# Rosner, Judah L. 1999

## Dr. Judah L. Rosner Oral History 1999

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Judah L. Rosner, Ph.D.

This is the first interview in a series on the career of Dr. Judah L. Rosner, NIDDK. It was conducted on 17 December 1999, in his office on the third floor of Building 5, National Institutes of Health, Bethesda, Maryland. The interviewer is Dr. Buhm Soon Park.

Rosner: Part of your job is to look backwards and to look forwards, and then you... We've been so privileged to live during a period when genetics and molecular biology just took off and has come into such fruition that now, today, every company is named Gen or Bio. And congressmen know about it and people are subject of *New Yorker* interviews and articles. So scientists are slowly moving, quickly moving, from being people who were just considered as quiet little strange people who worked in some dark place and had some interest in something which nobody could relate to really, to people who are now part of the popular culture almost.

Park: A kind of icon.

Rosner: Kind of an icon. And it's slowly changing, because up until recently, in popular culture, the scientist was seen in two different ways, I think. If you watch the Saturday--I don't know if you have children, but if you watch Saturday morning comics, so there are two scientists. There's one scientist. He's an older guy. He has a beautiful daughter. He has--somehow or other, he doesn't have a wife, so he's kind of asexual. He has gray hair and he just sort of is some brilliant guy who thinks about things and has come up with fabulous ideas, but he doesn't have any social sense, he doesn't have any business sense. He's just this dreamy guy.

Park: Einstein.

Rosner: Yeah, Einstein kind of a guy. And some crooks want to steal his thing very bad reasons, and then some good, handsome guy has to come along and save him and the daughter and the science. So that's one. Then, the other one is a scientist as the evil guy, as the wizard, as the guy who has the ability to do terrible things, and he's going to control the world, you know, the James Bond kind of scientist.

Park: The Frankenstein.

Rosner: The Frankenstein, that's right.

Park: That's one of the popular images of scientists.

Rosner: And I think those are two opposite images and neither one looks at the scientist as a real person. They look at the scientist as sort of an extreme kind of strange person who doesn't really live in the ordinary world that most people live. And this is slowly beginning to change, partly some popular books. I mean, even if you think about it, the Harrison Ford movies where he's an anthropologist or an archeologist...

Park: Indiana Jones.

Rosner: Indiana Jones. Even there, he's a scientist, but now he combines this muscular, good-looking aspect so necessary for acceptance in popular culture.

Park: Right.

Rosner: But if I can take a hint from here, going back to my education, you asked who influenced me to become a scientist. I was raised as an Orthodox Jewish person in an Orthodox Jewish family, but at an early age, I decided that I didn't believe in God. A lot of it had to do with the fact that some things that were described in the books that we read just had no relationship to reality. So I had this big conflict in my life between, on the one hand, religion, which was the world to come, and the other hand was the real world of science, and science seemed to satisfy. Science seemed to give answers. Science showed how, step by step, you could understand something, and that really appealed to me.

Park: And your secondary education period?

Rosner: In early, I would say before second, I would say in elementary school already, I was interested in science. And I can remember several very crucial events. One was, my sisters were older and they worked as secretaries for the Metropolitan Life Insurance Company, and they brought…they knew I was interested in science. Metropolitan Life Insurance Company at that time produced a number of little booklets on Louis Pasteur, Robert Koch, Samuel Weiss, Lister, all of the great people in bacteriology, infectious disease.

Park: Yes, right.

Rosner: And they were these pastel-colored books on the outside, but inside they were readable, even for somebody who was maybe 12, 13 years old and they were very interesting. And I think... I have looked back and I've realized that that was one of the earliest seeds in my interest in microbiology and bacteria. In fact, there was a woman that gave a lecture at the NIH, and I'm really sorry I couldn't go to her lecture. She was a science historian from Pennsylvania, and she talked about, according to the topic of her title, she was talking about Metropolitan Life Insurance Company and how it had some influence on these things. I was planning to get in touch with her, and somehow or other I lost the piece of paper and it didn't happen. So I think that was one important thing. And then I read *Arrowsmith*.

Park: *Arrowsmith*, yes.

Rosner: Very important.

Park: Yeah, very important.

Rosner: And then another one by DeKruif and those two books together, again, really affirmed sort of an idea of studying bacteria. It was really fascinating. It was really very interesting.

Park: And also studying biology on the basis of physical chemistry.

Rosner: Well, at that point, that wasn't there so much. There was more sort of from a medical point of view, looking through a microscope, slides, staining, seeing which bacteria was present someplace, can you connect this bacteria to this disease. Snother important person, a very important person, was my brother.

Park: The name?

Rosner: His name is Benjamin Rosner, and he was almost 10 years older than me. He was kind of an intermediate father for me, and my real father was this very religious person, very kind, very good person. My brother was much more practical and he was very much interested in education, and he rose to a very high position. He was the dean of the School of Education for City University of New York, and then later at Temple University in Philadelphia.

Park: Did you grow up in New York City?

Rosner: We grew up in Brooklyn, yes. My parents were immigrants. I remember one day--my brother was already in college, and he was, when he first started college, he was pre-med. He very quickly changed to education, and that's what his life interest became. But while he was there, he had to take some biology courses and he had to take some first aid course, so he had this first aid book at home. And I remember very clearly, one day he explained to me how breathing occurs, that when you take a breath, you expand your rib cage and you lower your diaphragm, and by making that extra volume, you cause air to come into your lungs. It was such a startling revelation to me that there was a physical basis to something that we did automatically, because back in the '50s, and probably for most people today, you don't think of yourself of being composed of cells, of something running around in your blood, of your heart beating, of being part of the physical world. Your body is something different. Until you're much older and you start going to doctors and they start telling you this, what hurts you is your liver and what hurts you is this, you have no concept of all of those things. You know if you get stuck, you bleed, and if you run fast, you get out of breath, or maybe you have a headache, so you know pain, you know touch, you know sensations. But you don't really have any idea of what's inside your body or how it works. And I remember what a revelation this was, that there was something, just a simple physical process of expanding the volume that caused the air to come into your lungs, and that was a very important point for me.

Park: Yesterday, I _____ my daughter _____ the kids' program, and there was an actor--I don't know whether you know the actor _____--and that program was designed to show how lung is operating and _____.

Rosner: Yes.

Park: And the school kids all of a sudden became very _____ take a look at _____ and have some problems. And my daughter is only three years old and she asked me, "What is lung?" and I said, "It's inside." I couldn't give a correct answer. But I--this is physical basis and things like that.

Rosner: Yes. Actually, you reminded me that my, back in those days, the housewife would get a whole chicken. You didn't go to the store and get parts wrapped in cellophane. You got a whole chicken. And it may have been cut up for you by the butcher, but once you came home, she took out the inside of the chicken, and sometimes she would show that to me, showed me the heart or the liver. So you also begin to get a little bit of an idea about those things.

And another important person in my life was my first high school general science and then he was my biology teacher. And he was an Orthodox Jew, but somehow he made it possible for us to think that while, of course, the Bible and creation story have to be absolutely true, that we could also think in terms of evolution as maybe having occurred within the biblical framework. So he made it possible to not have just the total clash, but to see a middle position between religion and science.

Park: When was the time?

Rosner: That was when I was in the second year of high school, so I was 14 or 15 years old then, 14 years old.

Park: In the 1940s?

Rosner: So that would have been 1953.

Park: Fifty-three.

Rosne: So that was also very important.

So there was a lot of emotional involvement, so, for me, the decision to be a scientist was more than just, oh, I'll be an accountant, I'll be a chemist, I'll be a this, I'll be a that. It had a lot of implications for me as a way of leading my life, not just as something I'm going to do earn some money. It turned out I didn't even think about it in terms of earning money. I thought in terms of, well, there must be some way I will get paid while I'm doing this which I want to do.

Park: The search for the truth.

Rosner: Yes. So that was certainly a very important part of why I became a scientist.

And when I went to college, I went to Columbia, and the people at Columbia College--this was from 1956 to 1960--it was a group of people who had been in the lead in biology, say, 10 or 15 years before. They were people who really enjoyed the cell, and my most important influence there was a man by the name of Theodocius Dibjanski [sp.]. And I don't know if you know his name. He was a Russian by birth, back in Kiev, I think, and he came to the U.S. and he worked on the drissophilla [sp.] with T.H. Morgan and the beginning of the whole genetic rebirth with Morgan. And he wrote a textbook. So, you see, I have Albert Einstein next to Martilyn Monroe.

Park: Yeah, very impressive. Two cultural icons.

Rosner: And then I have--this is Monet, the artist.

Park: Painter.

Rosner: Yeah, here's the painter. And that's Hank Greenberg, baseball player.

Park: Oh.

Rosner: Dibjanski [sp.] wrote this book, *Principles of Genetics*. I think it came out in the '50s, '58, and if you look at this book, it has some chemical stuff, understanding about some metabolism being involved in defects. But what does it have about... This is '58, so Watson was 53. Okay. What does it have about... Part of chapter 28, okay, 28, "Organization of Genetic Material." So first it shows chromosomes of drissophilla [sp.], which have very large chromosomes under certain conditions. Wait a second. Probability. Okay. So here is physical structure of the genetic material. So when does DNA get mentioned here? So we have HB 74, it starts with DNA, it tells you the basic structure of DNA, shows the double helix. That's it.

Park: Two pages.

Rosner: Yeah. So maybe another page here, something about RNA, polypeptides.

Park: Right.

Rosner: That's it, that's it.

Now, he was actually an interesting scientist. He worked mostly on population genetics. And he was interested in genetics and evolution, and he put together what's considered a great synthesis of showing how genetics and evolution go hand in hand, how, if you understand genetics, you can now begin to understand evolution, and that was one of his major synthesis.

He was also a great humanist, and he always made clear--heredity, what is heredity? What you inherit from your father as a trust of $500,000 is not the same heredity that you inherit from your father through the genes. So genetic inheritance and social inheritance are very distinct kinds of things.

He was a very interesting, very _____ man. And I think a year ago or so there was an issue of--there were some reports about him in PNAS where somebody went through and talked about his life and was dedicated to him. He was a wonderful man. _____.

So I came under his influence, but he was still an old-fashioned geneticist. And then there were embryologists, cytologists, physiologists, but still a little bit more connected to cells as understanding life through cells, but not through understanding of the cell through chemistry.

And when I left Columbia to go to graduate school, I went first to Yale to work with another drissophilla [sp.] person, but I found very quickly that my interest was growing in bacteria and the new bacterial genetics that was coming up. And Joshua Letterberg [sp.] wrote a book, a collection of important papers in bacterial genetics, and that book was very important to me.

And then, just very shortly after that, the man who I went to work with wrote a wonderful collection of papers, this man, Edward Addleberg [sp.], at Yale, of papers on bacterial genetics, and this was just the beginning. So '51 was the paper by Letterberg [sp.], and this then was about how we know what the nature of mutation is, the nature of infection with viruses, the nature of bacterial mating, where two bacteria come together and DNA is transferred from one bacteria to another, and that was basically Letterberg's [sp.] major discovery. It really opened up entirely the way to manipulate bacteria so that you could understand the nature of genetics, so you could move genes from one cell to another cell. That was very important.

So, if you looked back, you could see how you can trace a series of mentors and of developments that occurred at a particular time, sort of tended to channel interest in a particular direction.

So I left the drissophilla [sp.] laboratory and I went to work with this man, Addleberg [sp.], who was over in the medical school in another department, the Department of Microbiology, and that's where I got my Ph.D.

Park: I see, under him.

Rosner: Under him. But, in fact, he was--I hadn't completed my degree, and he was going to go away for a year on a sabbatical in Paris, and I met a man by the name of Michael Yarmalinski [sp.], and Yarmalinski [sp.] had just moved to the NIH, to the Laboratory of Molecular Biology. And he invited me to come and finish my degree in his lab.

Park: In his lab.

Rosner: At the NIH, and then stay on and do a postdoc with him.

Park: I see. When did you come?

Rosner: So I came here in 1965.

Park: Sixty-five.

Rosner: Yes. So it was, I think, September of '65, is when I came here, and I started to work with Michael Yarmalinski [sp.]. And then he subsequently left the NIH. He went away to Paris for a few years and I stayed on at the NIH.

Park: In which section?

Rosner: Okay. So, Michael Yarmalinkski [sp.] and Bob Martin [sp.] were the two main people in Bruce Ames' [sp.] section.

Park: I see.

Rosner: Bob Martin [sp.] worked very closely with Bruce Ames [sp.]. Bob Martin [sp.] did most of the biochemistry; Bruce Ames [sp.] did most of the genetics, and they had a very, very fruitful collaboration for many years. They wrote scores of papers on histidine biosynthesis and the genetic basis of it. Many things came out of that lab.

And then Bruce Ames [sp.] left, and I don't know--I would say the late '60s, early '70s, about '69 or something.

Park: When Dr. Davies told you _____ Gordon Tompkins' [sp.] lab.

Rosner: Right.

Park: And Bruce Ames [sp.] said, "Well, it's time for me to leave."

Rosner: Right. So he went to Berkeley. Bruce went to Berkeley. And then Bob became the section head. So then I was--I then became an independent worker in that section, but I didn't really have very much to do with Bob Martin [sp.] at the time. It's only been about 10-12 years ago that Bob, who had after a while stopped working on bacteria and started working on SP40, did a lot of work on SP40 and... He was interested in origins of replication of chromosome in cells. And then he decided that he couldn't really keep that up and he had an interest in writing plays. So he went to part time and thought if he's going to be part time, it would be better to be associated with me and to work with bacteria, because that's something you can do faster. It doesn't require a lot of help to maintain the tissue-culture cells.

Park: Writing plays?

Rosner: So, he has written a number of plays, at least two of which are directly related to science, and he's now finishing a huge work having to do with a Baltimore case. So when you speak to Bob, you'll have to devote a lot of time in talking to him.

Park: Well, this year, one of the prizes for the History of Sscience, in the History of Science Society, goes to a historian who deals with a Baltimore case.

Rosner: Which one?

Park: It's written by Daniel J. Kevelross [sp.], who is a professor in Cal Tech and who deals with, who describes, who traces history _____ Baltimore case and the NIH.

Rosner: Well, you'll get another point of view from Bob. He'll give you another point of view. He's followed that case very carefully.

Park: Oh, really? Interesting.

Rosner: But Bob has written--he wrote one play called "Experiments," having to do with the question of how you make a decision as to what is right and what is wrong. So this was a long time ago, and it was performed by some amateur groups, having to do with a question of whether somebody had found some virus that caused cancer, and was it proved or not proved. And then he wrote more recently one called "A Stampede of Zebras," which is really fabulous. It's been produced by a number of college drama departments and it's been read and used for many ethics, biological ethics classes, because he takes a laboratory, a fictitious laboratory, where there's a whole chain of command. There's the head laboratory, the head investigator, then his major postdoctoral fellow, who really runs the lab, and the postdocs and all that, and takes an issue of a finding that they had made and they had published and which was very exciting and new and was going to be the basis of getting more funding and proceeding on very important work, and a new postdoc comes to the lab, and her job is to reproduce the experiments to start with, and she can't reproduce the experiments. And, of course, she's blamed because she's stupid, she's new, she's doing something wrong, and no matter what happens, she can't get it done. And then, to make the story short--he'll give you a copy, I'm sure; you should ask for a copy of this--the main guy, the one who runs the laboratory, a senior postdoc, decides he has to go and do the experiments himself, and he has to go to the freezer and to get out his old samples and to reproduce the experiment, that it's very important. And somebody has pulled the plug on the freezer, and all of the samples are ruined. So now the question is, who did it? Who's responsible for this? And then it's--what I think he points out in this play is that, how each person in the laboratory has a certain vested interest in what the results of the experiment are. So the main investigator says, "Look, if it's not for me being able to go and sell these results and get grants, I can't keep the lab going, I can't offer you fellowships and scholarships and positions." And the guy underneath, "Well, I have to go on and move on to my next job, and I want to get a good job. I've done this great work, and why can't you get this stuff done?" So he sort of shows each person in the lab their particular motivation and how each of them could possibly have a reason for the experiments not working or for trying to make the experiment work, or did the experiments ever really work in the first place. So this is close to the Baltimore case. This touches very much on the Baltimore case. So it shows how there are a lot of forces going to cause pressure in the laboratory as to what is to getting the experiments to work.

Park: You know, there are lots of case studies of scientific laboratory in the universities and industry, and not many of the laboratories in the government, especially on NIH. There is no case study.

Rosner: Really?

Park: And so, I'm really interested in how the laboratory has been _____ and in what components, you know, something, that there may be social layers and administrative, director, or lab branches, _____ and postdocs and, as you mentioned, the experiment means differently to each person. And I am particularly interesting in how, to what extent NIH laboratory is different from the university laboratory, and did you have any first impressions when you came here in 1967?

Rosner: That's a good question.

Park: Just coming out of your Yale University laboratory? And what was your, you know, first impression?

Rosner: Well, at that time I was still actually a graduate student.

Park: Right.

Rosner: But I would say--and I think it is still absolutely true today--that, in the labs that I have worked in... Well, my career has only been in the Laboratory of Molecular Biology, so I've been here from 1965 till today. But I think it's true in the other labs that I know about. If you have an idea, if you are a summer student, a high school summer student, and you have an interesting idea, your idea is thought of seriously, so that you are immediately a scientist. A scientist is not necessarily your degrees or your qualifications or your experience. A scientist is a person who is thinking about an experiment and who has a critical mind about it, and who has a creative mind and starts to think about variations and how could we find this and how could we prove that. And as long as you are talking, thinking seriously about the science and you talk about it, what you say is accepted and dealt with just as equal as anybody else.

Now, if it's a question of opinion, of course, you know, somebody more senior and who has more experience, of course, can say, "No, this will never work. You don't really understand what goes into it." But in terms of saying something, if you say something, if it's intelligent, it's appreciated. It's not, "No," and it's dismissed.

So I would say that immediately you're a scientist. You walk into the laboratory, you are immediately accepted as a scientist. And you go to a seminar and anybody can speak, anybody can raise a criticism of an experiment, every... And that's definitely true about this laboratory, it's true about the Molecular Biology Laboratory as I know it. I don't know the more clinical laboratories. I just don't have any experience. But if I go to any seminar here at the NIH, I feel that I can ask a question.

Park: So, as compared with the university, this is less authoritarian or...

Rosner: No. I wouldn't say it's less, but I would say they're both non-authoritarian, in my experience.

Park: Uh-huh, I see.

Rosner: You know, we understand, we hear about the German kind of university system where nobody stands up until the professor stands up, and nobody asks a question until the professor asks the question.

Park: Actually, I grew up in that.

Rosner: Oh, is that right?

Park: I came from originally Korea, and in Korea, only professors can raise a question. The students cannot _____ sitting behind and see what the show is going on.

Rosner: Right. Very different. I think it's completely different. Anybody can ask a question.

Park: Right. So, at Yale and at...

Rosner: So, at Yale and...

Park: You felt kind of continuous, continued.

Rosner: Yes. It did not seem different; it did not seem at all different.

Park: So, you may feel like coming to another university.

Rosner: It feels like a university campus. And in the early '70s, when I started to look around for maybe taking a teaching position, and there were lots of opportunities in those days, I would go to a university and maybe the first person who I would be introduced to would be the provost, and the provost would ask me, "What size grant will you bring to this job?" And I realized that at the NIH, I was in a more academic institution than the universities, so the university was so much more subject to the pressure of money, whereas at the NIH, we don't talk about money. And in this laboratory, which is maybe different than others, the budget is not broken up into each section and to each individual. So the laboratory gets a budget and nobody is told, "You can't buy this chemical today because I need to buy some."

Park: So, flexibility within...

Rosner: Great flexibility, great flexibility, and a great lack of authoritarian. So it's the section chief. The lab chief doesn't say, "You must do this," to the sections. It's all on the basis of equality. And after Gordon Tompkins [sp.] died, then it was a rotating lab chief. Every year, there was one lab chief change. And the chief did not have any special advantage.

In other labs at the NIH, the lab chief is very important, determines everything that's going to happen in his lab.

Park: I see.

Rosner: So this lab has always had separate individuals who had small labs, like myself, at most one postdoc with me. Many times I worked by myself. But even Kawazawa [sp.], who at most had three people working with him, if he went to Japan, he could have had 20.

Park: Right.

Rosner: Easily, very.

Park: I was very intrigued by the fact that the NIH senior investigators do not have Ph.D. candidates. I mean, do not have _____. They have some postdocs, not many.

Rosne: Right.

Park: And so, probably some of the scientists here may miss a candidate in laboratory because of that, you know, producing a lot and publish a lot and get in the limelight in the society. But, as you said, here, it's _____ running a small scale, but it's very satisfactory _____ every time. In the university, if you're a professor, you never _____.

Rosner: Right. And that's something which, unfortunately, the academic world doesn't understand. So if you are in the academic world, you get a grant for $3 million, the chairman of the department will give you anything you want. And you can have as many students as you can get, as you can pay for. Here you're limited. You're limited by space, you're limited by your director as to how much you can have. So there's much more... So we enjoy the fact that we don't have to spend six months of the year writing grant requests, but we have the down side that we don't have the unlimited resources of a grant. So, people here, many of the people who are lab chiefs, up until recently, mostly all of them worked in the laboratory. Kawazawa [sp.] worked from 9:00 till 5:00 and then came back at 8:00 until 1:00, and he wasn't shuffling papers. He was doing experiments. Whereas...

Park: _____ doctoral students.

Rosner: Yeah, yeah, exactly, exactly, exactly. If you see Bob Martin [sp.], he'll tell you he comes in the morning, 6:00, and leaves at 4:00 or 5:00, and he'll be here... He went on a trip to Venice for vacation. He came back Friday, Saturday he was in the lab; he was in the lab Sunday. He was in the lab Saturday, he was in the lab Sunday. And he made a number of new oligonucleotides. He synthesized new genes to start experiments going.

Park: It's not because he was under pressure of getting tenure or...

Rosner: Nope. Not under any pressure. Just, he wants to do the work.

Park: Oh. I think it's _____ different from academic atmosphere where each lab has a professor and the professor has postdocs and the Ph.D. candidates and _____ laboratory and some hierarchy.

Rosner: Right.

Park: But here, I find that there is a structure in paper, the laboratory chief and section chief, and the investigators.

Rosner: Right.

Park: But more or less all of the scientists are _____ experiment and _____ on an equal basis and...

Rosner: Right. When I was in graduate school, my professor kept a lab coat on a hook in his office, and when some visitor came, then he put on the lab coat to take the visitor around and show him the laboratory. But otherwise, he never was in the laboratory. He was in his office.

Park: I myself was in the Molecular Mass Spectroscopic Laboratory for my master's thesis, and I came _____ in the morning and optimized _____ and ______. And then the professor just came in to take attendance, who is coming and who is not, and _____ come back again and whether they are working hard or not. It's very under pressure and probably because of that situation, I wanted to study some humanity things, and that's why I became interested in the history of bioscience. But, I mean, here, something, somewhat different.

Rosner: Yeah, yeah.

Park: And did you... You came in late 1960s and you're still here.

Rosner: Right.

Park: And did you find any changing atmospheres or changing morals _____ over the years? Probably the 1960s were different from 1990s.

Rosner: Yes. A big difference was the fact that, because of the Vietnamese War, there were lots of M.D.s or graduate students who came to the NIH to be in the Public Health Service. And, in fact, after I had been here for two years and I had received my Ph.D., I became part of the Public Health Service so that I could avoid being sent to Vietnam. But because of that, we had a large number of people coming through for two or three years at a time who were very smart, very good, hard-working, very interested mostly, and so there was a constant shuffling through in the labs. And there was no problem about getting postdocs. People wanted to come here, for sure.

Park: So the postdoc position was not heavily advertised.

Rosner: No. You didn't advertise at all. There was no need to advertise.

Park: I see.

Rosner: People found this place.

Subsequently, I think then, after that died down, I don't know whether _____ starting, let's say, in '73, '74, something like that, then I think there was more--people were more interested in staying. So somebody like myself, who had come earlier, now began to think, "Well, maybe this is a good place to stay. I'd rather not go on to another university." And then I think in the '80s and the '90s, it became a place which became very hard for anybody to receive tenure. So now, it's a very, very hard road to get tenure. You have to be invited to come, with the understanding that you're on the tenure track. So you couldn't just be some postdoc who shows up for one reason or another, works for a couple of years and says, "Oh, I like this place. I'd like to stay," and your boss says, "Okay, you're a good guy, you can stay." Today, you have to be invited by basically, with the _____ of the director that you're going to be in a tenure-track position and you're an important person, and you have a period of five years or whatever to prove that you're worthy of being in tenure. So things have changed a lot in that respect.

Park: I see. It's 1980s.

Rosner: That was late '80s up to now. So, virtually everybody who comes now as a postdoc is told immediately, "Don't think of staying here. You're not going to stay here, unless you're specifically invited."

Park: How many percentage was invited of that tenure track?

Rosner: I couldn't give you percentages. I don't know those numbers. But very few, just a few of us. And then those people now are invited, and they're immediately given some postdocs, technical assistance, more space, so there are people who are invited with the eye that they should succeed and stay at the NIH. So when I came, "Well, you came, you're a postdoc." Some postdocs like to stay longer. Eventually they became permanent.

Park: When did you get your tenure?

Rosner: Gee, I don't really remember. I would say it was probably in the '70s, early '70s, I would say.

Park: Early '70s.

SIDE B

Park: ...from my daughter's friend's parents, and she described that her husband got a tenure, "Scott got a tenure, Scott got a tenure, Scott got a tenure." He _____. "Scott got a tenure." _____ was a computer scientist. And it took about 10 years.

Rosner: Yeah. And I think during my time, it was almost--you almost drifted into it. You know, at some point somebody said, "Well, let's put Lee up for," you know, let's make this permanent. But, actually--I can't remember now when that happened. But now there are very strict rules. You can only stay as a postdoc a certain number of years. You can get renewed a certain number, and that's the end of it. Then you have to go. It's very, very strict. In those days, well, if you stayed, you said you want to stay, he'll take you _____. Okay. It was much easier, much more relaxed about that. Because there were so many jobs on the outside that you only stayed here if you really liked the atmosphere, because you could get maybe better pay, maybe more fringe benefits, less restriction if you were in in a university position. And very few people in those times went to industry because industry was only... You either went to a chemical company or you went to a pharmaceutical company. So there weren't that many positions. Today, many of the postdocs are going to biomedical, genetic engineering, that _____.

Park: Did you miss teaching?

Rosner: Yes, I did. Yes, I did. I think I missed students more than the teaching, the having people come through, young people who had no bias to their thinking. They thought for the first, from the beginning.

Park: Right.

Rosner: So that I missed a lot. And for many years, I would have summer students come, high school students, college students, minority students. Sometimes they would come in January if they would have a January semester. So I tried to have that kind of contact with students that way.

But that is one of the things that changed a lot, and it's been a little bit reversed now because somebody discovered this idea of pre-ERDA students. So people who've just graduated from college come here now for a year or two years and they work, depending upon the lab, depending upon their abilities and interests and excitement, they can become quickly with their own project or they can just be doing technical work. But, still, it's a new face, it's a young person, a person who asks questions and has ideas that are different than the accepted ideas. So I think that's an important thing.

And I think that Varmus's idea of having a graduate school here was to address that issue, but for other reasons, I disagreed very strongly with the idea of the graduate school. I did not think it was a good idea at this time.

Park: I see.

Rosner: But I, in fact, was a graduate student when I came here. And there still are... I don't know. There's something like, I don't know, 50 or 100 graduate students at the NIH who are working at somebody's lab or getting their degree from some local university or Johns Hopkins, so like that.

Park: The idea of granting a degree from NIH is actually not a new idea. It has been around for a while, and I think that was _____ that was one of the main _____ that idea.

Rosner: That's right, a long time ago, in the '60s.

Park: In the '60s. Did you see it coming and going ____?

Rosner: Yeah. I read that _____ lecture he gave talking about the possibility of having a university at the NIH, and at that time I was very excited about that idea. I'm not excited about it here today because, first, I think there ought to be universities who do the work, is not a great need for many, many more Ph.D.s. Ph.D.s have a hard time finding jobs now.

Secondly, the graduate school is more than just being in somebody's lab and having a mentor. It's being able to go to the engineering school and go to a lecture. We don't have any engineering school here. We don't have any chemistry school here. We don't have a mathematics school, we don't have the arts or literature, we don't have science here. So, it's not a university. It's a narrow, it's a very narrow, narrow kind of place. And what they were talking about, like bio informatics, that's very, very narrow kind of... Maybe it's what's going to happen in the future. I have my doubts about it. But, so it's a very, very narrow kind of thing, so it's not--so it's going to affect some very few labs. So they can still have a few graduate students come in.

Why would I want to teach a genetics course to somebody who isn't going to come back to me? You know, in the university, people teach courses and they get students through their courses. So, but if somebody's going to be _____ or is what they call translational science, you know, bringing science to the clinic, so I'm going to teach somebody bacterial genetics and never see them again because they're going to go into something completely different. So, from either...

So, I think there are lots of reasons. The most important reason, I think, is the first one, that this is not a university. This is not a campus of students, lots of different crosscurrents. That's what a graduate student should get. It shouldn't be _____ locked-up laboratory working on this narrow experiment.

Park: Well, after one or two years in the graduate school, you are just _____.

Rosner: Yeah. That's true.

Park: One topic and one laboratory and one mentor and not many projects.

Rosner: That's true. So when I came here, so I was already in that state, so I didn't miss it. But being in a graduate school, being on a campus at a university, you just soak in some different kind of attitudes. Maybe some student in your dormitory or married student housing or whatever, you know, says to you, what does your work have to do with helping people? Or aren't you creating... You know, you have opportunities for different ideas, different discussions. Here, it's so homogeneous. I think it would be... I'm a big believer in liberal education, in broad education. We specialize enough.

Park: History _____. Yeah.

Let's go back to the science side. Could you describe how the revolution of molecular biology took place and how it affected the study of bacteria and the other parts of _____ in the 1950s and so on?

Rosner: Sure. Well, I guess the major thing that is well known is the physicists that came in and that idea about what is life from Edward _____.

Park: Did you read it?

Rosner: I have the book. I've seen parts of it. I haven't sat down and read the whole thing in a careful way. But he was responsible, I think, for getting physicists interested in biology. And the physicists brought a different viewpoint to biology. They believed you could make an experiment, you could either, it's either this way, it means that, if it's that way, it means this, and that you could draw conclusions from your experiments.

Much of the thinking, I must say, when I was in college, even though it was very well known and very good professors, was a little fuzzy. I wasn't quite sure how, what the meaning of any experiment was going to be. There was always the attitude of, well, life is so complicated. The biological system is so complicated, you can't ever really pin something down. There were just too many variables. And the physicists said, "No. We're just going to think of things, it's simple. It's either like this or it's like that. And we're going to design experiments and we're going to answer questions. And then we're going to walk away with a piece of information." And I'll tell you...

I made an observation. This business of being able to come to a hard conclusion has really in a sense gone overboard in the sciences. And I showed you Watson's book, and the chapter titles were declarative sentences. So, conclusions. So, "bacterial cells do not have nuclei." "Bacteria grow under simple, well-defined conditions." "E. coli is the best _____ growth _____." "Even small cells are complex." "Cellular proteins can be displayed on _____." "The anatomy of E. coli." So, most of these are sentences, and they are conclusions. If you are high school student or a college student, phage form plaques, phage also mutate, phage do not grow by gradually... Conclusions, conclusions, conclusions, conclusions, conclusions! You don't have to read this stuff. You just write down all these conclusions and you pass the test.

So, this kind of _____ favors the... I call this assertive sentences. Okay? And I notice that, starting in the--let's see, I can't remember when it was; in about 1970, there began to appear as the title of the paper, of papers now, assertive sentences. So the title of the paper is...

Park: Very interesting.

Rosner: "Azide-resistant mutants of E. coli alter the _____ protein." Okay? This is a fact; this is a fact. You can't argue with this. It says so. So, you can read this...

Park: Thank you very much.

Rosner: This has, unfortunately, got very, very condensed because of the size limitations for *Nature*. And I have some beautiful graphs. Bringing you this idea of, you get conclusions from science, and what I dislike in the paper is that when you put it in the title, it sounds dogmatic. Okay? And science is the opposite of dogmatism. Science, you never want to say, "This is the way it is." Your experiments lead you to a conclusion. Your experiments could have some problems. How you measure could have some problem. Your interpretation could have some problems. There could have been some contamination. There could have been this. You always tend to... It's a difference from religion. Religion, you know exactly. This is what God said, this is how it is. So we're back to religion and science.

Park: Right, right. That's very interesting. And I think in some way it has to do with Watson's _____ of doing science.

Rosner: Exactly, exactly.

Park: Very assertive, and I'm the only one who knows truth, and a kind of preacher's _____. It's similar to Linus Pauling chemistry. I don't know whether you're familiar with...

Rosner: No, I'm not.

Park: Linus Pauling. He said that this is *the* nature of the chemical bond.

Rosner: Right. It's the nature of the chemical bond. Right.

Park: And things like that. He knows everything and nobody can challenge him. And in my paper, I deal with that problem. But in some way, _____ similar things in science. And, as you said, science is more subtle.

Rosner: Right.

Park: Subtle than the conclusion.

Rosner: Right, because, in fact, what you learn from a lifetime in science is that you get some kind of a beautiful image of how things work, but it's not true. It's a nice way of thinking about it, but it's just--we have words, we have pictures, we have boxes, we have diagrams. That's what's inside my head when you ask me E. coli. I think of certain... But that's not what E. coli is. That's something inside of E. coli, and that's not how things are happening.

Park: Right.

Rosner: So, I mean, most mature scientists know that, that it's the latest formulation of the best way we can understand something, but tomorrow it's changed.

Park: Right, right. But the history of science shows that.

Rosner: Exactly, exactly. Exactly.

Park: Newton's law does not, cannot be applied to the microscopic, atomic world.

Rosner: Right, right, yeah. But it's very difficult today. There is so much arrogance because there are so many great discoveries that have been made, and the techniques are powerful and the instrumentation is powerful that people fool themselves into thinking that they have proved something.

Park: Using the jargon in history of science, we call it is "black-boxed" in instruments. We never uncover the _____ instrument. We just trust the instrument, and the instrument produce something and _____ follow. But when the instrument was originally designed, there might be a lot of discussions and debates over whether it can be trusted _____. But after _____ done, it's just a black box. And that is a _____ black box.

Rosner: Well, sure, sure.

Park: In terms of using, in terms of physicists' interest in biology, molecular biology, this is the Lab of Molecular Biology, and there may be a physicist like Dr. Davies--he was a former physicist--_____ and chemists and biologists. And how did you find communication _____? Or did you have any _____ because of the educational backgrounds?

Rosner: Well, you knew that there were differences, but at that time we all went to each other's seminars, and there was a possibility that you could understand every seminar.

Park: How could you?

Rosner: Well, I mean, not necessarily, of course. Somebody was talking about, I mean, if David was talking about something that's purely x-ray crystallography, I haven't got the slightest idea what it means by this value or that value. Is it big? Is that good? Is it small? Is that good? But you had some basis. You could understand, you could go to a seminar with some expectation of understanding things.

Park: Learning something.

Rosner: Learning something and discussing it and being critical. So the Laboratory of Molecular Biology had, every Friday, had seminar, and it was a seminar, either an outside speaker or given by somebody within the laboratory

Park: Still today?

Rosner: No, no. It stopped.

Park: It stopped?

Rosner: That's a big change.

Park: When did it stop?

Rosner: Probably about 15 years ago. I'm not a good historian. I don't know dates; I don't remember when things happen.

Park: The 1980s, something like that?

Rosner: Well, maybe earlier, maybe even earlier. I would say part of it happened when Gordon Tompkins [sp.] left. Gordon Tompkins [sp.] was a bifunctional catalyst. He was the kind of person who could grab you. He would meet you and he would say, "Oh, you're interested in this. I was just in Chicago and I spoke to somebody, and you should talk to this person." He was a great connector of people and ideas and things. And he understood mathematics and he understood genetic regulation, and he could go to any seminar and understand what was going on. And people like Bruce Ames [sp.] and Marty Gellert [sp.], Gary Felsenfeld [sp.]. Gary Felsenfeld [sp.] was more chemical at that point, but he's become more biological. But between the group of people, there was a very broad understanding. So the seminars would be very lively. There would be arguments and questions and criticism.

Today, if I go to a crystallography seminar, the guy shows me a picture with lots of beautiful colors, that's it. I have to leave. I have no real way of thinking, it's right, it's wrong. You have to be a crystallographer to look at the data and look at the numbers to... And, similarly, even within your own field, you read a paper that's a little bit different from what you're doing, and there may be different techniques, and you're not confident, really, to criticize, to critically evaluate everything that comes up. So because...

And now there is so much powerful ability to find out certain things that, you know, everything is with a kit, everything is with a protocol book, so everybody's, anybody can take any gene and can clone it and can sequence it and put it into this organism, into that organism. You could be a high school student and do it. You don't even have to understand what you're doing and you can do it. So anybody can do miracles today. And behind that is, then, a tremendous power to do things and to understand things that you can develop a whole area where somebody else has no idea what you're doing. I mean, the number of factors that are out there now, how many transcription factors are there, how many cytokines are there, how many different proteins that people work on, different organisms, that there's such a huge amount of information...

Park: Right, right.

Rosner: ...that it's hard for any one person to be competent in all of these different areas. It's hard to follow, let's say, transcription in E. coli, let alone transcription in other bacteria, let alone transcription in yeast, transcription in mammalian cells. Each has huge complexities around it. It's very hard to follow. So we're now very segregated. So Dr. Felsenfeld...

So we don't have any lab meetings anymore. We used to have, every Friday was seminar, every Tuesday was journal club, and journal club was, somebody would discuss something in journals, or occasionally they might present some of their own work maybe. But if I give a journal club on bacteria, the crystallographers aren't going to come, the eukaryotic people aren't going to come, the chemists aren't going to come. So we have all become much more specialized.

Park: I see. When do you think was the critical point, the period or year or event... You said that Dr. Gordon Tompkins [sp.]...

Rosner: Yeah. After Tompkins [sp.] left, it started to fall down, I think, especially after Tompkins [sp.] and Bruce Ames [sp.] left.

Now, I was very lucky in that we had, from the very early time I was here, from '66 or '67, we had something called the lambda lunch. It was the first one of these--what do they call them now--interest groups. And, in fact, Varmus was part of that, just for a short period, and Mike Fisher [sp.] used to come to that. And it was a group for people, let's talk about bacteria, and we're also interested in the bacteria phage lambda. That started in 1966. I went to one. It hasn't stopped for 33 years. We get together every Thursday. We either have an outside speaker or, much more usually, it's one of ourselves. You know, sometimes in the summer, it gets quiet over the holidays. It gets quiet and we don't have it. But, otherwise, we have every week's lambda lunch.

Park: It's called lab lunch?

Rosner: Lambda.

Park: Lambda.

Rosner: For the bacteria phage lambda.

Park: I see, lambda lunch.

Rosner: And it used to be in Building 2, and then it moved to Building 35, the Mental Health Building. We have a room there that we use. And this group of people are from all the different institutes.

Park: I see.

Rosner: Mental Health; Cancer; Blood, Heart, Lung, Blood; Child Development; different institutes.

Park: This has to do with _____ collaborating across institutions...

Rosner: Right.

Park: ...at NIH. It's not just the lab here.

Rosner: Right.

Park: It's not bacteriologists here.

Rosner: Right.

Park: This is a group of people out there that's _____ the NIH boundary. And did you do that kind of collaboration or exchange of information or cross-fertilization starting in the 1960s?

Rosner: Yeah.

Park: And it's going on...

Rosner: It goes on strong, strong, strong.

Park: And, let's say, within the interest group or lambda lunch, you maybe exchange some ideas. "Why don't we collaborate on this project?" And that happens?

Rosner: Oh, yes.

Park: Very often?

Rosner: Oh, yes. Yesterday's seminar, as I was leaving, somebody was saying to somebody else, "Why don't we work on this?" And I came to the seminar, I received some strains from somebody in the Cancer Institute. We exchange strains very freely, information very freely, telephone, e-mail, and this once-a-week lambda meeting.

Park: So, that interest group is really a medium to go through institutions.

Rosner: Right.

Park: That's very interesting. What is the name of the interest group?

Rosner: Lambda lunch.

Park: It's lambda lunch.

Rosner: That's what it's called.

Park: Not the bacterial...

Rosner: Maybe it's called bacteria phage lambda interest group, but I don't think so. Probably I could find it for you on the Web. But that's what it--it was called lambda lunch because we used to have lunch. It was at noon and we used to have lunch. Now we don't have lunch anymore. It's at 11:00.

Park: Lambda before lunch.

Rosner: Well, maybe I'll have to find it. I won't waste time.

Park: Have you ever collaborated with Dr. Davies?

Rosner: Yeah, yeah. So, we just collaborated two years ago. A paper came out in 1998, where a protein that we were interested in. We talked to him and explained why it's an important protein and why it's interesting to us, and he assigned one of his postdocs, a Korean...

Park: Sakiri [sp.]?

Rosner: Sakiri [sp.], yes. And he solved the structure.

Park: Oh, good.

Rosner: That was very important for them. It was more important for us than for them, but it was an important structure for the whole class of structures, of proteins that hadn't been solved, so that's very satisfying.

Park: Could you comment on the kind of style, of scientific style or managerial style of Dr. Davies or Dr. Gary Felsenfeld _____, if you can. _____.

Rosner: Well, I think that, I mean, I don't really know so well, to tell you the truth, how they actually worked within their groups. I've had two collaborations with people who were part of Marty Gellert's [sp.] group over the years, and one of them was Dr. Michael Gottesman [sp.], who is now the deputy director of the NIH. He and I worked together on a project of transposition. And the other one was Mark Geier [sp.], who is now, I think, a third of the Human Genome Project. But they were both people who worked in Marty Gellert's [sp.] lab, and I had some interest. I had some observations and I went and talked to him about it, and he and Marty had no problem in allowing this kind of interaction to occur.

So, I would say, certainly in those days, if somebody had an interest, if a postdoc had an interest in working with another postdoc, I don't think anybody was going to say to them, "You can't do this." I would say today that, with Dr. Davies, it's probably a little more strict because he has a certain number of people, there are lots of proteins that need to be solved, so he has to decide what's worth doing. And so he is more at a point where he has to make a decision, do we try to solve that structure or don't we? If we solve it, is it important? If we try to solve it, is it too hard? Can this person afford to spend one year, two years, working on that problem? So he has to make that kind of decision.

From my point of view, it seems like in a certain way it's a little bit more straightforward, that it takes time to figure out how to make enough protein, it takes time to figure out how to get the condition to crystallize it. Once you have the crystals, then solving the structure is not such a complicated thing. Whereas the kind of experiments that we do are much more every day it's changing. You get some result, you go in this direction, you go in that direction, so every day there are decisions that have to be made about where the project is going. So it's a little bit of a more fluid, more physiological, whereas his is much more structure _____.

Park: More biological.

Rosner: Right, right.

Park: _____ simply put _____.

Rosner: Right. Now, I don't know how Gary Felsenfeld works with his postdocs. I don't know what happens when they come. Does he give them a project to work on, or do they come and suggest something, or do they have a discussion? Maybe all of those ways. I don't have any direct experience with how that works.

Back when Normalinski [sp.] was my boss, what he would do with a new postdoc is he would come and he'd talk to them about the laboratory, what we're doing, and then send them to the library for two weeks and have them come back with an idea, so very unstructured. Okay, we now have done this. Now, the next step is to do this, and you're the next person, so you're going to work on this--much more fluid.

Park: I see.

Rosner: So, but I don't really know. I'm not in those laboratories, so I don't... It's not that I'm shy to tell you. It's that I really don't know.

But then the... I think a big change that also occurred during this period is that, towards the end of the '60s, as the work with the _____ to be very powerful, then you slowly began to see the beginnings--not right away; I would say more in the '70s, maybe even the '80s--when companies started coming around the laboratory to find out some way they could do something to make your life simpler.

Park: Uh-huh. What do you mean by simpler?

Rosner: A kit or a tube that, this sample goes here and that sample goes there, or here is the bottle, you know. You don't have to--you just buy all of the bottles. You take one microliter here, one microliter here, one microliter here, put together this tube, spin it three minutes, pour it over a column, you don't know what the column is, out comes something magic.

Park: Right.

Rosner: So, kits. And specialty equipment, making equipment specifically for the needs of the scientist.

Back in the earlier days, you went and you looked and saw what was there, and you made use of what was there.

I remember when I first--the first summer after graduate school, I went to work with a very prominent bacterial geneticist by the name of Demeretz [sp.], and he, in order to agitate the tubes, they had a motor--this was at Brookhaven, so they just had a motor from the shop, and with a shaft and an eccentrically placed rubber stopper, and you turned on the motor and it had a rheostat so you could change the speed, and you'd put your tube next to that rotor, and as it went, it mixed it.

Park: Right.

Rosner: The _____ vortex gene, you know, the vortex, so it's made for you. There was a time when it wasn't made for you. If you didn't have your own... And in the... Michael Yarmolitsky [sp.] told me that at Pasteur Institute, they would take the tube and they would--that's how they mixed it up.

So there's been a change where industry has adapted to the fact that science itself is an industry, and there are people who want things made for them. So you could open any issue of *Science* magazine...

Park: Is there any attempt on the side of industry to commercialize the techniques or knowledge out of NIH today? Did they.;.. You know, in the universities, there are some fellowships, industrial fellowships or grants.

Rosner: Right.

Park: Funds available.

Rosne: Very little at the NIH because everybody here is government, unless you have a fellowship... Let's say a postdoc could have a fellowship from the Cancer Society or from Helen Hayes--not Helen Hayes... Some things... But those were philanthropic organizations.

The tobacco industry always was around in the academic world, giving funds, but not here.

Park: Uh-huh, I see.

Going back to the size of the funds, who decides the size of the budget? For example, _____?

Rosner: The budget is decided by Congress.

Park: By the Congress.

Rosner: By Congress. So Congress decides the budget for the institute, and I guess they decide the budget for how much will the extramural go into grants and how much is intramural going to the scientists at the NIH. And then, within that, it's the director, I guess.

Park: But it seems like, still, I know that that _____ from up to down, but it seems like there should be kind of a justification of the size of a budget from the Laboratory of Molecular Biology. We want to expand in some way, and _____ the more people and, you know, everybody can argue, and in some way they make... They do not have to write a grant proposal, but in some way they may argue that we need this money or...

Rosner: Yeah. For that information, you have to go to Gary Felsenfeld.

Park: I see.

Rosner: I don't have any information. It's my impression that once there has been a budget for the laboratory, then it's a negotiation with the director about, we want some more people, I need some more slots, or we need some more room. Can you find some more room? We want to hire somebody. But that's a piecemeal kind of, you know, a little addition here, a little addition there, that gets worked out with the director. The purchasing of large equipment goes through the director's fund. It doesn't come out of the laboratory budget.

Park: That's very interesting. I had the same feeling when I interviewed Dr. Davies, and he's kind of _____ from administration.

Rosner: That's right.

Park: Especially nowadays. It's just a matter of administrator or director, and I'm just doing my own _____.

I met Dr. Levine [sp.] downstairs, and he's the computer director of the scientific director. And he's dealing with those _____.

Rosner: That's right.

Park: And he says that, well, we are kind of screening scientists from getting involved _____ problems.

Rosner: Right. And it's very true. That's why I was saying that this is a more academic environment than the Academy's, because we don't have that as the constant concern. Now, maybe in June, Dr. Felsenfeld will send out a notice saying that we're running a little bit ahead of our budget, and so can we tighten up a little bit. Or if we go at this rate, we'll be out of money two months early. Okay?

Park: I see.

Rosner: So, you know, so he'll give some suggestion to be a little more careful with spending.

Park: That's democratic.

Rosner: Very democratic. And it's just voluntary.

Park: Voluntary, yeah.

Rosner: But, of course, it means that if in June I decide I need a new instrument that will cost $30,000, then that's a bad time to ask for it. But it may go to--it'll go through the director's office. But let's say all of a sudden I'm doing some project and I need to buy a lot of tissue culture media or I have to buy a lot of radioactivity or something like that. But I have never known anybody to stop an experiment because of budget.

Park: Before closing, we have a little bit of time. This is a one-and-a-half-hour tape, and I'm probably 10 minutes or so.

Rosner: Okay.

Park: How would you summarize your life at NIH as a scientist generally, not only just doing science, but as a...

Rosner: As a person?

Park: As a person and, you know.

Rosner: I would say that, for the most part, it's been a great privilege and a pleasure to not only be able to do my own experiments, but to be around really great scientists, and for the great, great majority of them, people who are willing to give time and materials.

Park: _____.

Rosner: Yes, yes. To talk, to give advice, to give criticism, to share materials, to give ideas.

In looking back, I would say that I probably made some bad decisions personally about working by myself, and that I realize that working by myself, which happened accidentally but which then came to be a certain number of years, was bad, because working by yourself is very lonely and is not an efficient way to work, because if you want to go to a seminar to hear what somebody is saying, the experiment stops. You go to the library, the experiment stops. So that's bad. Whereas if you have two people, one guy goes and comes back and tells you what happened, the other guy watches your experiment and does this or that when it's needed.

So it's been a really great pleasure to, since working with Bob Martin [sp.] over 10 or 12 years, where he is an incredibly hard-working and productive and thoughtful scientist, and we get along, fortunately, very well, so together we've made a very good team, I think.

I have missed some of the lack of teaching and the lack of relationship with young students, some of which I've gotten back by the summer students.

Park: Have you ever taught at the AES?

Rosner: I did very briefly a long time ago. Yes, I did. And I'm also involved now with the pre-ERDA students, pre-ERDA workers--they're not students anymore--in our institute, so I was appointed to be kind of an ombudsman, to make myself known to the pre-ERDA students, to be a little bit involved in their activities, to help them to get oriented at the beginning, to resolve any conflicts that may come up. So, in that way, I have a little bit of a connection with younger people.

And, intellectually, it's also a wonderful place to have discussions. If you go to the Library of Congress chamber music series, you see NIH scientists. If you to go the Emerson Quartet, you see NIH scientists. I...

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