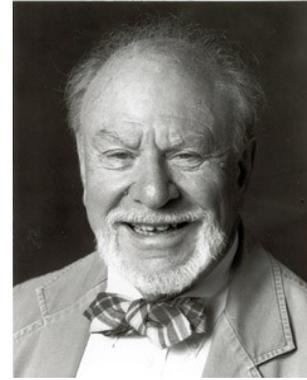


## **P. Herbert Leiderman**

- Born 1/30/1924 in Chicago, Illinois
- M.S. in Meteorology (1949) California Institute of Technology, M.A. in Psychology (1949) University of Chicago, M.D. (1953) Harvard Medical School

### **Major Employment**

- Physician, Children's Hospital at Stanford: 1984-present
- Associate Professor to Professor, Department of Psychiatry and Behavioral Sciences, Stanford University School of Medicine: 1963-present
- Physician, Stanford University Hospital: 1963-present
- Assistant in Psychiatry, Massachusetts General Hospital: 1958-1963



### **SRCD Affiliation**

- Program Committee (1977-81, Chair 1979-80), Long-Range Planning Committee (1971-72), *Monographs of the SRCD* Editorial Board (1971)

## **SRCD ORAL HISTORY INTERVIEW**

**Herbert Leiderman**

Interviewed by Robert Scott  
At Stanford University  
May 3, 2000

**Scott:** It's three o'clock on May the 3<sup>rd</sup>, 2000 and Bob Scott, Associate Director of the Center for Advanced Study in Behavioral Sciences, is interviewing Herb Leiderman for the Society for Research in Child Development. So Herb, tell me when you were born and where you were born and where you grew up.

**Leiderman:** All right. I was born in Chicago, Illinois on the 30<sup>th</sup> of January 1924. I lived more or less continually in Chicago until about age 17-18. I lived in the city except for travel with my parents until going to college.

**Scott:** Tell me about your parents.

**Leiderman:** Okay. My parents were both born in Europe; they came to the U.S. about age one to two. My father was born in England, mother probably born in Russia. They came here as infants.

**Scott:** Where in England?

**Leiderman:** In London. My parents, a little bit about them. Both of them were very assimilated in the sense that they were Americanized, though strongly identified themselves as Jews. My mother did speak Yiddish to her parents. She spoke the language fluently. Father was trained as a lawyer, but in order to work his way through law school he became a Recreation Director to earn money. This was an afternoon and evening job, in fact he dropped law a few years because he wasn't successful. He became a full time Recreation Director for the city of Chicago. My mother grew up in Muskegon, Michigan; he grew up in Chicago. She graduated from high school there. Her family then moved to Chicago where she obtained the degree of Associate in Arts at the Chicago Library School. She was a professional librarian.

**Scott:** I see. Okay. And any brothers or sisters?

Leiderman: Yes. I have sister, two years younger. Are grandparents important here or not? They may be.

**Scott: Yes.**

Leiderman: Should I do it now?

**Scott: You can say if you want.**

Leiderman: Yes, grandparents. My paternal grandparents came from Odessa, a cosmopolitan city located on the Black Sea in the Ukraine. They were urban folk. They immigrated to the United States as young adults, about the late 1880's, settling in Chicago. Grandfather was a cigar maker, grandmother was a merchant woman, buying and selling property. They died relatively early on in my life, when I was 11 or 12. My maternal grandparents had a more important influence on me because they lived until I was in my early 30s. They were rural folk, born in the Chernigov region of the Ukraine. My grandfather was in the Russian Army, which was unusual then for someone with a Jewish background. He trained as a railroad machinist in Warsaw, served five years in the Russian Army. After discharge and marriage, failing to find work in Kiev, he decided to come to the United States on the basis of a pamphlet put out by the Pere Marquette Railroad offering free agricultural land available in Michigan for new immigrants. The land was cutover forestland, unsuitable for farming though advertised as such. Their first years, by family lore, were very difficult. Soon after settling on the farm he brought his mother, four younger brothers, his wife, and my mother as a baby to join him. Four additional boys were born in the U.S. He was a railroad machinist all of his life, retiring in 1935 from the Rock Island Railroad in Chicago. As a teenager spending summers with him and my grandmother I learned about their life experiences. Grandfather was anti-Russian, later also anti-Communist, a strong labor union supporter, likely a major influence on the development of my political attitudes and values.

**Scott: I see. Good. Okay. Tell me about your early schooling.**

Leiderman: All right. I lived in the northwest side of Chicago in a neighborhood where I was one of four Jewish youngsters in the primary school.

**Scott: How large was the school?**

Leiderman: It was standard size for Chicago about a thousand students in attendance. We lived there because my grandmother owned a building that my father managed. We lived in this Polish-Catholic, Italian-Catholic, and Irish-Catholic neighborhood along with assortments of non-Catholic American and Central European youths. This childhood minority experience was traumatic for me because of the overt anti-Semitism of some of my fellow students.

**Scott: Say more about that, I mean --**

Leiderman: Yes. This period of the early 1930s, apart from the economic depression, was characterized by radio broadcasts of a Father Coughlin from Dearborn, Michigan, every Sunday afternoon, praising Hitler and excoriating Jews as evil people to be despised by all good Christians.

**Scott: Yes, sure.**

Leiderman: The children in the neighborhood obviously were aware of these broadcasts. The neighborhood playground reeked with anti-Semitic comments; Nazi swastika chalk marks appeared on the apartment building where we lived. Some teachers in the school would point out I was Jewish when I would be out of school for a religious holiday. These stigmatic comments felt as if I had chalk mark on my forehead to warn others to stay away. I had few close friends, one Czech youngster and one Italian youngster. The two of them were my only playmates until another Jewish lad moved

into the neighborhood while I was in the fourth grade. He became my best friend and remains so to this day, 60 years later. From my vantage point it was a traumatic experience, though it should be stated that I was never physically assaulted, just ignored or tormented by name-calling.

**Scott: Yes.**

Leiderman: However, in thinking about this experience further, I must counter this view somewhat since I was elected chairman of my eighth grade class. This stunned me since I clearly was not one of the popular students. Further the other Jewish student was elected to a school-wide office despite being known as Jewish. The cross currents were obviously more complex than mere religious/ethnic differences or conflict. One cross current I could identify was my mother. She was militant about discrimination and let it be known if she detected any trace of it. An amusing example worth mentioning here was that she wouldn't allow me to wear long pants. I wore knickerbockers, as did my Jewish friend. All of the other male students in the school wore long pants. Finally, when the time came to graduate from primary school, I prevailed upon my mother I should wear long pants for graduation. But being my mother, she said, "You can do so, but if you are to wear long pants for graduation you must wear a dark coat with white flannel pants." She convinced my friend's mother that this was to be the case. So the two Jewish youngsters went to graduation wearing white flannel pants and dark jackets, and all the other boys wore dress up suits, of course with long pants. My mother had to emphasize our being different as if we needed that.

**Scott: Wow. Okay. Did you have any early work experience that might be of interest?**

Leiderman: I did go into high school, we'll do that later, is that it?

**Scott: Yes.**

Leiderman: Work experience, well this gets to the major theme of why I'm in the field that I'm in. Our economic conditions during the depression were fairly harsh. My father wasn't paid for a period of time as a civil servant in Chicago. He received tax warrants, which could only be used at one or two stores in the city that would accept them. They were essentially loans from the merchants to the city government. My mother decided that she had to do something to increase income for the family. She started a children's summer camp; that was in 1935. So from 1935 until 1956, except for the War years, I worked at the camp. My parents did not permit me to work in any other place. I wanted to get a paper route, I wanted to get a job in a shoe store, but they said, "You're working here," though put more politely. I was non-voluntary unpaid employee. They and I (sister stayed with friends) went to the camp almost every weekend during good weather in the Fall and Spring to build and repair the place from 1936-1942, until I went into active military service in early 1943. I was not formally paid since it was a family enterprise. It included my maternal grandparents in Spring and Summer until the early 1950's when they died in their late 80s.

**Scott: I see. And that was summer -**

Leiderman: Well, it would be all year round at times because we were building the place, which I'll get to a little bit later.

**Scott: I see.**

Leiderman: That was the major work that I did.

**Scott: Okay. So now tell me about high school. Where did you go to high school and what was that like?**

Leiderman: Yes. I went to Foreman High School, which was in that same neighborhood as my primary school. The high school consisted of the same population as before - (Interruption)

**Scott:** So what we were asking is about work experience and then high school?

Leiderman: The high school was the topic that I recall. I went to the public high school, with the same type of student in attendance as primary school except for one difference. In this high school the teachers likely were bored with the lower middle-class students who weren't interested in academic subjects, so they offered Latin and other so-called advanced subjects for small groups of students. Since they had a Latin class, my mother insisted that I had to learn Latin because I was going on to higher education. That was understood!!! The Latin class consisted of about eight students, with a very fine teacher. I even remember her name: Mrs. Meyers. I think two of the eight youngsters in the class were Jewish, the other students came from assorted backgrounds, mostly female for that matter. She made a big difference, because she turned me on to more intellectual matters. She really took care of us, a positive buffer against the other unfortunate experiences of high school. It turned out to be a remarkably stimulating experience absent before this time, emphasizing the more academic side of my adolescence.

On the social side, I had two or three male friends continuing from primary school, but as soon as adolescence approached and girlfriends became a consideration, my erstwhile friends dropped me. There would be parties, birthday parties and the like, that I knew about. I would be excluded, with one exception which I can still recall, since the boy's mother made me especially welcome despite my "background." Essentially I was a social isolate at school except for my Jewish friend.

**Scott:** Wow.

Leiderman: We remained in that neighborhood for two years, when my parents learned that my friend's family was leaving for another neighborhood. He was going to a Jewish neighborhood located on the North side of the City. His parents told my parents that I would be there all alone. My parents finally said, "We've got to move," so we moved.

I have not mentioned anything about my religious education during this period. My parents were members of a Reform Jewish Congregation several miles distant from where we lived. My mother was President of the Sisterhood and my father was Executive Director of Programs at the Temple. I attended Sunday school every Sunday from age 7 to 14. I frequently attended Friday night services with my mother and sister. I was a member of the Jewish Youth Group and the Cub Scouts, meeting every two weeks on a weekday evening until age 12. I completed rudimentary studies in Hebrew enough for my Bar Mitzvah at age 13. I continued along with my sister in confirmation studies until age 15. The result of all this was a rather poor education in religious studies, but a positive experience in identifying with a group of youngsters who accepted me.

A second important experience for me during this late primary school and early high school period was trips to the museums of Chicago, first with my mother and later alone or with friends. In addition, my mother was an enthusiast about children's theater, so mother, my sister, and I attended the famed Goodman Theater in downtown Chicago on Saturdays over several childhood years. It was these experiences outside of formal schooling that profoundly shaped my knowledge of the humanities and sciences, quite apart from the school curricula.

**Scott:** I see.

Leiderman: The school curricula and fellow students became important for my third and fourth years of high school.

**Scott:** And so where did you move to?

Leiderman: In Chicago, but to another neighborhood on the North Side, more clearly middle class, entrepreneurial and government employee, along with some professional families.

**Scott: I see.**

Leiderman: It definitely was a more friendly school despite being on the border of two neighborhoods: one Jewish and one Swedish. The student population of the school was approximately 50 percent Jewish and 50 percent Swedish. That was quite a scene, tall blond-haired Swedes and shorter darker-haired Jews walking peacefully in the corridors of the same school. There was no conflict I could discern, and in fact some mixing of the groups even cross dating for a few individuals. School life there was very different, of course, because I was accepted. I also had relatively high prestige because my father was Director of the Public Park in that district, having been transferred from the Ghetto area. I had "connections" at the Public Park, important for my peers because through me they could use certain athletic and recreational facilities at convenient times. The principal of the high school and the coaches knew my father, which probably was an advantage for me. I joined the ROTC (This was 1939 and the War in Europe had started. I wanted to be prepared despite the fact that my parents opposed military training). ROTC gave me contact with a number of the non-Jewish boys in the ROTC who were there in somewhat greater numbers than Jews, though the student noncom officers included both Jewish and Swedish background students. I became more comfortable in moving across social boundaries.

I also had become a competitive athlete from my camp experiences. I was on the swimming team at the high school for two years, won letters for my performance for both years. I also was a good student, became a member of the honor society and even had a girlfriend, of course from the honor society, for a period during my senior year. So going from my early non-social environment to an environment where I was accepted enabled me to blossom, which leads into the tale of my subsequent career.

**Scott: It sure does.**

Leiderman: -- and the rest of my life.

**Scott: Well, now let me just ask one other -- did you go directly into college then from high school?**

Leiderman: Yes, I did.

**Scott: Okay. And where did you go?**

Leiderman: Firstly I will tell you where I wasn't allowed to go. I wasn't allowed to go to the University of Chicago, where I was accepted, for family financial reasons. I also applied to Northwestern where several of my honor society friends were applying. Initially I wasn't accepted at Northwestern University, because the admission office said my Latin grades were deficient. Since I had been an invited competitor in a city wide Latin contest two years earlier based on my academic record in the first two years of high school, and had taken a total of four years of Latin I felt a mistake had been made. Northwestern finally admitted that a mistake had been made when my mother complained to the admissions office (Northwestern was known to have Jewish quotas at that time). I now believe my mother was testing the system in encouraging me to apply. I didn't want to go there anyway. I also didn't want to go to the University of Illinois, where I was admitted. I ended up at the University of Michigan, the state where many of my mother's relatives lived and where my cousin was entering into the same class as mine in 1941.

**Scott: Okay. I see.**

Leiderman: So that's where I went. It's a long story.

**Scott:** And when you say you weren't allowed to go to Chicago, it's because they wouldn't admit Jews?

Leiderman: No. Chicago was different. By reputation Jews could be accepted to Chicago in the 1940's. My parents didn't have or would not expend the tuition money for a private university.

**Scott:** I see.

Leiderman: Chicago was a place that was open to all. You could always go to Chicago; it was a default choice for Jewish academic aspirants from the city of Chicago.

**Scott:** Now did you go to Michigan for four consecutive years?

Leiderman: No. I went to Michigan only for one and a half years. Michigan becomes very important for another reason, because there I was introduced into a more radical political world. My professor in a sophomore rhetoric class (selective for only 20 students) was likely a Trotskyite, and as well a devotee of the economic philosopher James Burnham of Managerial Revolution fame. Through this class, attending open lectures at the Rackham Graduate School, visiting the Liberal Unitarian Church for Sunday sermons on social justice, I met a lot of Trotskyite and Communist students who lived in co-ops rather than in the dorms as I did. They were engaged, exciting individuals who I instinctively liked. My political awareness was blossoming. However, I should mention that my awareness possibly began during my attendance at my second high school when one of the students from a previous class was killed in the Spanish Civil War. He was in the Abraham Lincoln brigade. I was aware of this young man who I knew about through his younger sibling. He had graduated earlier, had gone to Spain to fight on side of the Republic. His death became an important aspect of my political thinking because of his sacrifice for a radical idea and a noble cause.

**Scott:** Okay. I see. Alright. So you were at Michigan for a year and a half, and then what?

Leiderman: Then I joined the military.

**Scott:** And what year are we at now?

Leiderman: Fall 1942.

**Scott:** The Army?

Leiderman: The Army Air Corps as it was known then.

**Scott:** The Air Force.

Leiderman: Yes. I was a physics and math major at Michigan, though I took general courses in the social sciences, English, and one and one half years of French. Most importantly was the entry of the U.S. into the War on December 7, 1941. The War became uppermost in my thoughts. I decided not to enter the ROTC program. Rather I elected to prepare myself for service in my sophomore year by talking myself into an outward-bound-like program run by the Forestry School for their students on Saturdays (I was the only non-forestry student in their special program. They accepted me on the basis of my extensive children's camp experience). I met rural students, most from the Upper Peninsula of Michigan where they still had trees to cut, who were majoring in forestry. The group met from 8:00 AM until 8:00 PM on Saturdays for one semester "out in the bush" learning survival techniques. This experience with rural types from the Upper Peninsula contrasted sharply with the urban types I had met in the dorms. It certainly increased my appreciation of the survival skills of a social group very different from my own. Further I was completely accepted by them because of my enthusiasm for the program, and my own outdoor skills carried over from my camp experience.

In considering my military options in 1942 I decided I was going to serve, that I did not want to be a foot soldier, that I would volunteer. Furthermore the Air Corps would be my best option for becoming an officer, which I wanted to do. Also I had a girlfriend in high school whose brother was a Captain in the Army Air Corps, a meteorologist in fact. When I reached 18 he pointed out to me, "You're smart enough and as well a physics and math major, why not go into meteorology? It's safer and you'll be an officer." After enlisting in the Aviation Cadet Training Program of the Air Corps Reserve in November 1942 for pilot training, I was given an exam for specialty placement. I apparently passed it at a high level because after induction on 22 February 1943, and completion of basic training at Wichita Falls, Texas, I was transferred for pre-meteorology training at the University of New Mexico where I remained for seven months. I then continued on in meteorology training at the graduate school at the California Institute of Technology in Pasadena, graduating as a 2<sup>nd</sup> Lieutenant, Army Air Corps on 6 June 1944, D-Day. I elected to complete a thesis for which I later received an MS degree in 1949, after earning sufficient undergraduate credits after my discharge from service in 1946 to qualify for an advanced degree. I also was one of ten graduates out of a class of approximately 100 cadets who actually remained in forecasting, the rest assigned to other duties because of a surplus of meteorologists in the Air Corps

**Scott: And how many years were you in the military?**

Leiderman: Three years and nine months, until July 1946.

**Scott: And where were you stationed?**

Leiderman: After training in New Mexico and California, I was first stationed in the Northeast U.S. (New Hampshire and Maine) for short time, then transferred to Goose Bay Labrador for 18 months (two winters), and finally served for a short period in Newfoundland before discharge in late July 1946. At each weather station there were five to six forecasters and about 20 enlisted men who prepared weather forecasts for the entire North Atlantic area, which included Northeastern Canada, the islands of the Central and North Atlantic, Greenland, Iceland, Scotland, and England. On each day of reasonably good weather approximately 60 bombers and their crews would pass through the air base at Goose Bay Labrador on their way to their British bases in preparation for air raids over Europe. What was remarkable to me when I think about it now was that I and my fellow officers in our early twenties were given such responsibility for the lives and equipment so essential to the war effort. At the time I did not give it a second thought; only when I became a physician did I consider age and maturity as important elements in decision making. Planes could take off only if the weather officer on duty approved, otherwise they had to remain another day (or days) if there were storms at take-off time or forecast for the proposed landing sites in Britain. Our command headquarters was in the States, so if someone objected to our forecasts, they would have to contact headquarters there, something even Generals were loath to do. Morale was high for the weather group because our work was extremely important for the mission. Accordingly our status as officers and enlisted men on the base was high (we did not have administrative duties or "Fall Out" for the morning inspection at 6:00 AM as part of our military duties since we operated on a 24 hour schedule).

**Scott: I see.**

Leiderman: That's what I did. Are we going to go back to childhood, because there are a couple of important -- ?

**Scott: Yes, let's go back and collect those ideas.**

Leiderman: Yes. The other aspect of my childhood I believe important to mention is that my parents wanted me to be aware of my social surround. My father was Director of a public park in Chicago that had been located in a Jewish neighborhood (the famous Maxwell Street District). This neighborhood near the renowned Hull House of Jane Addams had changed from Jewish to Black and to Mexican. I would visit my father many Saturdays since I did not see him during the week since his hours during the

week were from 1:00 PM to 10:00 PM. Frequently I would be the only white kid in the place. I started visiting my father at work from about age seven years old until about age 11 when my mother began the family children's camp. I would see only children and adults of color, no white folks, except when I'd go to Maxwell Street to have lunch, then I'd see white people. I became very aware of racial differences and racial conflict in the ghetto neighborhood, where the experience was more physical when compared to my home neighborhood where it was verbal (athletic competitions and social events had to be segregated because of the potential for fighting. I could be a spectator of both group activities because I was the son of the Director).

A second important experience related to diversity and conflict was my father's two-week's vacation every year. I don't know whether planned or not. Before 1936 my parents would visit a different part of the U.S. every summer. The entire family would go to the Ozarks one year, the next year to the Indian reservations in South Dakota, next year to the coal mines of Harlan County, Kentucky, another year to Canada, New England, and Washington, another year to Pike's Peak in Colorado. However at my urging during Christmas Holiday in 1940 my parents agreed to drive all the way down to Mexico City and Acapulco for three weeks just after the Pan American highway was opened. My parents introduced me to the "outerworld," particularly families and children who differed in lifestyle and social class from our family. Travel and surveying the diversity of people has been a theme in my clinical work and research ever since.

The other very important experience during my high school years was my work at the summer camp. The camp was located in a rural community consisting of economically-modest Midwestern rural white folks, but nearby there were also Amish and Mennonite communities, and in the summer there were Appalachian Mountain folk from Kentucky looking for rural day work. As an adolescent before and after camp I would work with these folks who would be hired to do various skilled and unskilled jobs at the camp. My role was that of an informal overseer, meaning when my parents weren't there, I was to convey the information of what my parents wanted done. Essentially this was errands for the Amish (they came in horse and buggy) providing them with tools and such if needed. For the Appalachian people since the work was less skilled I literally worked with them on fence building, pier construction, painting, and clearing overgrowth in the forest. I spent several summers learning about people who were very different who taught me skills I never knew, and attitudes and values about working people I detect traces of at my present age. Most of all I learned to respect people who worked with their hands for a living and observed how they respected their work and were not ashamed of their particular niche in the vast scheme of rural life. (My mother maintained correspondence with her Amish friends for almost twenty years after the closing of the Camp. Since she knew Yiddish, a low German dialect, she could communicate directly with the monolingual older Amish women who also spoke a low German dialect, different from though in some ways similar to Yiddish.) So those are two important points.

**Scott: Okay. Now, so now we're up to 1946, you get out of the Air Force and now what?**

Leiderman: Okay. The other point I want to make was the covert and overt anti-Semitism I experienced earlier in my life suddenly shifted in 1946. I had not realized until this morning that all the way through childhood there were a whole series of incidents designed to convince me I should know my place. It stopped in 1946. There was something magical about that year. It was a feeling of liberation. It wasn't just because of my entering the eminent University of Chicago because these positive experiences continued even after leaving the University of Chicago. It was post-World War II, being liberated after a long period of being "under cover" in a bigoted world.

**Scott: Yes. Yes.**

Leiderman: So I just wanted to say that.

**Scott: To what do you attribute that?**

Leiderman: One likely factor, I had changed because I was older, another because I had been an officer, though I often had served with openly anti-Semitic fellow officers and some enlisted men. These verbal incidents were not just directed at me, but general comments. I never engaged into physical fighting though I witnessed a Jewish officer badly beat a fellow officer who insulted him with an ethnic slur. There was talk of a court martial for the Jewish officer (a Captain) until several witnesses, including the Catholic Chaplain testified about the circumstances. The insulting offending officer was transferred off the base within a week or two. The Jewish officer was not charged.

I attributed part of the change in attitude to the discovery of the Holocaust. Suddenly people became aware that something seriously had gone wrong. This bigoted evil beginning merely as slurs and name calling, carried a potential for real harm when acted upon as seen in the Holocaust. As another explanation for change, I also might include the idea of a State of Israel being discussed at that time. Of immediate import for me, however, after discharge from service I entered the University of Chicago, where there were many Jewish students. The social atmosphere on campus was entirely different, most tolerant of students from many different backgrounds. And the University of Chicago under Robert Maynard Hutchins, who took over as President in 1930, created a true university with an outstanding faculty who attracted first-rate students regardless of background, including students from the East Coast and New York City.

**Scott: So now let me just pick up on that. So when you got out of the military you came back and now you went to the University of Chicago?**

Leiderman: Yes, Chicago. I entered directly into the Social Science Division, (the quasi-graduate school program) rather than to the four-year undergraduate college where one could conceivably be admitted after only two years of high school. This admission to the Division was based upon passing the college exams in the biological, social, physical sciences and humanities offered to returning veterans so they could accelerate their university program by entering courses in the Divisions which reflected their previous education and/or military experiences.

**Scott: Okay. And you're now entering your university years.**

Leiderman: Well, I hadn't graduated from any school I already attended: Michigan, New Mexico and Caltech. I did a Master's degree thesis while at Caltech and was told when I received a BA degree, they would actually award me the MS degree from Caltech (they honored their commitment in 1949). Still I had to decide what I wanted to do apart from meteorology. I didn't know much about academics and had only work experience through working at my parents' children's camp. That I knew!!! However I also knew I did not want to continue in the physical sciences. I thought I wanted to do history. I did that for two quarters and decided I wasn't a historian, although I received good grades in Ancient and European History. I took one course in psychology and decided that this would be my area. I decided to major in psychology, along with acquiring a general education at University of Chicago courtesy of the G.I. Bill. I cannot sing the praises loudly enough for this socially transforming act of the government after World War II. My life would not be the same without it.

**Scott: I see.**

Leiderman: I completed a Master's degree in psychology, not developmental or social psychology, just old-fashioned physiological psychology (cats and rats). I liked the idea of working in an experimental mode. That was possible in physiological psychology at the University of Chicago then. The University of Chicago also was very important for my personal intellectual history, because I found it to be a true university. There one could be interested in and discuss intellectual ideas and not thought to be deviant. In the period from 1946 to 1949 Robert Hutchins was President, Mortimer Adler of the Great Books fame was a presence, Joseph Schwab a great science educator, physical scientists Fermi and Urey Nobel Laureates of the atomic bomb fame were in residence and teaching beginning courses. There also was an excellent Medical School though I wasn't interested in medicine then. The Psychology Department was only fair, but they had a superb human development and education group:

Robert Havinghurst, Allison Davis, Ralph Tyler, and many others. Sociology, the biological sciences and physics were probably the best in the U.S. at that time.

**Scott: Right.**

Leiderman: Sociology - I took lots of sociology courses with Burgess, Goldhammer, Ogburn, Blumer and one or two others. And then courses in human development with Bettelheim and Carl Rogers, two important figures in the field. Ralph Gerard in physiology and Anatol Rappoport and Nicholas Rashevsky in mathematical biology. I had a fine, fine education. And I didn't just stick to my own little bailiwick of physiological psychology, but branched out in the social sciences, humanities, as well as biology by attending lectures and auditing other courses.

The end of this phase for me came when James Grier Miller came to Chicago to head the Psychology Department in 1948. He had an MD and PhD from Harvard, member of the Society of Fellows of Harvard, became Chairman of the Psychology Department, though fundamentally he was a systems theorist. He encouraged me to consider medical school; "you shouldn't just be a psychologist, because it's too limiting." I was very suspicious about this advice because I was quite happy at Chicago, but he encouraged me in this direction and I did take the appropriate premed courses, which I did not particularly like because of the memorization.

I completed the pre-med courses then later decided to apply to medical school. I applied to Chicago because I wanted to continue towards a PhD and an MD degrees. Miller said I should apply at Harvard. I said I didn't particularly want to go to Harvard; being a true Midwesterner and having served in New England (New Hampshire and Maine) in 1944-1946, the region did not particularly appeal to me.

I felt I could get a PhD at Chicago and get an MD if my plan would work out. I nonetheless applied to and was accepted at three other medical schools besides Chicago. Much to my surprise and shock that one of the acceptances was from Harvard. Since I had married in November 1947 and because my wife, Gloria, a 1946 graduate of the University of Illinois in sociology was in graduate school at Chicago, the decision to leave Chicago and go to Harvard was not easy to make. The issue became decided when Gloria, who had met Robert Sears of Iowa at a child development colloquium at Chicago learned that Sears was on his way to Harvard. Gloria called or wrote him about our situation, that we needed to have dual Harvard acceptances if we were to move to Cambridge. She applied and soon received an acceptance in child development to work with the Sears and Whitings at Harvard's Palfrey House in their graduate program. Our decision now became obvious, and we accepted both appointments.

**Scott: And so when did you enroll there?**

Leiderman: September 1949.

**Scott: And how long were you there?**

Leiderman: I completed Harvard Medical School in four years along with doing research in biochemistry on the CNS in rats for two and a half years with a fellow medical student also from the University of Chicago. My wife received her PhD at the same time. I remained in Boston for my internship and continued with residencies in neurology and psychiatry at Boston City Hospital and Mass General and Mass Health Centers. I continued on at Harvard until 1963, firstly as a post-doc fellow, later becoming a junior faculty member at Harvard Medical School in the Department of Psychiatry. Our Boston experience totaled 14 years where we had intended to remain only four years. At this point with four children and a non-income producing PhD wife, it was necessary that I think seriously about increasing my salary of \$13,000 per year. When I was offered a position with a salary of \$20,000 per year at Stanford Medical School, a new department under the chair of Dr. David Hamburg, I accepted with a proviso that as soon as practicable there would be help from David Hamburg to get an appointment for Gloria.

**Scott:** I see. Okay. Now you have talked about some of the teachers and research mentors and college experiences and so on. So looking back, you talked about your experience in the military, and I wonder if you have any comments to make about the role it might have played in your subsequent career and your life?

**Leiderman:** Okay. I went into the military in 1942 against my parents' wishes. I was an 18 year old and I joined the Aviation Cadet Reserves. I did this because I didn't particularly want to serve in the infantry and wanted to become an officer. My overall plan was to become a teacher in high school or a college. I liked and was good at mathematics and physics in high school, so I thought about this area as the locus of my teaching. I became a physics and math major at the University of Michigan. After I entered the reserves my girlfriend's older brother, a meteorologist captain in the Air Corps, said, "Why don't you join us?" He thought I would be good for such a position because of my background. After entry in the reserves I applied to the meteorology program.

I entered active duty in February 1943, was transferred to meteorology after completing basic training. This meteorology experience was very important because it gave me a sense of scope about the complexities of the interaction of the global physical, social, and political forces operative on our little planet. In my training and my forecasting I routinely drew weather maps extending from the western Pacific to the shores of Europe, and from northern South America to the Subarctic. I did this from September 1943 to July 1946, first as a student at Caltech then as a weather forecasting meteorologist. I had to deal with large masses of data covering a vast sweep of the world. I began to look at processes very broadly, how massive changes taking place in the atmosphere would also affect humans on the surface of the planet. This viewpoint of the world has never left me, and I believe has strongly influenced my subsequent career. Certainly my concern about context, family dynamics, neighborhood, and social class stem in part from my military experience.

**Scott:** Oh, I see.

**Leiderman:** The second set of experience in the military was being stationed in New Mexico for my training. There one of my teachers was a professor of anthropology who taught the Cadets about the human experience. The entire world came under his purview. Locally he would take three or four of us who were interested in weekend trips to visit various Indian villages (Hopi, Navaho, Acoma, Pueblo) in nearby New Mexico and Arizona. I became fascinated with the diversity of people, how they adapted to their social and physical worlds. He was a marvelous teacher in that he not only knew the cultures, but knew many families personally. They would take us into their homes and in the case of the Hopi down into the Kivas as one event for outsiders. After the war I returned to the Hopi area for a visit, made friends with a Hopi family, the Monongyes with whom my family and I have maintained contact for the past 50 years. Our eldest daughter, Deborah, who served as a physician in the Indian Health Service in Chinle at the Navaho reservation near the Hopi second mesa, became acquainted with the Monongyes extending our family ties to the next generation. She and her husband geologist were included in tribal ceremonies, which is unusual for most Hopi families.

**Scott:** Wow. Yes.

**Leiderman:** After completion of my training at the U. of New Mexico and Caltech, I graduated as a 2<sup>nd</sup> Lieutenant, serving briefly in New England before moving on to Labrador and Newfoundland for 18 months as a weather officer. As part of my work as an officer, I would fly in the summers when the ice would melt on the glacial lakes of Labrador to inspect the isolated weather stations where some of our enlisted weather observers were stationed for a year "in the bush." This task fell to me because I was the junior officer, landing on isolated lakes in a seaplane did not appeal to some of my senior officers. I used the opportunity to visit the local peoples (Native American Cree and the Eskimo Inuit) adapting to life in the forests (Cree) or along the desolate coasts of Labrador (Inuit).

A third transforming experience for me in the military was being an officer. This role and accompanying status was very different from my situation as a minority child residing in northwest

Chicago. My military experience essentially changed the way I looked at the world. Though I spent a little less than four years in the military, I was proud of what I did in contributing to the victory over the Nazis. My horizons were expanded by travel to Western Europe as part of my meteorology work (so called Orientation Flights), opening my eyes to European (French, German, English, and Italian) worlds I had hardly imagined before, despite my growing up in multi-ethnic and museum-rich Chicago.

**Scott: Sure. Okay.**

Leiderman: And lastly, another transformative event was the decision that my academic interests were not in the physical sciences, but more inclined towards the social and behavioral sciences.

**Scott: And we're all grateful for that. Well, now as you look back on those life experiences in the formative years and through your schooling and all that sort of thing, and think about your subsequent career in child development, do you see the development of that interest as being kind of a linear thing or did it follow some other course?**

Leiderman: Well, it's an interesting question. Relevant to your question and perhaps directly or indirectly related to my decision, likely of interest to social and developmental psychologists, was the following experience. In 1937 and 1938 the Zionist Youth Organization of the United States rented my parents' summer camp after the season was over for their youth meetings. Visiting professors came from various universities to spend three or four days at the camp meeting with Jewish students doing workshops on various topics, directly or indirectly related to Zionism, during a ten day camp period. There were lectures and seminars and evening campfires with much spirited dancing and good fellowship. As a 13 and 14 year old and the son of the owners, I was permitted to sit in on the seminars and participate in the social activities. I was fascinated by the students, male and female, but awestruck by the knowledge and erudition of the faculty. One of the seminar leaders was someone by the name of Kurt Lewin, whose name I recognized from my father's discussion with my mother about a professor from Iowa who studied democratic and non-democratic behavior in small groups of people. (As a Park Director my father had to attend periodic workshops sponsored by the Park District of Chicago. Lewin had given such a workshop to the Park Directors a year or two earlier.)

**Scott: Oh, my gosh.**

Leiderman: I remember him vividly, a sweet gentle man quietly intense, who easily captured the attention of the group. In quiet voice he easily became the focus of attention of these adolescents/young adults. He gave a seminar on group dynamics that simply transfixed me, because I didn't know experiments could be done with people. (He was at Iowa then before he moved to MIT.) I was most impressed by the academic aura. His image remained in the back of my mind, though I never thought I would do that type of psychology. I also became acquainted with the Zionist movement and the fervor that accompanied political movements. This experience prepared me for my later entry to the University of Chicago after discharge from the Air Corps in 1946 where the students showed the same enthusiasm about political and social movements as did the Zionist students at the camp. Entering the University of Chicago was in some ways a continuation of the camp experience

I started off in history because I wanted to do something very different from meteorology. I wasn't a good historian in terms of the technical sense of writing well and easily, but I discovered I was very good in psychology. I became acquainted with (here comes Kurt Lewin back again) the psychology group at Human Development in the School of Education, with psychologists and a myriad number of sociologists. I can recall a few of the famous names of professors who really were important to me in furthering my education, such as Burgess, Ogburn, Goldhammer, Blumer, Havinghurst, Allison Davis, Carl Rogers. These were my teachers in those years, plus some people in Medicine such as Ralph Gerard, a neurophysiologist. An important person for me was James Grier Miller, a MD-PhD and theoretical model builder. All of these individuals helped me crystallize my thoughts. I was in psychology, wanted to get a PhD in psychology, but my interests were broader than experimental

psychology. I was always interested in brain and behavior, therefore decided I would attempt to work at the interface of these two areas.

**Scott: I see.**

Leiderman: So that was how it came together for me. Then I got seduced into going to medical school. Or let me put it another way, I was told that if I really wanted to make it, I should get an MD as well as a PhD, that is how I ended up in medicine at Harvard Medical School.

**Scott: I see. Okay.**

Leiderman: Now I didn't mention one other important component. I was married in 1947 while at the University of Chicago. My wife was trained in sociology, became interested in child development partly because of my parents' ownership of a children's summer camp. This explanation has caused no end of controversy in our family later on. She felt I intruded on her field, or as I try to explain to her, she mentored me towards the field of child development enlarging on my camp experience. I mentioned her here because her career was important to me. She was in graduate school at Harvard with Robert Sears and Pat Sears, John and Bea Whiting, Eleanor Maccoby, Harry Levin. It was close knit academic group, the faculty and graduate students included me in all social activities while I was in Medical School. I was part of the Palfrey House child development scene for all four years of my medical education, making my medical school experience much less onerous and much more academic.

(Note: In reviewing this interview I discovered that I did not include any of my medical school experiences which are considerable and most formative, because of focusing on child development and psychology. Perhaps these views will appear in another form later on because I am writing a piece for my medical school class reunion.)

**Scott: I see.**

Leiderman: During my four years of medical school I always was included in all of their social activities. There was an exemplary relationship between the faculty and the graduate students (I should mention Bill Hartup here who was an important fellow graduate student of Gloria because of his sociability). The image for me was working together as a group towards common goals rather than only individual accomplishment. Research and academic interests began to come together for me in many ways. I still don't know how to tease them out, but I know these experiences influenced my subsequent career.

**Scott: Okay. Yes. Alright. Now I want to ask you a little bit more about that career, and if you go back all the way to the very beginning of your interest in child development from a career point of view, how would you characterize your early interest in that? I mean, what were the kinds of questions you were interested in? What were the kinds of topics you wanted to research?**

Leiderman: Well, that's a very good question. I thought about it after reading about the type of questions you might ask. I was interested in the social world. I was interested in how the environment interfaced with the individual. I began with brain-behavior paradigm because I began studies when I was in graduate school on perception, animal perception, and how the brain processed visual perception (I attempted to test the hypothesis of Karl Lashley on brain waves mirroring the perception of the visual field perception). While in medical school I also did research. My interest then was on the effect of adrenal hormones on the metabolism of electrolytes in the brain for which I shared the Borden Award for Research with another medical student at graduation from medical school. As a resident in neurology and psychiatry I witnessed the last of the polio epidemics in 1956, taking care of paralyzed patients who had to be placed in respirators because they could not breathe on their own. Some of these patients became psychotic (I was on the neurology service) and that led me to studies in sensory deprivation and social isolation, attempting to tease out the effects of the viral disease from the effects of confinement. I spent about five years doing research on the neuropsychology of the absence of sensory stimuli and how it affected these patients. The interface between the organism

and the environment was the central theme of my work. I also did studies of the sociophysiology of group interactions. I hooked individuals up to polygraphs, measuring their physiological and psychological reactions to their contrived status as a minority or majority member of the group (I used a variant of the Ashe experiments, looking at the effect, not only on the minority person, but also on the majorities, when they didn't know their social status. I continued this line of research even after I came to Stanford, until a transforming experience occurred which moved me towards the area of child development).

The issue: a neighbor of mine (many faculty members at Stanford lived on campus in the "faculty ghetto" so faculty members across fields could easily get to know one another), an academic pediatrician, was concerned about the sensory and social deprivation of mothers of premature newborn infants who were separated for weeks to months from their prematurely born newborns who were housed in special nurseries for their care. Mothers were not allowed to be with them. He felt that these mothers, deprived of an important biosocial experience in the newborn period were less capable mothers after reuniting with their infants. He asked me to come to the premie nursery and look at it and see what might be going on in the nursery, that is, how the social structure of the nursery might be affecting later mother/infant interaction. My observations in the nursery and interviews of mothers led us to develop a large grant proposal covering four years, hypothesizing that the "infant deprivation" early on in a mother's experience, had an effect on her later behavior. We did a complex experimental study manipulating contact of mothers with their infants in the newborn period. By modifying the nursery for contact and non-contact six month periods for all infants and mothers (unbeknownst to the participants), we were able to demonstrate that the deprived environment affected mother's social bonding, having an effect on her later behavior with her infant. The findings from these experimental studies changed clinical practice in premature infant nurseries throughout the U.S. after they were published. Mothers and fathers were thereafter allowed into premie nurseries with suitable surveillance for infectious disease. THUS began my career in child development.

**Scott: I see.**

Leiderman: The same conceptual scheme of the influence of context operated. I took it to a different setting.

**Scott: Now at that point were you in psychiatric training? Had you completed --?**

Leiderman: No, I was an academic psychiatrist at this point. My training in psychiatry was classical Boston, meaning I had to know enough about biological sciences underlying the CNS, psychology which I already knew, but also had to undertake psychoanalytic training. Then if one wanted to have an academic psychiatric career it was mandatory to have a personal analysis. I did this five times a week for five years all the while doing research at the Massachusetts Mental Health Center, attending evening psychoanalytic seminars once a week, seeing patients, and being the husband of one wife and the father of four children (when I think about it now I do not know how I survived). There was some help from an NIMH Career Investigatorship, which gave me a salary and paid for my analysis. But when the opportunity came for me to leave for Stanford, where David Hamburg, new Chair, was setting up a department with faculty who were trained both in a basic science and trained in clinical psychiatry, I immediately accepted his offer of a position as Associate Professor in the department.

(Addendum note: I transferred to the San Francisco Psychoanalytic Institute for completion of my analytic training and to do my three required cases. They informed that I had to attend additional seminars because they felt the Boston Institute was sufficiently different from San Francisco that such training was necessary. I completed the three cases, each four hours per week with weekly supervision for three years. Needless to say I was vastly overcommitted.)

The first year at Stanford was a rocky one for me because I discovered we could not live on a salary of \$20,000 per year. Finally I was given a raise to \$22,000; my wife obtained a half time research position giving us enough funds to muddle through, despite paying for part-time childcare. We hired a Guatemalan woman part time who remained with us for over 30 years, long after our children were out

of the home. (Note: We also had to have home help in Boston with German au pairs for each of two years when we had four children, the oldest being five years of age. This WAS THE PRICE PAID FOR TWO PROFESSIONAL PARENTS WITH YOUNG CHILDREN).

**Scott:** So the studies that you were doing of maternal deprivation were being done while you were a faculty in what department?

Leiderman: In psychiatry.

**Scott:** In psychiatry.

Leiderman: I've always been in psychiatry.

**Scott:** Okay.

Leiderman: While I was a member of the department of psychiatry at the medical school I was in charge of the program funded by NIMH enabling students in medical schools to get Master's or PhD degrees in the social and behavioral sciences. I became acquainted with anthropologists, sociologists, and psychologists at Stanford, because I was administering a program to fund medical students who would get degrees in their academic department. I had to beg, cajole, bribe, and/or seduce faculty to supervise medical students who were supported in their 5<sup>th</sup> or 6<sup>th</sup> year of medical school while also enrolled in a social and behavioral sciences department. I believe 5-6 students received MA or PhD degrees under this program, along with their MD degrees.

**Scott:** I see.

Leiderman: The program was very difficult to sell at Stanford, because many of the academic behavioral science departments thought themselves as the purest of the pure. Would medical students be up to the level of their graduate students? It turned out that several times the medical students were the number one students in the department on exams. There were some very bright young people in the medical school. Several students, two PhDs and four MAs, worked with me under that program. Other students worked with other faculty in psychiatry and the social/behavioral sciences.

**Scott:** And was the medical school here at the Stanford campus at that point?

Leiderman: Yes, they were here. Bringing the professional schools together at one site was part of the rationale for moving the clinical part of the medical school from San Francisco to Palo Alto to allow more interactions between medicine and departments on campus. This new program on one campus was how I got acquainted with the behavioral/social scientists. It was included as part of my official role. From that point on I renewed personal friendships and relationships with Bob and Pat Sears in psychology and education. I knew them through Gloria who was one of their graduate students at Harvard.

**Scott:** Yes.

Leiderman: In some ways it was very easy to be at Stanford. We knew Eleanor Maccoby from Harvard. She was an important figure at Stanford, perhaps less warm and friendly than some others yet also very helpful at times in my work. Being on campus with easy access to the social and behavioral sciences was a joy. I could combine my earlier interest in academia with medicine. I loved it!

**Scott:** Okay. So now do you have anything more you want to say about those early years and the kinds of questions that you were studying?

Leiderman: One more. Being an opportunist I took advantage of situations. Let's see, this would be 1967 or 1968, I would have been four years at Stanford, and I was working on the program in the

newborn nursery. John Bowlby and Mary Ainsworth came to visit the Department of Psychiatry. They were both very remarkable people, though somewhat different from one another. Mary had done studies in Uganda, East Africa, on the paradigm developed by John Bowlby on attachment of young infants to their mothers. They gave a seminar in the Department of Psychiatry where they presented their work and ideas, which continued over several months. I realized that this could be a paradigm for aspects of the work I was doing in the newborn nursery. On the basis of my work in the newborn nursery I also attended Eleanor Maccoby's seminar on child development. She asked me in 1968 what I was going to do during my upcoming sabbatical in 1969. I said I hadn't made plans. Whereupon she asked me if I would be interested in going to Kenya, Africa, because the Whitings were looking for a field director of their child development project in Kenya. I responded with, "Well, I never thought of that possibility." I didn't know what I was going to do for my sabbatical, which was a year hence. I talked it over with Gloria and she said, "Well, maybe we should do it." So I informed Eleanor and shortly thereafter the Whitings called me, I visited them in Martha's Vineyard where they offered me the position of Field Director of the project in East Africa. I accepted. I would be administratively in charge of five graduate students doing various studies of children in several different communities in Kenya. Along with administration, Gloria and I could do our own work in a Kikuyu Village. Thus began one of the greatest experiences of our lives for both our four children and us.

**Scott: What was transformative about it?**

Leiderman: The original study I was going to do in the Kikuyo there was examination of the effects of very early contact of infants with their mothers on infant growth and development and maternal social bonding. I had studied the deprivation of maternal contact with premature infants. I wanted to examine the bond between infant and mother where mothers were very close to their infants from shortly after birth, and were physically close with their infants in the infants' first years (at least so I was told). I thought it would be wonderful to study the attachment relationships and social bonding in a real world situation. The study did not quite take place as planned and became another study. When I got to my agricultural village in Central Province Kenya I discovered that mothers were very good in managing the household, but in this community they had little free time with their infants because they were working mothers. They were mainstays of their families working in the fields cultivating crops, and as well frequently hauling drinking water from the river two miles distant when older children were not available for the task. My original study of early attachment had to be transformed into a study of the young girls taking care of the infants while their mothers worked in the fields. These baby-minders, generally girls between the ages of five and nine years, not in school for social and economic reasons, were the caretakers. My study focused on the bonding relationships of five and nine year olds to their mothers' infants and the attachment of infants to their youthful caretakers and of course to the biological mother. I saw the complexity of mother-infant relationships in this traditional community, producing bonding and attachment similar to but not identical with the bonding and attachment in middle class nuclear families. Attachments to older sibs (or peers) became more significant which I labeled a "polymatric system." The paradigm developed for the nuclear family in the U.S. didn't fit this population. The demands of the situation in this village required not only a change in my study of attachment and bonding, but also brought me to a different theoretical formulation of attachment and social bonding among family members and other important non-family individuals.

A second transformative event for me in the Kenya experience was that I became much less interested in psychoanalysis and the psychoanalytic developmental theory. I decided that this model was insufficient for the development of social relationships for non-urban and perhaps non-European families. The Kikuyu model of attachment to peers and to the extended family provided the major cathetected figures for these infants and young children. The psychoanalytic model of psychosocial development probably served well for the upper-middle class nuclear families in urban Europe and America, but did not serve well for psychosocial development of children in the extended agricultural families of a Kikuyu village.

**Scott: Yes. Yes.**

Leiderman: I changed both clinically and conceptually in my thinking and behavior. I became much more involved in understanding the contextual and cultural components influencing the family unit. My clinical work was more directed towards families, couples and small groups

**Scott: Yes, I see.**

Leiderman: I was fortunate to have new experiences and to use the opportunity to think about developmental processes in ways that made sense to me.

**Scott: Okay. Very interesting. Now what I wanted to ask you was exactly about that and the sense in which, as your career evolved and your research interests developed, whether there was discontinuity or continuity, and here is an example of a kind of discontinuity where you kind of just, not so much discard, but set aside one paradigm in favor of another, and I wonder if there were other shifts that also occurred.**

Leiderman: Yes. Another important discovery was that I learned that I really loved to work collaboratively. I'm not sure whether this was defensive because I wasn't formally trained as a child developmentalist, although I did know a great deal, or whether it was because I liked to work with other people. I decided this was a model for me. My ideas were more imaginative, more creative when I could work with other people, whereas when I sat down in front of a blank piece of paper it remained blank for too long (that was before computers and word processors). I was much less creative when working alone.

The other insight out of Africa was that I became convinced that I wanted to do research that was applicable to real world issues. I saw fantastic amounts of poverty in Kenya and through my travels in Central and West Africa. I saw good people really struggling to make it. I decided my research would require some practical application, that is, the research or my ideas might lead to something that would better human kind. When I did the premie nursery study, which was a scientific experiment, I thought it might prove to be helpful for mothers and fathers if it worked out. But it wasn't what I had in mind for the study. I was interested in the biosocial implications of social deprivation in the newborn period. The pediatrician I worked with, Marshall Klaus, did have that idea that it would improve prenatal care, but I was more interested in the science. When I came back from Kenya I wanted to do research that was much more applied.

**Scott: I see. Okay. Well, you describe your work as being collaborative and applied. I wonder if you would - how else might you characterize its contributions? In other words I'm inviting you to brag a little bit here.**

Leiderman: Okay. Let's see. I think the model of comparative cross-cultural research that I introduced was my major contribution. It was the comparative method, trying to bring diverse groups into the study paradigm, that is, either national, gender, social class, where appropriate. The other major contribution I'm really pleased about was the study I did in the premie nursery that was one of the very few controlled, randomly assigned clinical experimental studies done in a field setting. It was a very good model of a new type of research, looking at an important clinical problem, examining whether there was a critical period in human maternal to infant social bonding. It was parallel to biological work done in sheep and goats. Our study took much longer than I wanted, but it was a major study. I liked that.

In reference to the studies I did in Kenya, I didn't mention one very important figure for me, anthropologist Robert LeVine, then of University of Chicago and later Harvard University. I was interested in doing for infants what LeVine had done in Nigeria and the Whittings in Kenya in a variety of social and cultural settings for older children. I, along with the anthropologist William Caudill who unfortunately died before the conference began, arranged for and conducted a conference sponsored by the Wenner-Gren Foundation on cross-cultural studies of infancy. This work eventuated in a book

edited by me and a colleague published in 1976. My infant studies with the Kikuyu in Kenya were followed up later by LeVine, Brazelton and others with the Gisu in Western Kenya. The paradigm in both of these infant studies was for our team of anthropologist, sociologist, pediatrician, and psychiatrist to examine biological, familial, communal, and cultural factors influencing infancy. These studies were one of the first comprehensive ecologically-oriented studies examining culture and infancy. It was recognized by researchers and frequently cited.

The other contextual-oriented work coworkers and I did was more narrow by focus, not as well recognized but important for its implications. It was a longitudinal study of abused and neglected children in foster care. This work was done in collaboration with a psychologist and a lawyer examining the effect of foster care placement on the wellbeing of children aged 6 to 10 years. The study was important because it defined some of the parameters such as family structure and conformity that are important for the child wellbeing in foster care.

Michael Rutter, Professor of Child Psychiatry at the Maudsley, London, was complimentary about my courage in doing these experimental studies in the field that should be done but rarely were. A little later I'll speak about the influence of my training in medicine on my approach before I moved into child development in contrast with researchers classically trained in child development and psychology.

**Scott:** Well, now you talked about your coming out of the experience from your work in Kenya wanting to make an impact of some practical sort, and that leads me to the next question, which is what's your assessment of the impact you've had? Let's break that into two parts: one is the impact that it's had in a world of everyday life, and the impact that it's had in the literature in this field of child development.

Leiderman: Let's see. I'm now going to also speak about something that I am not altogether pleased about.

**Scott:** Okay.

Leiderman: This concerns the direction of my clinical work. I decided in order to do what I had hope to do in this area, that I should become better acquainted with youth growing up under difficult circumstances, generally coming up from the lower reaches of our society. I became consultant to the California Youth Authority at Stockton in 1980. The CYA was the final destination of youngsters who committed crimes and who failed to adapt at the community and juvenile detention level and who warranted incarceration in a closed setting. I saw youngsters at the recommendation of staff. My task was evaluation and recommendation for treatment. The typical situation were youngsters who were in trouble with peers and staff within the institution and who the staff believe might have psychological/psychiatric rather than merely behavioral problems. I believe my work helped the staff. I am uncertain whether I helped the youngsters. But what I didn't do, possibly through caution or fear of breaking regulations, was to make these consultations part a research program. This part of my activities stood outside of my academic endeavor, important and satisfying, yet not part of a research program, which as an academic I felt obligated to do. My satisfaction was the feeling I was actually helping youngsters other than the middle-class youngsters I saw clinically at Stanford. I really don't know whether or not my work at CYA had any impact other than support the staff with whom I came in contact.

What other impact did I have on the field? I would include my earlier work, certainly the work I did on sensory deprivation, and the work that I did on social bonding and attachment especially in cross-cultural perspective. I think these areas would be my scientific contributions. I did not think through the paradigms, as I should have done, perhaps; they'd be discovered later. That's how I now rationalize these issues for myself. A major part of what my difficulty was deciding how to bring my professional work and my research work into some sort of harmony. Frankly I never really solved it. I was really torn between the two.

I always tried to do things that I found enjoyable and where I could engender some enthusiasm. Sometimes I would get distracted by something that looked like fun, without it being truly programmatic. It is due partly to my character as well as taking advantage of opportunities that might be available. It would have been very different if I had been single-minded about what I was doing. I believe I could have made more important contributions this way.

**Scott:** Now this is a harder question --

Leiderman: That last one was hard enough. Okay.

**Scott:** No, no. This is a harder one, and that is to look back on the programs of research you engaged in and think about where you might have been in error, where you went barking up the wrong tree. Any particular lines of research that now you look back on and say, "You know what, I kind of disavow that now?"

Leiderman: Here I would include embarking on a program of psychoanalysis and psychoanalytic training. I began psychoanalysis in 1959. It took me about ten years to decide it was not good for me. This occurred after I returned from Kenya after working in a village for a year. Another example of a deviation was when I joined a project on abused and neglected children. I enjoyed that research, it was important and it made a contribution to public policy. I don't think it directly was in line with what I really wanted to do. Okay? It was a deviation that I now regret, because I didn't really complete this phase of work. I was interested in the child care relationships and I spent too much time on that project for what it did for me, without sufficient control of its direction. Though I was interested in the issues of the social nexus of foster children when the environments or context change, this project did not contribute to these issues. My two colleagues from law and psychology were less interested in the clinical components that I was, so the direction of the project was public policy rather than psychological.

What I would have liked more directly, was to have more direct access to graduate students. The disadvantage of being in a professional school, even in such a wonderful university as Stanford, was the lack of access to graduate students. That was always a major problem. I did a lot of the work myself because I always had to scrounge for graduate students, because they weren't readily available. Medical students were hopeless in that regard because of their schedules, and residents were a problem even though they did papers with me. It remained that way until I retired when I had graduate students available to me through the School of Education. It made a big difference.

**Scott:** Why?

Leiderman: I was a member of the Family and Adolescent Seminar Group on campus. In attendance were graduate students who would come to the seminars where they would meet me. They were mainly from the School of Education, they had some difficulty to find faculty to work with (many of these students were of minority Hispanic background). They would ask, "Can we talk to you about this work," and I'd say, "Sure, let's talk." It was fun. I had three PhDs since I've retired, because I could spend as much time with them, and they liked that. In fact, I got an email recently from one student who said he just received a Spencer Foundation Fellowship and he's being put up for tenure at his university. One of the big problems of being a professional and an academic is the hybrid issue. There are many others, but that's one I can think of.

There were other deviations, which I thought were superb. I thoroughly enjoyed my African experience. I was acquainted with anthropology because I had done lots of reading for a long time, so I felt very comfortable being in the field. I didn't feel at all uneasy about collecting data, meeting with village folk. In contrast my academic contacts were sometimes difficult, especially when issues of territoriality would arise.

**Scott: Okay. With all that research experience that you've had, were you successful in getting support for it?**

Leiderman: Yes. Not as much as I would have liked. I received support from the Grant Foundation, the Carnegie Foundation, NIMH, the Spencer Foundation, and the Foundation's Fund for Research in Psychiatry. I also received support from the various groups with which I was associated, such as the Family and Youth Studies at Boystown Center at Stanford who paid part of my salary. They literally bought out time from the medical school for me so I could do research. That was most helpful.

**Scott: Do you have anything particularly you'd like to say about granting and granting agencies and their policies?**

Leiderman: Oh, my gosh! Am I going to get a chance to talk about some other things too? I was on study sections for about fifteen years in Washington. I participated in several of these study sections, and as well was recipient of grants from one or two other study sections.

Firstly, the issue of giving out money. I found that the system for the most part was fair, honest, straightforward, except for the parochial academic interests. If the proposal didn't have the "proper" paradigm, or in other words, if there was anything truly creative or different about what you proposed, and it arrived at the wrong study section, the proposal could be in deep trouble. Many times I felt I was the odd man out in many of study sections, an MD among a group of PhDs. There might be one or two other psychiatrists, however the majority were psychologists, anthropologists, or sociologists. Frequently, the standard studies were done in standard ways. If the proposal was unusual or different or occasionally even creative, it would be talked to death and all too often received low priority when the score of everyone was tabulated. The situation was most evident in some study sections where the secretary had an agenda he or she communicated to the group. Having said that, I can't think of a better system than peer evaluation. Therefore I commend the relative honesty and fairness of the system, only I would like to see it improved.

Another aspect that I didn't like, even though I thought opinions were honest, was the tendency to support established (older) researchers. Younger people had a tougher time, except for programs designed to encourage Ro 1 (young investigator) proposals (I must report I was supported as a career investigator as a younger person). The private foundations I had considerable contact with, such as the Spencer and Grant Foundation did somewhat better in this regard. They were willing to take chances, and I liked that idea. Overall I think the grant system was a marvelous mechanism for the distribution of funds, only I wish it had been somewhat better.

Now for the other position, applying for grants as a senior investigator. I would get annoyed when I applied for a grant, which I thought to be very good, but would get turned down. I recall recently, George Bohrnstedt, Claude Steele and I proposed a study on ethnic conflict and social minority status in high school adolescents. I had done several interviews of adolescents around the issue of ethnic conflicts and ethnic identity in numerical majority and minority status high schools, where non-white groups were in minority, and other schools where they were in the majority. It was a well-designed study with excellent controls for social class, ethnicity, and geographical distribution. We were turned down with the implication questioning our experience with members of these minority groups (Claude Steele is an African-American and I had done work in cross cultural settings and spent considerable time interviewing adolescents of Hispanic, Asian and White backgrounds developing an interview schedule in a previous study). I felt the review group wanted more ethnic minority members as principal investigators, though I could be wrong here. That's the luck of the draw.

**Scott: Yes. Well, now I want to ask you some questions about the institutions you've worked for, but before we get on to that are there other things that you'd like to -- observations you'd like to make about your research career and the programs of research you've been involved in and how they've evolved or changed, or continuities or discontinuities.**

Leiderman: Currently I'm working on a fascinating study examining adolescents from a cross-cultural and cross-national perspective. This study stems from work I did with a small group of psychologists and sociologists. My role in that earlier study was to do the ethnographic portion of the study. I played a major role in this earlier work in getting a grant from the Spencer Foundation for the project. My colleagues working on the questionnaire portion of the study published this portion of the work on their own without even informing me of their action. This was a major disappointment for me personally and professionally. Anyhow, in the course of my ethnographic work I found adolescents who despite being at the "bottom of the heap" because of social class or familial status seemed to be performing very well in school. Socially they were of minority status of various sorts or came from broken families. I decided to look at non-cognitive factors that make for successful performance, especially as it might be influenced by familial, cultural and/or other factors. The current study I'm embarked upon, has taken me to Chile, Bulgaria, and to various parts of California, rural Colorado, and Japan. It is an exciting study, and I believe an important study. In fact, I was looking at some data today with very interesting implications for how such factors as responsibility, perseverance, cooperativeness, planfullness, efficacy, and autonomy play across these various communities. My data indicates this scale predicts academic success in high school. It is an important study and I am intrigued with the issue, different from what I've done before. Perhaps this will be the major contribution I'm going to make at this point in my career.

**Scott: How did you get interested in the question of resilience, because that doesn't connect --?**

Leiderman: Can I give my polemics now at this point?

**Scott: Of course. Yes.**

Leiderman: Okay. I was working with two psychologists and one sociologist in the earlier study, I was the fourth member of that team. We did this large study in nine high schools in Wisconsin and California. I insisted that we interview a subset of the sample in addition to getting information from questionnaires. This approach was my style in this type of research: questionnaires and subset of interviews. I never like to do studies unless I have direct experience with some of the subjects. I like this paradigm because I learn something every time I interview. I took it upon myself to visit three schools in California. I interviewed 40 youngsters selecting both low and high performers. I hit upon the idea that I would interview students from both well to do and at risk (poor, single parent, etc.) families. My problem was how to get a sample of high performing at risk youngsters. I hit upon the idea of finding these students from the school secretaries who really know the students and I asked them to select youngsters who came from families that really had a hard time financially or came from single parent families, but who seemed to be doing well in school. They did it easily. I collected 40 such youngsters in three schools in the Bay Area. I interviewed them for about an hour to find out what they believed enabled them to make it despite financial hardship. The common element turned out to be the qualities mentioned above. I termed these adolescents resilient youth that were going to make it despite the odds against them. I decided this research was something I really wanted to do because it reminded me of aspects of my own childhood. I thought this would be a good way to spend my research time in retirement. That's how I arrived at this topic of study.

**Scott: Interesting. I see. Okay. Now can we turn to institutions and --**

Leiderman: Yes. Let me just say a little bit more about my critique of American child development research. I will elaborate on this topic right now. I knew wonderful academics over the years that were models for me in my work. Yet, when I worked with them I found many never really got to know the individuals they were studying. Anthropologists were the exception who by and large did get to know their subjects. However, frequently the informants were the leading individuals in the village, which is acceptable if they specify that this group was the source of their information. To talk to the elites to learn about the people down the social status level is insufficient in my opinion. I learned this lesson in my research in Kenya, when I included mothers from the "bottom", as well as talking to those at the "top" of the status hierarchy.

Now to carry this over at the U.S. I felt that one major problem in American psychology was the absence of direct observation and interviewing by the individual who eventually was to write-up the study. Graduate student collection of data was necessary but not sufficient. Direct observation and interviewing by senior investigators was a necessary ingredient. A second issue for me was the selection of subjects and/or informants. Early on the counterpart to the white rats of physiological psychology were the white b-rats of American child development of middle class background or for adolescence white college sophomore males. The world I knew as a clinician and researcher in the nearby lower class communities stood in sharp contrast to this sample, very different from the world of the upper middle class. Only in the last 15 years or so has psychology begun to broaden the horizons to include populations who were excluded in the past. In my research I approached subjects as participants in a more personal way, based upon my personal experience in childhood, my experiences as an adolescent, experiences in the military service, working in hospitals in central cities, and my experience in Kenya. I knew that to get to know people, you really had to contact them directly to understand them. The best psychologists did just that, most sociologists understood this point from the start.

**Scott: That goes without saying. Okay. Well, now let's talk a little bit about the institutions. You were at Harvard?**

Leiderman: Yes.

**Scott: And then Stanford?**

Leiderman: Yes.

**Scott: And those are the two principle institutions?**

Leiderman: Well, one other: University of Chicago.

**Scott: Well, yes, but I'm talking about in your professional career.**

Leiderman: Professional, okay. Alright. Harvard Medical School.

**Scott: Okay.**

Leiderman: Yes.

**Scott: And you've been at Stanford since 19--?**

Leiderman: 1963.

**Scott: '63.**

Leiderman: Yes. But first I want to say something about the University of Chicago. I always knew I was to go on to university. My family never discussed it directly, yet everything that happened to me in school and outside was with the assumption I would go on to university. My choice was the University of Chicago. For financial reasons I could not be there.

After discharge from the military in July 1946, I enrolled in the University of Chicago where I had been accepted earlier. The then President, Robert Maynard Hutchings, was a hero to the students and most of the faculty for his "Great Book" ideas about education -- classical while not ignoring the pragmatic aspects -- and for his emphasis on excellence in whatever students and faculty did. There was high morale amongst the students who responded to a faculty who seemed to enjoy teaching bright and enthusiastic students. Chicago raised my sights to consider becoming a university teacher, to be

associated with and contribute to such an enterprise. No institution I have known since -- Harvard (daughter), Stanford (daughter), Princeton (son), Vassar (daughter) -- seemed to reach such heights, though possibly Swarthmore and St. Johns and other similar colleges might well be equivalent to the "historical Chicago."

**Scott: Well, let's start with Harvard, because I think that's prior.**

Leiderman: Harvard Medical School was a daunting place for me initially. The sheer size and complexity of the institution; the Medical School, the research laboratories, and hospitals scattered around Boston was almost overwhelming. I felt most of my fellows students were better trained in biology than I was. They certainly could memorize better since I almost failed gross anatomy. When it came to histology, biochemistry, physiology, pharmacology I felt far more competent. Gradually I began to take on the mantle of a competent medical student who belonged in medicine. By the time I reached my clinical years I became more comfortable because I liked patients, was a good interviewer because of my previous work in psychology, summer camp counseling. Further, I felt able to integrate information from many different sources and make sense of it because of my military meteorological experience.

Most stimulating for me to become a physician was the outstanding preclinical and later clinical faculty. The faculty of Harvard Medical School wanted to teach even those volunteers in the clinical years who weren't paid for the effort. In the preclinical years my favorite teachers were Dr. A. Baird Hastings and A. K. Solomon in biochemistry, both of whom supervised my research; Assistant Professor Avram Goldstein in pharmacology -- later a colleague at Stanford -- was a fine teacher, courageous to defy the then Massachusetts law forbidding the teaching of contraception; Dr. Professor Herrman Blumgardt at Beth Israel Hospital and Dr. Professor Walter Bauer, Chair of Medicine at the Mass General Hospital, provided models for elegant bedside teaching; Dr. Professor Derek Denny-Brown in neurology at Boston City Hospital taught me precision of thinking; Dr. Philip Solomon at Boston City Hospital, Dr. Erich Lindemman at Mass General, psychiatrists provided me as models of caring, knowledgeable, scientifically-oriented physicians who were dedicated to their profession; standing with them was the Dean of the Medical School, George Packer Berry, who captured the post-war atmosphere of Harvard University inspired by then President James Conant in opening Harvard to greater diversity of the student body.

My wife and I both graduated from Harvard in 1953 with our degrees, PhD (wife) and MD (me). Following our graduation, she was the only one with a paying position teaching child development (I was paid between 300 and 1800 dollars per year during my internship and residency years). In 1953 she replaced Professor Pat Sears in the School of Education at Harvard because Pat was leaving for Stanford with Professor Bob Sears, both of whom were taking professorships at Stanford. We decided to stay in Boston for my internship in internal medicine until I could decide on a specialty. During my internship, I decided on a residency in neurology to further my interest in brain and behavior. I completed two years of neurology at Boston City Hospital, followed by three years of psychiatry at the Massachusetts Mental Health Center and Mass General Hospital. By this time we had four children so we needed paid employment. I applied for an NIMH career investigatorship, received it, and became a junior faculty member of Harvard at Mass Mental Health Center. We ended up a total of 14 years in the Boston area where we had intended to remain only four years.

**Scott: I see. Wow.**

Leiderman: Five of those years were as a junior faculty member at Harvard at the Mass Mental Health Center, where I did social psychophysiological experimental research. For a time I studied the effects of sensory deprivation and social isolation for the Air Force based on my clinical research on paralyzed polio patients in respirators. I also received funds from NIMH to develop a program of research in psychosomatic medicine. I developed a small moonlighting practice for income, became recognized by senior clinicians so could receive patient referrals. However most of my time was spent on research, teaming up with a social psychologist. We decided to develop a field of socio-physiology, the psychophysiology of small groups. We published several papers and a book on this topic.

Despite the stimulating academic atmosphere of Boston and Cambridge I always wanted to come west because I just wasn't a native New Englander. I felt that New Englanders were not my kind of people: distant, parochial, and self-absorbed. The lifestyle was different from the Midwest I had known as an adolescent and young adult. For example, we didn't have a long lineage, we didn't have a place in Martha's Vineyard that we could go to on holiday. We didn't have an "old homestead" up in New Hampshire because we were not old family. Furthermore, we didn't have any money (that was one of the main problems). We also had four children, so the problem to solve was earning a living to support my family.

The first chance I had to leave was when I was offered a job to come west to Stanford. I seriously looked into it. I'd been out to Stanford as a tourist in 1944, during World War II, when I was stationed in Pasadena. I traveled up to the Bay area, saw the Stanford campus briefly in early 1944. I can recall saying, "I sure would like to go there someday." When David Hamburg, the new Chair of Psychiatry at Stanford asked me if I were interested in a position (he was setting up a research-oriented Department of Psychiatry at Stanford in '62) I answered in the affirmative. I had come on a visit to the Psychiatry Department at UCSF in 1962, where I had friends and colleagues who were interested in my work. David Hamburg found out and asked me to come down to Stanford to see him. Several months later he offered me a job. I took about ten minutes to make my decision. I said, "Yes, I'd be glad to come," so that's how I got out here. That was the impetus for me and my family to come west where we remained for 40 plus years.

Coming to Stanford was to arrive at a world distinctly different from Chicago and Harvard. The University at first seemed more relaxed, fun-loving, scholarly but certainly not driven towards excellence as I experienced at Chicago and Harvard. Also what was somewhat confusing was the presence of seemingly laid back President Wallace Sterling who was calm, relaxed, humorous, yet was on top of matters, suggesting changes were done. (Note there was also a fine Provost, Lewis Terman, who likely was the architect of creating an international university out of a regional college.) Sterling and Terman were instrumental in bringing the Medical School from San Francisco to Palo Alto. This was an act of administrative genius. A second act was to make the medical faculty full time emphasizing research in addition to clinical practice. This was a very brave act at that time. New faculty also were being recruited in the social and behavioral sciences strengthening a regional undergraduate college and engineering school into a first rate internationally recognized university. This was done with the acceptance of the dominant group on campus: the engineers. Sterling, along with Hutchins of Chicago, Conant of Harvard, showed what could be done to transform institutions through good ideas and calm, coherent, firm leadership.

**Scott: You were in psychiatry**

Leiderman: Yes. Though I was centered in psychiatry I also knew people in psychology: the Sears and Eleanor and Nate Maccoby from Harvard days. I also knew Lincoln Moses because my brother-in-law and he were colleagues at Teachers College, Columbia, and they completed a couple of papers together. Somewhat later in 1964, Ben Paul of anthropology came out from Harvard; a bit later Alex Inkeles in 1972 came here. We were friends with the Pauls and Inkeles in Cambridge in the 1950s and continued this relationship at Stanford.

**Scott: Wow!**

Leiderman: So I knew people out here.

**Scott: And was your entire academic career at Stanford now? I mean, you stayed at Stanford until you retired --**

Leiderman: That's right.

**Scott: And was your appointment always in the Psychiatry Department?**

Leiderman: I was in the Psychiatry Department. However I had close contracts with the Social and Behavioral Sciences because I ran the program in the Medical School sponsored by NIH for training physicians in a 5<sup>th</sup> and 6<sup>th</sup> year in the Social and Behavioral Sciences where students might obtain combined MD and MA or PhD degrees.

**Scott: But it was basically there.**

Leiderman: Yes.

**Scott: Now how would you describe the climate for research at Stanford?**

Leiderman: Stanford University was superb for me as a professional, because I had fifty percent of my time for research since I was on a full-time salary. I didn't have to earn outside income then. The salary was low at the beginning. I was paid 20,000 dollars a year, so it was very difficult to survive. My wife had to get a job in order to support us, which she did. She initially worked with the School Mathematics Study Group doing national research. On several occasions we exchanged children at the San Francisco Airport. I would be returning from meetings in Washington and she taking off for her research program in Miami. Later she became Director of Peninsula Children's Center for Disturbed Children in Palo Alto and our life became simpler. (Note: we always had domestic help from the birth of our first child in 1954.) But the climate was wonderful. David Hamburg had a wonderful attitude about research. He wanted the faculty to do research. Eventually we both received enough raises in salary to live on.

I liked Stanford especially because most of the people I met on the campus were welcoming to a member of the Medical School. I became close to faculty, especially in sociology, psychology, and anthropology, possibly because I was in a charge of a research grant from NIMH, but partly because of our outgoing personalities, ages of our children, and living on campus. Intellectually it was very exciting, because the university was in an active growth phase, I couldn't think of a better solution. I had two chances to leave and I finally decided it really was too good here to do so. We had four children at that time very young in age (ages 10-15), carting them around would be difficult. Here at Stanford, I could sneak off from my office, take the children some place and pick them up without violating some major rule: 'are you on duty today'. I would say it was a wonderful situation. I also liked the administration of the university. I thought Wally Sterling, the President, was just a marvel in the way he ran things. I knew him from the monthly dinners he gave for the chairmen of the clinical departments in Medical School, welcoming them to the "University" after the move from San Francisco (I served as acting Chairman in 1967 when the Chairman was on sabbatical). Don Kennedy was a good president and so was Dick Lyman. I liked the way Stanford was run. Al Hastorf, the psychologist, later a provost, also a very important person in welcoming me to the campus. I served as an outside member of lots of thesis committees in psychology because they needed an outside member on PhD committees. As a psychiatrist, I qualified. There was one other factor about Stanford I should mention, the Center for Advanced Study and Behavioral Sciences was here, and that later became a very important part of my life here.

**Scott: I see. What year were you a Fellow?**

Leiderman: 1973- 1974.

**Scott: And did you have any affiliation with the Center or familiarity with it prior to that?**

Leiderman: Yes, a lot. David Hamburg, the Chairman of my department, was a Fellow here in '67 (I was Associate Chairman of the department then, so I was up to CASBS for lunch maybe once a week or at least it seemed that way). I knew several people who were Fellows at the Center, individuals who I had met at meetings in cohorts '71, '72, and after my fellowship year '73-'74. There was a small hiatus

in my contact with the Center until Gardener Lindsey took over in 1977. I knew Gardener from my medical school days in 1950s. We resided in the same Harvard Graduate School ghetto, Holden Green in Cambridge. Gardner was a superb director of the Center for 13 years, setting the scholarly orientation and socially friendly atmosphere of the institution for the many years of his service. He truly cared for the place and maintained the high standards of this remarkable institution.

**Scott: Yes. Okay. And then you've since then been --?**

Leiderman: Since 1980 I have been associated with the Center in various ways helping Gardner with the Foundation's Fund for Research in Psychiatry Program, which met annually at the Center. The group consisted of 8-10 psychiatrists who would discuss issues related to psychiatry and advise Dr. Fritz Redlich, Chair of the Foundation, Dean of the Medical School of Yale, also a Dean of American Psychiatry regarding public policy issues of American Psychiatry. I also served as a consultant to the Director of CASBS on medical/psychiatric matters as they occurred with fellows and staff. I had lunch at the Center twice a week and attended Wednesday's seminars. In this way I could keep abreast of the social behavioral sciences, meet outstanding people and enjoy myself in the process. I continued in this role until the present date.

**Scott: Okay. You mentioned that half your time was freed up for research.**

Leiderman: Yes, it was.

**Scott: What were you doing with the rest of your time?**

Leiderman: The rest of my time? I taught resident and medical students, lectured about a quarter time in formal courses (I rarely taught full formal courses); I did it two or three times. I do not think I was terribly successful at it. Team teaching was the norm in psychiatry. I supervised residents in training and bedside teaching of medical students. I also was a very good citizen, serving on several committees of the University and judicial council. I was elected President of the Medical School Senate serving for two years and also served for three years on appointments and promotion committees in the Medical School. I also was involved in departmental committees until about the late 70s or early 80s when I decided that the same problems always arose and remain unresolved. I finally concluded they were insoluble. I gave the committees my best shot, and obviously despite my "genius" at getting people to work together, the problems remained.

**Scott: Now you mentioned earlier that one of the regrets you have is that you didn't have graduate students who you could work with. Were there other people though who you were training as researchers?**

Leiderman: Yes, I did. There were medical students; I had several students whom I supervised on their research papers. They were interested in my critiquing their research. I was competent about methodology, I could understand the research design, how to write up a proposal, how to get grants. I did a lot of that. I had many medical students who worked with me all during their medical school years. Recently I visited with one of my students in Washington who's now a Senior Director of the Food and Drug Administration. He was with me in Africa. I brought him over to Africa where he spent five or six months working with me and Robert LeVine in Gusii area of Kenya. I enjoyed medical students who were interested in research, but they were relatively few in number. Their interest lay in clinical medicine even though Stanford Medical School was noted for being a research-oriented medical school.

**Scott: Sure.**

Leiderman: There were always a few residents, one or two every three years or so, interested in research who I would spend more time with. Also, I did have graduate students early on, 2 or 3 got PhDs working on my projects, and after my retirement I served as senior adviser of three students from

School of Education who worked with me on adolescence projects. I was willing to spend time, even if not officially recognized. They wanted somebody who would listen to them about their research, a bit of handholding and such. Some of the time I received acknowledgement, some of the time not. I received lots of satisfaction from working with students, since it was not an overwhelming number as it is for some faculty in arts and sciences. After that I arranged to be senior thesis advisor through a multidisciplinary program on campus. I could directly supervise graduate students. I believe had a total of five students who received their PhD's under my supervision.

**Scott: I see.**

Leiderman: Several of my students became senior faculty and/or administrators. I don't know how many who remained purely academic. One, Aimer Dorr Leifer, was a very good student and researcher in psychology. She spent three or four years with me. She now is Professor of Psychology at UCLA. I nominated her for the Behavioral Sciences Center. I was disappointed she didn't get higher ratings, since I thought she was a certain winner.

**Scott: Are there other facets of your affiliation with Stanford or Harvard that we haven't touched on here that you would like to comment on?**

Leiderman: My most notorious public service was in 1967-1969 period of the Vietnam protests when I served on the Judicial Council of the University for Undergraduate Student Affairs. It was a sobering experience. The crowd mania would take over from student leaders encouraged by some radical faculty who would disappear when the "going became too warm." (Literally fires were started in several buildings on campus including at the CASBS where an anthropologist from India lost all of his field notes.) This experience took about a year out of my life, because I was up until two o'clock in the morning day after day hearing cases. It was an incredible experience learning about law through Jack Friendenthal, Professor of Law, who chaired the committee. He asked me to be on a seven-person panel. I think the Provost asked for me because he thought the Council needed someone from the Medical School, that is outside of the academic campus, to serve. I felt that it was my duty to do so. I certainly learned a lot about the faculty from this experience, about those supporting the "insurrection" secretly, and those who opposed it because of ideological positions, regardless the evidence for or against a student. I learned about the uncaring narcissism of some of my academic colleagues, the reluctance to see the community (Stanford) as part of their obligation. This facet of academic life, concern chiefly with self, always bothered me. However there were many others who were most conscientious in their obligations. Most stayed on the sidelines. The view surfaced yesterday at lunch with a Fellow at the Center, talking about his own university as if he was not an obligated member of the faculty. His arguments against this duty were that he had his own work to do. In my Department of Psychiatry we had people who never participated in anything but their own work, probably rightfully. That's what they were here for. I took a different stance, because of my background. My parents were socially participating and responsible. My parents educated me in this mode. There was no way I was going to be different despite five plus years of personal psychoanalysis. I generally enjoyed what I did, so I didn't feel I was being punished when I included others in my purview of concern.

**Scott: Right.**

Leiderman: So that was my "service" on the campus. My work on the national study sections was also important. I liked that and did that for over a decade. I also want to mention the Behavioral Science Center here because I felt I made a contribution to this institution.

**Scott: This would be a good time to do it right now.**

Leiderman: Okay. I knew Gardner Lindsey from 1950s when we both lived in Holden Green, Cambridge. When he came to the Center as Director in the late 1970s, he felt he could not deal with the then Associate Director. He felt he needed someone else up to talk to about issues and/or

problems at the Center. He asked me if I would do two tasks. The first one was the administration of Foundation's Fund for Research in Psychiatry Program, which was giving its founding grant to the Center. I had been a member of their Board for nine years. He said, "Since you know the Foundation's funding, you would be the logical person to take over if you were interested in this job. Take it out of my hands and just run it under the guidelines." The second task was to be a consultant to him around medical and psychiatric issues as they arose at the Center. The position evolved over time. Gardner was easy to work with and the fellows were most interesting. At about that time I met Robert Scott I liked him a lot; I thought he could be a superb Associate Director and a most decent, intelligent scholar/administrator who had the proper dose of self-interest and concern for others and for the institution. It turned out that my assessment was correct.

**Scott: He's a wonderful man.**

Leiderman: He was a superb Associate Director. My thought was if the Center did not have Robert Scott as Associate Director, the place would be in deep trouble. I could say it even more so today, by the virtue of the way he kept the institution going when he was Associate Director despite indifferent directorships after Lindsey retired. With Bob's retirement as Associate Director a sea change of direction seemed to occur. I liked the concept of the Center, saw it working well, though with problems, over at least 30 years. It was one of the most successful enterprises I have been associated with. There was a goal: scholarship at the highest level pursued by individuals with time to work on important scholarly issues, in concert or alone, with excellent support by staff for their work. While I do have some criticism of future directions, or perhaps the lack of appropriate change, I have not been associated with another organization doing as well in meeting stated goals.

I met lots of wonderful fellows at the Center during my own fellowship, '73-'71, had known through my work. Especially good was the project organized by Emde/Sameroff on relationships and Rutter/Garmezy on resilience. I maintained wide interest in the social behavioral sciences through these activities. Certainly the last 15 years of my professional life was enhanced because I had the collegueship that I wanted. The Center was always a place that I felt I could visit where by virtue of listening, talking, or meeting I would be kept current in the field. I consider this contact a very positive way to maintain an active retirement.

**Scott: Wow. Alright. That's very interesting. Now do you belong to the Society for Research in Child Development?**

Leiderman: Yes, I did for many years. I don't know how many now. I reviewed articles for the journal. I was a chair of the Program Committee for the biennial meetings for two years. I was the Program Director for the New Orleans meeting. I guess about 10 or 15 years ago, which took a hell of a lot of time. It was an incredible experience planning and reviewing in order to produce a fine program. Professor Eleanor Maccoby "seduced" me into doing it. Once was enough.

And I would go to meetings. I found as I got older, I wasn't following the literature that closely. I also didn't feel close to some of the younger scholars. I didn't feel like presenting papers, and I wasn't sufficiently political to get on boards and the like. I did get invited to meetings abroad, especially by people who knew my earlier work on cross-cultural studies of infancy.

**Scott: I see.**

Leiderman: I was involved regularly from 1968 on to '85 in SRCD.

**Scott: So 1968 is when you first joined?**

Leiderman: Yes, I think it was '68. Yes. Eleanor, and I think some others, encouraged me to do that. And since Gloria, my wife, was always a member. It was very important for both of us to go to meetings to meet old friends.

**Scott:** Who were some of the people who you remember were part of the Society at that time with whom you worked?

Leiderman: Well, one person whose work I admire tremendously was Glen Elder. He was really a first-rate thinker and researcher. Paul Mussen also comes to mind. The problem is not a paucity of individuals to mention, but rather my remembering their names now.

**Scott:** Right.

Leiderman: Let's see. Pat and Bob Sears were there. Another very important person was Lewis Lipsitt as well as Frances Horowitz. I participated in several symposia with them on early mother-infant relationships. Alan Sroufe, Mary Ainsworth, John Bowlby were important figures for my attachment work. Sir Michael Rutter and his group for excellent science they brought to real world issues in child development.

**Scott:** Okay.

Leiderman: Let's see, I wrote down some names here to make sure. Arnold Sameroff, Norman Garnezy, a very important person, Urie Bronfenbrenner and Mavis Hetherington were the individuals whose work was important for me. I almost forget to mention Jerry Kagan, Diana Baumrind and Robert LeVine all were major presences in SRCD. I don't know if they were members of the Society, but they were very important for me whenever I would meet them and talk about research. These were Judy Dunn and Robert Plomin and Rick Schweder, Barbara Rogoff. Schweder was a first-rate anthropologist and developmentalist. I admired his work, beginning with his graduate student work on the Whiting project in Kenya in 1970 after I left the Field Directorship.

**Scott:** Oh, really!

Leiderman: Yes. He was there, so I've known Rick since the '70s, but his work has really developed. More recently he and Hazel Markus, a colleague at Stanford, have collaborated. So intellectual relationships continue.

**Scott:** Yes. Okay. Now, so it sounds to me as if the Society played an important part for you at a point in your career that, as your interests evolved and as you matured, it became less important to you, is that --?

Leiderman: Yes. It was very important at a certain point when I was just getting into the field. It peaked from about that time with what I was doing. I found it less interesting over time because I couldn't participate and communicate enough that would make it central to my concerns. I was disappointed I was not an officer. Then I realized that's not right because you have to be more visible and I was not.

**Scott:** Sure. Well, now we've talked about training and we've talked about institutions and we've talked about the Society and all those sorts of things. Let's turn for a minute to the field of research in child development. As you look over that in the course of your career, do you have any observations you might like to make about the history of the field during the years you've been in it and how it's been changing, and whether that's for the good or the bad?

Leiderman: Yes, good question. Overall I'm very pleased with what's happening in the field. I've asked myself the question, "Why did it take so long for certain things to happen?" Early on in the field when I was a student I felt we should not make a pretense of science by describing our participants as subjects rather than people. I believe when one describes humans as subjects it may tell more about the researcher than it does about the individuals being studied. I believe we should not objectify the participants as an attempt to be scientific. The most defensible stance for me was to describe the

participants as individuals as best I could, or as a group with descriptive terms most relevant to the hypotheses being tested. Another gripe of mine was the absence of culture, ethnicity, or social class variables in the analyses. The concept of social class didn't seem to exist in psychology until about a decade ago (it existed, of course, in sociology where they eschewed psychology instead). The characterization of people of color, diversity as part of a culture was generally absent. It was difficult to get research published with that type of variable included. Context, despite Bronfenbrenner's work, did not seem to have much of an effect. In my work I attempted to include context as a variable, sometimes successfully and sometimes not. I believe we are more sophisticated now with the diversity of our population. It just wasn't reflected in the early research by some researchers, even when they could do so. That's point one.

A second point is my feeling we should move beyond the simple experimental or observational methods to include selected ethnographic interview and similar techniques to supplement the findings. For some work there is no other way to do the study properly without interview methods. There appears to be an artificial division between ethnographic, experimental, and observational approaches. All of these methods contribute to the research and help provide more valid data. Within a given study it is more difficult to do interviews, but I think it's essential for the best research.

The third point is that we should pay more attention to what I call the "historical tides" that sweep into the field. I am referring to the situation as of ten years ago. Today young scholars are more comfortable with diversity and the examination of issues in a variety of ways all to the good. Their work will differ in perspective from research done 20 or 30 years ago. Direct comparisons should be done taking this into account, both for what is studied and how a research problem is formulated.

A fourth point is that we are truly global in the behavioral sciences. European and Japanese research was rudimentary then. Now behavioral and social science research is international, at least for the developed world. Gradually we will get to the 2<sup>nd</sup> and 3<sup>rd</sup> world. Eventually I hope we will have a comprehensive biosocial-developmental schema with many different paradigms and various diverse samples. I would say that the research has changed much for the better. It's taken too long, but it is here now!

**Scott: Sure. What's your sense about its future? Do you think it's here to stay?**

Leiderman: Yes. I think it's going to be robust, in part because there are many new approaches which will improve our developmental psychology. I am thinking of better biology, especially modern genetics, but also looking to contemporary studies of brain/behavior processes. These approaches are from biological source as well as the external contexts contributing to understanding of behavior.

**Scott: Yes.**

Leiderman: Parenthetically, that's what I'm going to suggest to the Center for Advanced Study in the Behavioral Science Directorate to think about in selection of fellows. We're now on the threshold of great breakthroughs in biology understanding processes and mechanisms at many different levels. If we brought biology, genetics, and understanding of context together we could look forward to a tremendous future for the field. If we do not understand more of what's happening at these various scientific levels we cannot logically help the youth in serious trouble. I see this in California Youth Authority all the time. The cutting edge for public policy may lie in the integration of the biological, psychosocial, and cultural perspectives. It will require lots of money, though I guess not more than the level we spend now on bombs and other military equipment. The "bomb" we now fear is physical, but most likely in the future will be social.

**Scott: Right.**

Leiderman: And the terrorist may not come from abroad, he/she may be right here. They're born every day and living among us.

**Scott: Right inside. Sure. Sure. Okay. Now I want to go back just for a minute and pick up a couple of other bits and pieces here, because you've talked about Gloria and the important role she's played in your interests and also as a kind of collaborative, not to mention life partner. Tell me about the rest of your family.**

Leiderman: Alright. We have four children much influenced by our sabbatical year in Kenya. Our oldest daughter is married with two children. She is a physician, a neurologist. She is a graduate of Harvard/Radcliffe, majored in Anthropology, wrote her thesis on African women. She received her MD and MA in History of Medicine program from the University of Pennsylvania. She has been interested in clinical work combined with research. Being a married woman with children, she stabilized her life by not going into full time clinical practice. She worked for two years on the Navaho Indian Reservation in Arizona after she graduated from medical school as part of tuition payback. She has worked at NIH in research, became interested in drug development, and did that for a time in the private sector. She finally decided to stay with the Federal Government. She works for the FDA as a Director of the Anti-Addictive Drug Development Program. She has a senior position in Rockville and plans to continue in a quasi-administrative and quasi-research role.

Our second child is the family home-leaver. She graduated from Stanford (BA) and Brown (MA) Universities in the humanities, much influenced by the '60s. We haven't had much contact with her since her young adulthood. She is married, a published writer, lives her own life in the hills of California on a ranch. It's a disappointment for us because she's a very gifted woman, and was a most interesting addition to the family dinner table conversations.

Third is our son, who is intelligent, scholarly, humanistically-oriented, and fluent in four languages. He never had a clear direction until recently. He started in literature, has degrees from Princeton (BA) and Harvard (MA). At age 40 he decided on medicine. He completed his MD training at McGill in Montreal, specialized in family medicine, has worked for Indian Health in outback Canada, and in a clinic in the Cameroons. He is he planning a career in international medicine.

Our fourth child is married without children. She has been interested since high school in public policy and political science. She has undergraduate degrees from Vassar (BA) in Public Policy (MPP) and Public Administration (PA) degrees from U.C. Berkeley. She has worked in Congress, now is the Director of Government Relations for Kaiser Permanente Foundation for Santa Clara and San Mateo Counties. She is Elective Officer of the School Board of Santa Clara County and formerly a member of the Central Committee of the Democratic Party in the State of California.

**Scott: Okay. Do you in your own mind see any connection between your family and career that strikes you as interesting in terms of the directions your career took?**

Leiderman: Yes. Certainly my wife (BA Illinois, PhD Harvard), personally and her career, has been very important to me. She was a strong support not only directly, through introduction to people (Sears and Whitings come to mind) who provided me for the opportunity for positive academic experiences, but encouraging me when the going became rough for me in medical school. Medical school became tolerable because I engaged in intellectual stimulation through my wife. In addition I gained socially and economically because she was a Director of a Center for Disabled Children in Palo Alto for over 20 years; a public figure, recently honored by the Palo Alto community for public service at her retirement.

**Scott: Sure. Now, I've been asking lots of questions here and trying to lead this along, but I wonder if there's anything else you'd like to add, or you think ought to be added to this?**

Leiderman: Yes. I should mention another interest because it was an important part of my family life. Our family was interested in the outdoors so we did lots of backpacking in the Sierras from the late 1960s to the early 1980s. We were brought together in the midst of beauty, serenity, and collegiality.

In contrast I should mention some disappointments. I've been disappointed in people I misjudged, or should have known better. Their own personal interests were the only consideration despite the amount of time I committed to them. They departed without so much as a goodbye. I did not understand it. My wife assures me that "you're not the only one." I replied, "Okay. You're right."

**Scott: Yes. No, that's true.**

Leiderman: I don't mean for that to be the typical situation. It was a disappointment. Overall I feel lucky that I do feel positively 90 percent of the time. I truly enjoyed where I lived and the circumstances that I've created for myself, that other people created for me in this environment. This stands in sharp contrast to the imposed environment that I had experienced during my formative years growing up in Chicago.

**Scott: That's right. I see what you're saying. Yes.**

Leiderman: There was something I always vowed I would do if at all possible. I would establish an environment for myself that I had some control over; that I would do even with the sacrifice of money or other perks. They were much less important. By God, I think I did it at Stanford!

**Scott: Yes. I see what you mean.**

Leiderman: So therefore, I feel that I lived in a very, very positive era for me and my family. I wish it could be true for the future generations, for those young people now coming along. Given the regressive political climate in Washington, the rampant thievery and corruption in corporate America, and the lack of communitarian values in the society, I must confess that at this point of my life I am pessimistic about the future.

**Scott: Yes. Well, alright. Thank you. That's great.**

Leiderman: Thank you, sir.