

Lee Nelken Robins

- Born 8/29/1922 in New Orleans, Louisiana; died 9/25/2009
- B.A. (1942) Radcliffe College, M.A. (1943) Radcliffe College, Ph.D. (1951) Harvard University/Radcliffe College

Major Employment

- University Professor of Social Science, Washington University: 1991-2001
- Department of Sociology, Washington University
 - Professor: 1969-1991
 - Adjunct Associate Professor of Sociology: 1963-1969
 - Lecturer: 1957-1963
- Department of Psychiatry, Washington University School of Medicine
 - Director, Program in Psychiatric Epidemiology: 1987-1997
 - Professor of Social Science in Psychiatry: 1991-2000
 - Professor of Sociology in Psychiatry: 1968-1991



SRCD Affiliation

- Distinguished Scientific Contribution Award Recipient (2003)

SRCD ORAL HISTORY INTERVIEW

Lee Robins

Interviewed by John Constantino
At Washington University in St. Louis
February 6, 2006

Constantino: This is an interview for the Oral History Project for the Society for Research in Child Development. My name is John Constantino and I am interviewing Dr. Lee Robins. This is taking place at Washington University on February the 6th of 2006, and we will begin with a sort of general background history on Dr. Robins, so Lee would you describe your family background along with any childhood or adolescent experiences that might be of interest.

Robins: Sure, well, I guess the most dramatic thing that happened though I didn't know it at the time was that my father died when I was four. He was a urologist and died of heart disease as his ancestors had before him. He had been very successful and taught at Tulane University School of Medicine. He was in private practice as well. He thought he was leaving my mother very well taken care of because he had bought a lot of property, which is supposed to be the safest thing to do. But then came the Depression just after he died, and she had a very difficult time managing the property and trying to keep it rented. She did very well and we lived in a very nice house, but she had a very wealthy aunt and felt she was poor compared with the aunt. One of the things she had to manage was a small hotel, and she couldn't find someone she thought was adequate to run it. She finally decided that we would move there, and she would manage it herself. And so at 14 we did that.

Constantino: Lee where was the hotel?

Robins: It was in New Orleans on Napoleon Avenue.

Constantino: Okay.

Robins: I attended public school early in my life, as did my siblings. I was the last of four and the only one born at home because my mother thought she knew how to do it by then. The other three were sent to Newman, which was a very good private school, when they were ready for high school or close

to it. But in fourth grade I was telling someone goodbye who was getting on to a streetcar near the house and a car hit me. I was in the hospital only briefly, but was dizzy for quite a while, and so out of school for about a month. My mother decided at that point that I might as well make the change then rather than waiting. So I went to the school from fourth grade on. It was a fine school and I got lots of training in writing which was very nice. I can't tell you much about my interests in high school except that I did take both trigonometry and mechanical drawing, which were primarily boys' courses, I'm not sure it was because I was interested in science or liked boys -- it was one or the other. The only thing I missed there which I really have regretted is that I never took physics, neither there nor later, so I don't understand very well how the world works. I was very young for my class because in the public school I went to we had two half grades in each room, so they could take children in for the spring semester if they wanted to. I was born in the summer so I was in the usual pattern, but entering first grade in the fall. So when I started school there was also the older first grade in the same room. I listened to what they were learning and so I skipped half of first grade. And then in third grade I skipped again, so I was a year younger than the rest of the class. And in those days, and I think it's changed but I'm not sure, southern schools only had 11 grades, not 12. So I graduated very young, at 15, just before my 16th birthday.

When I graduated I got a scholarship to Newcomb, which was the female part of Tulane. I went there for two years, because the family was worried about money and it was hard to give that up, but I wanted to go elsewhere and I asked a friend of the family where the best place was to go, and she had been to Radcliffe so of course she told me that was the best place to go.

Constantino: Right.

Robins: I had decided to major in sociology because my older brother, who acted as sort of a father figure to me after my father died, when I asked him, "What should I study so that I could stop wars?" told me sociology. I did it, but I haven't stopped any wars as a result.

Constantino: Maybe not directly.

Robins: Not directly. I loved being at Radcliffe and did well there after first getting to learn the ropes, but I didn't know what I wanted to do next. They offered a test when you were a senior to tell you what field to go into. I took the test, and it came out "social work" so I thought, well I don't know maybe that's right. When I told that to my professor he said, "No, no absolutely not. Go to graduate school." So I did, but I only went for one year at first because the war had started. World War II had started and I wanted to do something to contribute to it, and decided to go to Washington and see what I could do. So instead of continuing grad school, I got a job with the Division of Program Surveys which, after the war, was moved over to the University of Michigan and became, I think, the Institute for Behavioral Research there. Washington was very exciting for me. I learned there how to interview, how to design studies, and how to write interview questions. And I even learned how to run the counter sorter, which was the way you did data analysis in those days. During my one year of graduate work I had met Eli Robins in a course called the Morale Seminar, and it was supposed to be about keeping up the morale of soldiers, but we said that we didn't know if we'd achieved that, but the course was very good for our morale.

Constantino: Right.

Robins: We enjoyed it a lot. During the year I was in Washington Eli was an intern in New York and came down to Washington occasionally. At the end of that year he went back to Boston to the Mass General. And I wanted to be with him, and I'd also figured out that I was never going to rise in my current job unless I had a PhD, so I followed him back to Boston.

The only time I've ever been fired was when I first got back to Cambridge. I tried to work as a nursery school teacher, but my housekeeping skills were not great and the women who ran the school got mad at me for leaving doors open and other such villainous things.

Constantino: And toys on the floor?

Robins: So I was really upset. Eli was at the Massachusetts General as a resident in Psychiatry and I got a job with Eric Lindeman, who was one of his professors, and who was very, very nice to me. I worked as sort of a mixture of secretary and research assistant. He was generous in letting me take courses while I was working for him. He really didn't have a tremendous amount of work for me to do and it was okay with him for me to go. I took courses at Harvard and also one at MIT while I was working for him.

Also, during that period I had a summer vacation that I spent as a counselor at a camp for disturbed children, run by Fritz Redl. He had primarily aggressive children. This was my first experience with antisocial children. And although it was a little bit frightening at first I was impressed with how accessible they were, and how pleasant much of the time. I had a very good time there with them. I also began to see how hide-bound psychiatry could be about its ideas. One of the children there was about 11 years old, a Canadian of East Indian background. And the write up on him said that he was very sick because he had violent fantasies. His fantasy of what he wanted to do in life was to train lions, to be a lion tamer in a circus, and they thought that was a very bad sign. Well, I got to know him and talked to him a little bit about life at home and his family. It turned out that one of his uncles was a lion tamer in the circus. He just wanted to be like his uncle. I don't think there was anything pathological about it at all.

Constantino: Much less heritable.

Robins: Right. Anyhow, after that summer I went back to my job. And soon thereafter got married to Eli Robins. And we had not been married very long when he got sent to San Antonio because he was in the Army and there was an Army hospital there. We thought we would be there for two years. We had a lot of trouble finding a place to live. While looking, I volunteered to do some political work for a liberal candidate for governor. Then we found a little house. I was busy unpacking when my husband was notified that he had to go to Germany, but we had no phone so he couldn't stop me. So I didn't find out until he came home after I'd been unpacking all day.

Constantino: Oh my gosh.

Robins: We decided that we would go back to Boston to see some friends before he had to report. But he got sick when we got back and had to turn himself into the army hospital eventually. There he was misdiagnosed as being psychiatrically upset over being sent to Germany. In fact we were thrilled at the idea of going to Germany, somebody paying our way to Europe. The problem was resolved when the physical therapist found out that he had a paralyzed arm. He had not known it because it was so painful that he didn't want to move it and didn't try.

Constantino: Right

Robins: So he left the hospital on a weekend pass and saw Ray Adams, the neurologist he'd worked with at the Mass General, who told him he had polio. There was a lot of it in San Antonio that summer and we'd done a lot of swimming in the pools there. So the hospital, as you can imagine, was pretty embarrassed over their misdiagnosis, and to make up for it they appointed him Head of Psychiatry at the hospital. While he was doing that, I went back to graduate school to finish up my course work. Once I had finished the course work, I had to do a dissertation, and didn't know what to do. But Planned Parenthood offered me \$500 to find out what people thought about birth control, because at that point Massachusetts and Connecticut were the only two states in the Union in which birth control was illegal. So it was lucky that I'd worked in Washington where I learned how to do a survey. I wrote the questions and went to homes to do the pretest, pregnant at the time. And people would say to me, "Well dearie, we can see how you feel about birth control!" Then I had the baby and used my

\$500 from Planned Parenthood to hire students to do the interviewing. So it worked out fine. Well I don't know how much more you want to hear about that--

Constantino: Well I'm curious what came of that interview. What came of that survey? Do you remember?

Robins: Well, in the first place the reason for the survey was that the issue was going to be a referendum on the election.

Constantino: Right.

Robins: And it didn't win, but it did provide me with a favorite story. During the pretest I interviewed a woman who was only in her late 20s but looked 40. She had had six children, and was very thin, looked tubercular but I don't know that she was of course, but she looked sick.

Constantino: Right

Robins: With each person we interviewed we read a list of arguments for and against birth control. And the against arguments began with "Birth control is against God's law." And she said, "Well yes that's true, I agree with that. But you know things are different nowadays."

Constantino: That's wonderful. And the experience with that first survey, did that sort of make you want to continue to do that kind of work and to continue that or had you already a conviction at that time that survey research was--

Robins: I had not made that kind of decision. I just knew I knew how to do it.

Constantino: Right, yes.

Robins: So at that point, I got through with the survey and got pregnant again. I had decided -- well we had decided early that we'd like to have four children. And I thought it was a good idea, though I'm not sure I would recommend that any more, to have them close together, because if you're going to stay home with one you might as well stay home with the rest of them and then go back to work when they were school age. So we were planning to have them in quick succession. So I was already pregnant with number two by the time we had finished the study.

After getting out of the Army and finishing his residencies my husband was urged by his chief mentor, who was Dr. Mandel Cohen to go to Washington University because, as he said, Dr. Edward Gildea, who was head of the department then, was the last white hope of psychiatry. Dr. Cohen was the only psychiatrist we knew in Boston who was not a psychoanalyst. He thought it was a terrible waste of time, and that what was interesting was biochemistry and trying to link the brain to behavior. And so he recommended that Eli go there. That idea fell on very receptive ground because Eli had already decided that finding the link between brain chemistry and psychiatric disorders was of great interest to him. And he got a post-doc appointment. He was the first post-doc with a superb biochemist here at Washington U, who was Oliver Lowry, who at that time was the new chief of pharmacology. As I mentioned I was already pregnant at the time we left, and by the time I had two little boys to take care of I wasn't getting much data analysis or writing done on my dissertation. I might never have finished it if had I not gotten a warning from Harvard, which was simply, "Finish the dissertation this year or forget it." The reason they were in a hurry was that the Department of Sociology had disappeared into something called Social Relations and they were trying to get all of the old sociology people off the books to simplify their lives. It was a very good warning because, when faced with the ultimatum, I borrowed another post-doc's bachelor apartment in the mornings, hired someone to look after my little boys and wrote, and that worked. So I finished it and defended it in December of 1951.

Constantino: Lee, what was the title of that dissertation more or less?

Robins: Something about birth control, attitudes toward birth control or something like that.

Constantino: Okay, right.

Robins: It never got published so it didn't really have a title.

Constantino: But it earned you a title.

Robins: It earned me a PhD, and probably one of the reasons I got through so easily was that on my committee when I defended it, was Eleanor Maccoby, who I had worked with at the Division of Program Surveys. She'd been in Washington at the same time I was before she went to Berkeley. And so we were old friends and she liked the dissertation.

Constantino: Right, and even at that time she was doing work in antisocial development?

Robins: She was. She was working on kids--

Constantino: Right, and so was your interest in that in any way influenced by those early connections with Eleanor or did that come later?

Robins: I don't think so. I didn't know about antisocial behavior at that time, I think--

Constantino: So now you've moved to St. Louis, two boys, very busy--

Robins: Yes, after he completed his post-doc, Eli stayed on as a regular faculty member. He just loved Washington University, particularly in contrast to Harvard. He had not been happy with the atmosphere at the Mass General, nor had I. It was an odd experience. There was a great deal of in-fighting among the faculty and they would come and cry on my shoulder, which I thought was quite inappropriate. It was much pleasanter here.

So with the dissertation finished, I went on with my design to have more kids. And so I got pregnant again and again, and thought that taking care of the four little ones was what I would be doing for the next four or five years. However, during the very first year of the youngest boy's life (they were all boys), one of the young psychiatrists that Eli was working with, Pat O'Neal, had discovered that the records of the St. Louis Child Guidance Clinic, which were being stored at Malcolm Bliss Hospital, were about to be burned because somebody wanted the room they were stored in. And she had asked, "Would it be possible for me to have them instead?" and they said, "Sure, if you can get them out of here." And so she asked me if I would be interested in going with her to look at them and see if they were worth saving and whether we might put in a proposal to follow up with the patients. This clinic had opened in 1920; it was one of the first three in the U.S., started about the same time as the Judge Baker (in Texas) and it continued until it was closed during the Second World War. That's why the records were in storage. They were wonderful records; each child had a 26 page blank that had to be filled out in great detail, not only about the child himself, but also about every member of his family. They were very objective, not speculative records. We chose to take all of the appropriate children who had been seen between 1924 and 1929 because after that the psychoanalytical approach came to St. Louis and the clinic's records got to be more and more theoretical and less and less factual about what had actually happened. So the later records were not very appealing, but the early ones were wonderful. I formally joined the Department of Psychiatry as a research assistant, not a paid position yet, but I could use the connection with Washington University in grant writing. We got funding for our project from the Foundation's Fund for Research in Psychiatry. The first few months were very dirty physical labor trying to sort through these stored records that were really filthy. What we did was select everybody who'd been seen from 1924 to 1929 who was white and had an IQ of 80 or higher. The reason we selected the 80 or higher was that they did a lot of IQ testing as a service to various agencies for children who were suspected of being mentally deficient. These children had never been

sent for treatment. The reason we dropped the Blacks was that there were so few of them. St. Louis had almost no Black population in those days; it came in with World War II. We picked a control group, which was done by going to the archives of the St. Louis public schools. They had the records of every child on microfilm reels and we would just spin them and they would stop and we would look to see if that child was the right age and lived in the same neighborhood as one of the kids in our study. If so, the child became a control subject.

Constantino: Right.

Robins: So we picked a hundred at random as controls. And we had a lot of fun hunting for the patients and controls, finding that we could locate almost all of them and finding them quite willing to be interviewed. We'd had all sorts of warnings from people about what a ridiculous thing we were doing. They said that nobody would talk to us, and if they did they wouldn't tell the truth. And -- but in those days you could get all the records you wanted and we got an amazing variety of records, and were turned down only by the FBI and Social Security, and even those we got around. We got around the FBI because friendly police chiefs would just give us their FBI records. And that worked fine. The Social Security people said they couldn't give us individual records but they would give us grouped records. So they did that, but one of the things they gave us was the earnings records at various locations, and we could tell what city they came from. By then our St. Louisans had scattered all over the country. So in the long run there were only 11 children left in groups where we could not be sure which children were which.

Constantino: Right. So they were---

Robins: So it worked beautifully, we got--

Constantino: --fairly identifiable that way? Yeah.

Robins: --very identifiable even though we weren't supposed to be able to identify them. We got all the other records we asked for. We got police records, we got military records, we got social agency records, we got credit ratings. There was almost nothing we couldn't get. And the advantage of that was that we asked a lot of very personal questions in the interview, which we were told we couldn't do because nobody would answer them truthfully. They had told us very frankly about all sorts of possibly embarrassing things they had done. But we had the record, and found -- and we substantiated the old finding from social research that the one thing people don't like to tell you is how much money they made last year.

Constantino: But everything else, no problem.

Robins: Everything else was okay. We asked about sex, we asked about marriages, we asked about whether they beat their children and everything came out.

Constantino: Right.

Robins: Of course I'm sure there were some cases where there was no record and perhaps they didn't tell us, but if there was a record they were very honest, even though they didn't know we had the records.

Constantino: Lee what is your sense at the time of the extent to which that prevailing idea that people wouldn't answer honestly had impeded psychiatric or behavioral research?

Robins: Oh, very much.

Constantino: I mean, that's the idea that I get looking back, you know--

Robins: Researchers just didn't ask difficult questions.

Constantino: Right.

Robins: Particularly if they wanted to do a follow up study, because then they were sure that when they came back the second time they would be rejected. We just didn't have that experience, although we sometimes found very protective relatives who didn't want us to get the address because they didn't want the person disturbed. But when we actually found the address and went to the person they were very happy to be interviewed.

Constantino: Right. And so, even aside from the findings of your longitudinal follow up, which we'll talk about as we go along here, did you find that as you presented this at meetings and shared this information with colleagues that the sort of flood gates opened of studies where they thought, if they're doing this at Washington U and people are answering honestly, we need to be doing this everywhere?

Robins: I think it made a big difference, though a lot of people still didn't believe it.

Constantino: Right.

Robins: I mean it's amazing how people hang on to their beliefs no matter what. I gave a paper once at a school of social work and the guy who had invited me said, "Well, it's not been my experience so it's just not true." He'd probably seen very few people with such backgrounds.

Constantino: And why let the truth get in the way of a perfectly good opinion? Right?

Robins: Right. Okay, well the study really solidified my interest in anti-social children, which had at first been piqued by my experience at Fritz Redl's camp. What we found as I guess everyone knows by now is that these children who had been sent mainly from the juvenile court had disastrous adult lives in many cases. Not in every case, but about half of them had really bad outcomes in terms of not getting much education, not being able to hold a steady job, having multiple marriages, arrests, and most depressing of all, turning out children who were just like them, so that it was being passed on to the next generation. We found that out very clearly by finding school and juvenile court records for their offspring. We had, of course, a contrast with the controls, but we also had a contrast with children with other kinds of problems because, although the clinic was set up mainly for the juvenile court, they did accept children of other kinds referred either by social agencies or by their own parents. And what we found was that there was very little difference between the children seen for the various neurotic traits, which were almost the only kinds of cases being taken to psychiatrists in those days, and the controls. They were very, very similar when they grew up. That doesn't mean that the parents didn't need some help while they were children, because I'm sure the parents were disturbed and the children were disturbed, but in the long run the ones who really needed help were the anti-social kids. And the psychiatrists were not treating them, thinking they were hopeless. So the most important message, I think, was that child psychiatrists were avoiding the treatment of children who needed it the most. What was very distressing was that when they grew up they chose anti-social mates and transmitted their problems to their offspring.

Constantino: Right.

Robins: And there was a little bit of suggestion of a genetic factor because it didn't seem to make any difference whether they stayed with the mother or they left; either way the kids had a bad future.

Constantino: And Lee was this the same group that you had studied or that you subsequently studied in terms of fathers and sons? That is, in terms of profiles of outcomes?

Robins: We had two groups. What happened was I was worried about whether the results of this study were generalizable because this was a clinic population.

Constantino: Right.

Robins: And what I wanted to do was to find children whom I could study who had not been referred to clinics but who were anti-social. And at that time it was clear that Black children in St. Louis had a very high rate of not finishing school, and of being juvenile delinquents, and of being adult arrestees. So I thought if we could just pick names from elementary school records and follow them that there would be a sufficient number of anti-social kids to make it worthwhile doing. So we used very simple criteria; we chose kids who were and were not held back in school, and that turned out to be a very good predictor of whether they would be anti-social or not, and whether they had a father in the household or not and what the family occupation was.

Constantino: Right.

Robins: And so, using those three variables we got a stratified group and were able to look at the effect of family and school dropout or being held back. And what we found was that exactly the same predictors worked for the Black kids as had worked for the White kids, but there was a very different finding, which was important and very upsetting. Among the White kids if they had not been anti-social as children, it was almost certain to have no adult arrests at all. If they had been anti-social about half of them had an adult arrest history. But that wasn't the pattern among the Black children. If they were anti-social as children almost every one had an adult arrest history, and even if they hadn't been anti-social quite a few of them got arrested as adults, which suggested to me that there was a lot of prejudice involved in terms of who gets arrested and who does not. And we substantiated that in a later study, showing that the frequency of anti-social personality in White prisoners is very much higher than in Black prisoners. You have to be really bad to get into prison if you're white, and you don't if you're black.

Constantino: Right, and how did that play at the time in terms of the prevailing understanding of discrimination and, you know, racial inequities that were during those times and how did that information get received by the scientific community?

Robins: I don't think it's had the amount of reaction that you'd like to see.

Constantino: Right.

Robins: Everyone recognizes it is not fair, but no one does much about it. It's essentially racial profiling, but the argument that the police use is that, well, there's more of it, therefore it's okay to do profiling because the chances are higher.

Constantino: Right, a higher yield.

Robins: Yes it's a higher yield, so it's--

Constantino: And so it's a vicious cycle.

Robins: Yes, it's really very vicious.

Constantino: Yeah, and do you think that those same factors are operating today--

Robins: Yes.

Constantino: --or have you seen over the course of your career?

Robins: Well, I mean my last big study was in the '80s and they certainly were then.

Constantino: Right, right. And how -- and we'll get to this as we go along but I'm just curious while we're on this topic -- how has the understanding of the biology of aggression and personality development and so forth over the years in, your mind, how has that played into this whole issue of the discrepancy that one sees across race and social class?

Robins: Well I don't know of any studies that have shown the difference in biology.

Constantino: Right, well I mean the studies that have been done haven't shown much of a difference and so--

Robins: Oh quite the contrary. Another example of this kind of racial prejudice is that Blacks are much more often arrested for drug abuse, but they actually use drugs less than Whites. And that's true of alcoholism as well. So it's hard to be black.

Constantino: Yes. And in your thinking, where does that start to impinge on the life of an individual or how does that get perpetuated over time and across generations and in successive cohorts of children growing up? What can we learn from that as a society? What could we do? How do you see all that?

Robins: I have a very simple solution to discrimination; it's called miscegenation. If everybody had Black relatives I think the prejudice would soon disappear.

Constantino: Meaning...

Robins: I think intermarriage is a very good solution. I've seen it happen certainly with anti-Semitism, and it's diminished because there is so much intermarriage.

Constantino: Right.

Robins: Not that it's totally gone but it's certainly much less.

Constantino: Are there other precedents for that in sociology or anthropology for the idea that intermarriage is the final threshold to ending discrimination?

Robins: I don't know.

Constantino: Yeah, it's interesting, it's very interesting in and of itself, aside from the knowledge that we gain from longitudinal follow up, about when these problems first arise and what is the longitudinal course of anti-social development. Along the way you pick up this sort of profound insight about the way in which racial profiling plays a role and even our definitions and our understanding of who's affected and who's not, as well as this sort of profound understanding that people will respond to your questions when you ask them. These are almost corollaries of the original research objectives.

Robins: Well actually, we found that people love being interviewed if you do it kindly.

Constantino: Right, right.

Robins: We did a lot of pre-testing in the hospital and we had one woman with whom we were trying out a computerized interview. She asked if she could buy it and take it home, because her doctor had never asked her those questions.

Constantino: That's beautiful. Lee, as your work and your career went on, were there political events at the time that influenced your thinking or work you were doing and what you got interested in?

Robins: Well there were. Let me tell you what happened. I did these two studies on follow-ups of children. The second one, the study of the young Black men, I used to see if the findings of the first study were really true in the general population and not just in a clinic population. Because heroin had already come into the Black ghetto in the '50s, it had been available to young Black men, but it had not yet hit Whites. I happened to have a first study of a general population's drug use, and therefore, I got invited to be on a review committee for the drug reviews in NIMH, the group that soon became NIDA. All of the studies on drug abuse before had been in treated populations.

Constantino: Right.

Robins: And so obviously the concern over the great spread of drug use made them interested in my rather small study, but it was unique. And because I went there to be on that review committee I met Jerry Jaffe who had to stop being on the review committee when he was named to run the drug program in Vietnam. We had gotten to know each other well and he had admired what I had done. And so he invited me to follow up the Vietnam veterans. That was an irresistible opportunity because he had lots of funding and he had the cooperation of all the government agencies that could help. So we had tremendous ease in picking a sample and locating them and so on.

Constantino: Right.

Robins: It was irresistible. So that was an indirect effect of history.

Constantino: Right. And still all built upon the original studies of when does anti-social behavior begin, or what are its earliest signs. You know, it's one thing to sort of study the individuals, late adolescents, adults who are using heroin or who are veterans. But particularly because this is an interview about research and child development, if you could say a little bit about the prospective information that one gains from those earliest times. How did that inform that research?

Robins: Well, I think that it was very important for me that I learned to trust what people said.

Constantino: Right.

Robins: Because we had the childhood records and we could look back and see, yes, they were telling us the truth. Originally I had the prejudice that a lot of researchers have, that you can't get good historical information from people. But I don't believe that.

Constantino: Right.

Robins: I think you can get good historical information in interviews. It's not perfect of course, but every interview involves some historical information. You're not asking subjects just about today. You're asking them about a period of time. It's certainly better for dating if the time is shorter, but whether there are more events reported that way I don't know.

Constantino: And so integral components of those studies involve retrospective accounts of earlier development? Right.

Robins: Which turned out to be highly good.

Constantino: Yes.

Robins: And so the same thing happened with the Vietnam study, which was that we found that the amount of drug experience they'd had before they ever went to Vietnam was not only highly predictive of their using heroin in Vietnam, but also whether they would go back to it when they came back.

Constantino: Right.

Robins: I mean that's an extremely interesting study, because there were no alternatives to heroin in Vietnam. There just wasn't easy access to stimulants or sedatives. And so, if they were going to use anything strong, i.e., more than marijuana, which was available, they had no other option. And heroin's future use was exactly like the future use of stimulants and sedatives, and that's another place where discrimination becomes very important because in the Black community there is much less use of stimulants, or was before crack cocaine came in, and a lot of casual use of heroin without being addicted to it. And that's another reason -- I'm saying it badly, but because there is so much more emphasis on heroin than on other drugs by the DEA anybody caught with heroin or known to buy it or sell it gets tremendously rough treatment by the justice system.

Constantino: Right.

Robins: And they get absurd treatment, I think, by the medical system where they are sent to a methadone program ignoring the fact that every heroin user we found was also using every other drug available. It's sort of foolish to treat heroin addiction as though that's all that he's got.

Constantino: Right, right. So the issue is co-morbidity and the clustering of different drugs of abuse. Were there indicators, Lee, from those studies about the actual effects of drugs on the brain that perpetuated further use, aside from just the concept of habits and dependence as we knew it? What about in terms of changing the biology of the brain, did you have clues to that?

Robins: I don't really have the evidence. I can tell you about people I have known, but that's not science.

Constantino: Yeah, right.

Robins: I mean I've known people who took LSD who were never the same afterwards.

Constantino: Right, right. Well and there are, you know, now compelling models for ways in which, you know, drug use does reset the biology of certain parameters of brain function.

Robins: That's not my field.

Constantino: Well I understand, just to think about how those observations -- how those longitudinal observations sort of carried themselves out.

Robins: Well, all I can say is that, for most of the kids who used heroin in Vietnam, they came home, didn't use it anymore, had no problems. And they became just like everybody else: they smoked, and they drank and used marijuana, and if they wanted something more they used stimulants and sedatives.

Constantino: Right, right.

Robins: We asked them why they stopped and they said, "Well my girlfriend and my mother didn't like it."

Constantino: The ultimate in inhibition.

Robins: Right.

Constantino: That's fabulous. So -- and there again, another way in which marriage as an intervention comes up.

Robins: Right.

Constantino: So then, following that series of studies, how did your research interests evolve from there? What were the next most important questions for you to address?

Robins: I have to admit that a lot of what I did from then on was what came my way rather than my planning it. The big thing for me was the Vietnam study, and the next big thing was that I got a call from Washington asking me about interviews, asking me what I thought about certain interviews they were considering using in the Epidemiological Catchment Area, and I said quietly, "Well we've just done a better one."

Constantino: Right.

Robins: And so we all competed and we won, and that made a huge difference in the rest of my academic life.

Constantino: Right, well let's talk about the Epidemiological Catchment Area study.

Robins: Okay, well again we asked a little bit about childhood but not a great deal.

Constantino: How was that study conceived in the first place and how did it--

Robins: It was conceived by Darrel Regier as an answer to the fact that when he was asked for information about whether people with issues were being underserved and how many doctors were needed and that sort of thing, he couldn't find the answer and decided that they'd better do something. Roslyn Carter had a big interest in psychiatric illness, and I think had a hand in stimulating the report, but the report showed that very little was known. And so the idea was to do a study that would actually evaluate how much psychiatric disorder there was in American adults.

Constantino: Right.

Robins: There was a later effort by the government to do a similar study with children, but it never was fielded, and that was heart breaking as far as I was concerned. But even the adult study had important implications for children. For one thing, we found that what it was saying in the books about onset was wrong: that many, many adult psychiatric disorders seem to begin in childhood.

Constantino: Right.

Robins: The other thing that struck us was that there are child disorders that have different names but sound a lot like adult disorders, and maybe they're all the same thing. So maybe there are other disorders that begin in childhood. So the result for me, which is still in the hope stage, is that we added four childhood disorders to our adult interview, and hope that from now on we will find out what the relationship is between childhood disorders and adult disorders.

Constantino: Right, you want to review those four disorders?

Robins: I think that there's a great future in that. I think that there is a very false division between childhood and adult life, and things that we thought started later sometimes start early and things that we thought started early sometimes start late.

Constantino: Right.

Robins: I mean, one of the things that we found in the study, the very first study, was that children who did not have much anti-social behavior but who began using drugs heavily in adolescence had a future very much like the kids who had had a lot of anti-social behavior in childhood, so antisocial behavior can begin in adolescence. It doesn't usually. Usually it begins early.

Constantino: Lee, were you surprised by analyses of the various characteristics of children with conduct problems and whether certain types of conduct disorder symptoms, for example, predicted one trajectory or another? It almost seemed that no matter which sets of symptoms you chose the outcomes were sort of the same. Could you talk about that a little bit and how you see that now?

Robins: Well, the way I see it now is that the most important variable is the number, the diversity of symptoms.

Constantino: Right.

Robins: And until we did this study nobody was using the total number of childhood symptoms as a control factor to look at individual symptoms. I think you absolutely have to. For example, our best variable is running away from home. However, that's a very rare event, and any child who runs away from home has probably done everything else on the list. So is the true predictor the fact that they've done everything? Or is it the fact that they ran away from home?

Constantino: Right.

Robins: So once we realized that, we've always controlled on number of different anti-social behaviors. We don't have good data for frequency, which is too bad. I don't know what we can do about that. People can tell you whether they've ever done something very reliably but knowing how many times they have done it, they are very poor on.

Constantino: Right, okay. And, for example, there has been a whole generation of studies more recently that look at differences between types of aggression, say predatory aggression versus affective aggression. And did you see that in the data when you originally looked at it or not--

Robins: We looked for it.

Constantino: You looked for it, right.

Robins: But we found that anybody who has a lot of arrests will eventually have one for aggression. Again, you have to control on numbers.

Constantino: Right.

Robins: If you are doing things that are anti-social, eventually you will get into a situation where there's going to be a fight or you're going to hit somebody or someone's going to get hurt. And I don't think you can just look at it as this one is an aggressive person and that one isn't. I think it has much more to do with frequency.

Constantino: So this is an important factor for our generation of studies now to keep very close tabs on: what's the total array of anti-social characteristics?

Robins: Exactly.

Constantino: And not just to focus on one small component of it and track its course without looking at the rest of it.

Robins: I think that has been one of the big failures in criminal research is--

Constantino: Really?

Robins: --that they've looked at a particular outcome instead of the total array of things that happen to people.

Constantino: Right.

Robins: That's begun to change. But if you only look at violent behavior or sexual offenses or something of that sort, you're missing the fact that everything's wrong in their lives, and what you do about this one behavior is probably not going to make a whole lot of difference.

Constantino: Now you mentioned the finding that children who, in adolescence, start to engage in serious drug use have outcomes similar to children with earlier onset of conduct problems. I wonder if you'd comment, just for a minute, too, on this issue of the age of onset. One of the important findings that I remember reading about from your work had to do with even--

Robins: Early is worse.

Constantino: Yeah, early is worse, and so the retrospective accounts of when the careers begin show that more serious recidivistic criminals began early?

Robins: I think it is true and I haven't seen a study that didn't find that early is worse.

Constantino: Right

Robins: And there have been some studies now that started with very young children that show signs as early as three to five. There are some things that have a normal age of onset for kids. Drug use for example usually starts around 15, or did. Maybe it has changed over time but at the time that we were studying people, that's when it was, so earlier than that was bad. But I don't know, and I don't think anyone else does, whether, once you get to be an early user, whether it matters whether you're seven, or eight, or nine, or six. For some things we know it doesn't. For example, Naomi Breslau looked at smoking and very early onset is not meaningful because a lot of kids find cigarettes around the house and try them once. They don't like it and don't do it anymore, and it's not until they're older that they begin to really be a smoker. And it's when you become really a smoker that matters, not when you first take a puff.

Constantino: Right, although people argue about that for alcohol.

Robins: Yes, but I think it's true; it's certainly been true in our family for alcohol. I had a baby who used to try old beers that were left around, hated them and never went back.

Constantino: Never went back.

Robins: Right.

Constantino: Right, so when you think back on the whole span -- we've talked about many of the contributions of your research. I remember myself beginning my own career in studying social development in children, with an intense interest in anti-social development, because of the magnitude of a public health problem that it represented. I can remember at least 80% of the articles that I read, peer-reviewed articles, cited your work, *Deviant Children Grown Up*, in those articles about studying, understanding the longitudinal course, the importance of the stability of anti-social development as a landmark. When you think on your career what do you think have

been the most important contributions of your research and your career in the scientific achievements of your work?

Robins: Well, I think there are several things that I have been very happy about. I certainly am happy about the fact that it's being used by other people a lot. One of the things I thought was very important and keeps being rediscovered, but still unbelievably often, is that the great emphasis that sociologists, in particular, put on social class; we've never been able to substantiate. We find that it's the kind of people they have for parents that are very predictive of what happens. And people who are very anti-social can't afford to live in good neighborhoods, but we haven't found much to implicate the neighborhood itself.

Constantino: Right, so you have a sense of what would be termed gene/environment correlation, perhaps, or even "downward drift," if one wants to put it in that sense, that based on particular characteristics of a person they're more likely to find themselves in that environment rather than that environment producing these kinds of maladaptive outcomes?

Robins: Well, essentially what we found was that anti-social behavior was a very good predictor of socioeconomic outcome. But the other way around is not powerful.

Constantino: Right, right.

Robins: Now there is some work that suggests that maybe we missed something there, that there are communities that -- there are good poor communities and bad poor communities, and that's probably true. But one of the things that I think you know from American history is that most immigrants began poor and rose, and that's certainly what we found in that first study that, although we picked the control cases from the same neighborhoods in which the patients were living, they were almost all in the suburbs by the time we went to find them.

Constantino: Right, right, right. Well, I remember a study that was published not too long ago by, your close friend, Tony Earls and Rob Sampson looking at the extent to which family factors mediated associations between social class and anti-social outcome. Have there been other things that you've seen that have helped to illuminate that?

Robins: No, I think that the Earls/Sampson study is the major study that's worked on it.

Constantino: Right.

Robins: I'm sure that it does make a difference to children whether their family is respected and treated well, but it's not as important as what happens inside the house.

Constantino: Right, right. Do you have a sense of, from your work, if you had your ideal method for intervening, what have you learned from your career and all of your research, all of what you've learned from your colleagues, that you would say, "This is where I would think that would be the best way to approach these problems to stop them to intervene?"

Robins: Yes, well I've written some on that, but of course I haven't been able to prove it because I haven't done the work. What I'd like to see is daycare people being very well trained to do all of the things well that bad families do badly. I mean, not to do the things they--

Constantino: Not to do them--

Robins: --do badly, and to do the things good families do well, which is offering a lot of rewards, and a lot of loving, and, in-so-far as you can, ignoring bad behavior and hoping it just disappears. And it seems the problem is that our economic system pays these people so badly and gives them so little training that we're not doing that for children.

Constantino: Right.

Robins: I've been particularly interested in daycare because it fills such a large portion of the child's life. I think it's much more promising than going to see a doctor where you see them once every two weeks or once a month for a few minutes. And if you could really have good up-bringing in daycare and use it as a way of teaching parents how to do it, I think it has terrific promise.

Constantino: Right. What's been the biggest barrier to making that happen? I mean, have -- you haven't found very much disagreement with people about that have you?

Robins: I haven't found anybody willing to undertake it and do the -- put the money into it.

Constantino: Right. What would it take?

Robins: I don't know. I haven't--

Constantino: There -- well it's interesting, you know. There was the NICHD -- recently the NICHD study of childcare and it, I think, underscored many of the things that you were talking about in terms of what the usual condition or the average condition that many children find themselves in in daycare, and the wide discrepancy in quality, you know, that exists out there.

Robins: You have a big problem when the pay is so low. It makes the turnover of caretakers enormous.

Constantino: Right.

Robins: And that obviously is not good for children. They need to be able to count on people.

Constantino: Absolutely. So any other major research and theoretical contributions that we haven't touched on that you would like to talk about?

Robins: Well, there are certain principles I'd like to mention, which don't solve the world's problems, but I think would lead to better research. One of the things we've worked very hard at, and have been fairly successful at, is making our research transparent. For example, one of the things I like best about our interviews is that next to each question we put which diagnostic system, which diagnosis, and which criterion for that diagnosis is this question attempting to get an answer for. So that anyone who doesn't like our questions can criticize them and know what they're talking about, that it's not just a sort of "I don't like your wording."

Constantino: Right.

Robins: But it isn't getting at this issue. I think that that would be wonderful if it happened more often. I think that people ought to know what you're trying to do at every step of the way, and then they can criticize, and they should. We haven't had much criticism. I don't think it's in the tradition of how you look at other people's research that you say, "Well that's a stupid question because it isn't doing X, Y, Z," you just say it's a stupid question.

Constantino: Right. Now how - now, Lee, how has that -- how does that play with more the quantitative kinds of measurements, where the questions all revolve around certain themes for which the factor structure or the core structure of that isn't even known yet, that the instruments are designed sort of to derive what are the core elements? Because I think, as you know, over time there are arguments beyond that level that have to do with is this the right classification system in the first place?

Robins: Absolutely, well that is an issue I'm very much involved in. Many studies look at diagnoses but few look at their elements. And we often don't know how well the symptoms hang together and whether they are specific to that diagnosis. As an example, almost every physical and psychiatric illness includes fatigue. I don't think it's a good symptom to use solely for depression. It only means you're sick.

Constantino: Right.

Robins: Something's wrong, and I think there are probably many things like that that need to be looked at to see whether they're specific to a diagnosis or if it is a symptom of many illnesses, like fever.

Constantino: Right.

Robins: If you're sick you have fever. It doesn't tell you what you're sick with.

Constantino: It doesn't differentiate much?

Robins: No.

Constantino: Right, I guess the counter to that would be if that particular symptom, even if it's not specific, can add to some cumulative index where some of the factors or some of the items are a little more specific but others are less specific, that may warrant the reasonableness of including them.

Robins: Well sure, you'd include fever--

Constantino: Right, you'd--

Robins: --in the same way--

Constantino: --exactly--

Robins: If you claim that you're ill and you don't have fever then one wonders.

Constantino: Yeah, right, right, right.

Robins: I think that's true, but I think what's happening is that it's being missed in all the other disorders it also belongs in.

Constantino: Right, right. I was thinking about this before when you were talking about the ECA and the fact that there wasn't a child ECA. Do you think that, since that time, that the research using the methodologies that have been derived has actually sort of made up for it, and in so many ways answered the questions that a child ECA would have answered, or do you think that we still need to have one, using the same kinds of methods that were used in the original ECA study?

Robins: That's a very big question, because first I have to decide whether the methods used in the original ECA questions--

Constantino: Right, right. Well, but the ECA is a big study and so the question is do you think that our field is lacking still for not having had a child version of that, or do you think it's been figured out? I mean, do you think that the epidemiology of what we understand as child psychiatric diagnoses is well enough worked out that we don't need to do a survey in that structured a manner? Rather, there has been a push to explore different childhood disorders at another level

rather than looking at prevalence according to a set diagnostic schema? So how do you feel about that for child disorders at this point?

Robins: I think we still have a lot to learn. I don't know what the best way of approaching it is going to be. But I was very disappointed in the failure of my argument when we were doing the ECA, that once you also have a normal population of parents you have a normal population of kids. Why don't we interview the kids? I couldn't get the government to do it quickly. Instead there were fights over the instrument and how to do it. My idea just went down the drain, and I think it was a tremendous waste of money by not taking advantage of the fact that we already had a good population sample of children.

Constantino: With respect to the history of the child ECA attempt, Lee, also related is the issue that many child disorders, which had historically been thought of as categorically definable disorders, are now being construed as quantitative traits. And how do you -- you know, where a disorder is sort of an arbitrary dividing line imposed on something that's continuously distributed in nature -- how do you see that as sort of either a hindrance or an advance in the epidemiology of child disorders?

Robins: Well I've never been very much impressed with the fight over whether you use dimensions or categories. All categories are made up of dimensions and I don't see why we can't use both. I think it's an empirical test what's most useful.

Constantino: Right.

Robins: Let me try to put it a different way. I think that the early work in psychology, which was all only dimensional, is very unsatisfactory because there is this huge mass in the middle that is so big that the ends don't make much difference. And therefore you don't learn very much about disorders because so much of the information is about small differences between normal people.

Constantino: Or at least you don't learn very much about disease or--

Robins: Right, exactly.

Constantino: --or something that's clinically significant.

Robins: So I think you have to pay attention to the extremes. And where you cut the extreme, I think, is just a matter of convenience. It doesn't make any difference, but you do need to do something to look at the people who are really disabled.

Constantino: Right. Well, and this is one of the things that launched the medical model in biological psychiatry, especially at Washington U with Eli and Sam Guze and putting psychiatric conditions into that kind of a context where we're looking at -- they're always counting and there are thresholds for clinical significance. Do you think that to study these things in that way had tremendous implications? Do you think that the difficulty or the conflicts that researchers have over -- or arguments over quantitative versus categorical or those kinds of things -- do you think that those are impeding the progress of research, or that somehow kind of getting on with it in a way that people aren't right now would spring loose greater discovery? Have you -- do you have any thoughts about that one way or the other?

Robins: Well, I think sometimes the dimensional argument is made by people who just don't like the fact that they aren't free to do everything they want to do. So I think it's hard to judge. What I would like to see, as I said before, is using both simultaneously, and certainly counting number of symptoms is a very big help in looking at the likelihood of recovery.

Constantino: Right.

Robins: I mean, we looked, for example, at the study on whether people could ever become normal drinkers if they'd been alcoholics, and the answer is yes, if they weren't very alcoholic in the first place, so that they barely made criteria.

Constantino: Right, right, right, but not true if they were way in there.

Robins: Yeah, severity is obviously a very important factor, and we don't understand it yet. We don't know why some people have very serious disorders and other people have a light case. I think that's a very important area to start exploring, but we haven't done it.

Constantino: Right, and what of the integration of genetics and epidemiology? What have you seen over time in how those have helped to elucidate some of the counting of symptoms and how that's all worked out?

Robins: Well, I'm very excited by it. I don't know how long it's going to take for it to pay off in a big way. But I think it's wonderful if we can really put biology and behavior together so that they inform each other.

Constantino: That's right.

Robins: It's obvious that if you had good genetic measures, you could learn a great deal more about the effect of environment than we now know. You'd know who was at risk and who isn't, and we don't know that now. And it seems that some people are not at risk no matter what.

Constantino: Well, I think we're getting close. I mean, I'm very excited about that.

Robins: I think it's going to come. The most important thing to know is what can you do something about and how.

Constantino: Right and you've said that to me from the very beginning when I came to Washington U some 12 years ago, and I remember you making that exact statement, and it's never left me too, that somebody that had made your contribution and had been so careful about all the various things that you had done still said the most important thing is to sort out who's at risk and what can you do to help them, what you can do to change things. So I've never forgotten that.

Now, I was going to ask, too, if you've had any surprises. In other words, what were some of the most -- or one of the most surprising of findings or areas where you held onto something for a while and you found that that was the wrong approach, that there was something that was not right about that and you had to sort of change direction?

Robins: I really haven't had much of that sort of experience. One of the things that surprised me was that we had what I thought was a wonderful original discovery, which is that what used to be called "hysteria" and is now somatization disorder in women was strongly related to early anti-social behavior. And I thought, well we've really got something here, and I looked up some other writers and found they already knew it! And that was a shock.

Constantino: Right, right.

Robins: Yeah, I thought that was brand new and it wasn't by any means.

Constantino: Well you didn't have the advantage of an Internet Pub Med search at the time either did you?

Robins: No

Constantino: Right, you'd have to spend hours with Index Medicus to find others' work.

Robins: Yeah, there isn't a good way to do that. In fact, it was in a book.

Constantino: Right.

Robins: Even the new computer searches can't help you find things in books.

Constantino: Yeah, right. Well, let's talk about research funding a little bit. This was always very important to your career and you -- as you know all the -- you know, not only your own research efforts, but all the scientists that you've worked with, that the economics of it has been very important. And sort of your perspectives on that, how you tried to shape policy with respect to research funding over the years, and any particular memories about that that have been important.

Robins: Well, I was very fortunate. I was supported continuously for some 40 years and that was very nice. But it was a lot nicer--

Constantino: That would be an understatement.

Robins: --it was a lot nicer at the beginning than at the end. I served on a lot of review committees sometimes, I mean a lot of ad hoc ones as well as the NIDA one and on the NIDA Council, and what I observed was that the level of success you had to have to get funded got higher, and higher, and higher. And I think that's extremely dangerous because what it meant was that one enemy in the group could sink you no matter how much everybody else liked your proposal.

Constantino: Right.

Robins: And I don't think that's fair, and I don't think it's a good idea. And the other thing that changed dramatically over my career was, when I began, if I got turned down I would have been absolutely shocked and thought something was terribly wrong. Now, virtually every grant proposal gets turned down when it first goes in and everybody has to re-write.

Constantino: Right.

Robins: I think that that is terrible policy. And the reason I think so is that as a researcher you have to keep your team alive, and therefore if there is going to be a hiatus while you're re-writing and sending in another version you have no money. The result is that everyone I know now who's in this business tries to keep three or four grants going with different termination dates so that they can support people. That's terrible; nobody should be working on three or four grants at once. You can't give any of them all your effort. And I don't think that's good science. I think you're getting sloppy stuff done, because there's so much to do.

Constantino: Right, right and your efforts are so scattered across those different -- the administration of those things.

Robins: You're putting out fires all the time, one place or another

Constantino: Right, so obviously this is not the first time that you've made that comment. Tell me about sort of who you've shared that with, how you expect that to change if ever, and, you know, what your recommendations would be?

Robins: I really have no answer except bring back the old days.

Constantino: That, that -- we cannot accept that answer.

Robins: I know. It's not the right answer. I don't know how to solve the problem.

Constantino: Well I -- well let's think about it. People might say, first of all, you know, there is steep competition. I mean, many more researchers have entered the fray.

Robins: That's right.

Constantino: And so trying to have a system that's fair, that sorts out the very best applications, may necessitate this sort of insistence on perfection, you know, as opposed to a first round acceptance, because otherwise there would be no way to ultimately fund everybody. You know, people get rewarded for "stick-to-itiveness," being able to be persistent in terms of their efforts to get grants funded. So I am particularly interested in what you said about this issue of being scattered. There are great pressures on that. I experienced that myself in terms of sort of this phase of my own career, again, having been in this now, you know, about ten years after a post-doctoral fellowship and you do. There is a direction that one goes to sustain oneself and to spread, and how could the funding agencies re-direct the programs to focus on a scientist being able to focus and concentrate?

Robins: Well, I guess one way of doing it would be to have a fund that they put aside ahead of time for an interval between funding. I can't think of any other way, because the problem is not for yourself, because you have tenure, but to keep the people working for you in salary.

Constantino: Right.

Robins: And it's extremely unfortunate if you have to let people go and start over when you've spent years training them to do it your way. And I don't see any other way of doing it. Another possibility would be to give you -- if someone thinks it's a generally good idea, which I guess they would have to or you would not even reapply, would be to give you short -- to shorten rather than deny the award so that you have to come in after two years. One of the things that I find very difficult and always have about grant proposals is writing down what I'm going to be doing in five years. Who knows? It depends on how things go.

Constantino: Right, and that's probably a good thing to not know.

Robins: Right, yes, I mean I've kept feeling I was pretending when I said I was going to be writing X papers in five years from now.

Constantino: Right.

Robins: Maybe, maybe not. And I didn't like having to write down things I didn't really believe.

Constantino: In order to document the justification for year five of funding, I must say very specifically what it's going to look like or I'll get docked for not--

Robins: Yeah, right.

Constantino: --you know specifying that.

Robins: So I think it might make more sense to say, "I want funding for five years, and this is what I'm going to be doing the first two years, and then I will write to you about what I'm going to be doing the next two years."

Constantino: Right, right, right. And you know, and the whole system of yearly progress reports doesn't quite reach that because there isn't the kind of review associated with those kinds of reports, and the whole premise of them is just to make sure that you're executing that original five-year plan or something close to it. So--

Robins: And they take a lot of time and don't seem to serve much purpose.

Constantino: So aside from those sorts of frustrations, what sort of roles have NIH priorities over the last decades affected either, you know, complemented or diverged from your own research efforts and your own research emphasis? Were there ways in which the system sort of worked to your advantage? Certainly early on it did, it sounds like, but as things went forward were there ways in which the NIH priorities evolved that were specifically either stimulated by your work or that helped to move your work forward or both?

Robins: That's a good question, and I don't really know how to answer it. But what happened was that all of the principal investigators in the ECA were told that they would no longer be supported as a group and that they would have to each come in with an RO1 application. And so I wrote one and it got turned down. And I said I wanted it re-reviewed and they said, "Okay," and they asked me who I would like to see on the review committee. By that time I had some powerful friends and I gave them names, including the editor of the *Archives of General Psychiatry* and a few other friends in high places. It got re-reviewed and after revision was accepted, not because they put my friends on the committee, but because the proposal had been assigned to two review committees, but reviewed by only one. It was moved to the other committee and went through. Now again, I think that's very awkward and essentially I got it funded by complaining and not taking it quietly.

Constantino: Yeah. And would everyone have done that? And you know, one question is what's your sense of, had they not -- had you not done that, had you not gotten that funded, how would it have changed your career?

Robins: Well, I think it would have been unfortunate, because I think it was a very good project and I was just amazed that they turned it down.

Constantino: Right.

Robins: I guess I could have written it better and maybe I did the second time around, but I don't think it changed all that much.

Constantino: Right. And do you have a sense of the priorities for the NIH in terms of mental health psychiatric epidemiology versus medicine in general, how that's evolved over time and your role in that and how it's -- how appropriate you see that mix of things is at this point?

Robins: Well, I think very interesting things are happening, but I'm not on the inside anymore. I don't really know the details. All I know about are things that our University is doing. Some of them are fascinating and I wish them well. I hope they'll work. But it is very different from the old days.

Constantino: Right. In terms of creative ways of garnering support from the various institutes and-

Robins: Trying to make everybody work together.

Constantino: --exactly.

Robins: That's very difficult. I mean, everyone who's tried it finds that it's very valuable in some ways, but things work best when all the different disciplines are in the same head. It's very hard for people who don't talk the same language to collaborate.

Constantino: Right. And how about across institutions in terms of your career, you know, how that worked out for you and how any sort of overriding themes over the span of your research career--

Robins: Well, the only time that I've been in a collaborative enterprise was the ECA, and that had its good and its bad aspects. In the first place I liked it because I got to know people I wouldn't have known otherwise and I got to know them well and I made good friends that way. It probably was a very good idea to do it collaboratively, but there were times when it was difficult. For example, what it ended up doing was giving each university the option of a half hour of questions of their own, and that's fine, but then the results are limited because you don't know what the other universities would've found if they'd only asked your questions.

Constantino: Right, right. Tantalizing but, yeah--

Robins: Yeah, not entirely satisfactory, but it gives people some freedom, which is good. So collaboration requires everybody's doing things exactly the same way and that represses enterprise.

Constantino: Right, right. Well, and I think that's a very important issue in child development research, because there are frontiers in that kind of research that require innovation, but also require large sample sizes or, you know, wide geographic distributions, or different, you know, populations that are taken advantage of at different sites. And so trying to balance innovation with the integration of--

Robins: Well, I think that's absolutely right.

Constantino: --samples, yeah. And in some ways I think the -- I mean my observation right now is that the emphasis is on collaboration, inter-institutional collaboration. It will be interesting to see whether that pendulum may have swung too far in terms of not rewarding enough individuals or innovations.

Robins: Well, I think our solution was the best you can have in that situation, which was to give some freedom to each place. But it's not really a very fair system, because some people just are passengers and other people are really running the whole thing.

Constantino: Right, and that's got its own frustrations, I'm sure. We've talked a little bit about this, but if you could say a few words about your research and its impact on overall research priorities at the NIH and, more to the point, public policies involving prevention and intervention. In other words, there are so many very important implications that people like myself that are in science that are studying children recognize from the enormous contributions that you've made. How do you feel about how those contributions translate into public level knowledge, public policy, you know, about understanding children, development, anti-social behavior, prevention, all those kinds of things?

Robins: Well, I wish they had done better. They've certainly stimulated interest. I don't think we still know much about how to treat anti-social kids. There aren't obvious solutions from what we've found for what to do. There are certainly things you shouldn't do. Beating them every day doesn't seem to do a bit of good.

Constantino: Right, and probably hurts them.

Robins: Yes.

Constantino: The handwriting is on the wall.

Robins: Right.

Constantino: And you mentioned -- I remember sitting in your office one day -- you were my first mentor -- and sitting in your office and hearing you talk about, well, how can we accept even the possibility that that kind of maltreatment wouldn't affect a child, something as delicate and sophisticated as a child. Look at what it does to dogs. And I've never forgotten that. We had a talk about that, and we talked about animal models that never quite came to fruition. But, right -- and so what is your observation now about how science, especially in our field, gets translated into public policy, gets translated into awareness, public education, legislators and what are your thoughts about what we could -- how we could do better?

Robins: Well I think it's a terrifically difficult problem. I don't think I've got the answer. I was very much interested in the argument over whether you should do things for high-risk kids or do things for everyone, and I think there is much to be said on both sides. Most of the studies that have tried to choose high-risk kids and do something for them have failed, and the argument has been that perhaps part of the reason is that they're so stigmatized by being selected for the treatment that the treatment doesn't have a chance. On the other hand, if you take kids who don't need it and spend all sorts of time teaching them to be nice people, that's not what they're in school for.

Constantino: Right, not the greatest use of resources.

Robins: No.

Constantino: Right. Now, getting back to the high-risk kids, was it your thinking -- or is it your perspective now that there is a certain high-risk group that's just too high-risk? In other words, that they're so severe that they may not respond, whereas if you choose not quite as severe that you may get a better response? Or you don't see it that way?

Robins: Well, we have a friend in common who works with very severely disturbed kids, and some of the kids she describes seem just so dangerous that I can't imagine anything that's going to work for them.

Constantino: Right.

Robins: And we do know that a lot of kids do recover, and I think that for me the most useful thing to do next would be to study those kids, to look at kids who get well on their own and see what happened to them and copy it.

Constantino: Right, well and it's interesting. It gets -- in some ways some studies like that have come full circle back to the issue of risk and liability in terms of, especially, the gene environment interaction studies so that, you know, those children who are resilient, for example to maltreatment, may have genetic underpinnings that confer that resilience to them in the first place. And so when you say recover--

Robins: Well, I'm thinking about children who are anti-social--

Constantino: --who have become anti-social and then come out of it--

Robins: --and come out of it.

Constantino: --as opposed to children who have -- are exposed to some risk parameter that would otherwise lead or influence anti-social development, but they're somehow resilient to that. I mean, I think those are two separate things and even in the most sort of dire circumstances of a 50% poor outcome or even higher for clinically ascertained conduct disorder -- what I'm hearing

you saying is that studying those children who do actually spiral out of it is very important to the study.

Robins: I think so.

Constantino: Have they been studied very well?

Robins: No.

Constantino: No?

Robins: I think that's where I'd put my money and it's very parallel to the advice I gave the Robert Wood Johnson Foundation when they wanted to put money into drugs and I said, "Please pick nicotine," because many normal kids smoke, and those are the kids that you need to work on, kids who are clearly salvageable, they don't have to smoke.

Constantino: Right, right.

Robins: And if you could keep them from smoking, and if you keep kids who are being anti-social but don't have to be, because they have something going for them, that those are the ones that you could do the most for. The other thing I think, but I have no evidence for this at all, is that we have to find ways to have schools reward all sorts of skills and not just academic ones.

Constantino: So this is some of the work of your friend David Offord?

Robins: Yeah.

Constantino: Yeah.

Robins: I think that a lot of children hate school because they never get any rewards. They do everything badly and who could like that?

Constantino: Right.

Robins: I think the trend that has been "let's save money; we'll cut out art; we'll cut out music; we'll cut out athletics," is terrible because you have to find something for every child that he or she is good at so that they have some fun in school.

Constantino: Right. Fun, purpose, identity--

Robins: Rewards.

Constantino: --rewards, right, all those things. Have you seen over the course of your career, you know, this transition -- the transition to adolescence is an important, very important time, and has very interesting application to the life history of conduct disorder, because there are some cases that develop first onset in adolescence. And you know, a question that I have always wondered is, from your perspective, do you think that adolescents, in the course of your career, have been exposed to social factors that will more tend to marginalize them or more tend to isolate them or more tend to make it difficult for them to integrate themselves meaningfully? Or has it -- do you think it's sort of always been the same for adolescents, just a different flavor over the decades?

Robins: I don't really know. I think an awful lot of adolescents spend their time trying to be grown up and to get rid of parental supervision, and that can get them into trouble. But it's also a good thing to do--

Constantino: Yeah it's part of their job!

The other -- I want to ask one other thing about this very important issue about who spirals out of conduct disorder because I think -- and my hope is that it will be one of the -- of many legacies of your work is to build upon it by sorting that out. There have been a series of studies that have suggested a differential heritability for juvenile anti-social behavior versus adult anti-social behavior. And has it been your impression, and again, we don't really know about this, but has it been your impression that one possible explanation for who stays in versus who gets out of that trajectory is genetics; that is, that even those children who develop a pattern of conduct disorder early on, those who continue versus those who leave that trajectory, do so on the basis of genetic factors, and do you think that could fit or that that's not going to be the whole story?

Robins: Oh I think one thing we don't know that I'm very curious about is what determines when genetic factors have an effect. I mean, they clearly do not all happen at exactly the same time. If you look at identical twins, one of them gets sick before the other or does something else before the other, so there are obviously timing factors which are probably not genetic, but I don't know what they are. And I think that's a very important thing to learn, because I think the consequences are very different depending on when it happens. One of the things that I remember from the beginning of the drug era was people giving up on children and saying, "They're hopeless, they're ruined for life because they've been using drugs." Well, if they started drugs in college this was never true, or almost never true, because they had a substantial base already. They could change, because they knew how to read and write and they could do arithmetic and all those good things.

Constantino: Right, they had a way to get out.

Robins: Yes. Whereas if they got into it early, before they learned these things, they were really stuck. And I think that's one of the major reasons that age of onset makes so much difference, because they don't have time to develop other skills.

Constantino: Well, it will be extraordinarily important for us to work this out and, again, to build upon those findings. Now, in terms of your role at Washington University, you also had a teaching role aside from your research. Can you say a little bit about that and how you have employed child development research in your teaching and in your coursework, and so forth?

Robins: Well, I've often used it as examples when I'm trying to teach epidemiological methods or how to do follow up studies. So I've used it a lot, but that's because that's what I know, so it's easy to do. I've had a good time teaching advanced students. I started and ran the psychiatric epidemiology training program for a long time, and that was great. I've not been so successful with undergraduates, which I've tried a time or two, because I've discovered that they were really more interested in the kid sitting next to them than in what I was saying.

Constantino: They didn't have that direction and the drive for what you were interested in teaching them about?

Robins: Right. But I've had a good time teaching medical students.

Constantino: Do you think that the -- Lee, that the discipline of psychiatric epidemiology as you have practiced it and developed it represents a different -- a fundamentally different point of view than what is adopted by most individuals in child development research?

Robins: Well, I think there are a few things we know that are not widely recognized. For example, I think studies -- and this isn't just mine -- but I think studies that pick children early before they are separated from the family for any reason do much better because they're getting everybody. I think when most epidemiologic studies now are cross sectional and they take a child however old he happens

to be in a family and miss everybody who's not in a family, and there are a lot of kids who aren't in families.

Constantino: Right.

Robins: So you don't really have the answer for the whole population. You're losing your extreme cases and I think that's unfortunate. And there are kids who are lost because they've run away, because they're in detention, because they're in hospitals, and I think that's a mistake. I really like the studies that are being done now starting with birth records, because everybody's in them and must be accounted for.

Constantino: Right.

Robins: Of course some will die young and you'll lose those. There's no way to know what would have happened to them if they'd survived. The other thing I think that can help is interviewing families about a child who's died just to be sure about what happened up until that point. So I'm very keen on studies that start with very young children even if you don't see them when they're very young, but where the roster is of young children so that you know you're covering everybody.

Constantino: Right, very good. Your familiarity with the Society for Research and Child Development and its activities, the -- sort of the history of that.

Robins: It's really not been much.

Constantino: Okay.

Robins: I once gave a paper there and then I got an award from them, which I was very grateful for, but that's really been my only contact with them.

Constantino: Okay, although researchers in their field have heavily borrowed upon and built upon what you've done.

Robins: I know a lot of people who belong--

Constantino: Sure.

Robins: --so we've had other contacts but not specifically because they're in that organization.

Constantino: Right. Now in terms of your more over-arching perspective on the development of our field, continuities, discontinuities over time both in terms of child development and epidemiology, how do you feel like it's advanced, how do you feel that it could be advanced further? And we've talked about some of these things, but if you have some sort of--

Robins: Well, I think it's very much better than it used to be. For one thing, I think the standards have risen greatly. Researchers now worry about statistical power, which they ignored early on, so that you-

Constantino: Statistical power.

Robins: --have enough cases--

Constantino: Right, yes.

Robins: --to say something about four people, it's not very exciting. I think that's a big step. I think that the pessimism about being able to locate people has disappeared. There used to be a feeling that

if you could get 30% or 40% that was great. Nobody would put up with that sort of figure anymore because it turns out not to be all that hard to find people. There's also been a development, which I think came from polling, which was trying never to get a flat refusal so that you have another chance. We've found that a lot of people who say, "No, I don't want to do that," the first time will do it when asked the second time.

Constantino: Right.

Robins: The other thing that I feel strongly about is not always done, that is, paying respondents for their time. A graduate student, who worked for me to run the young Black men's study, convinced me that it wasn't fair that interviewers got paid but the respondent didn't. They were both putting in the same amount of time. And it not only is fair, but it makes for better science. People feel an obligation to tell the truth when they're being paid for it. And they feel--

Constantino: --most people--

Robins: --most people. Oh there are some bad eggs out there but most people aren't. And paying them gives the interviewer a lot of freedom to be honest about how long the interview would take. Our interviewers could say, "I'm sorry it's going to take more than an hour. When could you see me for two hours?" And the interviewers felt okay about it, whereas when they weren't paying people they would say, "Well it's not going to take very long." That was a lie.

Constantino: Right.

Robins: So I think both parties feel much better when there's payment. And that unfortunately is still not being done very much, because it's expensive. But I think it would be very interesting to do a cost analysis of payment, because I'm not sure it is really expensive. I think if you counted in all the failed appointments, people who didn't come when they said they were going to, or weren't there when you went to their homes, and changed their minds about being interviewed you might find it's really cheaper to pay them.

Constantino: Right, right. Yeah, I mean--

Robins: It also means it's important to you.

Constantino: Exactly.

Robins: And people want to do something important.

Constantino: Right. Not to be wasting their time and not to be taken advantage of. Right.

Robins: Yeah.

Constantino: Lee, who would you say you count among your -- the most influential colleagues over the course of your career? We haven't talked a whole lot about that in terms of other researchers that were -- that influenced you in whatever way, either in terms of their ideas, their supportiveness your interactions with them over time, who were some of the most important?

Robins: Some of the most important were here at this university. I mean, I was asked at one point by a sociologist to write about how do you get along in a department of psychiatry. And I said, "Well, you know, I'm really not going to do that, because the first line would be 'first marry the chairman.'" But it wasn't just that. The whole ethos of this department was so receptive to empirical work and so interested in what I was doing that it was wonderful. It was a very unusual experience for a social scientist. So certainly, the most important influences were local. I got teaching about psychiatry at home. The lack of knowledge about psychiatry has been one of the big problems I think for a lot of

social scientists' research on children. They just don't know enough about what really sick kids look like.

Constantino: Right disorder--

Robins: Yeah.

Constantino: Yeah.

Robins: And it's also a matter of knowing how to ask, I think. Psychiatrists learn how to ask very intimate questions without offending. And I think I learned from that.

Constantino: And without seeming too cold and aloof--

Robins: Yeah.

Constantino: --at the same time.

Robins: Being -- well, it's a very delicate business.

Constantino: Good. So we were continuing on with interviews and having psychiatrists as colleagues--

Robins: Yes I'm trying to think if there were other people.

Constantino: Well, first could you name some names for the record, for your Wash U colleagues that were so important?

Robins: Well, there were a lot of them. One of them was clearly Eli, who taught me most of what I know about psychiatry. I used to have long conversations with George Whitaker, and that was great fun and as well as his having lots of original ideas, making me think about things and being very willing to argue with me. I used to argue at length with Ted--

Constantino: Ted Reich?

Robins: Ted Reich, yes. And all of that was very helpful; it was a different atmosphere in those days because the department was so small and we all saw each other all the time.

Constantino: Well, and Ted, you know, in his career, applied so many of the principles of epidemiology and whole families in terms of studying the genetics of alcoholism.

Robins: Yeah.

Constantino: So taking a different spin in a clinically ascertained population, but using those principles.

Robins: Another person I had very warm feelings for was Remy Cadoret, who was working in very much the same areas I was.

Constantino: Right.

Robins: Abroad there was Fini Schulsinger, who became a good friend. Certainly a very big influence in my life was Norman Sartorius, who gave me all kinds of opportunities to do things I would never have done without him.

Constantino: Right, and how did you first meet him and what was the nature of your initial interaction?

Robins: Well, I already -- the other person I was about to mention was Michael Rutter, of course, who visited here when he was very young. We were trying to get him to come permanently, but he didn't do it because he thought our political system was not good. My first contact with WHO was a meeting on child psychiatry which published a book about what the future of child psychiatry was and what the past had been.

Constantino: And was Norman the head of--

Robins: Norman was the head of Mental Health at WHO, and I think it was probably Mike Rutter who got me invited. We spent a whole week writing the report staying up to incredible hours to do it, and got to know each other very well.

Constantino: You and Dr. Sartorius?

Robins: Well, yes but Mike was there.

Constantino: Okay, right.

Robins: It was an interesting experience, because Mike Rutter and Philip Graham didn't appear in the main group but they were preparing a preliminary draft that had to be approved by the end of the week. That's the only way you can get a publication out in a week. That's what they had to do. That was the rule. So--

Constantino: And have you kept in touch with both of them over the years?

Robins: Oh yes, yes we're good friends--

Constantino: What's the -- when you -- you know, especially Mike Rutter, obviously such a major force in child psychiatry and child development research, what kind of conversations do you have with him?

Robins: Everything.

Constantino: Yeah, yeah.

Robins: No, we're very good friends.

Constantino: And as far -- do you think that he shares many of your perspectives about what are the most important next steps, especially as they pertain to anti-social development or have you had disagreements about some of those things?

Robins: The only thing we've had disagreements about is that, because he's a clinician he doesn't like our highly structured interviews, he feels it reduces the interviewer's freedom and I don't blame him. If I were Mike Rutter I wouldn't like to use them myself, but that's really been the only major division.

Constantino: But I remember you telling me one time that it's a good interview if the computer can score it, you know, and to, you know, sort of really strive to have as much objectivity in those.

Robins: Well, if you're interested in doing big studies you're going to have to use a lot of interviewers that you can't train very, very closely and so the interview has to carry it.

Constantino: Right, right.

Robins: You can't trust people to be clever enough to ask the right question at the right time.

Constantino: Yep, even if under the ideal circumstances they might be able to derive richer information. You can't depend on that for--

Robins: Well, and it wouldn't be uniform from case to case. And so you don't know who doesn't have something they have asked about if they ask question that others don't ask or ask it of some subjects but not others.

Constantino: Right, right. So any other colleagues that you feel like you would like to mention in this particular history or that were particularly influential?

Robins: Well, I think those are the main ones that are specific to children. Certainly the people at NIMH were very important to me for the adult study, and I learned a lot from them. I think that's true.

Constantino: Good. Now, what about the sort of -- the development of your own personal life, your personal interests, your family life and how that's influenced your work, your science, the course of your career and so forth?

Robins: Well, the main thing that happened, of course, was that Eli got multiple sclerosis and at first that wasn't much of a problem. But as time went on and he became sicker, it gave me a great deal of time to write. When he was head of the department I did an awful lot of entertaining on his behalf and traveling with him to meetings and that sort of thing, which was great fun. I would have been happy to keep on doing that.

Constantino: Right, right.

Robins: But when the time came that he wasn't really up to doing that I had the time to do a lot of writing, and so I'm sure it made a huge difference in the way my life went. Also, working was something that he valued very highly, and so he didn't complain if I went off to a meeting, whereas if I'd gone off on vacation he would have complained.

Constantino: Right, right, so that kind of kept you to it to some degree.

Robins: Right.

Constantino: And in terms -- to following through on some of the things that he was most actively pursuing in science that perhaps, because of his condition, he wasn't able to pursue in as full a way as he wanted to -- you know, I know from many conversations with you that he would, you know, share so many of his perspectives and insights and recommendations and things like that. And if you could say a little bit about that, about how that shaped, you know, sort of your carrying forward your own research agenda.

Robins: Well, we never actually worked together on a project, and that was partly because he knew a lot of things that I didn't know, and partly because I didn't think it was wise. We were both pretty perfectionistic about what we did and I didn't want to fight with him.

Constantino: Yeah, yeah.

Robins: And so that was part of it, but we talked about work a lot, and he read a lot of things that I wrote and criticized them. And I did the same for him. He was a great thinker, I was much better at style and writing than he was, so I would fix his English and he would fix my ideas.

Constantino: What a perfect complement.

Robins: The other thing that was fun to watch was how much of the attitudes that he and I shared our kids picked up. I remember coming home from a review committee and saying, "Oh, this was the most awful proposal. I couldn't stand it. They said, 'Bah, bunk, bunk, bunk,'" and one of my youngsters said, "What they should have done is..." and he was absolutely right about everything!

Constantino: One of the kids--

Robins: One of the kids picked it up and knew exactly what was wrong. And I thought, I wish--

Constantino: They're quick studies.

Robins: --I said, "I wish I had had your proposal to review!"

Constantino: So let's talk about that for a minute too, sort of your role as a mom and your experience with the boys. How did that influence or shape your thinking about child development and your studies in children?

Robins: Well, I think my doing the work I did was good for them, because I had four very active boys who messed up the house a lot, fought a lot, quarreled a lot, didn't always do what I asked them to do, and sometimes I'd get very angry about that. But then I would think, you know, compared with the kids I've been reading about, they're really okay. And so I think it sort of soothed me a little, so I didn't expect perfection. I thought they were really pretty good.

Constantino: Has it ever seemed to you that researchers who did not have a perspective on what was just normal kid stuff or the typical range of variation, that they missed something?

Robins: Oh yes. When at Wash U I sat in on a course that Jules Henry was giving. He was an anthropologist and described with horror the sight of a mother feeding cereal to a six week old, pinioning the arm, and scraping the food off of the face, and spooning it back into the mouth with the spoon as though this was an act of terrible aggression! And I thought, has he ever tried to feed a six week old? How else can you do it?

Constantino: That's the way it's done.

Robins: Right.

Constantino: Right, right.

Robins: No, I think there is a good deal of that. I think the example I gave you of the doctor who thought that this child was abnormal because he wanted to be a lion tamer is an example of that. He thought it was a very bad sign, but didn't really understand this child's background and how he could think that and it would not be a bad sign.

Constantino: Right. And do you think that for young researchers in child development, from what you've seen, that there are adequate resources for them to learn about or to at least be exposed to normal development in the course of how they're taught or trained or developed as researchers?

Robins: I really haven't had responsibility for doing that, so I don't know how much they get.

Constantino: But it's a good idea for them to have--

Robins: Well, Hugh (my current husband) has a daughter who is a social worker who has worked with very disturbed children, and I've watched her with her nieces and nephews -- she has no children of

her own -- and she's fabulous with them. You don't have to be a mom to learn how to handle children. She's wonderful with them.

Constantino: Right, right, that's great. How about your own sort of personal interests over time, you know, the things that you'd like to do on your -- what leisure time that you had--

Robins: Oh, I had plenty.

Constantino: --and your travels and, you know, friendships and things? I know, for example, also that you were diligent and very involved with maintaining the connections and opportunities for Eli to interact with his colleagues at Washington U, and if you could say a little bit about that in terms of the personal side of that and how that's been an important part of your life and career.

Robins: Oh it has. When we first came to St. Louis we met a couple, where the husband was in economics, who had the idea of setting up a group. He often invited people for Sunday night supper, sometimes people that he didn't know, but whom he thought would be interesting. And it worked very importantly for almost all of us. We all became very close friends. The strange thing is we became better friends with each other than with the host, which I don't think was what he had in mind.

Constantino: Not exactly, right.

Robins: But through them we met people in a lot of different fields, particularly in literature, and some in economics. And we liked that very much and found it very interesting. And so we cultivated friendships outside of medicine, and found it a great diversion and fun. And when Eli got so ill that he found it difficult to go out we started having a salon once a month. This solved many problems for me. I always wanted to ask people over but I didn't get around to it, and so we set up an open house on a specific night once a month and I didn't have to invite them every time. They just came. We asked a lot of people we had met in various ways, hoping that they would become close friends as we had, and it did work but it took a longer time than I expected.

Constantino: You mean for the group that sort of stuck with it and--

Robins: For the group to go beyond the friends they already had and make good friends with other people in the group. But it did work in time and amazingly enough, although Eli died almost 12 years ago, the salon is still going.

Constantino: It's still going. Right.

Robins: I thought it would stop. I mean, the whole thing was invented just to give him a social life, but our friends love it and they keep coming.

Constantino: That's fabulous, that's just wonderful. Do you -- aside from how fulfilling it is, and interesting, and important personally, has that influenced your scientific career do you think?

Robins: I don't think so.

Constantino: You think of it more as--

Robins: My cooking career, yes.

Constantino: --your cooking career? That's great, that's great. So Lee, in terms of recording this oral history are there other things that we haven't gotten to that you would sort of like to remember to document?

Robins: Well, there's one thing, which was a result of the study I was doing, but didn't come to a happy ending. I got made head of a committee to try to revive the Municipal Child Behavior Clinic that I had started. It was a city clinic. And I worked on it for a long time and with a small committee, one member of which was Mrs. Gildea, Dr. Margaret Gildea. And we got along fine and wanted the same things for it, but the city was not willing to do anything for it. We almost succeeded. The city agreed to reopen it and then we had to find a head for it. We found a very good person, and he was willing to come. He came to visit and everything was fine. But they insisted he had to live in the city, and he didn't want to do that, because he wasn't being paid well enough to send his kids to private schools and the city public schools were terrible. And they would not bend. We tried every which way to try to get them to make an exception for him and they wouldn't. And then they hired another guy themselves who had no real investment in the idea. We had plans for it that I think would have tested a lot of the ideas we've talked about. The clinic was going to take kids after school who were having problems in school and give them the kind of environment we thought they needed, which was lots of loving care. And we had--

Constantino: So it was sort of a secondary prevention model that already were identifying children who were--

Robins: --we were taking kids who were in some trouble but not yet serious trouble. We were planning to take very young kids, first and second graders.

Constantino: Well, almost the situation of your African American children from the study who were being held back and that were a high yield sample.

Robins: And so we had lots of plans and at many times it looked like it was going to go, but the city wanted to get out of any kind of health activities. They were closing the City Hospital and--

Constantino: --what years was this occurring?

Robins: I don't really remember exactly when it was. It must have been probably late '70s or early '80s, I'm just -- oh it certainly wasn't early '80s, because that's when we were doing the ECA. It was sometime in the '70s.

Constantino: And do you think that it wouldn't fly, you know, thinking about how to actually make something like that happen, do you think that it would require the resources of either municipal funding or school funding as opposed to, you know, research funding to make such a thing go? Or do you think that it--

Robins: Oh I think you couldn't do it as a research project.

Constantino: Too expensive?

Robins: Yes, and you can't do it very short term. It takes a while to see the results.

Constantino: It would never look good on a grant application, in other words, nowadays?

Robins: Maybe you could write one, but I haven't figured out how--

Constantino: Well, I'll let you know and I'll let you review the drafts for sure.

Robins: We had a perfectly wonderful woman lined up to run the school project and it just never came to pass.

Constantino: So this is something that you would like to see happen or that you would have liked to have seen it--

Robins: Oh I'd loved to have seen it happen.

Constantino: --right. And at the time--

Robins: I really am a do-gooder at heart. I wanted to solve these problems.

Constantino: Well, at the time did it seem that there would be a good way to track these kids longitudinally to follow them closely over time?

Robins: Well, we were going to try to bring in the mothers as assistants so that they'd learn how it's done and learn how to treat the kids.

Constantino: Right, and also to have employment for themselves?

Robins: Yeah. Well, we really hadn't gotten that far in terms of talking about that but we -- the idea was to influence the families to--

Constantino: Right. Well, you know, and there is a sense to which -- and it varies by daycare center -- you and I have had conversations about this before, that even in the situation for high quality daycare centers that serve children at risk, that the kids are dropped off there and then picked up at the end of the day, but there isn't the sort of involvement that translates what goes on, you know, in daycare to what goes on at home. And--

Robins: Yeah.

Constantino: --but you had this idea to--

Robins: To try to get the mothers there for enough time so they would observe how the kids were handled and observe how the kids could behave in a group, which might be very different from the way they behave at home.

Constantino: Right, right. So any other things in terms of thinking back on your career and the history that we're recording here that you think would be important to add?

Robins: No. It's been great fun.

Constantino: Well, we've covered lots of topics of an illustrious career. I'll add to the end of this because it can be deleted if one wants it to be. But I remember my own entry into the field and this -- you know, it was sort of -- is irrelevant compared to recording the history of Dr. Lee Robins. But the way that I got into the field was -- and I forget if it was a phone call or a brief letter to Lee -- I had finished my training in New York and told her that I had read her work and that I wanted to do something about conduct disorder, I wasn't sure what, and was there any way that she knew that I could enter the field and get myself going and, you know, the door was immediately open, you know, not only to be invited to come to Washington U but, you know, ultimately -- you know, first for an interview, but all through those early years -- and observing this not just for myself, but for other students and colleagues, that Lee was always available, always open, always thinking, always, you know, receptive and open minded and consummately positive and encouraging and--

Robins: Thank you.

Constantino: I don't think that I would have been able to do a tenth of the things that I did without that kind of a start, so I'll always be very grateful for it.

Robins: That was very nice of you, thank you.

Constantino: Well, it's the truth, and that's aside from even the science and all of what the scientific contributions did to provide a base for which to, you know, continue. And we will continue, so we're very grateful and, Lee, thank you for the time and this wonderful history. And I'm sure this is going to be something that will be treasured by the Society for Research in Child Development.

Robins: Thank you.

Constantino: My pleasure.