- Spouse - Jeanne Humphrey Block
- Ph.D. from Stanford University (1950); M.A. from University of Wisconsin (1946); B.A. from Brooklyn College (1945)

Major Employment:

- Langley-Porter Clinic at UC San Francisco School of Medicine - 1950-1954, Research Psychologist
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- Clinical Psychology, Personality Theory/Development/Assessment, Longitudinal Research

SRCD Affiliation:

- Member



SRCD Oral History Interview

Jack Block

May 1, 1994

Interviewed by David Harrington
In El Cerrito, California

Harrington: It is May 1st and we're sitting here in 1395 Rifle Range Road, El Cerrito, California, talking to Jack Block. I'm David Harrington, from the UCSC Psychology Department. My first series of questions have to do with general intellectual history. The first question is to describe your family background along with any childhood and adolescent experiences that may be of interest; where were you born; where did you grow up; what was your schooling like; were you involved in the military; what was your early work history? The next question has to do with early adult experiences important to your intellectual development and so on. So this first one is pre-college, I gather.

Block: Well, I was born on April 28th, 1924, in Brooklyn, New York. My mother had emigrated from Russia or I should say Byelorussia given the way the Soviet Union broke up. My father was Lithuanian or Polish, depending upon when borders were drawn. My father died when I was 14 months-old and I never knew him. I suppose at some deep psychological level that was really a significant influence on who I am; that I really didn't have a father figure in my growing up--

Harrington: I'm sure.

Block: --and probably accounts for a number of things about me. I grew up in a Jewish context and had a lot of family around early on. My mother was a widow, and I had brothers and sisters, many aunts, uncles, and cousins. They used to somehow especially come to our house for holidays because of our big backyard (1927-32). It was a New York, Jewish existence in the 20s and 30s. I don't think I was especially intellectual. I used to do a lot of reading, and I remember going to the library often, picking up books, and taking them home. I guess I did a lot of reading. I was a chubby kid until I was about 13 or 14. I was--

Harrington: What was school like for you?

Block: Well, I went to PS #2 in New York (in Brooklyn). It was a crowded, but decent school in my memory. I remember the schoolyard was about eight or ten feet wide and a whole block long. My mother had a jewelry store--my father had had a jewelry store and she kept it going after he died--and it was on the street level in Borough Park, Brooklyn underneath the then Third Avenue El, not too far from the wharf area of Brooklyn. We lived up above the store on the second floor in an apartment there, and we also had some rooms behind the store. It was a lower-middle class existence. It was not a comfortable life financially, in reverse actually for my mother. Later on, because she had been able to save some money when prohibition ended, she had the money to buy a liquor license and family prosperity was in the offing. She was also able to provide money for liquor licenses for her three brothers. So the extended family had four liquor stores and was suddenly going to make a lot of money. Then it was recognized through some boastful indiscretion of one of her brothers--one of my uncles who became immediately the black sheep--that these stores were somehow linked to an illegal arrangement according to the law at that time. We had to sell them, which caused the liquor licenses to be lost. My mother tried, through an expensive and corrupt lawyer, to get her license back, but to no avail. Suddenly, we were really in an economically critical situation.

Harrington: How old were you when that was happening?

Block: I would say that my first ten years we lived in Borough Park, and when I was ten we moved to Bensonhurst for two years. Then we moved to Yorkville (in Manhattan, New York) and then to a rabid German-American Bund area. And with the liquor store owner license lost but legal tries for reclaiming it seemingly feasible, my mother, to save the store location, began a venture in ladies clothing with mostly things like, you know, slips and bras and panties, things like that. She did not do very well, and then we became really quite desperate monetarily. The ladies store didn't last very long, and then we moved out to Harlem at 121st Street and Lexington Avenue. I was in JHS and about to enter HS. I was accepted to go to Townsend Harris High School, but Mrs. Sher and a cousin advised my mother and me to go to Stuyvesant High School instead. At that time, Townsend Harris was the high school to go to in New York, as we'd say. I would have graduated Townsend at 15, which I was advised would be too young, especially given my uncertain state of maturity. So I went to Stuyvesant, commuting from 121st to 14th via the now removed Second Avenue El.

Harrington: You were a good student?

Block: Well, not a great student. There was a lot of reading. I wasn't a very scrupulous, dedicated student. My family environment was not especially pointing me towards an intellectual or academic life, you know.

Harrington: Right.

Block: My mother was a shopkeeper and came out of a small Russian village. There was a big distinction at the time between the Russian, Slavic Jews, and the German Jews. The German Jews had

come here before or relatively recently from unfriendly Germany and were better educated, more wealthy, and very academic or intellectual. Slavic Jews were much lower in class, knew it, and resented it.

Harrington: Was there any discouragement of school?

Block: No, not discouragement, but not the act of pushing--certainly not discouragement, certainly not. There wasn't the active pressure or expectation regarding the course you were into if, of course, you're to become a doctor or, of course, you're to become an academician.

Harrington: When you brought home presumably pretty good grades did they respond to them with pleasure and support?

Block: Oh yeah. I guess there was pleasure expressed. And I think it was expected and everybody was getting that--everybody in the 30s. It wasn't a strong, strong directive, simply an expectation.

So we went to Yorkville. Yorkville was, at this time, patriotically German-American where the Nazis were really popular. We lived on 86th Street between 2nd and 3rd Avenue for a couple of years, close to the liquor store; then we moved to 83rd Street between First and Second Avenues during the time of the ladies shop. When I went to Junior High School was when I was getting into lots of fights. Not that I was getting into fights; I was being victimized. At this point I was short and fat, chubby, unathletic. I'd be 12 years-old--not yet into my adolescent growth spurt--and there were these big, stupid 14-year-olds who would beat up on me. There was a lot of Anti-Semitism at this point, and so I'd been in fights and I'd run or fight also. And then, lo' and behold, in high school I had much of my growth and these earlier big 14-year-olds weren't so big anymore. And that's what led me to athletics, more or less. I'm sure I'm not being very organized or sequential.

Harrington: That's fine.

Block: As if you didn't notice!

Harrington: Don't worry about it. Did you work at all during high school along this time?

Block: I delivered tuxedos on Saturdays for a tailor. I took them to customers and reclaimed them. I was getting \$2.00 a day for a very long set of those days, plus occasional tips. I didn't really work subsequently until I went college.

Harrington: The next question has to do with early adult experiences that were important to your intellectual development and adolescent experiences. So I think, in general, the next question is how'd you get to college, and what happened?

Block: Well, I graduated from Stuyvesant. I was on the swimming team in high school. I got to be big and somewhat strong and spent good years in high school. I went to Brooklyn College at the age of 16 and didn't like it as I was really too young for college at that point. And I left college after the first year; I did badly and was not happy. Meanwhile, my mother and sisters had moved out to Long Beach, New York, on Long Island; it seemed to make sense to take a year off. During that year, I worked in gas stations, as a furniture mover, and as a Life Guard. I also was enrolled for a while in a machinist's school and was a very, very, very good machinist. Then I got bored with it and went back to Brooklyn College.

And in Brooklyn College again, I still did not do well at all. I must have told you that I had the lowest undergraduate grade point average--I want this on the tape--of anybody, OF ANYBODY, subsequently in academic life, at least all of whom I've encountered. For my four years at BC, my GPA was 2.23; for A is 4, B is 3, and C is 2, and only raised it my last year. And the only A's I got, besides in athletics, were in an elementary course in economics and a survey course in mathematics, but no courses in psychology. I actually majored in both psychology and American Studies for a double major. In the last half of my senior year, I decided I wanted to go to medical school for over-determined reasons. I started taking pre-med courses, and I graduated from BC. I had completed only one of my five pre-medical courses, but I was a 3-letter man in sports and the first 3-letterman in the history of Brooklyn College.

Harrington: Is that right?

Block: That's it. Yes.

Harrington: What were the three letters? Swimming?

Block: Swimming, wrestling, and football. I got a minor letter in track, for javelin throwing, but I didn't ever hit anybody. I finished Brooklyn College in June of '45. Regarding the draft, I was four F because of my quite extreme shortsightedness. This brought me into some trouble during WWII because I looked hale and hearty and because a war was going on. I once got into a big fight with some Marines on one occasion because I looked too healthy.

Harrington: Really?

Block: I was at some ballroom, I guess, and I left a party there with a college friend when we encountered a couple of Marines outside who really were looking for a fight. One Marine says, "What did you say to my friend?" And, of course, we hadn't said a word. They were looking for a fight! And, as I said, we obviously had just come from a party. We brawled briefly and then ran. Early on, I had tried to get into the army and been rejected because I was very nearsighted; I felt guilty about that.

At any rate, I enrolled at NYU in the summer of '45 to finish my pre-med courses in two intensive 6-week, semester-equivalent courses. I was in a motivational hurry, so I began by registering for the first half of organic chemistry and, simultaneously, its prerequisite, the last half of inorganic chemistry (I had started inorganic chemistry at BC, but I still needed its last half). And since you couldn't do that legally, I enrolled in NYU as two different individuals with the same name, but at two different addresses. My ruse was not discovered until the last few days of the first six weeks, but I was doing well and allowed to continue. For the 12 weeks I received straight A's, 16 units with straight A's. I had completed all the necessary pre-med courses with sterling grades.

Harrington: Right.

Block: Subsequently, I tried to get into medical school that very year, fall '45, and did not. And I was lying around in New York, not knowing what to do. I'd worked that summer up in the Borscht Belt as a waiter. For several summer years I had worked up there as a good way of making money at that time. And I was going with this BC girl now out in Rochester, Minnesota. I also had a friend in Madison, Wisconsin and Madison, Wisconsin is closer to Rochester, Minnesota than is the university at Minneapolis, as you would know. And a friend of mine in Madison asked the university whether they would accept belated admissions, and whatever the reason was, they said, "Yes."

So in October, after the semester at Wisconsin had already started, I asked for my transcripts from Brooklyn College and NYU to be sent to Madison. NYU responded immediately, but Brooklyn College

took a couple of months before they sent their transcripts. Well, the Madison Psychology Department said that since the semester has already started and they could see from the available NYU transcript that my grade point was straight A's and so forth, "Sure, come on." So I went to Madison and started taking courses there in psychology toward an MA. And then a couple of months later when my Brooklyn College transcripts finally arrived, I get called into the Dean's office and she says, "Oh my," you know, but they let me finish the first semester. I did well, was allowed to stay, and on that chancy basis, my career in psychology could begin.

Harrington: That's wonderful.

Block: It is marvelous. It was a very permissive time in that the war had ended and universities were receptive to students.

Herrington: Yes. And it probably couldn't happen today.

Block: Yes. Over the years since I've been on the Psychology Department's Selection Committee of graduate students, I judge I would never have gotten in. If I had not been accepted into grad school, I probably would have been just making a lot of money working as a waiter in the Borsht Belt. And I'd probably have gone out to Miami during winter, made money there also, saved it up, and bought a hotel or something. I often wonder what my life would have been like; I've often reflected on that because you can make a lot of money up there in summers and down there in Miami as a waiter. People don't appreciate how much money a waiter can make, and you can make good contacts. I got lots of money. It was only when I became an Associate Professor I started to make as much money as I did earlier as a waiter. Of course, Associate Professors then didn't make what they make now.

Harrington: What happened at Wisconsin? Were there people that you became interested in?

Block: Oh, yes!

Harrington: And how did that go? And is that where you were influenced?

Block: Oh, I got to Wisconsin, and that was very important to me because I think it was part of my transformation. This girl I was going with came down with tuberculosis and went back to New York and into a sanitarium. In Wisconsin, several people were very important to me intellectually. There was Norman Cameron, who was a psychologist and also a psychiatrist. He wrote an important book on behavior disorders, and he was important to me. There was also Ann Margaret, a clinical psychologist from Stanford. She was quite young and really quite smart, and clinical wasn't clinical the way it is now.

Harrington: Tell us about that.

Block: Clinical now is defined by psychotherapy mostly--all clinical students get involved in it. She was only about eight years older than any of her students. She was very smart and a very nice person. Afterwards, she got married and never really had a significant career although she was bright enough to have had a significant career. And then there was somebody named David Grant, who nobody knows about anymore. He was an experimental psychologist and a mathematical psychologist for that time. He was, I think, quite brilliant, and he was also self-destructive and, I guess, an alcoholic. And even with that he was a very nice, warm guy. I really admired him and connected with him, and he was very important to me in terms of giving me the sense of real methodology and logic in psychology, which was very important.

Harrington: Through courses or through hanging around him?

Block: Well, for a course I took with him, we had to write papers. I wrote one paper for him, which he quite liked. And it was very important the nice things he said about and to me. I had an insight, as it crosses my memory now, that he thought was an interesting slant on the nature of measurement and so forth. Anyway, I appreciated him a lot. He also could be a very stern, experimental psychologist. He was much more open than most experimental psychologists. The most known experimental psychologist at Wisconsin at the time just infuriated me in terms of the way he defined psychology as well as the un-psychological nature of their psychology. I won't mention names; maybe I will mention names.

Harrington: Why not?

Block: No. Wolf Brogden was there, and I thought he was sternly and confidently irrelevant. And he gave the Psychology Department its reputation back then. But I thought he was a narrow psychologist. I don't think you read about him anymore--historically, he may have been locally important, but not scientifically. There was also K. U. Smith, who had such an astonishing un-psychological approach that presented a machine view of human beings.

Harrington: Sometimes it seems to me the teachers--people that one runs into--have impact on one because you react against them, and they can lead you to sharpen your perspective. Is that true?

Block: Yes. I developed alternative rationales for the kinds of things that they would be talking about, perspectives much more psychological, and I think more correct. And of those faculty listed as being prominent, they were extraordinarily dense in my view. At any rate, I was less solitary, still working summers back at the Borscht Belt, and happy with Wisconsin. I received an MA there and was still applying for medical school--about 25 medical schools every year. I did that for five years.

Harrington: Wow!

Block: And never got in.

Harrington: Wow! Why not?

Block: Well, first reason: I had a bad grade point average, though it was looking better and better. But I did well on the MCATS of the time. While I did well in graduate school, I was also Jewish. There was a lot of anti-Semitic discrimination against Jews at that time. Anyway, I didn't get in. I wanted to leave Madison (and my older sister had moved to California). I suppose I had imprinted the slogan, "Go West, young man!" I had no idea what California was like, but it seemed to make sense to move to California to be near her. So, in my geographic naiveté, I applied to Stanford to be near my sister near LA.

Harrington: Of course.

Block: --and I applied to Stanford medical school. They rejected me from medical school, but accepted me in the psych department. So I came out and discovered where Stanford was, which was far from LA. And Stanford took all the money I planned on using for the year, for the first semester's tuition.

Harrington: Wow!

Block: I worked at a veterinary hospital for the first year I was at Stanford. I received a free room, no board, by virtue of cleaning the kennels on weekends and by helping the veterinarian on emergency cases, functioning as an operating nurse. I did that for a year. Meanwhile, I was trying to get into the

clinical psychology VA program, a restricted program at Stanford. Most clinical grad students at that time were with the Clinical Veterans Administration Program.

Harrington: Tell me a little bit about that?

Block: Well, if you got into the VA program as a 'would be' clinical psychologist, you worked at the local VA hospital and got a generous lot of money. It paid for something like 39 hours a week, and it was really a lot of money for that time. It was hard to get, but if you got in, you could really support yourself quite handily. VA clinical psychologist trainees had cars, often better than psychology faculty at Stanford could afford. Shortly after being accepted into the VA Program, I had a nice car.

Harrington: What was it?

Block: A 1949 Ford convertible.

Harrington: What color?

Block: Light green. I bought it from a bankrupted actor who was temporarily staying in San Francisco. The first year at Stanford I did well, and I was especially impressed by Quinn McNemar. I really imprinted on him. He was a superb statistician and had a heart of gold.

Harrington: Was he teaching the term statistics at the graduate level?

Block: Yes. And there was Ernest Hilgard, who was most dazzling to me. I really quite admired him, though later on I thought that he did not really use his talent well. To me, he did his best work after he retired and went into hypnosis, among other things. But at this time he was a learning psychologist and mainstream. He and Don Marquis had written a widely accepted, but sterile learning textbook.

Harrington: Right.

Block: He was in the learning psychologist mainstream; he was handsome, very bright, and articulate. I remember coming in to see him with a rough idea, which he took and shaped much better than I had shaped it. When he handed it back to me, I didn't want it anymore. He was important. McNemar was important. There were some other people like Don Taylor, who I played tennis with. He could afford tennis balls, I couldn't. He was a very nice guy, but died very young. And there's Paul Farnsworth, who had a course in the history of psychology and who was a marvelous historical gossip about other psychologists--about John Watson, who got involved in a sexual scandal at Columbia, and things like that.

Harrington: So tell us about some of these graduate students and how they were important?

Block: Well, of course, there was Jeanne. She was there. We started at the same time at Stanford, but we didn't start going together for a couple of years. Oh, Fred Attneave was there. He was a very impressive person then and I'm sure still is; I was impressed by him intellectually. There's Jerry Blum who went on to Michigan with the Blacky Test. There's Harold Rauch, who studied the effects of schizophrenia. Dick Bell was there, and Harold Stevenson was there. Wayne Holtzman was also there.

Harrington: Wow! That was really an impressive crowd.

Block: I'm sure I'm leaving out important people who were there. There were some very good people there.

Harrington: And was it the kind of place where there was real interaction?

Block: Yes, there really was a good feel to grad school there and then. I felt that there were a lot of discussions, arguments, and all of that. I really enjoyed Stanford; I owe them a lot. I would begin the day by biking from El Camino Real down Palm Drive, go into the inner quad to climb a tree for an orange or tangerine, then going on to the Union for a glazed donut and a plastic cup of piping hot coffee--thus breakfast, classes or the library, and two hours of impromptu basketball to end the afternoon.

Harrington: What was happening to the clinical interest? And let me interject--I hear about somebody who got all straight A's in all the premed courses, went to Wisconsin; was really intrigued with the guy who was kind of a methodologist, experimentalist; goes to Stanford, Quinn McNemar is the key person, but there is this clinical thrust. And I am curious about how those two--what's going on at this point?

Block: Well, I'd read a lot of Freud. I read a lot of Fenichel and his theory of neurosis, and I was fascinated by it. I thought that his was a much better statement of psychoanalytic theory than Freud had ever made. I thought Fenichel's first 186 pages is a modestly systematic statement of psychoanalytic theory. I got caught up by that. I also got caught up by Kurt Lewin. I guess I was interested in more formalization of psychological things than many people in clinical. And well, the reason I had wanted to go to medical school was to be a psychiatrist. I was interested in clinical psychology and psychiatry, and I guess I had a culturally conventional interest in doing psychotherapy. But when I started doing psychotherapy, I had all sorts of internal doubts about what I was doing and with what other people were doing.

Harrington: Can you talk about that? And did you get into the VA program?

Block: I finally got into the VA program. As a matter of fact, I tried for a year to get in and didn't get in. At Stanford, Howard Hunt was the clinician of the department. He would teach a course on psychoanalysis, and he really had no background at all being obviously only a few pages ahead of us in his readings. But he was in administrative control of who was to get into the VA program. I had expressed my interest early in it; I didn't get in. When I went one day after a year to tell him I was leaving Stanford for financial reasons and going to Florida, he told me I had gotten into the VA clinical program. Perhaps--even likely--he took pity on me. I was about to tell him of my necessary departure, and he tells me that I'm in the VA clinical program, so I stayed at Stanford. Otherwise, I would have left and abandoned psychology. Anyway, I got into the clinical program and that was a lot of fun, and I met people from Berkeley as well as Stanford, all working at the Palo Alto Veterans Administration Hospital, and that was important in various ways. I guess I met Tim Leary at this time, Abel Ossario, and a variety of people from--where am I?

Harrington: Talking about Stanford and people who were important in your intellectual development there. Who would you describe as your mentor, or did you have one?

Block: Well, not conceptually. The local person I most imprinted, I guess, was Quinn McNemar in many ways. And I didn't have really a mentor. In some sense, you know, I have never had a true, proximate mentor. I have never had an s-o-l-e or s-o-u-l mentor.

Harrington: To sustain you.

Block: Yes. And I think of all the psychologists I have read, I think that Robert White has a very resonating approach. I find him actually moderate and profound in many ways, and I especially liked the 1959 article on competence that he wrote. I never liked Henry Murray's personality views. Indeed, some of the recent things that have been coming out about him personally are quite alarming.

Harrington: I haven't read it or know about it.

Block: Some things I had heard about him are described in his biography; he was a scandalous individual, self-indulgent, narcissistic beyond belief, not scientific at all.

Harrington: What about Kurt Lewin? I know that Lewin had an impact on your thinking, and you mentioned him earlier. How were you exposed to his work?

Block: Well, I don't remember how I was exposed to his work, but I read him, and I became all excited about him in that he seemed to be formalizing things in quite a psychological way. Conceptual formalizers were far removed from psychology then, and most people were immersed and drifting in confusing psychological waters. I was impressed by the attractive formalization Lewin was offering; it was very psychological. I really would love to have worked with him, but of course never did get the opportunity. I read him a lot and later tried to do something.

Harrington: And he had important impact on your dissertation?

Block: Yes.

Harrington: Are you going to talk at all about that?

Block: Well, Jeanne and I, in our mutual grad student enthusiasms, had talked and worked out a way of bringing together some psychoanalytic aspects of personality and expressing them in Lewinian terms. The person system, according to Lewin, would have boundaries with the properties of these boundaries being permeability, elasticity, and differentiation. We'd talk formalistically and could coordinate these boundaries psychologically with aspects of what Fenichel was talking about psychoanalytically. Permeability had to do with monitoring or control of impulse, elasticity with the resiliency or a regulating function, and differentiation had to do with the articulation or degree of complexity predicating behavior. Thus, one can be complicated without having permeability in old age. This time was when we started thinking about over-control, under-control, resiliency, their coordination, their behavioral implications, and some deductions we tried to realize in the real world

Harrington: You and Jeanne wrote interlocking dissertations?

Block: Yes!

Harrington: Was that unusual?

Block: Oh, very unusual then and still now.

Harrington: Under whom did you do that?

Block: Well, we had different thesis committees, but our research arrangement was together. Jeanne and I saw about a hundred Stanford undergraduates, each experiencing ten or 12 different tasks. And for her thesis she took five or six of them, and I took five or six of the tasks. So, we did theses separately, but they were both based on the same sample. Both were on under- and over-control and a

variety of experimental procedures. It really worked out, I think, interestingly at the time. I received my degree in 1950 and Jeanne in 1951. In the course of our thinking and doing this work together, of course, we found we worked well together and thought we liked each other well enough to really, truly connect. I wrote my thesis in about eight days, each morning being locked into a soundproofed, insulated, windowless, padded anechoic chamber in the bowels of the psychology building sustained through affectionate guarding by Jeanne. There were no distractions and nothing to do but work.

Harrington: My gosh! Tell me about that. There are people in the world who would like you to expound.

Block: Ah! I describe that because that's what happened. I wanted to do it in one week; create this thing in one week.

Harrington: Well, that's how it goes. Wow! Had you been--why would she lock you in the room?

Block: Oh!

Harrington: Had you been wrestling with it for a long time and sort of not writing and probably avoiding?

Block: Probably preventing me from wrestling with her. But that--

Harrington: I see! Ha! Ha!

Block: No, no, it was a way of achieving concentration--

Harrington: Sure, of course.

Block: --no matter how distracting and so forth, but she didn't really lock me in a room. I mean...

Harrington: Yes. But--

Block: No, I really wanted to focus on it. This was a way of avoiding distraction. Writing in that anechoic chamber room afforded no echoes whatsoever; I mean, you speak in this room and it's really quite astonishing to hear your voice.

Harrington: Right. Quite seriously, did that follow a period of time where you had been sort of trying to sit down and write your dissertation, and it wasn't happening?

Block: No, not especially. I guess I might have been dithering a little bit, and indeed by the time I really sat down to write, I had obviously had a lot of preparation. I guess I started to work on a deadline or something.

Harrington: Have you ever done that since?

Block: Well, at various times I've worked in various intense concentrated ways, and it's been quite effective. At various times I've done that. I remember sometimes along those lines in writing research grants against deadlines and things like that.

Harrington: This is really kind of off this set of questions here, but it seems to me as though I remember your once saying something about having written some reasons for the apparent

inconsistency of personality--something close to that--very, very fast. I'm just personally curious about that.

Block: Well, I once wrote an article that took me less than an hour.

Harrington: Which was that?

Block: Oh, it's not an important article.

Harrington: Okay.

Block: I'd read something in a journal. I happened to have in my files the same thing, done even better, and I had a point to make. It's a short, a very short little article--I don't want to make much of it.

Harrington: Okay.

Block: There are times that you take an awful long while.

Harrington: Could take decades.

Block: As we both know, and take two lifetimes and one has only one.

Harrington: Yes. That's right. One of the questions here on the list is what are the origins of your interest in child development? And obviously what you've been talking about so far isn't directly around child development, you know, so let's kind of jump to that. The interview is sort of organized in terms of somebody who sort of starts out in child development and developmental psychology, but that wasn't your trajectory. So let's tell that story.

Block: I was in clinical psychology and decided that I didn't like doing therapy. I felt that therapy was practicing the status quo instead of advancing the status quo, so to speak. I felt personally uncomfortable because I was then 22, 23, 24, 25, doing psychotherapy. I had my own problems, and wondered what I was doing out there trying to stamp out mental disease. I thought I had research possibilities and liked research more than I had really enjoyed doing therapy, so I soon opted to become a research personality psychologist. My first job after my Ph.D. in 1950 was at the University of California Medical Campus in San Francisco, working at the Langley Porter Clinic.

I was working for Jurgen Ruesch and with Gregory Bateson on "communication." Bateson was working at this time on the double-bind approach. Ruesch was a very smart psychiatrist from Switzerland interested in communication and interested in, perhaps, too many things. Jeanne called him the Charles Boyer of American psychiatry. I had a lowly position with them. I met Bob Harris, Robert Harris. He taught clinical psychology, mainly the MMPI for the clinic. He was really an important person in my career, but died much too early as a relatively young man. He liked me and advanced my cause in various ways. Indeed, after earning my Ph.D. and just before I began my job at the Langley Porter Clinic, Bob, who was on the selection committee for UCSF medical school, told me if I wanted to go to medical school, I could get in!

Harrington: At last!

Block: At last! And then in five years of time, I would have an MD. I introspected a moment and said, "Gee, I really don't want to go." I'd seen what physicians do and I thought that being a psychologist was

more mind-using, much more interesting, and I still believe that. And so I turned that down. I also—and not irrelevantly--had just married sweet, vivacious Jeanne.

At any rate, I was now doing research on interpersonal communication at Langley Porter with Gregory and Ruesch. I published some stuff on parents of schizophrenic kids at this time. I began to develop ideas and procedures we subsequently used in personality assessment research at Berkeley. I soon sidled over to Berkeley part-time and then full-time. By '53, I was involved in a number of assessments that IPAR (Institute of Personality Assessment and Research) was doing on graduate students, significant writers, air force jet pilots, and students applying for medical school.

I was at IPAR doing various kinds of research when I developed quite a research program on emotion, how deeply or superficially people feel emotion, and also whether felt-emotion was relatively appropriate affect or inappropriate affect. I received a research grant on emotion from NIMH in 1955. I took some LSD about this time, before it became popular to do so. I had read and been impressed by Aldous Huxley's, *Doors of Perception*. I had separately developed a kind of theory of ego structure for which I thought the taking of mescaline might have behavioral implications, and also taking mescaline (which subsequently was recognized as having effects similar to LSD) was obviously a personal, over-determined curiosity. I thought that this drug approach might be a way of experimentally modifying ego structure. In anticipation, I checked my conjectures by referring to the established medical text of the time on pharmacology, Best and Taylor's *The Pharmacological Basis of Medical Practice*. As I evaluated the available information, I thought that LSD, mescaline, seemed to enrich and remarkably broaden perception, whereas tranquilizers dulled and narrowed perception. Alcohol, its effects I reasoned as having very different effects in the behavioral action category, but not truly affecting perception.

Harrington: Okay.

Block: Alcohol makes you more under-controlled, and amphetamines make you more over-controlled. So, I figured in terms of my little theory, if I were to take a unit dose each of alcohol, amphetamines, mescaline, and tranquilizers and if my conjectures were correct, nothing would happen.

Harrington: That's wonderful, Jack!

Block: I didn't try that, but I did try mescaline, not LSD. I said LSD, but mescaline is actually the same in its effects. It's quite a profound experience because I saw how that works, and subsequently in some indirect way, I became responsible, I believe, for Barron and Leary getting involved in drugs.

Harrington: Really?

Block: Yes, in a way.

Harrington: Oh, can you trace that? Connect it?

Block: Oh, yes. Well, I took this mescaline, and it was really a very profound experience. It became known I had taken it (although I wasn't talking about it).

Harrington: Okay.

Block: Frank Barron was also at IPAR. We were two young psychologists that were competitive at the time; we both had high priorities working at our careers and perhaps working against each other. He was intrigued by word of my mescaline adventure. I wasn't talking about it, so he went out and got some peyote or something, took some, and then he was modestly ostentatious about his experience. He

has always taken credit for being the reason why Tim Leary got into the drug scene, and since I think he got into it because of me and therefore, it follows.

At any rate, I was interested in the mescaline experience and, at this point, experimentally working on ego structure and personality. I started to realize that's very hard to do in a serious way after I saw about thirty people, in somewhat experimental ways, having first assessed them in various ways and then administering mescaline in proper ways in the hospital context.

I also was doing a lot of work on having people see an emotion-inducing film in order to make people cry.

Harrington: What was the film?

Block: It's "The Age--you wouldn't know it. It's a very effective documentary.

Harrington: Oh.

Block: It's a real, very well done tearjerker. Anybody, if you have a tendency to cry at movies, this would have made you cry.

Harrington: Oh.

Block: It was about a man who retires and doesn't know what to do with himself. He dies and his wife grieves and also doesn't know what to do. It's really--I don't mean this in a cynical way--it's a really effective tearjerker. I saw the film a hundred times, being personally much moved before, and after a while, becoming unaffected by the film. It was a real emotion inducer. I was trying to get reactions, affective reactions, as measured by skin conductance. The study actually worked out kind of interestingly, but I didn't have confidence in the results. So I never really published seriously on that. Subjects had a lot of nonspecific GSRs while watching that film, and the number of nonspecific GSRs turned out to relate to some interesting individual differences in the way they responded to a questionnaire on various felt emotions. At any rate, let's talk about that separately. I don't think we have to do it here.

Harrington: Okay.

Block: At any rate, I was publishing in various ways. Also, I had observed while I was still at IPAR and before I went into the Psych Department that the Institute of Child Welfare (subsequently called the Institute of Human Development, IHD) was getting ready to do an assessment of subjects that they had last seen in adolescence and planning for evaluating the subjects then in their 30s, which was central at this time. I had become involved in the Q-method, and I thought it was quite a useful method to employ in their context. I met with Jean Macfarlane several times and got her to agree to use it. She did subsequently use it, but only partially and not in the right way. So whatever data she developed ultimately was unusable. In any event, I began to realize that there's deep sense in the longitudinal approach. But then I entered the department.

Harrington: Now how did that happen?

Block: A position opened up in the department, and Frank and I applied for it. And we found that Don McKinnon, the IPAR Director, had brought us both forward and each for a half time position; we could share the one full-time appointment. But the department decided they wanted me full-time, on what I later learned was an absolutely adventitious basis. However it happened, it happened. When I later was

told of the reason for the decision, it proved to be kind of funny. Frank was departmentally judged to be too tied to McKinnon, while I was seen (validly) as more independent of McKinnon. On that basis, they voted me in, not because they saw that I had such sterling qualities or that I would be marvelous and so forth.

Harrington: Yes.

Block: Any rate, I now was full-time within the department, and I had tenure.

Harrington: Okay. I see.

Block: I had been doing some work on lie detection and externalizing and internalizing. It tied into something earlier done by Harold Jones, Director of the Institute of Child Welfare, which by then was about to be called the Institute of Human Development. It was really very congruent regarding what he had found, by quite different procedures. I got to talking with him about it, and he and I were both intrigued by the connection. He invited me into IHD, and I went there for a couple of months during the summer when he was off. I became intrigued by the longitudinal idea as requisite for really studying child development. He was most encouraging of me. Oh, yes! He also, in the late 50's and under a Ford Foundation Grant, actually saw some subjects again that he had started seeing in the 20s and 30s. So, in '57-'58, IHD was seeing subjects, doing skin conductance, showing movies, and so forth. Jeanne was doing some of that too; we both saw subjects.

Harrington: Did she have an appointment at this point?

Block: No, she was mostly preoccupied with our children.

Harrington: Okay.

Block: She did ad lib/ad hoc work, as her situation permitted.

Harrington: Okay.

Block: At any rate, I became intrigued by the longitudinal idea. I thought the IHD data there were a mess in many ways, but I was intrigued by the longitudinal idea. I don't remember the exact date when Harold Jones retired. After the going-away, retirement party for him, they left the following morning for Paris, and he died in Paris the next day. Mary Jones, his wife, returned much shrunken. It was very sad; they had become very good friends of our family. It was very, very sad and tragic. Mary was a marvelous person, too. She only died two years ago.

At any rate, I had become intrigued by the idea of longitudinal research and looked at some of the data, which were inchoate and in pre-computer form--I wouldn't call it data--looking at the files and a few collected things. I tried a couple of tentative, exploratory analyses and thought there might be some serious analytic possibilities residing in the packrat files. John Clausen, a sociologist, had come in as IHD Director and was selected unilaterally by Edwin K. Strong, the UCB Chancellor. I showed Clausen these possibilities, and I then requested a small grant from NIMH to try out some ideas, which worked out promisingly. I then wrote a large, multiyear grant application, in which John designated himself as Principle Investigator pro forma and I was designated as Co-Principal Investigator. I thought, naively, that being Co-Principal Investigator meant that you were right up there and on par with the Principal Investigator so far as the grant was concerned.

Harrington: You're smiling as you say this, yeah?

Block: Yes, because of some things that subsequently happened. Anyway, I got the grant. The grant was given to us with the anticipation it had serious possibilities, or so said somebody on the scientific review committee at the time. I was the prime mover of the research. I was young, energetic, and I'd done some things of good background for the venture. Anyway, we got this grant, and I started doing the data analyses that subsequently became the book, Lives Through Time. And that made me, if you will, a developmental psychologist. Earlier, I'd done some work on child-rearing attitudes, but I certainly was not then a developmental psychologist.

Harrington: When was that?

Block: This was in the mid-50s. I remember going to a SRCD meeting in Berkeley in the late 50s. This was before I was involved in IHD. It was a very small meeting. Everyone fitted into this little hall; now there's thousands and thousands of people. SRCD was then a very small, select club and I joined it. I obviously had interest in development by then, and why people turned out the way they turned out. If the study of personality tries to understand why people do what they do, and one naturally tries to understand why people have come to be this way, then you have to be in developmental if you're in personality, obviously. And so that led to less time, eventually, to dissuade after that. There's a long story after that, which I'm not going to go into right now. I will go, but we've got to take a look at that outline to see where we are.

Harrington: Let me ask you a question on the Q-sort because the Q-sort, you are so identified with it and so on. How did you come in contact with the idea of the Q-sort and so on? How and when did that happen?

Block: Well, it was in the early 50s. Carl Rogers had gotten involved in the Q-sort method and had done some interesting things, which caused me to read some William Stephenson. I became intrigued with the method, but not in the way Stephenson was using it. Stephenson thought the Q-sort method as being a "Weltaanschung," a cosmic philosophy of some kind. I don't know if you've ever read Stephenson?

Harrington: No.

Block: His was almost a religious approach to psychological research, but I thought of it as being no more than a very effective scaling technique. It was not quite a cosmic philosophy, and I was intrigued by it as a way of encoding observations, understandings, and etcetera. I was using it when I was working with Jurgen Ruesch and Gregory Bateson mostly to describe interactions, to scale artistic forms, to making various kinds of Q-sorts. We got published on sending and receiving of interactions. Remember that?

Harrington: I've seen--

Block: Yes. Well, I took one person, a single person, and had her describe the way, by the Q-sort method, she behaved with 26 other people significant for her. And she also described how they behaved with her, again by the Q-sort method. Then I went out to each of those 26 people and had them describe the way they behaved with her and the way she behaved with them. And then I would evaluate the congruence between how she behaved with her mother and how her mother said she behaved with her. And to the extent there was congruence, then she was having the effect on her mother that she thought she was having, so she was *sending* accurately. And then I went out to her mother to ascertain what her mother said she was doing with her daughter and what the daughter said the mother was

doing. I would get congruence scores to see if the mother was being *received* the way the mother wanted to be received. I did that with 26 pairs and did a factor analysis. I got all sorts of interesting things about types of role interactions. It was quite, quite cute. As a matter of fact, something quite similar to that has been done, but only recently, about three or four years ago. At any rate, I got into Q-sort that way. And then I moved over to IPAR--they were doing ratings and checklists.

Harrington: Right.

Block: Perhaps as an introduction of myself to IPAR, I proffered the Q-sort approach amid some uncertainty about it. Then, at a particular meeting I could not attend, they decided not to use the Q-sort, whereupon I heard about this and wrote an emblazed, passionate Q-manifesto. You know, it was intense and about 15 pages, which in some sense became the later Q-sort book.

Harrington: Yes.

Block: It convinced the IPAR people they should use the Q-sort, and they did use it. I explained what it did and how it minimized much of the response-set problems. IPAR used it, and it turned out to be a very useful method.

Harrington: There's a question here in this general intellectual history, what political and social events influenced your research and writing? It's a big question.

Block: Well, I don't know of events, per se, that influenced me. Politically, I've always been either liberal or radical, I guess. I read a lot of utopias when I was 13/14. I had been much impressed by Edward Bellamy's *Looking Backward*. I read a whole bunch of utopias, and I became a socialist when I was about 14 years of age; I still am socialist. If one wanted to achieve the kind of social system described in something like Bellamy or the utopias, I think that would be marvelous for humanity. I realize there are problems in doing so, but I also don't accept that it's ever been seriously tried. The Soviet Union, for example, has never been serious about Socialism.

When I was in Madison, Wisconsin, Norman Thomas, who was a great man of the time, came through. I organized a dinner for him and 150 other people by virtue of my Borscht belt experience, where I had been as a waiter and also in the kitchen; I worked the salad section. At any rate, I organized this roast beef dinner for Norman Thomas and all the other people.

Harrington: Ha, ha.

Block: --Afterward, he came back to the kitchen and shook hands with gravied me. Up in my study, I still have a marvelous picture of Socialist Presidential candidate Norman Thomas, the Unitarian minister.

Harrington: Ha, ha.

Block: Nobody would come.

Harrington: Oh yes, of course...

Block: What do you mean, of course?

Harrington: Well,--

Block: You're right. I was reading in the paper a couple days ago that 25% of American college students don't know that the holocaust existed.

Harrington: I know. They all--or doubt that it did.

Block: Or doubt that it did. Yes.

Harrington: I know, it's absolutely astonishing --

Block: Well, some say it's chilling! Yes--isn't it?

Harrington: I mean, just--

Block: At any rate...

Harrington: This perspective has influenced in your book that you're aware of?

Block: I don't know. It made me early on a strong environmentalist. I remember when I was at Stanford getting into an argument with Louis Wirth, who was an eminent sociologist at the time from the University of Chicago. He was spending the summer or quarter at Stanford, where he had done an attack job on hereditarians, which I kind of agreed with. For balance and I suppose just to be ornery, I guess,--I closely looked at the value systems of environmentalists, although I was myself an environmentalist. I found their scientific purity equally lacking, and we got in a big argument. This time I'm caught in between: on the one hand I was an environmentalist, but on the basis of strict analysis of the logic, and scientific purity or absence of ideology in conclusions, I concluded the environmentalist was as bad off as the hereditarian.

Harrington: I see. I'd like to pursue that just a little bit, just in terms of your own intellectual history and development. How did those two perspectives roll in together or become related? I mean on the one hand the real interest in environmentalism, and on the other hand, clearly your fundamental and personality kind of--?

Block: Yeah, I--you know that.

Harrington: And I happen to know you played around with trying to characterize and formalize the ways this environment situation and task, etc.--can you talk at all about that inter-play of interest or--? Maybe that question isn't clear. Do you understand what I'm getting at?

Block: Well, it isn't really clear. Do you mean-- I'm not quite--

Harrington: Let's just take it from here. I mean, you say that you really swung over kind of away from the environmentalist?

Block: Yeah, as a matter of fact--

Harrington: Was that a scientific move?

Block: I think that's a scientific move in the sense that I think the evidence is compelling on certain things.

Harrington: Okay.

Block: I think there are some things that are biologically ingrained that cause people to be certain kinds of ways. I mean some of it's genetic, and some of it is not. And they may just have certain developmental anomalies in the womb, for example, that cause you to be hyperactive, and hyperactive all your life, and that it wasn't genetically received and will not be genetically transmitted. And so, I just think there's a lot built-in from birth and certain dispositional tendencies. And so it left, but that doesn't mean I don't think that the environment is not important. Indeed, this partitioning of heredity and environment--

Harrington: Right.

Block: --seems kind of silly. I go along with the positions expressed by Robert Hinde and Pat Bateson in Cambridge. They are world-class ecologists/biologists. They really write very nicely the way about the family, the interplay of genetics and environment, how you need to keep these things connected.

Harrington: You were at Berkeley in the 60s? Did any of it--

Block: I was certainly among the faculty supportive of the free speech movement people, and I thought in many ways it was a rather beautiful movement. It got silly in various ways subsequently, and that was kind of sad and that always happens, but there was a purity to it initially that was very attractive, even moving to me. I certainly supported it, you know, in different ways. My 11-year-old daughter baked a cake, which she was able to have housed into Sproul Hall when the students took it over.

Harrington: Did the 60s influence your own work in your way? Did you see, or--?

Block: Not me, but Jeanne got involved in her work on activism; she did a lot of work in that area. And I was supportive of that; I mean, we talked about it and I might have had some input into it, but in my own particular professional work, I guess not.

Harrington: Let's move into the next section, which is called personal research contributions. The question, I think, we've basically kind of covered; what were your primary interests in child development at the beginning of your career? I think we really basically covered that, unless there's something you want to add--?

Block: In fact, there is something I would like to add to that. During the work that eventuated in *Lives through time*, one of my great dismaying recognitions in the beginning, in the middle, and at the end of it, was that the IHD data was really quite insufficient to respond seriously to some fundamental developmental questions. The IHD studies had been begun historically early but not carried through well, given the state of developmental psychology at that time. They simply didn't have good early childhood data. And subsequently, they did not collect or create good scientific data all the way through. Of course, I may only be manifesting retrospective wisdom, which is very cheaply attained.

Harrington: Right.

Block: I think now anyone can see lots of things that should have been done. And Jeanne and I thought it would be worthwhile to do a proper "lives through time." The phases of our respective careers seemed opportunely linked. So we came to the decision that together we would do our own longitudinal study to really get good early childhood data and then go on to follow the children into their later years. You know a lot about that obviously, but--

Harrington: I do, but the people ultimately listening to this audiotope might not, so why don't you elaborate?

Block: Sure, sure. Well, I was now finishing my book, *Lives through time*, and Jeanne and I concluded that we were at a point in our careers, such as they were, where it made sense to join up together and do really a right kind, or certainly a much better kind, of longitudinal study with proper early data...proper early data because they didn't have any. Indeed, there was some rather dreadful contaminated data in the ICW childhood study, and the Jones and Jones study had started with kids only when they were about 11 or 12, and that's too late. So, we decided that we would do our own longitudinal study and do it better from what we'd learned and thought. Drawing on this, we began a longitudinal study in 1969. I tried to describe the study in a chapter that's coming out in my Festschrift book, you know, from the Palm Springs Conference. I guess the book is titled, *Studying Life Through Time*.

Harrington: Something like that. Yes.

Block: Yes. At any rate, some of the reasoning, the plan we had, and the research desiderata are in that paper. But we explicitly formulated that and carried the study through. There were a lot of problems with that study, but it's a damn sight better than any other longitudinal study. I will also say, I think that's an objective statement.

Harrington: I know it's hard to answer this question, but in a sense, how long was that study in the active planning stages before it really started?

Block: Well, it's hard to say.

Harrington: I mean I realize that--

Block: You know, we--

Harrington: --are we talking three years, ten years, 20 years?

Block: Well, I mean, in the course of doing the "Lives through Time" study, you know, we were having dinner every night, Jeanne and I, having a drink before dinner together, and we drove in the car and we would talk during the day. And as thoughts would strike one of us, you know--this was the spontaneous planning. Suddenly, we sat down to write a serious research application. That was the active planning.

Harrington: Right.

Block: We had a lot of background in writing and preparation --

Harrington: And the writing of "Lives through Time" is really what's--

Block: --but--

Harrington: --the need for it and all --

Block: Oh, yes. Yes.

Harrington: --though obviously it was simmering before that?

Block: Yes. I think you can say, yes.

Harrington: Okay. What continuities in your work are most significant? What shifts occurred and what events were responsible? You've responded to some of that stuff, but I'd be interested in, you know, what continuities in your work that you see as most significant?

Block: Well, I think I was concerned to know why people turn out the way they do, obviously. I'm an introspective person, reflecting on my own life and the things that have happened fortuitously, and things have been over determined and do not happen this way, but we're going to have it happen something like that, like this was sure too--it's going to happen. And that's a fascinating thing to reflect on. And so I see that as being, in one way or another, in some deep way a continuity. Talking of more obvious ways of the kinds of things I've--I think my continuity is--I don't know. Oh my, that's a tough, difficult question. I need to reflect on that a little bit.

Harrington: We can come back to that.

Block: I've been interested in clinical problems and personality. And I think that we made a kind of contribution in this, interweaving more than most, perhaps often better than most, I can say, I need to believe, you know, methodology with notions or things that are really seriously psychological. So often, methodology will achieve purity by becoming of spacious content and while deep content becomes simply scientifically inaccessible. I've been very aware of that epistemic problem and tried to be, more than many, both methodological and conceptually meaningful.

Harrington: You've started to move into the next question, which is just a simple small question and that is, reflect on the strengths and weaknesses of your research in theoretical contributions, the impact of your work, and its current status in 25 words or less, Jack.

Block: Well, that's a confronting question obviously, what my contributions have been. I guess I should have prepared myself for that question; I certainly haven't. Well, I think that I've been involved in the primary issues of personality psychology during my career, and I think our notions quite early about some fundamental constructs in personality psychology have proven quite useful. Although there are other people who use concepts essentially the same, they use different names for them. You know about the "jingle, jangle" fallacy? Have I ever mentioned that to you?

Harrington: Tell us all about it.

Block: Well, the jingle fallacy is a label applied by a colleague of Thorndike. Thorndike mentions this in a book of his in 1904. This phrase was coined by a colleague named Professor Akins, who characterized the jingle fallacy as the situation in psychology where the same name is applied to two, quite different, phenomena. Thus, you label something anxiety that you measure by skin conductance changes in stressful situations and somebody else may label anxiety to be a certain pattern of responses to a questionnaire. You both are calling what you are doing "anxiety", but you are referring to really very different phenomena. At any rate, that's obviously something confusing to psychology and the source of many of our problems. Then in 1926/27, Truman Lee Kelly, the preeminent educational statistician at the time who knew about the jingle fallacy, added the jangle fallacy in his book. In it, he talks of the situation wherein two things that have different names in psychology are really the same. And, boy, is that a problem in personality and other psychology. At any rate, Jeanne and I came up with the ego-control (over-/under-control) and ego-resiliency constructs in 1950. I think those are two fundamental constructs, dimensions in personality psychology. There are many later similar concepts or dimensions, all with different names. Early on, we picked our names very carefully and for theoretical reasons. I

think those names have better conceptual meaning than most of the later names that have been put forward.

Harrington: What are some of those names?

Block: Well, people talk about reflection-impulsivity. Well, under-control improves impulsivity, but it also includes spontaneity and subsumes the good part as well as the bad part of impulsivity, and that's why we call it under-control. In reflection, there is a good part to reflection and there is a bad part to reflection. I mean an obsessive/compulsive can reflect too long; you can become immobilized by excessive reflection. We were aware of both the good and bad parts of being under-controlled and the good and bad parts of being over-controlled, and we wanted to recognize and respect that in our constructs. I think that often, when these related terms are used, but used without recognition of their conceptual basis, it leads to such peculiar findings as a measure of ego strength that has to be corrected if too high because if high, it is really a measure of over-control.

Harrington: Well, what do you mean too much ego strength? How can you have too much ego strength?

Block: Only if you are not measuring and conceptualizing right. In the field, a lot of things float around. There are actually various introversion concepts depending upon whom you talk to and how they mean extraversion/conversion. Sometimes it means something like under-control; other times it just means sociability; other times it means a messy, mashed together version of those two notions; and then you don't know what they're talking about.

At any rate, I think that the ultimate theory of personality psychology in the sky is going to have concepts very much like ego-control and ego-resiliency. It will have some other things as well, but it's going to have something akin to those two. We did a lot of work on those constructs, on the behavioral manifestations of control and resiliency, on experimental implications for the concepts via risk taking and tolerance of ambiguity, or the auto-kinetic effect, for example. We published a number of things during the 1950s on our concepts, and I think that was perhaps instrumental in my early reputation, such as it is, since we were doing experimental work in personality. And then I'm proud of the Q-sort method, and that has been a valuable, I think, contribution, methodologically. It certainly has been taken up, indeed, my Q-sort book published in 1961, more popular now than it ever was, which is an interesting fact.

Harrington: That's right.

Block: And then in the mid-60s I got involved with the response set controversy, with *The Challenge of Response Sets*. Oh, I can think of a continuity, a deep continuity in me.

Harrington: Okay.

Block: I'm an anti-nihilist. There've been a lot of movements in psychology that have become popular and achieved a certain cache, and built reputations for people that are based on saying there's meaninglessness; that what you think is there, isn't really there; it's only an artifact. So it was with response set furor, which argued that there's nothing meaningful in what people say about themselves, it's all due just "acquiescence" or "social desirability," or the Mischel thing: there's no continuity, no coherence, no consistency in personality, says Mischel. Jerry Kagan went on to say, "There's no continuity in personality," you know. These are nihilist positions, and it has been my view that they have really been naive nihilists. Nihilists are supposed to be sophisticated. It is only those who are poor, simple-minded souls who believe in the relevance of personality matters, whereas nihilists presume

(unwarrantedly) sophistication. I think the nihilists have had dreadful influence in psychology and disparaged people concerned with psychological meaning. In the long pull, I think the meaning seekers have done better than the nihilists have. In some important sense, I think I have been an anti-nihilist. I really mean that.

Harrington: Why? I'm glad you mean it. It is a very interesting point of view.

Block: In many ways with respect to response sets, with respect to the consistency of personality, with respect to the continuity personality, which is a consistency with temporal dimension added, with respect to developing an improved way of estimating the number of findings significant by chance; you know that effort? Things were often being ascribed to chance that were really meaningful. I worked out a computer method that made no stiff assumptions and then discovered that in many psychological statistical analyses the conventional assumptions were fundamentally incorrect--more things were significant than had been thought. If you look at certain data more closely, you can often identify things ascribed to chance that are really meaningful. That typifies a lot of things I did, and still does.

Harrington: There really is an order. There is an order and a structure in--

Block: Yes, it is in reality, I think.

Harrington: But the methods have to be good enough to kind of --

Block: Well, yes. Many were open to Mischel's nihilism, not recognizing that his selective research citations involved bad methods or bad conceptualization and so forth. Well, I'll tell you another version of this--

Harrington: Okay.

Block: -- namely, attribution theory and what I've always called allegation theory.

Harrington: Tell me about that.

Block: You see, attribution theory implies that there's no reality there. Thus, intelligence may be attributed to you. In attribution theory, we focus on why I happen to do that and what within me causes me to attribute intelligence to you? Attribution theory in turn implies that there's no reality to an attribution. Now suppose I want to suggest, I want to allege that you're intelligent. And if I allege that you're intelligent, maybe because of some peculiarities in me that cause me to attribute intelligence to you, but the attribution may be because it could be true. You might be intelligent, so I allege, which leaves the issue in doubt. Of course, there are certainly some interesting things in attribution theory, but the whole focus was on that there's no reality out there. Simply talk about the errors, the shortcomings and what's the phrase of judgmental processes without recognition. With that among other things, the judgmental processes might be correct. And they're probably because of experienced-as-valid reasons and they damn well have been evolved, valid judgmental processes.

Harrington: Right.

Block: At any rate, that's another--

Harrington: That is a very interesting continuity. That really is.

Block: You've known me well enough and long enough, so you can kind of see that's involved.

Harrington: But I hadn't realized it until you said it—

Block: Well, there is a point to it. Well, at any rate, going chronologically, there's the response set controversy, then really, as you know, a big issue--

Harrington: Right.

Block: --really from in the 60s and--

Harrington: It was almost immobilizing—

Block: It really was. And, I think my response set book really provided an explanation of how that interpretation could have come about quite accidentally, for understandable reasons, and how that interpretation could be tenable. But if you didn't confound response set with meaning, the response set interpretation had to be set aside. I think that the Challenge of Response Sets, that book of mine, quite ended the controversy and made me a world expert in something that was no longer of interest.

Harrington: Including you...

Block: Yes. Then it is said out of this *Lives Through time*--which, I think was a peaceful contribution in terms of organizing all of that data. And that takes me to, I guess, the 70s. I became involved in our own longitudinal study and some other things I suppose. And I wrote this chapter for this Magnusson book on, you know, recognizing the coherence of personality, which I think had a lot of effect at the time in responding to Mischel. I received about thousand requests for copies, before it was published.

Harrington: Well--

Block: Yeah, really! Maybe it was 800, probably not a thousand. So I already had a lot of influence, and I think I am pleased by that as we had positive influence. Of course, I think Mischel had much too much influence, given the nature and quality of his arguments. I think his assertions happened to fit in with the experimental social psychology movement at the time, but I think his were poor quality arguments in fundamental ways. I don't want to go into that, but it was methodologically naïve, and his literature was selective and ideologically driven. Except for that, "besides that, Mrs. Lincoln,--how was the play!" You know that joke.

Harrington: Yes.

Block: And now we are in the 70s. I also became involved in arguments regarding the continuity of personality. Jerry Kagan was talking about discontinuity and discontinuity was a big, big phrase in developmental psychology at this time. And I think the reason people were saying it was because they weren't creating good quality data, or weren't looking at it properly. I think that notion has gone away pretty much. And I think some of the things I was involved in may have helped that unfortunate nihilistic notion go away.

And during the 80s I was involved with--really our longitudinal study, you know. Things had started to come out of that of attractive consequence. And that study is maybe the point where really so many things can be done. And it obviously is going to be a body of data that can be approached in subsequent years by very many people in ever so many ways and will be fruitful in ever so many ways because it's going so many directions.

Harrington: So with respect to the issue of the current status of your work, among other things, you see the longitudinal study as on the verge of--

Block: Well, I--

Harrington: -- bearing an enormous--?

Block: Well, I think we've put out some very important things already, and I think I know enough about the data, which is really overwhelming in so many ways. Now it's so easy to do analyses, look at the analyses, and get some recognition of important things. Then you really need to put in a lot of time, not just look quickly. And finally you have to integrate it and write it up. We've done many more analyses and appreciable looking, but not enough serious integrative efforts. I would like very much, and I propose, to try to do a book that will use the longitudinal data organized around control and resiliency and with some of that theoretical section greatly advanced. That's my intellectual aspiration these days, years, or whatever.

Harrington: Does that bring it full circle?

Block: I probably wouldn't be able to do it, but I hope to--when you reach that full circle, you know, it's marvelous. I am reminded of a short story by a Russian, I think Tolstoy, entitled "How much land does a man need?" A peasant was told by a nobleman that he could have all the land that he can enclose in a circumference in the course of one day. He has to get up at sunrise, start running, return by sunset, and he will be given as much land as he has encircled during that time. Okay? You said I've come full circle, right? So the guy starts running at sunrise and he runs and runs, and he makes his first turn. He keeps running and running; it's a new direction. "I've got to make a second turn." When does he make that second turn? Reluctantly, he makes it and pushing and pushing onward, the legs are faltering, but he's pushing and wanting to make that last turn and head for home. Of course, he finally makes that last turn, pushing the limits, and he exhaustedly reaches the end. He gets there just as the sun sets and then drops dead. When you said come full circle, I didn't know exactly what you meant--where you mean how do you draw a circle? How do you draw a circle, you know, that problem?

Harrington: Yes. Yes.

Block: It obsesses us all.

Harrington: Yes.

Block: And do you have sufficient obsessiveness or do you have too much obsessiveness?

Harrington: Yes. The question also asks what you see as the weaknesses of your research and your theoretical contributions.

Block: Well, I think I have had some good theoretical ideas and by virtue of my obsessiveness, I have brought them forward. And it is really elegant, and maybe even egomaniacal to say so, but I think that much of what passes for theory in psychology, or controlling its use, is really an uncritical expression by people who are comfortable in saying things rather than being sufficiently self-critical. I think I am very self-critical. And some of that is good, but some of that has been immobilizing in me, and I haven't brought out some theoretical things or recognitions that probably would have been worthwhile to bring out earlier rather than waiting for that ultimate day when they achieve ultimate perfection as that's just never going to happen. So I'm critical of myself for not being as theoretical in print as I should have been. I have published in *Child Development* a paper, "Assimilation, accommodation, and the dynamics

of personality development,” which is, I think, solely conceptual, which I think says some interesting things. I should have done many more things like that earlier and since. I made excuses at one time or another for not being theoretical enough on the grounds of my doing useful research and doing other things that are of value as well. But I think that I should have published more on theory, and within myself been more sustainedly focused on such things. That would have been personally satisfying, and maybe even of help to the field. As I look around, I see people who made a splash in the field, and I think psychology is such an immature field in many ways. Some of the others get up on top of the mountain and shout loud and repeatedly and assertively; some of them get listened to because the audience down below thinks, “Gee, this person wouldn’t be saying things so repeatedly and strongly unless he knew something that I don’t know.” But maybe he really doesn’t know one thing more than you know. In other words, there are a lot of things that happen in psychology that are not evaluated in the right ways. It just irks me as a methodologist. I think I’m morally into methodology—but as a means, not as an end.

Harrington: I think you’ve started to answer the next question, which is what published or unpublished manuscripts best represent your thinking about child development?

Block: Well, I suppose, “assimilation and accommodation”--

Harrington: Yes.

Block: Funny, you say you looked at it recently. I think it’s a nice paper; I wish I had written it. I guess I did the paper that you think is really great. I would mention some other papers that were written by other people that I wish I’d written, by Norbert Bishoff, about 1975, which is, I think, a magnificent paper nobody knows about. You would like it, I think. If you haven’t known about it, I’ll dig up a reprint. It’s really a theory of functional systems and an analysis of anxiety. At any rate, I think he is a German psychologist. He hasn’t written very much, but that article by itself is enough for a scientific career. It’s a marvelous article.

Harrington: So far, assimilation, accommodation--

Block: Yes, that system on personality and on the motivation-processing theory of personality. I really wrote much of it thirty years or so ago that I could have taken further, but I never brought it out into really public view. I showed it to a few individuals, more or less, informally and privately. I think there are some interesting things in there, but it certainly has to be taken further. I should say I have taken it further, but not to circulate around. And I’d like very much to work on it on--my thought is to build part of that into this book I’m organizing longitudinal data around, the resiliency and control concepts. I think in our data those two concepts show up time and time again. They underlie all the conduct disorders and what’s called externalizing/internalizing; to review, there’s un-resilient over-control, un-resilient under-control, and things like that.

Harrington: It also asks about which of your studies seem most significant, and especially with respect to development? Let’s kind of hold it down to developmental.

Block: That makes sense in this context. Well, I think that our findings coming out in this chapter reported at the meetings on the continuities of ego control from age two to age 23 so far. Under/over-control, the correlations in ordering of subjects from age three to four, five, seven, 11, 14, 18, 23, for both boys and girls, that kind of continuity is rather impressive and an important finding. And the finding, that in respect to resiliency, shows that same type of continuity for boys, but not for girls. It’s very impressive and has much implication, so I think that’s a very important finding. Why don’t you get that continuity for the girls? The methodology is the same; in three of four comparisons girls show

continuity from adjacent time periods, but different kinds of sliding transformation as the girls enter puberty, you know, things start to get mixed up. I think that this has large developmental implications, and I would mention that as an important thing. I think some of the work on self-esteem continuities and changes has an analogy; our longitudinal paper on self-esteem is going to be coming out in *Child Development* in the next issue, I think, or the June issue it is. I guess June will be the next issue I guess and it would be considered an important paper. It's the first formal and sufficiently elaborated presentation and presents some interesting continuities as well as some interesting absence of continuities, which is not the same as saying there is discontinuity.

Harrington: Ha! Ha! Ha!

Block: Discontinuity is a very different word, and people should probably never use the word discontinuity. They should, at least, examine closely what that term means and use it much more carefully than it has been in the past. At any rate, I think what I find interesting is the different basis of self-esteem in young men and young women. You know, what self-esteem is predicated upon in young men is different from what self-esteem is predicated upon in young women and I'd say I think that's a very interesting gender difference. I think from our findings on that from a very early age you can identify the--by which we mean nursery school data. Our nursery school data identified the people who were going to be in a drug scene 20 years later. They identified from nursery school the people who would be relatively liberal or conservative politically 20 years later. You know, that's a pretty impressive thing how we can look at nursery school data, identify those who will have certain positions on civil rights and abortion and on a spectrum of social/political attitudes. You could identify at the age of three and four, people who are going to be at one extreme or the other, and the correlations are surprisingly high. The drug data attracted a lot of attention because of the topic, but it's implicated deeply with respect to issues in developmental psychology. And I think our work on depression, the antecedents of depression, and the different flavors depression has as a function of gender that is of interest and has some large implications in terms of understanding developmental psychopathology. The phrase, 'developmental psychopathology' is an attractive one, but it's nothing if you do not consider normative development and how it can go awry. You want to know the ways in which normative development proceeds. We've done some very useful and unprecedented things, widely ranging, in a longitudinal study and you can answer, or you can respond to some questions you can't respond to in any other way. It's a long enough longitudinal study, and we study both sexes, not just one sex, and we have spent a lot of time and energy and, I think, some intelligence in getting good measurement along the way. So if you don't study both sexes, you don't study long enough, and you don't get good measurement, then you're not going to show very much. One of my jokes about Mischel was that the best way to support Mischel as right is to do bad research. If you don't measure things well--in all you don't have measures, or they're not reliable--you're going to support Mischel with respect to the inconsistency of personality and personality development all over the place. I think by doing better quality research that you got a shot at it, and if it's really there you're probably going to find it.

Harrington: That's right.

Block: It's really in there, isn't it?

Harrington: It seems--156--it's really there.

Block: Yes, it really is. We got good quality research, and then you can know it relatively fully.

Harrington: I find this question somewhat interesting. Which contribution did you make that seem the most wrong-headed?

Block: I never made a WRONG-HEADED contribution in MY LIFE! Everybody knows that. And, oh, I don't know--

Harrington: Seriously, is there anything out there that you--

Block: That you'd--

Harrington: --now you might pull back or something, or--?

Block: No, I don't mean to be a--

Harrington: No!

Block: --at the moment, I don't know if I have anything I can or should pull back. No. I hope I'm not being hysteric, or repressive, disassociated, or whatever. Indeed, there are many things that inhibited me and things I might have said, but did not. I'll pass that question.

Harrington: Okay, that's fine-

Block: I've been privileged all the way through.

Harrington: That's fine. Please reflect on your experience with the research funding apparatus over the years? Comment on your participation, participation in shaping research funding policy, implication, securing support for your own work and related matters.

Block: Well, that's a large question.

Harrington: It is, but it--

Block: Well, I have some interesting--until most recently, I have been, you know, successful in getting support and I'm expecting to continue to gain support. I got my first research grant through NIMH in 1955. I had it for a couple of years, let it lapse as I entered the department, and didn't have any research grant for a year or two. Then, it must have been about 1960 when I again applied, again got money, and have had money since 1960 until a couple of years ago.

Harrington: That's quite a record.

Block: I don't know if it's a record, but--

Harrington: No, no what did I--

Block: I would apply and would get money. So I'd done pretty well that way, and I'd been involved in the funding situation at the other end while I served on NIMH committees and chaired one; first I served and came back as a chairman, and I guess I fulfilled that role reasonably well. And I've been an ad hoc reviewer often for NIMH, even after I left, and for W.T. Grant. So I've been immersed in research funding on both sides, and I think it's been a decent situation. The process has been pretty good though. It gets caught up in political considerations every now and then and goes flagrantly wrong. I think right now that our longitudinal study is at a time when, in my judgment and I think not only in my judgment, it's really ready for some serious harvesting and the notion that they should not have funded it or given it such an insufficient priority to get funding, is terrible, I think. This obviously irks me. It more than

irks me; it discourages me. They said such nice things about the project, but then we're not getting any money and that's really a shame. I say it not because it's self-serving--it is that, certainly--but I really do think, and as I say, many people think that our database going back 20 years can be speaking to so many issues and can be harvested in so many ways, but the harvesters now aren't there; the money for harvest isn't there. I think that's unfortunate, and I don't know why that is so. I felt a little paranoid about that. And I most recently began to wonder if it is because I have become "emeritus" or retired. I find that once they hear that you are academically retired from formal teaching, they presume you're retired intellectually or scientifically. I don't think I'm retired!

Harrington: It just means you can now get down to serious work.

Block: You're right! That was my thought. That was my thought, you know; my goodness, the university is giving me and other faculty this golden parachute inducement to retire, and then I can get out from under and I can really finally get down and do some serious work. I mean I've been doing some serious work, as you know.

Harrington: Yes.

Block: And lo' and behold, the world out there with its expectancies or stereotypes, or whatever, means I haven't gotten the funding that I need. I find that, I'd say, more than annoying.

Harrington: Your role in shaping research-funding policy, in connotation, securing support for your own work related matters?

Block: I don't know if I've shaped policy. I've sat on various boards, but not the conferences, which are research-funding conferences. I do think largely that time and our longitudinal study has been encouraging people to recognize that there are certain continuities and that certain things can be done. I think, although we're not getting money right now, I think longitudinal research is more in vogue now that it was ten, 15, 20 years or so ago.

Harrington: And then since you--your work has had a role in that?

Block: Yes, I think so. Yes.

Harrington: What about the general attitude of funding agencies toward longitudinal work?

Block: Impossible.

Harrington: Can you talk a little about that?

Block: Well, the federal government, by virtue of four year political reigns and sociopolitical considerations, has made it very difficult to plan in the way one really should be able to plan a longitudinal study. You know, you have to come for money at regular, not long separated intervals. Writing applications is a strain and drain, and there are political vagaries. Somehow it should be easier than it is.

Harrington: Did it used to be?

Block: It was easier, though right now, of course, we're not doing well. It was never easy. It was never easy. Right now it seems to be especially difficult, at least for me. Maybe other people are doing better, but I'm not doing very well. And I thought, quite frankly, we reached a stage and been

productive enough that it should now be especially easy; instead it has proven to be especially hard. I don't understand that. Whether it's that at this moment in May 1993, there is such a shortage of money or whether in some ways that I don't know about what people back there concluded is not a productive study.

Harrington: Some people--this isn't on the questionnaire here--but there's general talk that to some extent the system perhaps ought to pay more attention to funding those who have track records of being productive people. And on the one hand, there's sort of the idea of funding the person and on the other, there's the idea of funding specific projects. And there is some talk that perhaps the system is much too much in the direction of funding specific projects with too little willingness to just keep upping money into people who keep doing good stuff. Do you have any thoughts about it?

Block: Well, yes. There are dangers either way, and profits either way.

Harrington: Sure.

Block: And you're probably right that it's tilted too much in the one direction and not enough in the other direction because of funding "productive individuals." When you define productivity, of course, you get in many problems just like that.

Harrington: Sure.

Block: But I would agree with that, yes. I really think that getting funding should be made easier. I think that there are particular problems in longitudinal study in showing continuity.

Harrington: Right.

Block: So many of my colleagues in my department do research that they can stop and start at a moment's notice. You know, with longitudinal research you just can't do that. You don't have that flexibility; you cannot stop time. If you stop it, you may miss something. There is a necessity of detail. In our longitudinal study I've been involved in things--people in the project have been involved in things we never mentioned or need to mention in writing a formal scientific manuscript: like laying carpet in an experimental mobile home, or making curtains for it, or responding to a subject who is calling up for some help on something or other. You respond in ever so many ways, you're caught up by it, and nobody knows. Or a lot of people write not for just reprints, but rather with elaborate questions. You speak to a peer sometimes about the time you spent in responding to a letter from a graduate at Yale. I never heard from him, but he spent days and days costing so much per hour and so forth. You do a lot of things that you don't get paid for, and a lot of things that happen don't get mentioned in research reports. The special problems in longitudinal research are not recognized by granting agencies or research review committees, which are often peopled who have no background along those lines. Often they're people who don't have any interest, even if they did have background or vice versa. And I don't think the granting foundations, such as the Macarthur or W.T. Grant, are cognizant enough or if they are cognizant, they'll make the right decisions. And the grant foundations should certainly be supportive of this. We have not been encouraged by them. But it's maybe me, actually. At any rate, I've got this reputation in the field of a certain kind as being prickly, and I'm not very political. Actually, I may have alienated various people who've been influential in the field, and I think that may be now operative or has been operative in the past in various ways. And as you know, I've been argumentative in the literature and I don't apologize for any of those arguments that I've been involved in. I think they've been helpful to the field. But nevertheless, there are some influential people out there that I have not either paid attention to or given the respect they feel they're entitled to. I was a young Turk, so now

I'm an old Turk. I've never been really part of the establishment, and there is an establishment. Some of that's okay to be included in the establishment and--

Harrington: Do want to talk about that, why you haven't been?

Block: Well, part of it is my style, my choice, or not knowing how to do it is part of it, I think. Some people would socialize in certain ways that makes it easier for them; I could not. Jeanne could be part of the establishment without it influencing her integrity, and I never knew how to become part of the establishment.

Harrington: Right.

Block: But I really haven't known how to do that. I'm more so standoffish. I'm not the most sociable person in any context. Also, when I don't like somebody I really find it very difficult not to show it. And there are some people in the field that I don't like in terms of what they say and on the basis in which they say it. And then my feelings may be, in some sense, rather critically expressed by a strong word, but I mean it. When I saw some very visible influential people who I thought did bad work or should know better or that have misrepresented things, I can't shrug away my feelings about it. And that is not compatible with the edict: to get along, you have to go along. I don't go along. My argumentative strength was also my professional weakness.

Harrington: Right.

Block: You've seen that in me in lots of ways.

Harrington: Well, yes. Is there a part of you that kind of enjoys being the outsider?

Block: No, I don't think so. I don't really think so.

Harrington: Okay.

Block: I am reluctant along both, yes, like you. Now, maybe at some deep level, something does get in--I've come up with some phrases, some of which I've used that I figure are absolutely and deliciously malicious, but that's just joy with words and they come up especially after. But I've felt for long unappreciated, and so, maybe deeply, I suppose I do get some kind of perverse pleasure, but I'd give the one for the other.

Harrington: Well, in a way, that helps us as I go into the institutional questions having to do with-- these are a set of questions having to do with what institutions have you worked in, dates, capacities, etc., and then kind of describing your role with various people or in the institutions and so on? And you, at the start, you know, you sort of covered some of this stuff, but backing up and talking a little systematically if you want to--

Block: Sure.

Harrington: --about the IHD experience, the IPAR experience, the psychology department at Berkeley experience, insider/outsider, etc. Whatever kind of way you want to take this up.

Block: I was at IPAR long enough. IPAR was quite an interesting place for me at the time in many ways though.

Harrington: What were those years again?

Block: From about '53 to '57 when I entered the Department--

Harrington: Okay.

Block: --and there were some interesting people there at the time. But I was a junior person mostly. Don Mackinnon was director, Richard Crutchfield was there, and Harrison Gough was there as senior people with more clout. Frank Barron was there before me, and I guess I was somewhat junior to him. And they were doing interesting things. I was interested in moving onto an academic position and had an opportunity for one of these junior professorships at Harvard. Jeanne and I talked back and forth about whether to go or not.

Harrington: Right.

Block: It was one of these 'up or out' things.

Harrington: Right, mostly out.

Block: But almost, almost always out. Nevertheless it was, you know, tempting, given the aura that surrounds Harvard, much of which is on the surface, I think, at least as far as psychology is concerned. I was tempted to go,--I got this nice letter from Robert White--but decided not to go hoping I'd have a chance to get into the Berkeley department, and a couple of years later that happened. So, I got in the position of having rejected Harvard. There are three levels of status in the field: never having been invited to Harvard is lowest; better is to have been invited to Harvard and gone; and the highest form of prestige is to have been invited to Harvard and turned them down. Well, I'm in the highest level.

So we stayed and a couple years later in 1957 that Berkeley departmental position opened. Frank and I applied jointly, but also vying. And it happened to come up for me, I think, quite by accident; it could have gone the other way, and so was my life determined. I went into the department and was there for the rest of my career, basically. I never went back to IPAR. I was interested in going back, but there was some tension, I guess, which I hadn't been appreciative of, I suppose; or perhaps they didn't have the degrees of freedom to let me go back when I wanted to go back a couple of years afterwards, part-time. It may have been Harrison at the time; I really don't know what was involved, but when I expressed an interest in going back they weren't interested in having me, so I never went back. And when they really wanted to get me back, I just wasn't interested, even if only out of pride.

IHD was percolating with respect to the Ford Foundation follow-up session they wanted to do in the late 50s, and I was trying to influence that in terms of doing GSR work with their subjects. And when I was suggesting that a good way of coding their observer data and interview data might be through the Q-sort, Harold Jones invited me into that study, and I was there for a summer. That was the summer of 1960 when I finished my Q-sort book. And then John Clausen came into IHD, appointed as Director by the then Chancellor who had become impatient with the Psychology Department's nominations for the Director of IHD. There's a big long story about that. They needed to get a director for IHD after Harold Jones retired, and the Psych Department was then in a very turbulent and contentious time. The Psychology Department was recommending people totally inappropriate for the position of Director of IHD. Richard Solomon was recommended for the--

Harrington: Richard Solomon?

Block: Yes. He was a rat psychologist. He was voted in by the department to be Director of IHD. They rejected Lee Cronbach, although I think he would have been a good Director of IHD. But having selected Solomon, the administration decided that he would be inappropriate. Some contrary letters had been quietly sent by opposing members of the department, which allowed the administration to receive a wider perspective. They realized that the Psych Department could not be trusted to make a sensible nomination. And so, then Nevitt Sanford was explicitly appointed as Director of the Institute. Then he wanted to come in from Stanford with a lot of people and, indeed, he gave an opening lecture as the new incoming Director. But in subsequent negotiations with the administration, what he wanted they weren't going to give. So he never really was Director; and there was no director for a while. Then the Chancellor, visiting Washington, met John Clausen briefly and appointed him right on the spot.

Harrington: Wow!

Block: So now you know why it was Clausen came in.

Harrington: Wow.

Block: -- I had my difficulties with Clausen, and I was not alone.

Harrington: What nature were those difficulties?

Block: Well, I viewed him as very bureaucratic, as really violating my integrity in various ways, and he had me in powerless positions in various ways. Another reason was about a grant I wrote. I wrote it, the money came, and he wanted half of it. And so it was a study that he never did. He would appoint people--two wives of colleagues of his in the Sociology Department--to positions within my section of the project. One was okay, the other one wasn't. That's not the way you do things, you know. I went off on sabbatical from 1963-1964 to Norway, and while I was away I was working with somebody, Norma Haan. Norma Haan was a failed MA who was brought in before I came to IHD. She'd been doing some interviewing. When I came into IHD, I connected with her. She was a feisty, spunky, and I thought, intelligent woman. And we were working pretty well together. She was a rather emotional person, but I like intelligent, feisty women. I don't know; I didn't want a doormat. She was very helpful to me and wanted to work with me--a lot of crud needed to be done, and she did quite a bit of it. I did a lot of it, too, I might say. But then when I went away on my sabbatical, I set it up for her to be in my role while I was gone. And while I was away, Clausen and she put out a grand statement granting accessibility to all the data in my NIMH IHD study. I was developing and had developed already multiple Q-sorts on each subject using a panel of thirty closely supervised clinical Q-sorters, carefully attending to quality, checking on stereotypes, their quality, and controlled analyses. I lost my youth on that study! And--I had been involved in that study for a couple of years working day and night on all sorts of things, setting the design. At any rate, I set it up so Norma would have control of that study while I was away in Norway. Clausen and she put out a statement indicating people could use the Q-sort data I'd been instrumental in organizing and controlling, coding and testing, and evaluating. And from Norway, I protested. And I received assurances from Clausen that I would have full control if anybody else wanted to use it. I'd be able to control whether they could and could not use it. Separately, Norma wrote me a letter saying I would be jeopardizing her position at IHD if I complained. And so I came back, and actually I didn't formally complain as I believed Clausen's assurances. I came back a year or so later, I think, and there appeared in my mailbox one day a manuscript ready to be sent to the printer that had been based off the Q-sort data without my knowing it. The person involved had been separate from my research project; they simply had gotten hold of the Q-data (while I was arduously testing out its reliability) and used it unbeknownst to methodologically concern me. I complained and, ponderously, an IHD committee of Clausen and some others agreed that, yes, that wasn't very appropriate and that therefore this manuscript would be withheld or delayed until I had a first chance at publication. Two

months later, a second manuscript came forward that I didn't know was being done. This time I was told by this committee that was, of course, shaped and controlled by Clausen and Brewster Smith that since I had received a favorable ruling the first time, then it was only fair to rule the other way the second time. Two and two was four the first time, but not the second time; I found that intolerable. Also with Norma, I decided that--oh yes, I chose then to leave IHD. I'd gone back to the department full-time because of Clausen. I had created this big NIMH project within IHD with a halftime appointment, my other half appointment being in the Psych Department. The academic year was going on and I had to make arrangements for the following academic year at IHD. Being the grant's prime mover and co-principal investigator, I expected to be invited again; there was plenty of NIMH money and I early on mentioned to Clausen that I had to make academic plans for the coming year, but he didn't invite me. Perhaps he was waiting for me to ask him again; I wasn't going to ask him. I went back fulltime in the department.

Harrington: Right.

Block: There were some other things going on--and this kind of detail maybe is a little too much detail. Meanwhile, Norma, who had a lot of energy, was writing some things and as she wrote, I realized that she couldn't write. She was writing simply beyond the data as far as I was concerned. I tried editing and re-writing and so forth, and she claimed my changes were ruining things. And it became clear intellectually that her writing in that way and the words she produced were words that I couldn't accept; and she couldn't accept mine. At one point she drafted a chapter, and I re-wrote it quite entirely and I really mean fully.

At this point Clausen was no longer, having seen to it that his old friend, Brewster Smith, was Director. Crossen was still there. Brewster had been a part-time colleague of Clausen, and Clausen brought him into IHD first as Associate Director. Brewster then replaced Clausen after a five-year term. Norma Haan decided she couldn't accept my rewriting of her chapter; she didn't like mine, and I couldn't accept hers. I tried to talk to her, negotiate, and urged her to keep our disagreement to ourselves, but she said, "I'm going to Brewster!" She went to Brewster against my request that she not. I didn't want a colleague and peer in my department trying to--I didn't like Brewster for various reasons also. I won't get into those. And Brewster read those two chapters and privately came to me saying he had not realized how bad Norma's chapter writing was. He had been sympathetic to her complaints about me and my arrogance, of course. And we talked about--I was angry with the whole situation because I didn't like this way of doing things, and Norma unilaterally had gone to Brewster Smith. That really settled my relationship with her. I felt that she'd been disloyal to me earlier with Clausen and was being disloyal to me now in invoking Brewster, and also her own creative work wasn't very good.

Harrington: Right.

Block: I actually severed my relationship with her. I had joined IHD, received a NIMH grant, and I couldn't bring my grant with me. Clausen knew full well someone defined as Co-Principal Investigator was without NIMH influence compared to the person being defined as Principal Investigator, so he had all the money. I therefore wrote another NIMH grant from the Psychology Department and had some money, and it was enough money to do the remaining analyses. I did the analyses and wrote the book, *Lives Through Time*, entirely by myself and entirely outside of IHD, which did not see that book until it was published. There was a lot of tension between IHD leaders and me. I would not talk to Norma, who would also not talk to me. I wouldn't talk to Clausen, who would not talk to me. Clausen over the phone once said, "I'll get you for this." I quote that; he was very deadly. And afterwards, in some respects, he did get me. He was much more political than I was. In any event, there may have been some rumors, and you may have heard somebody's rumors about this, but I walked out of IHD. As far as I was concerned, it was a matter of self-respect to do so. And with respect to Norma, her writing, and oh, if

you look at some of it yourself, I've still got some of her drafts actually someplace. It was opaque and fundamentally incorrect. There are some jokes I can tell that are too detailed, and it's not very nice. She was once writing up a whole bunch of pages based upon correlations with values well over one.

Harrington: Those are quite strong correlations, Jack.

Block: Yes, she blew up the data. She looked only, and uncritically, at the summary computer output. This was a time when the sequence of program cards was all important. Her correlations were fake correlations. The batches of cards were out of order, putting all the correlations above one. But the summarizing program didn't know that. It considered any correlation above, say, .3 as significant. Therefore any number above .3 is collected as significant, including nonsensical correlations with values of 3.2. At any rate, there were things like that. There were all sorts of problems. I won't get into all that. At any rate, it didn't work out. And so I broke off from Norma, and I stayed out of IHD subsequently for many, many years. Jeanne knew all about it and had a convenient position in there just because she had an NIMH career research award and had to be located there. That was a peculiar arrangement.

Harrington: For the historical record I feel the need to follow-up on your--you said, later Clausen did get you?

Block: Oh, yes. He conveyed privately and unethically to the Budget Committee at Berkeley, which controls promotions, that I had not been the author of *Lives Through Time*. I was not told then of the accusation, and I was not promoted for some years. I found out informally what was involved, but I could not get explicit acknowledgement from the Administration that they had done so. And then in about '77 or '78 there was a law passed, or whatever, which meant that they had to give me an abstract of their directive.

Harrington: Right.

Block: I asked for that abstract immediately. I knew what was going on and got the abstract, but it did not explain something so I had to request it again. The second abstract finally had a sentence that was a smoking pistol, so we went to the Privilege of Tenure Committee. They thought I had a case, and we went through a whole bunch of things together. All sorts of things were involved; confidentiality was an issue. And the administration did not want certain things to happen; they were scared green that I would take this open because it was, of course, a violation.

Harrington: Right.

Block: I could have taken them to court readily; they were scared I would. And they gave me some back pay, but really it didn't make up for it.

Harrington: Sure. This could be for the record.

Block: Okay, it's kind of interesting actually. When Clausen came to NIMH in Bethesda, there was a rebellion against his autocratic ways there and that's one of the reasons he left.

Harrington: Really? Wow.

Block: He used to make secretaries cry.

Harrington: Wow.

Block: You know, he was sadomasochistic--what I've called pecking-order personality. Pecking-order personality is somebody that looks at you and sizes you up and where you are on the status hierarchy; if you're above him then he's deferent; if you're on the same level then he's nice; if you're lower than him, he's nasty. I would bubble up to John and say, "Gee, you got a moment?" And he would say, "How about a week from next Thursday?" Secretaries would not work for him, they would cry, leave, and quit. And that's just terrible.

Harrington: Another part of these questions have to do with your assessment of what achievements these institutional units played in the history of child development research. And again, you've partly addressed that, but do you have some kind of general comments about how you think IHD--I presume that IHD would be the central--played a role in the history of developmental psychology and child development?

Block: Well, IHD is a, you know, historical institute in child development. Its longitudinal studies by McFarland and Bayley and Jones--I like to identify them by the names of their prime movers rather than label them with confusing initials--

Harrington: Right.

Block: --those initials are very confusing. But those are historically important studies. I think in many respects they are deficient studies, you know? And I think that people think there's lots of data in these longitudinal studies. Lots of things in the files, but much of it is not--are not data. The studies probably get more respect than is warranted, seems to me. But they have been influential and some of the things coming out of them have been consequential. At the time I came into the Institute longitudinal studies, in particular those at IHD, were in great disrepute because--the words I believe were from Lee Cronbach--they had issued promissory notes that were never redeemed. That was the phrase. I think that's Cronbach's phrase, but I can't vouch for that. But that's the phrase that was used and it was said of the IHD studies. They got a lot of attention and there were references to IHD, but really what had they done? And really they hadn't done very much. And so a test of that, or one of the hurdles/obstacles confronting this research time, was this attitude. And I specifically tried to address this attitude. In the early 1960s, this was the perception. But historically, they've been influential--

Harrington: Right.

Block: --subsequently, I think, since then.

Harrington: Were there things, institutionally, that should have been happening at IHD that would have changed the course or influenced in some positive way the course of developmental psychology?

Block: What were defined at that time, and until very recently, were almost exclusively the several longitudinal studies existing within IHD. But did you know that experimental studies there--for example, if you want to do experimental, developmental research with children, nobody's doing anything at IHD. There were only these longitudinal studies. They had a nursery school associated with IHD, but it's not being used for research purposes. When we got into it experimentally with our assistants, it was very unusual.

Harrington: I did not realize that. It's amazing.

Block: Very unusual, yes. There was a nursery school, but it wasn't being used by the developmental psychology people, and it wasn't being used by the IHD people. IHD was ONLY, only, the longitudinal studies, and the studies were not active enough to keep it vital. People there were not the right people. Jean Macfarlane picked people that she preferred, and they were not productive people. And the Jones and Jones study really was not too active--Harold got involved, and it was a very nice--I really quite enjoyed the Joneses, both of them. He got involved in administrative work and also in national professional work--you know, the Social Science Research Council. He was a significant figure beyond the university in terms of various kinds of councils, foundations, and so forth. So he hadn't been too active locally for some years, though he had some very interesting research earlier. But the main activity was really the Macfarlane project, and that really was packrat. They had taken photographs of their people in part with Sheldon's--

Harrington: Oh yeah!

Block: --marvelous typology and his marvelous photographs. File after file, they'd had somebody making graphs of relevant data, you know, for each subject.

Harrington: They could have really done that too?

Block: Yes. And that's what I called a packrat place. But there was almost no substantive research coming out of that place. That's why I said they had issued promissory notes that were never redeemed. And--but that's what changed in the 60's, what Jeanne and I were trying to do when we started.

Harrington: Do you have any comments or thoughts about the kind of relationship between the Psychology Department and IHD --

Block: Well, they were really separate actually.

Harrington: I know.

Block: IHD wanted to have more connection to the Psychology Department than the Psychology Department was willing to have. Teaching a necessary course in the Psychology Department on mental testing meant you got a new status and credit for it, you even were paid. This was part of the department's orientation toward psychology--what the influential powers defined as psychology. It was really towards memory drums and nonsense syllables in the 50s, for example, verbal learning and experimental psychology; the softer psychology was looked down upon. Indeed, there was really a lot of conflict in the department because the people in clinical, personality, and social were really looked down upon. Developmental was not developmental before the Second World War; studying the child was in Home Economics and that's a--

Harrington: Was that true at Berkeley, or was it kind of generally accepted?

Block: --around the country. But the people who took child psychology were nice, bright young women who were going to college to prepare themselves for being a proper wife and mother as well as to find a husband. And nationally that was what child psychology was like: temper tantrums, bedwetting, and proper child rearing. It was really only after the war that it became a serious science; it became developmental psychology. And developmental psychologists started to come in, which was really in the mid-50s and subsequently, even at this--it was getting, you know, lumped together. Starting work on pigeons or rats, you know, or the retina--whatever it was. We weren't quite as biological as we are now. That's what got the cachet; what got the credit, the kudos, and the powers in the Department were in that camp. And it often made it difficult for people in softer psychology. And you find people,

staunch representatives of experimental psychology, that were critics of the other psychology, but not themselves doing any experiments. I won't mention any; it would be too cruel to do so.

Harrington: Okay.

Block: Very vigorous and nasty people whose scientific records were really literally empty, but that didn't mean that they weren't in positions of influence and directing nasty criticisms towards other people. The Department at Berkeley had a national reputation for being conflict-ridden, and it was well deserved. Throughout my career at Berkeley, soft psychology has not been valued as much as hard psychology.

Harrington: Right.

Block: I think that's a national thing, and the power is transferred to people with the same values or ideology. That tends to be the case in our department now; it's very biological, neuro-scientific, and cognitive sometimes in very narrow, uninteresting ways--sometimes in interesting ways.

Harrington: Sure.

Block: But it's certainly a place for being in the softer fields of personality, social, clinical, and developmental. Developmental here is trying to be scientific by focusing on being very experimental and focusing on the first couple of years, I think. They're very cognitive at Berkeley in developmental now, but social affective aspects of developmental psychology are not really considered anymore; children more than two- or three-years-old are not considered either.

Harrington: You mentioned earlier that you in many points, and perhaps that the theme throughout your work felt frequently kind of lonely and unappreciated. That really sums up your particular configuration?

Block: Well, yes. It may be true that I get under-appreciated more generally. Academic people have their emphases--what I'm doing is obviously more important than what you're doing; of course, if what you're doing is more important than what I'm doing, I'd be doing what you're doing. I don't mean to tell you that as a joke, but I know Berkeley was especially an isolating place. I understand Stanford is like that as well. We don't talk to each other or to other departments. So I certainly have felt isolated. I've had some good friends also, and I have some people elsewhere that I really, in terms of values and interest, connect with. I use electronic mail now, and I'm connected to all sorts of people I want to be connected to, much more so than in the department. In the department there are two or three, maybe four people I can talk with. Most people are simply irrelevant for me, or I'm actively uninterested in them so I'm not sure that I'm unique in this regard. I think I've heard other colleagues in this department say equivalent things also. I've heard people in other departments say that as well. So--

Harrington: Unfortunately I don't think you're unique in this regard.

Block: Yes.

Harrington: The next set of questions has to do with teaching and possible tensions between teaching and research. Specifically, they say describe your experience as a teacher of child development research. I imagine you can kind of broaden that to encompass and/or trainer of research workers. What courses have you taught? Please comment on the tension and research in the field of child development.

Block: Well, that's an awfully general question. I mean--

Harrington: Grab at this, grab at this.

Block: I've enjoyed some teaching; I've hated other aspects of teaching. I've never really liked large lecture courses because I felt that there was too much variance in the knowledge and interests of the students, so that at whatever level I teach the course, it's wrong. I figure the most I could ever reach is one third of the students, and should I enter the bottom third, the middle third, or the top third? Which two-thirds should I ignore? And I think that's, again, a general problem.

Harrington: Sure.

Block: I have enjoyed some small classes or seminars, and I think I'm much more effective in that kind of situation. I think students can connect with me or appreciate what I may have to convey in that context. In one course that I taught, I taught longitudinal research on subsequent issues. I taught a course on self-concept, on ego development, on personality, and I taught methodology courses. There's lots of tension in teaching; the more time you put into teaching, the less time you have for research. What is being emphasized in this university, Berkeley, is the emphasis on research, so some say teaching has gotten a short shrift, a relatively short shrift. I think I've done well in certain kinds of courses; others I probably have not done so well.

Harrington: You're basically not teaching now, right?

Block: No.

Harrington: Just being in seminars or so, but you're not responsible?

Block: I'm not doing formal things that I don't miss. I might like to take on a formal seminar. You know, I'm meeting weekly, in fact, I'm doing some of that right now, but I'm not being paid for it. But do I want to do lectures? No, I don't want to do lectures.

Harrington: Okay. Please describe your experiences in so-called applied child development research. Please comment on your role in putting theory into practice.

Block: Well, I have stayed away from that pretty much. I have been very troubled by the way media and even the university want me to do or say certain things, or get caught up by a writer for a child health magazine, to talk to them and they want quotes and so forth when they haven't read what I've written. And writing is much more careful in what you say, as compared to when you're asked spontaneously over the telephone. I haven't really sought to have public attention; indeed, I've tried to avoid having publicity. I have let my work be in the scientific realm, which has had some ramifications. If it filters out, fine; if it doesn't, so be it. But I don't want, nor have I sought to be a public figure. You know, other people have. Some have done it very well. I think I really am impressed by the way Ed Zigler at Yale, for example, has been in the news. A lot of people I do not respect also have sought public influence; I have been really very upset by their public influence.

Harrington: Okay.

Block: When did I join SRCD? I don't exactly recall. I recall attending a meeting at SRCD in the late 1950's, but I couldn't really say. I hope the matter is of record someplace; they probably store that information away.

Harrington: You said that first meeting you went to was very small?

Block: Very small, indeed. I think there were folding chairs set up in the hall.

Harrington: Your history of participation in the scientific activities of the Society?

Block: Some research work, which is published in the developmental journals; that's the extent of my participation.

Harrington: Okay. Anything in SRCD governance?

Block: No, I have not been in the governance.

Harrington: Okay. What do you believe are the most important changes to occur in SRCD and its activities during your association with it?

Block: They got enormously larger.

Harrington: Is that good or bad?

Block: It was good up to a certain point, now it may be bad in the same sense. You go to the meeting and there will be many scheduled things. It's always the case that there are so many things going on at the same time, and you can't be several places at once.

Harrington: Any solution?

Block: No.

Harrington: Okay, the field...please comment on the history of the field during the years that you participated: major continuities and discontinuities, events related to these? Have your views concerning the importance of various issues changed over the years? How so?

Block: Well, I don't know. I think too many things are published. They talk about there being an information explosion; I think there's been a literature explosion. I joke that each journal is proud of having a high rejection rate. And each likes to have, say, a 90 percent rejection rate. But if there are ten journals in the field, everything can be published. Something akin to that happens. I really think that much of what is published is not good conceptually or methodologically. I think that often developmental research is lacking in sufficient methodology, particularly in respect to what might be called psychometric issues. I'm not talking about fancy statistics, but rather a simple level of psychometrics establishing in some serious sense both reliability and a better conceptualization; I think that's lacking in much research. Many people I run into are not good enough and haven't been trained well. I don't know; it's a little supercilious to say that. But that's my judgment for whatever it's worth.

Harrington: Follow that up with comments. Do you have any general comments or perspective on the training of developmental psychologists these days?

Block: Well, there's such compartmentalization. I know in my own department the people often have been primarily concerned with making good measures. There are people in assessment; that's what assessment means, you know.

Harrington: Right.

Block: And people in developmental psychology, I think, are very often naive. They're interested in large matters, but naive as how to get at them and how to assess them. And I think that's not just here.

Harrington: Have you had any influence locally in shaping the dev--?

Block: Well, I used to teach a methodology course, which was really oriented to be generally oriented with emphasis on developmental context, and I got tired of doing that.

Harrington: I see.

Block: It was a hard course to teach, and developmental people were pleased to have me do it. And, of course, it meant they didn't have to do it or confront the issues.

Harrington: What are your hopes and fears for the future?

Block: My hope is that it gets better; and my fear is that it won't.

Harrington: Would you care to elaborate, Professor Block?

Block: Obviously things happen, but things happen much too slowly. I think there are fads and fashions in the field and maybe that's inevitable, but it certainly wastes a lot of time. I think there's a lot of compartmentalization in the field--things going on that should be connected that aren't connected. I think that was true when we started our longitudinal study in 1969 or so. One of our intentions was to try to interconnect the scattered assessment measures we were studying developmentally, to connect things that are usually kept separate--

Harrington: Right.

Block: --either kept separate or happened to be kept separate. And I think that the opportunity to connect things was very attractive and powerful. But other studies, in the field as a whole, have not really gone that way. I think things that are kept separate are preventing us from seeing certain coherencies in the field and cumulateness as it might otherwise be. I think also the "jingle-jangle fallacy" is large in the field and if that was to be attended to seriously, again the field would seem to be more coherent, more accumulative than it is. I think you see this when people study minimal brain damage, which is a way of saying that you can't see anything wrong with the kid neurologically, but the kid's hyperactive so you call it minimal brain damage. Hyperactivity, I think, is usually, probably an under-control, for example. And attention-deficit may be really under-perceptualization in a lot of people--it has been called being stimulus bound or controlled by the environment. I think of relating these to my own notions, obviously.

Harrington: Sure.

Block: Consider people studying conduct disorder. Well, conduct disorder is usually under-control coupled with low intelligence or perhaps low resiliency. And so there are all sorts of things that can be integrated that are not being integrated. I think risk research is often ill-advised or not properly conceptualized. I think people--

Harrington: Tell me about it.

Block: Well, it's epidemiological research.

Harrington: Risk research?

Block: Epidemiological research usually works with confounded indicators and also considers base rates or something like that. If you look at the risk factors involving drug users, risk factors involved in the conduct disorders, and the risk factors involved in alcoholism, the same risk factors are involved in all three, yet they're talked about separately. Nobody stops to look at, -Gee, isn't it interesting; these factors here are the same as that; what encompasses it all? In my view, something like under-control is central, but not alone.

Harrington: Sure.

Block: Indeed, I've written that in this paper on the antecedents of drug uses; I try to address that issue.

Harrington: Right.

Block: I talk about what is common to all these things, you know. People usually don't see the commonness. Maybe I'm seeing a coherence that isn't there, but also maybe I am seeing the coherence that is really there.

Harrington: Quite a number of them.

Block: Yes.

Harrington: Again, there's an order out there or under there.

Block: Yes, oh yes, exactly.

Harrington: There's an order out there.

Block: Yes.

Harrington: Final question: Please tell us something about your personal interest in your family, especially the ways in which these experiences may have had a bearing on your scientific contributions?

Block: Oh my, my personal interest and me personally. I guess as I've gotten older, I'm a New York intellectual. I read the New Yorker, the New York Review of Books, the NY Times, although ambivalently. I have read the New Yorker for about 55 years.

Harrington: What do you think of the new format?

Block: I'm still ambivalent about it; some of it I like, and some of it I don't like. I read The Nation. I read the Progressive magazine.

Harrington: You are a socialist aren't you?

Block: Yeah. I was an American lit major in college. I enjoy lots of music; I have lots of opera and a five compact disc changer. I often have a couple operas going while I'm working upstairs on my PC. I

used to be athletic, but my orthopedic difficulties have encroached on me. I'm not athletic now: I walk and swim and when the joints aren't bothering me, I'll even dance. I used to be a very good dancer.

My family—well, with Jeanne there obviously was an integration of work and home that was marvelously efficient and mutually reinforcing. Work and home reinforced each other, and we reinforced each other. We had four kids and so were developmental in practice as well as theoretically. I guess some things I've learned academically I could see and apply at home, and some of the things I learned at home I could see and apply conceptually. I still recall, with respect to Piaget and conservation, going to my daughter, Carol, asking if, “you want one piece of gum or two pieces of gum?” She was about two years old at the time and she replied, “Oh, two pieces, daddy!” So I took a piece of gum and broke it in two. She looked at me as if I was a con artist, although she was not yet at an age when conservation should exist. I also thought that with regard to conservation of liquids: you have a narrow glass and a broad glass, and change the portion in a narrow glass to a broad glass whereupon the level changes, and so forth. I thought if we did this with Coca Cola, or a desired soft drink, that things might be different or earlier than Piaget suggested.

Harrington: Were they?

Block: No, I never--

Harrington: You didn't do it?

Block: No, I didn't do that. I didn't do that one, but I did the one about breaking the piece of gum in two. So--

Harrington: Okay. You'll have a chance obviously to amend, elaborate, add, etc., but as we--

Block: Or CENSOR!

Harrington: Or censor, sure absolutely. But as we come to--again, I mean as you kind of think back over stuff, is there anything you would like to add, any thoughts that kind of occurred to you later about issues that got talked about before that you want to toss in now? As I understand you'll have a chance to do that later, but--

Block: Oh, I don't know. I found this--I'd never done this kind of thing before. I found it very--as I was going through, there's all sorts of memories that were going through my mind--

Harrington: Sure.

Block: --that it's very, at some level, moving through some self-evaluation; a summing up, so to speak.

Harrington: I've enjoyed it very much. Thank you very much.

Those who inspired and were influenced by Jack Block:

Mentors

Frank Barron

Gregory Bateson

Norman Cameron
Richard Crutchfield
Paul Farnsworth
Harrison Gough
David Grant
Ernest Hilgard
Howard Hunt
Don MacKinnon
Ann Margaret
Quinn McNemar
Jurgen Ruesch
Robert White

Colleagues

Professor Atkins
Fred Attneave
Dick Bell
Jerry Blum
John Clause
John Clausen
Paul Farnsworth
Norman Haan
Robert Harris
Wayne Holtzman
Harold Jones
Jerry Kagan
Tim Leary
Kurt Lewin
Jean Macfarlane
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Harold Rauch
Carl Rogers
Harold Stevenson
Don Taylor
Norman Thomas
Robert White
Louis Wirth