Leon Eisenberg

- Born: 8/8/1922 in Philadelphia, PA
- Spouse: Carola B. Eisenberg
- A.B. (1944) University of Pennsylvania, M.D. (1946) University of Pennsylvania School of Medicine

Major Employment:

- Maude and Lillian Presley Professor of Social Medicine and Professor Emeritus, Harvard Medical School: 1993-Present
- Professor of Psychiatry, Harvard Medical School: 1967-93
- Chairman, Department of Social Medicine and Health Policy, Harvard Medical School: 1980-91
- Chief of Psychiatric Services, Massachusetts General Hospital: 1967-74
- Professor of Child Psychiatry, Johns Hopkins University: 1961-67
- Chief of Child Psychiatry, Johns Hopkins Hospital: 1959-67

Major Areas of Work:

- Child psychiatry, autism, social medicine, global health, affirmative action

SRCD Affiliation:

- Child Development Editorial Board (1962-68)

SRCD Oral History Interview

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Interviewed by Emily Cahan
At the Department of Social Medicine
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Cahan: Beginning at the beginning, could you describe your family background along with any child and adolescent experiences that may be of interest including the educational, occupational characteristic of your parents, where you were born, where you grew up, what was your schooling like, any military experience, early work. Go back to this little laundry list and refresh any time you want.

Eisenberg: Well, I was born in Philadelphia. My birth was a problem because my mother suffered from severe postpartum hemorrhage and died. So that you see she was someone I never met before and whom I remember only from a picture. I was raised by my grandmother for the first three years of my life, my grandmother and three maiden aunts, until my father remarried and reclaimed me. Probably it was the very best thing that could have happened because my grandmother and aunts had spoiled me rotten by three and God knows whether I would have survived much more of that.

Cahan: And when did you get back to your father?

Eisenberg: At three. So I got back to a father and a new mother who really was quite wonderful to me or at least as wonderful as she could be as a person. That is, we had our disagreements but it had nothing to do with my nonbiological relationship. I had subsequently two younger half-sisters; we all acted as one family. And I think the good fortune for me and maybe for my mother as well and for my father was that I had two sisters rather than another brother because whatever feelings of biological

loyalty there might be toward a son it was not the same toward a daughter because I was the one on whom the family's academic hopes were pinned and there I was and I did well. Grew up in Philadelphia, went to public school. I think I remember, or at least the family story is I was an indifferent student until about the fourth or fifth grade when I became a good student, was visibly a good student and certainly was a good student all the way through, so that when I finished high school in Philadelphia everybody was asked to take the April college entrance boards -- they used to have April and June exams -- and I won one of twenty mayor scholarships. It's a long history of the relationship with Penn but the city gave scholarships that were something or other, and it was exam-based. So I got a scholarship to college, although, by that time, the family was well enough off that tuition would not have been a problem.

Cahan: What did your father do?

Eisenberg: My father came to this country, probably at about age ten, as an immigrant with his father, grew up in extreme poverty, in fact was thrown out of the house by a stepmother that his father married. His mother had died in Russia somewhere.

Cahan: Do you know where in Russia?

Eisenberg: No. My father had to shift for himself. He had to go to work, so he left school at maybe the seventh or eighth grade, worked hard and became a salesman, saved money, started a business, and by the time I was growing up he was a moderately successful small businessman.

Cahan: Sounds very much like my grandfather and his father.

Eisenberg: So I certainly had no experience at privation but I was certainly aware of privation. Not that I heard stories about their early years from my parents very often, but rather that my father had an extended family, many of them were in trouble. He provided jobs for some of them, gave handouts to others, so one was aware of poverty. I was born in '22 and was ten at the time of Roosevelt's election and the Great Depression.

My adolescence was marked by the arrival of Hitler. Anti-Semitism was in the air. I knew from a long time back -- I can't say when -- that if I didn't have superior grades, I'd never get into college. I truly wouldn't get into medical school, and maybe I wouldn't get into medical school even if I had superior grades, so that anti-Semitism was an issue that I remember very distinctly. The way history looks in retrospect with American strength you would think that there was never any question that we would win the war. Living through it we had a lot of questions about what would really happen and what would happen to the United States. There was a German-American "Bund", people who wore Swastikas. There were all sorts of other fascists like Father Coughlin on the radio stations, so it was a time of anxiety. I remember -- I think in just talking to my father it may have been his idea, I remember it also as being my idea -- that what mattered was what you had inside your head because that was the only thing they couldn't take away from you.

In any event, I got into Penn. I really would have preferred to go to Haverford, but I didn't get into Haverford, and my father couldn't understand anyway why I would not want to go to Penn since I had a scholarship, and there was never any thought of going away to college. I didn't think of it, and he didn't think about it because, as I remember, of the kids who graduated from high school with me maybe one kid of the forty or so that were in my class went away to college. It was just not done, at least in my lower-middle class Jewish circle. I went to Penn and did very well. So I wanted to go to medical school, and I had the damnedest time getting, got in at the last minute off the waiting list. And I can repeat the story which someday, when asked to tell, is going to be a narrative. I was first in my class at the end of the first year, and I stayed on top for all four years.

Cahan: And that was at what medical school?

Eisenberg: Penn. At the end of the first year it had been standard that the students first and second in the GPA -- because they calculated these things in those days to two decimal points -- were supposed to serve as proctors in the pathology course; that is, you assisted in doing a resection of the cadaver. They passed me right by and went on to somebody else. It wasn't a great intellectual loss, but it made you know who you were and how they felt about you. After finishing first at the end of the fourth year, every graduating student had an interview with the woman who was then the Associate Dean of Students, a woman named Elizabeth Glen Ravitch, who was the wife of Isadore Ravitch who was Professor of Surgery and had been Eisenhower's surgeon. He had a certain amount of fame for one who had once been a Jew but had thought better of his ways. Mrs. Ravitch had never had that lesson; he was a convert, she wasn't. She said to me in that final interview, "We've never forgotten how you got in here." They still remember the fact that my father had put a lot of effort to get me into medical school. If anyone had proven his right to be there, it was I. I wish I could tell you that I made a dramatic speech or that I quoted Thomas Jefferson. I just felt sick. The University Hospital at Penn took, as I recall, some 21 interns. There were seven Jews in the top fifteen of our class (I know that because, if you were in the top of the class, you were admitted to Alpha Omega Alpha), but they didn't take one of us. So it's not my paranoia, it's my experience. There could have been something wrong with me that I didn't recognize, but not with all of us. So there we go, I interned at Mt. Sinai.

To look back in reflection on my childhood, there was a sense of cultivating intellectual values, certainly a deep personal conviction about the importance of social justice, both as a victim of anti-Semitism and watching people who were poor through no fault of their own, etc. During the years of the Depression, I was an ardent New Deal Democrat, and fifty, sixty years later I remain an ardent New Deal Democrat. The world has changed, but I have not. To finish that theme, anti-Semitism was a barrier to medical schools that began to crack not long after I entered. Two things happened, on one hand the sudden arrival of the NIH research funding and the need for medical schools to cultivate active research programs meant they had to pick the brightest and most able people. They could no longer choose faculty on the basis of how well their wives would fit into the country club. The competitive atmosphere made it possible for Jews, Catholics, and other undesirables to get on to the faculty. My own commitment to affirmative action for blacks is a residue of that experience. Jews are not the problem now, it's black and Hispanics. The remedies are different, culture is different, the world is different where affirmative action is concerned. So, I'd say all of that personal experience made me very sensitive to the issues of poverty in childhood. It was also the case that, when I was an adolescent, I had severe and unremitting acne. There were pimples all over my face. Single, multiple, nodular, cystic; it looked just terrible, and I became proud of being bright but ashamed of the way I looked. My social relations became an issue. Part of what lead me into being a psychiatrist is my familiarity with unhappiness, shyness, and all the rest.

Anyway, I got into medical school, graduated, took an internship, then I had to decide what I would like to do. I had thought for a while of a career in research, biochemistry, or physiology, but then realized that I was much more interested in interpersonal relations rather than a job in a laboratory. I did take a year off to do a fellowship in physiology, but then I went on to residency training and applied to a number of places. I got into a program on the outskirts of Baltimore, a private Quaker hospital named Sheppard Pratt. I wanted to train in child psychiatry and went to Hopkins with Leo Kanner and became his trainee, and ultimately his successor.

Now, some of my early research interests. When I became a psychiatrist, there wasn't anything you could do other than listen to people and try to be nice to them, supportive. There were no drugs. The only "medical" treatments were electric shock and insulin shock. Electric shock is a modern, useful treatment for depression. It helps patients recover. It's not anything that is congenial to me, but if I have a severe depression, I'd rather have shock than persistent depression. We also gave insulin shock thinking it worked, and there was never any convincing evidence that it did. The field became revolutionized shortly after I entered because of the chance discovery of a series of drugs. They were all chance, unpredictable observations. The investigators were smart enough to notice what happened, and it suddenly became possible to give medication which reversed a severe psychosis within hours or days. The field began to change rapidly, whereas research methods had been relatively

sloppy before. Most people don't remember that the first randomized control trial in medicine was a trial of streptomycin for the treatment of tuberculosis that was done by the Medical Research Council in the United Kingdom, published about 1947. That's just after I graduated from medical school. To the credit of psychiatry, when any drugs came along, RCTs were widely used in psychiatry. So it wasn't that I brought them in, although I did one of the first trials in children.

Cahan: RCTs meaning?

Eisenberg: Randomized Control Trials. I did one of the first such trials with children supported by NIMH. In those days, anti-anxiety agents were all the rage. In the first several trials we ran, we found the placebo was much better than either of the active drugs, with fewer side effects. The children felt well because most kids do get better, and it was in that context that we wanted to see the one drug that worked in clinical experience: Dexedrine. It was about the time that Ritalin came and was used with overactive children, so we did a series of trials on drugs which had been around for a long time. But ours were the first controlled trials and showed symptomatic results. My own discontent, in retrospect, with that work is that we followed a research tradition that continues to the present. The longer you do a clinical trial, the more defections you have in your patient population because they don't stick around long enough. So, if you can do a trial in a reasonably short period of time, you have a much neater, nicer trial. If what you are really interested in, or should be interested in, is the development of the child and his learning at school in six weeks, you don't learn a damn thing about whether he is progressing. We satisfied ourselves that, because they were quieter and had longer attention spans and our studies showed the attention span was longer, that meant they must be learning. Well, it ain't so. Good studies using stimulant drugs for appropriately long terms and designated numbers of children remain few and far between. Learning remains a problem; parent child conflict remains a problem. If you provide the counseling and if you take the burden off the parents of a grossly overactive child, it should facilitate progress. There are not very many studies or integrated treatments, and I can tell you individual instances where there are just absolutely overwhelming results. We had a mother bring in a child who was extremely overactive, and the mother looked psychotic. She was ill-dressed, her hair wasn't combed, she was down in the dumps. She couldn't get anything done. Happily, this kid took Ritalin and responded. When she came in a week later, she was a different person, and he was quiet, but usually that's not the story.

So my career began with a series of drugs trials. We did one study which we only in retrospect fully appreciated. And it never got the attention -- though it was published -- that it should have as a psychological rather than a pharmacological study. The purpose of the study was to use a new drug that somebody hadn't used in a training school for delinquent children. That's another easy, ideal way to do a study because you have a large population in one place at one time. You start with everyone on day one, you finish on whatever day you are going to finish on, and everybody is still there. Whereas in an outpatient study, this week you have two patients, next week you have three more, and it takes a long time, even if it is a short study. So here we had a captive population. We used a study design which had consisted of one cottage of children on the active drug, Dexedrine, one cottage of children on a placebo, and one cottage of children who got no drug. In our minds, the reason for the no drug cottage was to watch what we thought would be the relative stability of the behavior ratings of the cottage parents. We didn't pay much attention until we got an urgent call that there had been a massive elopement from the control cottage. Some 14 of the 30 kids made off from the premises. When we got out the behavior ratings, which we hadn't been looking at, they were getting worse week by week. In retrospect, we realized that the myth of the belief among the children in the training school was that the pill would make you better; as a result, you would get out earlier because most delinquents are detained on an indeterminate term; behavior in the institution has a good deal to do with release time. Both the kids getting the drug or the placebo figured they were getting the treatment that was going to get them out of the jug and these other kids were being denied what they thought was a great benefit. It's really ironic now that drugs are regarded as terrible things for kids. These kids *wanted* to be in the experiment because of what they believed, whether it was true or not. What we offered as a control condition actually had a major psychological effect because we weren't

thinking of the larger culture, the institution. Getting no drug uncovered a "nocebo" effect. I think that was one of the most important findings we had.

I got very much interested in reading disability and did a population-based study in which I got figures from the city of Baltimore on public school reading scores during the sixth grade. I got scores from the county of Baltimore for the same grades and scores from two private schools. The private school scores on the average were well above grade expectation based on national standards. The county schools were about at national standards because their economic situation was so much better than the inner city schools. The private schools, for one thing, excluded kids with problems by an entrance exam. They had smaller classes than the rest. That is sort of the point we used that study to make; namely, the impact of the social conditions on academic performance is one source of commitment to social change.

My teacher, Leo Kanner, was the man who discovered the disorder called Early Infantile Autism. When he retired about '61, I was chosen to succeed him, and I stayed at Hopkins until '67, when I was invited to come to Massachusetts General Hospital as head of the Psychiatry Department, and later became head of the Harvard Medical School Psychiatry Department. In 1980, the new Dean asked me to found the Department of Social Medicine, and I chaired it until I retired. That was a logical progression.

Now, to look at the field. When I came into it (I finished medical school in 1946), academic psychiatry to the extent that the system was growing was predominantly psychoanalytical. My teacher, Leo Kanner, was not an analyst, so I was free to become a critic of psychoanalysis, partly because of Kanner's influence, partly because psychoanalysis seemed absurd as part of the health treatment. Its stance on women was absurd. I remember being very much influenced by a little book that was called *Why War*. Albert Einstein, a socialist, asked the great figure in psychiatry (Sigmund Freud) the question "Why war?" Freud's response came back as, war is a response to deep, instinctual, hostile feelings. I rejected analysis as a class-biased and unscientific theory.

Cahan: Who actually authored the book *Why War*?

Eisenberg: I don't remember; Einstein had an essay on Freud. I think it's Freud. With my reasons for being against it and a mentor who himself was a critic, my career didn't suffer, and Hopkins was in general not captivated by psychoanalysis. I was recently asked, "Why was psychoanalysis so dominant?" a question I really hadn't asked myself at the time because it was like asking, "Why is there oxygen in the air?" That's the way it was. I came to realize that what psychoanalysis had contributed was at a time when there was no effective medical treatment for psychological disorders of any kind. Freud's method required the doctor to listen to patients, and people respond to being listened to. For a long time, psychiatrists were the only doctors who listened to patients. We're giving that up now for psychopharmacology.

Now the field was also characterized by the premise that everything had a psychogenic cause. Schizophrenia was caused by schizophrenogenic mothers. Kanner was unusual in suggesting that autistic children were born different and had an unspecified brain disorder. For many psychiatrists in my era, the brain was in the skull to keep the head from floating into outer space. There were neurological diseases, of course, but neither schizophrenia nor manic depressive disorder had any obvious brain correlations, so therefore we assumed they were all psychological. There was a good deal of resistance in both the adult and child psychiatry to the introduction of medications when they first came out. As I said, it wasn't because they were not rationally designed, but it was apparent that they worked. When I first came to Boston in 1967, I remember consultations with severely depressed people who were in psychotherapy with psychiatrists who refused to give the patients medications. They had to work their way out of the depression psychologically. Now there is current evidence that cognitive behavioral therapy works about as well as anti-depressant drugs for moderate degrees of depression, but that's not psychoanalysis, and it's not the standard mode of treatment. Refusing help to people who were suffering was -- just seemed ridiculous and ideological.

One of my first encounters on moving to Boston was with a woman who was clearly in need of psychotherapy. She had had a history of several previous depressive episodes, and the depression -- she consulted me about in the middle of seeing another psychiatrist for treatment -- resulted from an extramarital affair. She saw a man who was very attractive to her at a social gathering. She flirted with him, he seemed responsive, she wanted the relationship to go further, but he was married and wouldn't go to bed with her. She felt terribly depressed because she saw that as a reflection of her getting older and no longer being attractive. I told her anybody who regards the response of a man to her sexually as the essence of her being and gets depressed when he turns her down needs psychological help to think about her own self-esteem. Yet, here was a very bright woman who was being offered an important faculty position, and she felt too depressed to take it. I prescribed an anti-depressant drug. This time it worked: she took the job and succeeded as an academic, I don't know about further progress on her psychotherapy. The rest of her, that was left to another psychiatrist. I tell that story to reflect the fact that drugs that lift the depression don't deal with the rest of what's wrong in your life, and I do think that the psychological reasons were the cause of the depression, but that doesn't mean the depression won't respond in any amount to drugs. But it perhaps illustrates a time when the dichotomy between when the one espoused psychology or psychotherapy and when the other came from a more biological orientation. There are extremists at either, and you've got practitioners from one side or the other, because there were psychiatrists who used electric shock for everything. Now that drugs are around and available much more, psychiatrists use drugs for everything even when they are not indicated. And I guess there are still people who are so wedded to a psychoanalytic mind that they won't prescribe medication.

Now the battles in psychiatry are battles today that have a number of different dynamics per se. One of them is the terrible market for providing parenting care. The entrance of for-profit HMOs means that the mental health care, which was never readily available, is even more difficult to obtain. So practitioners are pushed to get patients symptomatically over the hump as rapidly as possible, and drugs are seen as a less expensive way to go, and psychiatrists often become collaborators. The HMOs use cheaper therapists, i.e. social workers or counselors, and the psychiatrist makes the diagnosis and writes the prescriptions. So that's one thrust. A second thrust is that, even before for-profit medicine came in, there was a growing fight between psychiatrists and other mental health providers, psychologists and social workers, over psychotherapy. I don't know of any evidence that psychiatrists do better psychotherapy than social workers or psychologists, so it becomes harder to claim superior skills in that area. What we can do by law is that we are licensed to write prescriptions. We are also supposedly medically competent -- some of us are and some of us aren't -- and so there has been a major thrust, I think, toward the emphasis on the biological component of medicine that came out of the marketplace. If we are providers that can take care of the brain as well as the mind then we have an edge on everybody else.

Cahan: And now you are talking about really quite contemporary--

Eisenberg: Oh, absolutely, absolutely. The thrust is so forceful that, in some psychiatric training programs, there is less and less time that is devoted to teaching the residents to do psychotherapy and more and more emphasis on diagnosis and prescribing. Pharmaceutical diagnosis has become the new real. They carry it out to so many places and they have so many different terms as if they are real conditions when there is still a good deal of remaining uncertainty.

Cahan: It's almost like they are surrounding--

Eisenberg: Yeah, it really is like -- well, because there's been enormous progress in the neurosciences, there's another reinforcement for the biological argument. I think that neuroscience makes it all very exciting. It's really terrific when investigators at Yale can give men and women different kinds of tasks and show that there is a sexual asymmetry in the way that the brain responds possibly related to the fact that girls talk earlier and better, whatever the hell it's all about, it's fun. I don't know that it tells you anything that you'd want to use in social policy but it's still interesting. It becomes more than fun when you can identify a defective gene correlated with a disease. Almost all these things that

have been so far discovered relate to relatively rare diseases. I have no doubt about the fact that, as progress continues on the genome project, we will begin to identifying genes which increase the risk for A, B or C. Note that I didn't say "cause A, B, or C" but "increase the risk". Somebody just issued a claim, I haven't seen it yet, this guy Polman does all his work on genetic theories of intelligence, claims to have found, by studying a group of average IQ children and a group of relatively superior IQ children, a difference in heritability between the two groups. He was smart enough to say that maybe this contributes 4% of the variance, which ain't much, but. Fine if you find genes that influence 4%, 2%, and 8%, 3% of intelligence, but it won't explain intelligence. It won't account for it all, but you begin to get something, an idea that may indeed tell you that the kid who has this kind of genetic composition, whether he learns better and more rapidly or not, so I don't see it as a threat. What I see as the threat is the misinterpretation of all of this as genetic determinism. Freud once said that "biology is destiny" (in relation to sex differences). But some folks resurrected dead and dormant data, and they got marvelous publicity for it because they fit a right wing message with Freud's data. So that's the risk; it's an ideological factor, not a fight about data. There have been a number of claims that weren't ideological; they just turned out to be lousy. The Amish are particularly interesting because they intermarry, and they've kept their bibles full of genealogies that go back for four or five generations. And some investigators found an Amish kindred with a lot of bipolar disorder, and they seemed to think it was associated with a gene with one particular chromosome, and published the finding. Fortunately they used a large cohort. The difference had just about reached the statistical level of significance and, when the next two cases were added to the series, the effect disappeared. That's been the story again, but it's not evil. You know there is something that's going on. Depression does run in families, there are more schizophrenic persons in some families than in others. Surely there is something to it, but until we find the something, the predisposing factors, until we find the genetic mechanism, until we find a way to intervene, this becomes more of a theological battle then a scientific battle. So that's sort of what's going on.

Cahan: Well, you have skipped ahead and have been quite conclusive. Is there anything more about your general intellectual history or political and social events that may have influenced your research and writing?

Eisenberg: Well, let me tell you about some aspects of child development that are closer to the topic, that is, to the historical questions. Child psychiatry in the United States emerged out of a number of streams. One of these streams was the movement of concern about juvenile delinquents stemming out of the social work that started at Hull House in Chicago with Jane Addams.

Cahan: I think that it won't be out for about a year.

Eisenberg: There was also some connection with pediatrics because there were pediatricians who paid attention to what we now call maternal deprivation before it was given a name and recognized as such. And it really grew up away from and outside medical schools in free standing community clinics like with the Judge Baker. Judge Baker's attachment to Harvard Medical School was to come later in its history. It went through all sorts of interesting permutations and combinations. I think my history is correct that, during the time George Gardner was the head of the Judge Baker, which would have been maybe between 1940 and 1960 roughly, psychologists were allowed to test the patients but they couldn't do the treatment.

Cahan: Yes, that's what goes farther back.

Eisenberg: Oh no, I know it goes -– I wanted to begin with that, sorry. I was just beginning with those years. But the psychologists did Rorschachs, and they did IQ tests. And there was a big fight inside the American Orthopsychiatric Association -- which was more of a multidisciplinary practice than a society -- about who had the right to do the treatment, and the psychologists gradually emancipated themselves from psychiatric hegemony. There was a long period of time when my boss, Leo Kanner, referred to the "Holy Trinity reign;" you couldn't have treatment for a child unless the trinity of a

social worker, a psychologist, and a psychiatrist were involved. No single person could do the treatment.

Cahan: That was really the ideology of the clinics.

Eisenberg: Right, and the community clinics joined and became part of something called the American Association of Psychiatric Clinics for Children, the AAPCC, which were mostly formed from free-standing clinics, although some had university connections. They were the only group setting standards. The psychiatrists in the 1960s got together and formed the American Academy of Child Psychiatry and began putting together rules that you couldn't be officially trained unless the clinic had a required connection with a medical school. And then all clinics scrambled to get some medical school connection. But what was at issue was politics and money and control and had little to do with intellectual issues and the Academy of Child Psychiatry. I remember that one of Leo Kanner's graduate trainees, much earlier than I, applied for membership in the American Academy after it got started, and he was told that he would have to have some proof of his training cases to qualify because he had been trained at Kanner's service, which was not an AAPCC member. Kanner wrote a blistering letter to the woman who was then President of AAPC (Cheastle, a Hopkins graduate), and the man finally was admitted. But then the psychiatrists petitioned this American Board of Psychiatry and Neurology to make child psychiatry a subspecialty. And I can tell you that I was not quite old enough to be grandfathered in. When they start a certification board like that, people who had the usually proper credentials that were beyond a certain point in age are "grandfathered". But I fell on the wrong side of it, which absolutely infuriated me because I thought I'd taken my last examination, and I had to be examined once again. I really don't know what's happened to AAPCC now that there is the American Academy.

The American Academy of Child Psychiatry in its early days was all psychoanalytically-oriented; it wasn't all medical. It would have meetings in which people would communicate with each other, sharing their speculations on the mindset of a child, discussing development without bothering with data. It's now a much more scientific group than it was, with the use of statistics. You have no idea how far that was from today; it's so hard for me to remember what it was before.

In the early 1960s, I was invited to Washington University in St. Louis to give a talk about one of our drug studies. That university occupied a special role in American adult psychiatry as one of the first places where research was a major item on the agenda and where attention was paid to diagnosis. All of the various editions of the DSM which have come up owe their origin to Washington University. But I remember going there and being hailed like a hero by the faculty there who felt alone and appreciated a psychiatrist who would count. Let me tell you, counting was very elementary and primitive. It wasn't that I was doing higher mathematics or fancy biostatistics, I used a randomized controlled trial and was simply counting the number of patients who entered, left, who got better, who didn't, having some simple accounting for all of it, because that was so foreign to the tradition of case studies and argument based on theoretical principles. Now, we must be ready to go on to something else.

Cahan: Sure, you've spoken of your primary interest in child development, continuities in your work that are most significant, shifts that may have occurred. You've spoken of the reactions to contemporary trends in your book.

Eisenberg: Well, there certainly are theoretical continuities at a certain level, if you like, a fundamental continuum to, if you want to call them, ideological positions, such as not accepting genetic determinism, being convinced of a major role for environmental input in determining developmental outcomes, and a commitment to assisting the socially disadvantaged. They were all there from the beginning, and they became more sophisticated but were essentially unchanged in the basic sense of being a commitment. My career shifted after I left child psychiatry at Hopkins to move to general psychiatry at MGH. I stopped doing any research personally. I did much more writing on the more general themes or reviewing their literature and the like. And then, when I moved on to the Department of Social Medicine, I was at a level even further removed from empirical psychiatry. I

think I've stood for something in psychiatry that I hope has made a difference, although Lord knows how you'd establish that. I wrote a paper which got a fair amount of attention in 1986 that appeared in the *British Journal of Psychiatry*. The title tells its story. It was called "Mindlessness and Brainlessness in Psychiatry," It was taking a stand against the false dichotomies, and almost ten years later in 1995 or '96 a paper in the *American Journal of Psychiatry* with a title, "The Social Construction of the Human Brain," using "construction" both metaphorically and literally. The evidence, one of the most exciting things about the paper is the evidence that the brain is socially constructed. You know, I cited a delightful study showing that the finger representation for the left hand of a professional violinist is much larger on the right cortex than in you or me unless you have violinist's hands.

Cahan: Or a cello.

Eisenberg: All right. Well, you've got to do a lot of playing. Even more striking, somebody actually looked at a couple of, I didn't know, they were Braille readers, who learned to read with both hands. I don't know what the difference between one handed and two-handed Braille readers is, but they looked at the brain representation of the newly-learned technique and showed again a remarkably large representation on the tips of the two fingers in Braille readers. So, it changes even with experiences, and researchers have shown acute changes in women who've undergone a mastectomy and volunteer for the experiment. When you stimulate the skin on what would have been that area, I mean the parts around it have spread into or have taken over what use to be the old representation. Or also, if somebody has an amputation. So cerebral representation is a dynamic changing phenomenon, constantly subject to input. And you know, all the animal studies are really quite remarkable. Owls have a remarkable capacity to localize sound in space, and they don't see that well, although they do better at night when they hear sound, attack, and bang, they get the mouse or whatever they're after. Some investigator took young owls who had already learned to correlate sound with sight, and put prisms over their eyes. Owls can't turn their eyes; their heads turn. The prisms over their eyes shifted what they see 30 degrees from true coordinates. In no time at all, the young owl was able to relearn the coordinates. When the prisms are taken off, they readjust to the new situation. If the prisms are put on when the owls are mature, those who had been exposed when they were young can learn how to do it. If they are put on an old owl or a mature owl, they can't master the task. Furthermore, somebody just reported showing that the length of the time, in months, I guess, you measure in an owl's age during which this capacity is retained depends on the richness of the visual environment. If they are raised in a bare cage with nothing in it, they lose this capacity much sooner.

Cahan: Oh, gosh.

Eisenberg: Ain't nature grand; isn't that wonderful? So, if all of those concepts have remained themes that I reiterate, which I believe need to be said, especially in a climate that is driven in the other direction for economic and pragmatic reasons. But my work got further and further away from that. I now do much more reading in general medical scientific journals than I do in psychiatric journals, so it's shifted away from the earlier emphasis, and I think the ideology has changed, too.

Cahan: Is there any particular, I guess there's probably--

Eisenberg: Well, you mean you wanted to see some citations? I could give you, I can send you some.

Cahan: Okay, sure.

Eisenberg: I can give you a half dozen articles.

Cahan: Reflect on your experiences with the research funding apparatus and any participation in shaping research funding policy, implementation, or securing support for your own work.

Eisenberg: Well, to put it in a rather cavalier way, when I began to research an applicant with an IQ of at least 70 could hardly fail to get a federal grant.

Cahan: A lot has changed.

Eisenberg: Boy, has that changed! You know, there are some statistics which show that, though I exaggerate, it isn't such a large exaggeration; that is, the success rate was very much higher in earlier years. It's painful for me to see very competent young people with good ideas unable to get funding. Now, part of the reason is that so many more people applying for grants, and research is much more expensive, but the limited growth in the availability of federal funding is a basic problem. So that's observation number one. Observation number two, which is not on the question, was, is a part of my personal history. By the time I would have been ready to serve on an NIMH study section, every federal agency had an FBI political review for clearance before anybody could get an appointment to a study section. It's incredible to think that there was political clearance for psychology, mental health, or whatever, as well as for biochemistry.

Cahan: I think--

Eisenberg: Right. Well, I'm proud to say I was screened out because I was involved in left-wing political activities and pointed in my criticism of the political right.

Cahan: When would that have been?

Eisenberg: This would have been in the early 60s. Let me tell you something interesting about it. How did I learn that I had been screened out? Dale Prugh, who was a child psychiatrist and a good friend, had served on the study section. When he completed his term on the study section, he was asked to nominate people who might be good candidates for the study section. He suggested my name among others. I didn't know anything about it; there was no need to discuss it. When I saw him several years later, he asked how I liked serving on the study section. I asked, what study section? And that's when we both discovered that I had been screened out. That stimulated him and a number of others to begin an initiative that the American Orthopsychiatric Association -- I think is how it got started -- protesting political clearance for study sections. And I think, ultimately the National Academy joined, and a number of other groups joined, and then the process was nominally ended it. There was a period when they decided it wasn't necessary, but it actually kept going on for long after it was nominally halted. I know it was ended because, when Julie Richmond became Secretary of HHS, he put my name in for an advisory committee to Don Frederickson, who was then the head of NIH, and I was appointed to the Committee. So everything about me was true, although it was so much older than it was then.

Cahan: That period of censorship is so important to remember.

Eisenberg: Oh, absolutely! It was really a dreadful time for civil rights. It didn't -- I mean, I was saved an awful lot of time. People who worked on the study sections put in a lot of effort. I didn't learn as much as I would have because, I think, I did miss a very valuable intellectual experience, if you are contentious and you read and so on, but I never served on a study section. There was no Robert Wood Johnson Foundation. There were fewer foundations and fewer very wealthy foundations. Foundations certainly have become much more a part of the picture ever since. By the time I served on the Advisory Committee to the Director of NIH, the big issues were the fights about overhead and the fights between scientists and universities about the allocation of grant funds. I could give you a whole long song and dance, not so much out of my personal experience, but in the 50s as the NIH budget was going up, Representative Fountain, a Congressman from North Carolina, imposed on the NIH the requirement that people getting government grants had to submit time and effort reports to justify the amount of research time relative to the proportion of salary paid from the grant. I felt we all should have told Fountain we wouldn't do business with him. It's not that I wanted universities to overcharge because the government, I know, wanted the research done. If we had rejected the conditions of the work, they would have been changed. Instead, universities kowtowed, and we got into increasing absurdity.

I don't work a 40 hour week, I'm sure you don't either, so how do you figure your hours? Does the government, if it's paying 50% percent for 40 hours hold 100% of my time, including evening hours, weekends, holidays, and the rest? What about when I think at home, what about, etc, etc. I don't want to give you the song and dance; you've already rehearsed this to yourself.

Cahan: It's a little bit like the Department of Labor.

Eisenberg: Yes, right, it's sort of like the administration of a college. But, my God, they went through that and it caused all sorts of analysts trouble with accounting. And with that kind of stupidity one understood that, when government supported a researcher, it really expect that the researcher would do something else, like teach and see patients. But then some NIH auditors would come around and ask, "How come you are taking time away from research?" Now it's a problem because, if you don't work 200 hours in your research, you won't have enough to give the NIH to get your next grant, but that's a different problem. Well, so that was a major issue, so it was the business of kowtowing again and again and, of course, one of the more fundamental things that I think concerns all universities, but surely in medical schools, too. And that's my experience.

Cahan: Thinking about the problems about psychiatry funding strategies--

Eisenberg: Well, the academic politics of child psychiatry are partly a fight about the relationship between the child department and general psychiatry. Child psychiatrists always felt they never got a fair shake. Maybe yes, maybe no. There was a constant press for independence. Child psychiatry often has problems finding its relationship with pediatrics. Pediatrics undervalues mental health, although it does so less now than it did 40 years ago. I don't really think that it was so much the matter of the research community. The real problem was, you realize, that medical schools taught medical students by "stealing" money so to speak, by using time, part of which was paid for by research grants and part by patient care. We never stood up and said, teaching costs money, it's important to the future of the country. We ought to be paid directly instead of having to use the Robin Hood principle. So it was both out of patient care money, it was an indirect subsidy. I mean, the patients paid for it because the doctors who took that money were expected to give some time to teaching, but that meant that they put higher prices on what they did for the patients. It should have been direct and unequivocal, and then we wouldn't be in the mess we are in now, but now we're in the mess worse because budgets are being squeezed from all sides. What's going to pay for teaching?

Cahan: That actually brings me to the next question on the agenda here, which is, describe an experience as a teacher of child development research, or a trainer of research workers, or -- you've already spoken a little bit about the tension between teachers and research.

Eisenberg: But that's another whole story.

Cahan: Right.

Eisenberg: I don't have a lot to contribute to that topic because it really is phrased with a college graduate school environment because medical schools generally don't have required courses.

Cahan: Straight child development.

Eisenberg: It's always a fight to get any time you need to teach straight child development, so the complaint from child psychiatrists is not legitimately that, "My God, you want me to teach too much," but "Please give me a chance to talk to the students," so it's very different. In medical environments there is an interesting thing: there is much more training of fellows, both clinical and research fellows, than there is of medical students and undergraduates. Very few child psychiatrists see undergraduate medical students, though maybe on some campuses they are common and maybe some people volunteer to take--

Cahan: Have you had any experiences that you can describe -- and again, this question comes for mainstream child development folks -- experiences in so-called applied child development strategies?

Eisenberg: Well, I guess, when the war on poverty started and project Head Start got under way, there was no time for planning to get the thing off and running. And a real call was issued to academics who cared to figure out a way to do some research on outcomes that would support the burgeoning program. A psychologist who was working in our department with me, Keith Conners -- who has done a lot of work on ADHD since -- and I got medical students and psychology students, and we did a study on the first Head Start program during that summer in Baltimore. We gave the Peabody Picture Vocabulary Test to the children in the program before and after the summer.

Cahan: That's all you had!

Eisenberg: That's right, and we had a nominally matched group. Now, we knew very well that the mothers who had been active enough to get their kids in kindergarten were already different than the ones who missed this opportunity and let this thing float by. But we showed a big difference on a test we chose that would be sensitive to differences. And I remember discussions with Ed Ziegler, he didn't believe we'd proved anything; he knew we hadn't cheated, but he said those differences don't matter. So we threw ourselves into that particular find.

Other people continue to be alive, you know. There were people who managed to get research started and done around Three Mile Island. That was really something. Remember when the emission of radioactive materials evoked fear? And there were people who got themselves going when there was a big flood in West Virginia. I don't remember the name of it, but Kai Erikson at Yale did that. And, of course, people would try to study Bosnia and Africa. But that was our one big foray, and we talked about it all over the place.

Cahan: So that was your involvement with Head Start at the time?

Eisenberg: Yeah, right. And most of the rest of the things I have done have been testifying letters to the editor, op-ed pieces, and the like, supporting things, but that was the only applied piece of research, was that one.

Cahan: How about your experiences with SRCD?

Eisenberg: Well, as to SRCD, I don't remember who invited me to join, but it sounded like a great thing to do. I think, over the many years I've been a member, I made great use of the journal.

Cahan: Do you remember about when you joined?

Eisenberg: Late 50s, it's a long time, it was before Head Start. I guess SRCD had as part of its heritage something I never much liked, that is a tradition of academic work using faculty children as the subjects. And lots of studies that were put together Ph.D. thesis, but I think didn't make much difference to the world; it's certainly got much better than that.

Cahan: In terms of relevance to the Committee on Social Policy and such--

Eisenberg: Oh, yes absolutely, and all sorts of good people have been Presidents and leaders of SRCD. I think the last SRCD meeting -- no, wait, I went to one not long ago when they gave an award to Julius Richmond, and I was a part of that, but I went to another meeting on development where Harriet Rheingold spoke, and where one of my long time intellectual heroes whom I met at a meeting once, whom I've seen only rarely, passed on. You know, a woman named Meredith West who is a comparative psychologist and who studies the development of cowbird song, just marvelous studies of

bird songs. About 1990 she wrote a piece in the *American Scientist* called "Mozart's Starling", which I commend to you to read just for pleasure.

Cahan: Did she write a piece in *Science* as well, *Science* magazine?

Eisenberg: No, she has had pieces in -- I don't remember. The *Science* one I think I would remember. But in this one, Mozart recorded in his diary having been in a pet store where he bought a starling who learned to sing eight bars from one of his piano concertos, and then Meredith uses that to discuss starlings who, since ancient times, have been known to mimic sounds and speech; it's just a marvelous story. Some people put netting around open areas of the house so the birds fly around. The birds won't learn to speak unless you, so to speak, address them; you have to get them involved with you. They don't learn to speak just because you feed them, I mean, with food in there. It's not a Skinnerian affair. It's hard to know what makes them do it, and it's, in fact, a very intriguing thing because birds like that will learn to imitate automobile gears, radios, and what have you. They pick up phrases and they will take up melodies. They do odd things with them. Anyway, it's just a charming piece. Her study that I think is really terrific work, with her husband, Andrew King, is on an ugly bird which lays its eggs in the nest of other birds, which may be tricked into feeding it.

Cahan: Risky and intrusive.

Eisenberg: Oh, but the other birds don't know the difference, and you get a free ride, you get a free ride. The problem is, how does the bird learn his own song; it's not much of a song, but it is distinct. If you rear the cowbirds in isolation, they don't do very well. If you put the cowbird with an adult uttering something it can hear, the cowbird will learn to imitate one of his own. But she did the marvelous experiment of rearing young male cowbirds in a cage with several females; females don't sing, but they teach the males how to sing. Now, how do they do that? In isolation, the male cowbird makes noises, and those noises come out as a very crude approximation of a mature song. When the resemblance is great enough, the female flicks her wings and signals excitement.

Cahan: So, it is Skinnerian!

Eisenberg: To get the female to flick her wings, the males learn how to sing. It's such a clever innovation. I mean, Meredith West really is a developmentalist. She and her husband, Andrew King, are developmentalists in the very best sense of the word. They ask what is the process and what are the stimuli; they just don't accept the word innate, which lots of people use to get the problem out of the way and end up knowing nothing. What are the stimuli that are effective? What is the process? So Harriett Rheingold talked about the woman at Cornell who did the visual studies, Gibson. It was Gibson, Harriet Rheingold, it was Meredith West, and somebody else was involved in it, too. So, I've been to relatively few meetings, and that's never been for me a central point. I don't know why. I mean, Julius Richmond was much more active in SRCD, but then, it's true that Julie and some of his people have a long history of doing research. He was very close to Betty Caldwell who did the, monomatric-multimatric family research. He had a young colleague who died prematurely named Earl Stein or Shriver, something like that, who studied autonomic developmental variability in the newborn, which was reported at SRCD, but I like what I've seen. I read their publications, but most of my stuff has been, either when I was in psychiatry or active in psychiatry, it was either in the *American Journal of Psychiatry* or the *American Journal of Orthopsychiatry*. Do you know that group at all?

Cahan: Not well.

Eisenberg: In two sentences, Ortho was founded in the 1920s by a group of people who were working with delinquency.

Cahan: Right, out of child guidance.

Eisenberg: Yeah, out of the child guidance movement, and it was, Ortho meant straight, so it was straight psychiatry, like orthopedics. And interestingly enough, Karl Menninger was one of the founders of the group in the 1920s. They had a number of people who became -- and Lawrence Frank, a leader in SRCD. Frank is a name to be reckoned with in Ortho. Ortho has fallen on hard times. What made it distinct, by the way, was that it included psychologists, psychiatrists, social workers, and some teachers as well as others, so it was multidisciplinary; it was the only one, whereas SRCD is mostly psychologists.

Cahan: It's very disciplinary--

Eisenberg: Yeah, and Ortho -– I don't know why Ortho -– part of the reason was, there was a woman who was the long-serving Executive Secretary, who, since the beginning of time had run the Association with a tight hand. And despite the efforts of several Presidents, she would stand by and hold down the expenses, and it worked well for a long time. And then they began to get liberal and to expand the budget after she retired and died. And then Ortho went into financial difficulties. I think it also suffered because social workers were very heavily dependent on institutional support to attend meetings, since the meetings cost money, and they couldn't go anymore when the tight budget era came and the agencies needed to cut back their budgets.

Cahan: Right. Those kinds of multidisciplinary efforts suffered terribly during that time.

Eisenberg: It was doing fine until this period, and I think they just couldn't get money to come. It appealed only to sort of left-wing psychiatrists -- went to the APA to talk to your own.

Cahan: Mingle with the underlings!

Eisenberg: That's right, I didn't go the medical school to have to talk to social workers! So, any other questions on your list.

Cahan: You've spoken quite a bit and quite well about various aspects of the history of child psychiatry. What are your hopes and fears for the field?

Eisenberg: Well, I think my fears for child psychiatry are the same as my fears for adult psychiatry: that there is no reason to fear science, but there is a reason to fear scientism. And scientism is alive and well. There is no reason to fear the human genome when understood and dealt with properly, but, boy, it can so easily be made into predeterminism. Actually uncovering the genetic contribution variance ought to make identifying the environmental contribution easier; that is, if schizophrenia is a disorder which is, as you know, a genetic predisposition for it takes some configuration of the outside to bring it on. It's a rare disease; 1% is the lifetime population risk. So if we studied 100 people and 99 people who don't show the symptoms, what happened? But only one of the 100 persons does express schizophrenia, and you are trying to figure out what makes that one hit. Now, if you have a way -– I understand the problems of identification and expectation, but if you could, without doing all that, find people who were vulnerable and then look at the environmental differences between the vulnerables who succumb and those who don't, what a psychological study that would be. So I think we have everything to gain and nothing to lose at an intellectual level; but, boy, as to the public understanding, we have lots to lose. I mean, the stuff is all over the country, Merrian Hertz are not the ones responsible for the valid initiative in California, who then did affirmative action, but they sure contributed something to make the intellectual climate which makes that possible. And it's that kind of thing that I think we are increasingly having to deal with; it's all over the place. There was a synagogue blown up in Moscow two days ago, three days ago; the head of the Jewish community, whoever he is, protested and listed other examples of anti-Semitism, and they quoted some malcreant who is now in the Russian government as saying, "No, no it couldn't have been in the government; it must have been in one of the other parties," and as saying, "If the Jews weren't so prominent in the present government, this wouldn't happen." Well, it's that kind of--

Cahan: Sounds familiar.

Eisenberg: Yeah, yeah, so it's around. I mean it's not so visible here openly; you know it's under rocks, they don't need much stirring up. And the genetic belief, the very people who think Jews are better because they are smarter are ready to say, but they are ready to cheat you. I have a colleague who had a -- I guess still does -- a large pediatric cardiology practice in a Los Angeles not far from us, and even after they set up the Drew Medical School, black patients wouldn't go to those doctors because they were black and therefore couldn't be good because they would come to him because he was a Jew doctor. So there is still a very considerable danger that the misuse of a kind of genetic term is a false biologist because it's all in the genes, so I spend my time writing that it ain't.

Cahan: When in fact one truly understands nature and the nature, as you said, it makes the -- that much more compelling.

Eisenberg: I mean the story is just really annoying. Look, polyphenism is absolutely a genetic condition, absolutely! You can't get it if you don't inherit the gene. Can't have the gene and take phenylalanine in your diet, and you're almost certain to suffer microcephaly, severe mental deficiency, seizures, and perhaps even psychotic behavior. So, by discovering the genetic condition or its predisposition, you have the mechanism of that genetic condition. We now have an environmental treatment that reverses it all together. I could go down a long list of conditions for which that's also true.

Cahan: You're not supposed to teach that in a child development class.

Eisenberg: It's a message you can't repeat. You get all these fire storms that go off in the wrong direction, but boy, there are people, and there are smart people. I mean, that's the worst of it, isn't just dopes who are saying this stuff. Noam Chomsky is a left-winger and a socialist and so on. Noam Chomsky uses phenomenology to argue that language is innate; well, what I think what he means is it takes a human brain to master language, and there are syntactic structure and logical relationships, and indeed, it does take a human brain to master language as we understand it. You can't teach a chimpanzee to speak French, but you don't speak a word of any language if you don't hear it spoken, if you don't interact with other people who speak that language, and so on. And there are just so many studies showing that the richness and the character of the language you speak is the function of the language to which you are exposed to and the relationships in which that language is used. So, that fight goes on endlessly. I don't know what made Chomsky choose that particular way to express his ideas. You know, I suspect that it was a fight against naive Skinnerism.

Cahan: Right. And it was a direct assault historically.

Eisenberg: Have you ever read that marvelous paper by Chomsky taking apart Skinner's theory of language? It goes back many years.

Cahan: I probably have read that.

Eisenberg: I was a marvelous piece, and Skinner--

Cahan: You mean Chomsky's review of verbal behavior?

Eisenberg: Yes.

Cahan: That's the one; yeah, sure. That's just classic.

Eisenberg: Skinner was unbelievable. I once heard Skinner give a talk. I mean, I once came up to Skinner after his talk and I said, "You know, behavior is a reinforcement. If something doesn't work in respect to the learning of birds in the first place, you get some birds who sing without ever hearing it,

and then many birds who have to hear their song to hear it." He didn't blink an eyebrow; he said, "Well, they are matching to an innate template. Well, then it's not behavioristic anymore; then you are back to psychoanalysis."

Cahan: Skinner said that?

Eisenberg: Yes, yes, not publicly, but in a private conversation. I invited him to give a talk at MGH when I was there because I found him amusing. I want to tell you one nice thing about him. Skinner said you could get everybody to like Beethoven; just put him in charge of the music, and they would all learn to love it. And the idea of somebody being in charge and designing it. The best thing I heard about Skinner is when I heard Skinner say, he was interviewed on NPR as he was dying from leukemia. There was a marvelously reflective interview, the point he was making was leukemia is really a nice way to die because there is no pain, you feel somewhat tired. It takes a while, you have time to talk to your children, you have time to ready your affairs. It was just so marvelous, it was absolute courage in the face of this kind of thing. My friend, Richard Herrnstein, who did a lot of work in pigeons and developed Skinnerian theory, actually did a study published in *Science* in which he showed that you can teach pigeons to peck when they saw, on a moving screen in front of them, pictures of people in a landscape. It was a marvelous thing to have in Vietnam when they were trying to find the Viet Cong. So he was a fine fellow all the way around. One of the things Bernie did, or wanted to do, but the Dean wouldn't do it during the days of student unrest at the university. Edwin Land, the owner of Polaroid, was going to give a talk on his trichromatic theory of color, and there were students who were absolutely disruptive and made it impossible because Polaroid was selling these things to photograph identity papers in South Africa. Bernie wanted Bob Ebert, our Dean, to invite Edward Land to speak at Harvard and to alert the police, and the Dean said no. The thing that really soured my friends was that Bernie wrote a scurrilous attack on Steve Gould; it was scurrilous.

Cahan: So--

Eisenberg: --no, no, no. It's too bad. I don't know. He sort of threw away a good reputation, and then he wrote that terrible piece that appeared in the *New England Journal of Medicine* saying this medical school was letting a lot of poorly-prepared blacks come through and use the opportunity. But I think I've said as much as I should on this topic.

Cahan: One more. Turning to the personal for a moment. Is there something you can tell us about your personal interests, your family, and your person background -- your work?

Eisenberg: Well, as I told you, my father was an immigrant and so was my mother. I was one of the first people in my generation of the family to go to college. I have one cousin who did. It made going to college, I mean, I realized what a privilege it was coming from an extended family that hadn't. My father wanted me to be a doctor, so did my mother, but I never wanted to be anything different, least so far as I can remember. So their wanting and my wanting appeared to be the same thing, without any ambivalence that I ever found, led to a wonderful career in which I got enormous gratification, and they were so pleased at how well I did, and I was so pleased by being able to please them without having to do anything I didn't want to do myself.

Cahan: It's nice when it works out.

Eisenberg: Yes, it's nice when it works out that way. I can't imagine a better career. I am a member of the last cohort in academics who necessarily had to retire, so I'm no longer on the payroll. But, because my successor allowed me the use of my office, I'm able to come in everyday around 7:30, just as I always did, and leave around 6:00 just as I always did because it's fun. You don't carry bricks, you don't sweat; it's a marvelous, marvelous job, and I can't imagine anything I would rather have done. And, if I'm lucky enough, I'll drop dead one day in the office or on the way to or from it. I don't want to sit back and quit. I don't want to get in the road of my successors, and I think that, if space is a problem, I would. Since I don't draw a salary, I'm not blocking an opportunity for a younger person. I

think, personally, the abolition of the mandatory retirement age for academics is an error. Extensions should be possible for exceptional people. Research-intensive universities are finding that professors staying is an increasing problem because of the space crunch. I think it's here to stay.

My first marriage ended in divorce after a lot of unhappiness. It gave me two children, one of whom is a physician working in Charlestown who also gave me grandchildren, and a daughter who teaches in Philadelphia. My new wife became a widow when her husband, whom I knew professionally, died. We married. This has been a happy marriage. She had two children by her former marriage, one whom is a psychiatrist in Rochester, New York, and the other the Deputy Director of the National Human Genome Research Project in Bethesda, Maryland.