

Robert W. Berliner Interview
April 21, 2000
Conducted by: William Summers

Summers: Well, today's the 21st of April 2000, and I thought we'd start with the beginning, where you were born, and your birthplace.

Berliner: I was born in New York City March 10, 1915. When I was about four or five years old, my family moved out to Long Island, and that's where I really grew up, in a place called Woodmere. I went to a school called Woodmere Academy, and the main thing there was that the science teacher in the beginning of the seventh grade really was the one who not only got me started in science, but got me started on birds, and I've been birding ever since. I guess I was 11 years old then.

Summers: Tell me a little about your family.

Berliner: My father was in the wholesale meat business. He had a place on 14th Street in New York, and they supplied butchers and hotels and restaurants in New York. And I was the second of four boys. My older brother went into my father's business after he quit college after his freshman year because he thought he wanted to get married. He didn't but he went to work, and this happened to have been 1928 or '29.

My younger brother, with whom I was very close, was also into the birding and everything. We did everything together in those days. Then there was a big gap between my youngest brothers, 10 years younger than I am. Nothing much to tell about that.

Summers: What were your brother's names?

Berliner: My older brother's name was Herbert and my other two were Ben and Bill. Ben is a pediatrician, now retired, and a big orchid grower. He's very well known in orchid societies.

Summers: So it sounds like learning and perhaps science was instilled in you at a young age.

Berliner: Yeah.

Summers: The whole family or . . .

Berliner: No, not particularly, mostly me, Ben to some extent, although he was not into the science nearly as much as I was.

Summers: And tell us a little about your mother.

Berliner: Well, my mother had a high school education, not particularly the intellectual type.

Summers: Were they from Europe or were they born here?

Berliner: Oh, no. They were born here. Their parents were born in Europe. My mother was born in Waterbury, in fact. Her father came from Prague. He came here at the age of...sometime in the late 1850s at the age of about 17 or 18, and he said -- I don't if it's true -- that he had left to go make a visit to England, and he got so sick on the English Channel that he decided it was better to come here than to go back. How much truth there is to that, I don't know.

Summers: What would you say was the sort of milieu of your upbringing? Was it typical suburban Long Island or . . .

Berliner: Yeah. I guess you would say. We didn't see much of my father during the week because he used to leave to get to work at five o'clock every morning. And we would go to school. When we got out of school, we'd go birding usually and, of course, the athletics. It was a small school. Everybody did all the athletics, played football and baseball after a fashion,

Summers: What did you do?

Berliner: Hmm?

Summers: What sport did you do?

Berliner: I did football, baseball; up to a point, I did basketball. Basketball you didn't have to do because they only needed five people. There were only 17 in my class when I graduated.

Summers: So this was a private school?

Berliner: Yeah.

Summers: And was your religion -- were you a religious family or . . .

Berliner: No, no. My mother thought I ought to go to Sunday school, reformed Jewish Sunday school. I went sometimes, but whenever possible, we hid so they couldn't find us and make us go. But there was no real religion in our home at all.

Summers: Politics?

Berliner: My family was always Democrats, as I am.

Summers: But, I mean, was there a rabid quality to -- I don't know if rabid is the right word, but active?

Berliner: No, no. They felt strongly but not very active.

Summers: You spoke of your high school teachers or your seventh grade teacher as being somebody who you remember.

Berliner: Oh, Dave Harrower. He was a very important . . .

Summers: How do you spell his name?

Berliner: H-a-r-r-o-w-e-r. He had grown up around Philadelphia and had been a birder since he was a kid, very enthusiastic about it. And actually, fortunately, he lived right next door to us. Well, I should tell you this. When I got out of elementary school and in the upper school, which began with the seventh grade, he was the science teacher. The first marking period, I got a 45. It's the only red ink I ever had on a report card. And the next year I got the science prize for the whole school.

Summers: And your project for your science prize was . . .

Berliner: It was just for performance. Anyway, that straightened me out in a hurry. There was a group of us, about six or eight, who got very much into birding. We used to go together all the time and he would meet with us once a week for our club meetings and so on. His stay -- he would have been the science teacher all the way through, except that he left to get a Ph.D. in ornithology at Cornell, and then he came back to the school.

Summers: And was birding a science or was this . . .

Berliner: Oh, no, it was not a science. It was just birding.

Summers: Birding, right.

Berliner: Just looking. Fortunately, when we were kids, we only had to go about four or five blocks to be out in the country with woods and extensive fields and marshes. So, today you couldn't do it that way.

Summers: Were there any other teachers that you remember positively or negatively?

Berliner: The only negative teacher I've had any particularly negative feelings about was a French teacher. He was awful. But other than that, you know, they were a good bunch.

Summers: And then you left there to go to college.

Berliner: Yeah.

Summers: How did you choose a college?

Berliner: Damned if I know. They all sounded good. It really was, you know, not particularly well informed decision. I think it was a good one, worked out well, but I had no particular reason for picking Yale, maybe because of the football team. I've always been . . .

Summers: Was it a choice between Yale and some other schools or . . .

Berliner: Well, I didn't apply to any other schools.

Summers: Right. And so when you came to Yale, what was your impression when you arrived here?

Berliner: Well, first of all, everybody was much bigger than I was. In the small group that I had grown up with, nobody was much more than maybe a couple of inches taller than me. But I was -- that was the most amazing thing. All these guys were much too big. I had a good time as an undergraduate. See, we were the first class that moved into the new colleges. We were the first ones ever in the one that we lived in.

Summers: Which college were you at?

Berliner: I was in Davenport.

Summers: Who was the master, the headmaster?

Berliner: The master was Emerson. Oh, no, I'm sorry. Emerson Tuttle. We didn't see much of the masters in those days. He was an artist. He did very nice etchings. But the college was where you lived and where you had your meals, and sometimes in the spring you'd go out and play ball on the lawn in the middle. But it wasn't particularly any college activities. There were no college seminars.

Summers: Whom do you remember from college, the teachers or people who you interacted with?

Berliner: Well, the people in -- Nicholas [John Spangler] in the Department of Zoology. There was a nice old gentleman; his name was Coe [sp.] -- who taught one of the courses. I can't remember which one. But in chemistry, Coghill. As a matter of fact, the most impressive guy I remember was a fellow by the name of S.K. Mitchell, who taught freshman history. He was mainly a great character, not because I learned a great deal of history.

But those were the Depression days, and you could get through Yale in three years if you could get your fourth year to count as your senior year, your first year of medical school. And the usual thing was to go to Yale, but I didn't. I went to Cornell. And I got special dispensation from Yale to go there. So I did get my degree after my freshman, my first year.

Summers: So you came in planning to do medicine right away?

Berliner: I came in planning to do science, not necessarily medicine, and I was persuaded to do medicine by people who'd say, "Well, you know, you'd better do something that you can make a living at if you have to."

Summers: That was when scientists were independently wealthy.

Berliner: That's right. Yeah. In fact, that was still true. I was going to tell you about that when I got to going to NIH. But I was trying to decide what I was going to do after I finished my residency. I went to see Bob Loeb, who was the most scientific of the professors at Columbia and with whom I had done work my first year. And he said, "Well, you know, you didn't have the good fortune to inherit money, and you don't have the good sense to marry it, so you'd better go into practice."

Summers: Well, at Yale, did you do any what you'd call research or have any exposure?

Berliner: No. I did a half-assed study as part of a physical chemical course, but it wasn't worth a damn.

Summers: And what did you do at Yale in terms of activities that might

Berliner: Well, the first year I played lacrosse -- not very well, but I played it. And I went birding when I had time.

Summers: Was there a birding club or a birding group there?

Berliner: No, just me. As a matter of fact, the first year it was just me, and after that a fellow who had been in my birding group back in high school came to Yale and was a year behind me there, so we used to go together.

Summers: Who was that?

Berliner: His name was Arvid. He later became the editor of birding journals.

Summers: How do you spell his name?

Berliner: A-r-v-i-d. His father had come from Egypt.

Summers: Were there faculty members who were interested in ornithology at the time?

Berliner: Not that I remember.

Summers: How was Yale during the Depression?

Berliner: Well, it was very different. I mean, the student body -- you read this thing and . . . You probably don't get the Yale...

Summers: Yes.

Berliner: Do you? Did you read this thing about Kingman Brewster and the changes?

Summers: Oh, yes, yes.

Berliner: That's very true. I was still in my prep school days. There were not very many. I think there was something like, in my class, something like 80 from Andover, 40 or 50 from Exeter, and there were two from the school I was in.

Summers: Did you feel . . . Dan Oren has written about Jews at Yale at that time. Did you feel that you were a school quota or . . .

Berliner: No, not really. I never had that feeling, although there were some things you didn't expect to do. You didn't expect to get tapped for one of the secret societies. But I didn't feel particularly...It's interesting. When Dan Oren was doing that study, he found -- he sent me a copy of something he'd found in a letter somebody had written. I guess it's written to... about the Jewish boys who were applying to medical school and saying you'd better take Berliner.

Summers: And so, why did you decide to go to Columbia as opposed to Yale?

Berliner: Well, I applied to both and I was accepted to both, but I didn't like Winternitz Milton Winternitz. It was really a very unpleasant interview. I decided the hell with that.

Summers: Can you remember why it was unpleasant?

Berliner: Well, he just asked some nasty questions. I don't remember specifically what they were, but I felt very uncomfortable, as opposed to the situation at Columbia, a very pleasant time.

Summers: Winternitz was still dean at that time?

Berliner: Yes. It was 1935.

Summers: And so you went to Columbia. Did you have any preconceived ideas about what you were going to do or . . .

Berliner: Well, I never thought I was going to go into practice if I could help it, although I can't say . . . When I was an intern, I really enjoyed clinical medicine, and I was so sorely tempted to go into it, but I decided not to. When I finished my internship, I got a job with Jim Shannon, who was at that time a leading renal physiologist.

Summers: So while you were at Columbia, do you remember what that school was like or teachers that particularly positive?

Berliner: In particular -- Bob Loeb was a particular influence, and as a senior, I had worked in the laboratory that was sort of his, although Joe Ferrebee was actually in charge, and the first paper with my name on it -- I held the tubes; I didn't really do anything -- came out of that.

Summers: And what was that about?

Berliner: It had something to do with T1824. Did you ever hear of T1824? A blue dye used to measure blood plasma volume.

Summers: Yes.

Berliner: What we were doing was collecting lymph and showing that the blue dye didn't stay in the lymph, what did get into the lymph we collected. It was done with one of the surgeons, and he cannulated thoracic...

Summers: And do you have any recollection of, you know, the excitement of research or investigation or things that turned you to that direction?

Berliner: Well, not particularly, not really. When I finished my internship, I had a fellowship briefly with Loeb, and it was the first time I really studied... We studied: Why did people get pulmonary edema in the middle of the night? And what I did was, the reason would be because their blood volume expands, and so we measured plasma protein throughout the day, every two hours around the clock, and we found that the plasma protein had dropped quite sharply in the night reaching a minimum just about the time people would get pulmonary edema. So that was the first research with him that I ever did. And that was presented at the Young Turks Meeting in 1940, I guess -- not by me but by the other guy. I did all the work, but the other guy... I was the one who got up in the middle of the night and got the blood. He didn't, not quite sure why . . .

Summers: As a student at Columbia, you saw firsthand what academic medicine of the time was like.

Berliner: That's right, yeah. And in those days, people in academic medicine were almost all people who had some independent source of income, full-time people. There were lots of part-time people, of course, but full-time people all had other sources of income.

Summers: And who were the teachers you remember?

Berliner: Well, a fellow by the name of Franklin Hanger. These are in medicine. In pathology, a fellow by the name of von Glahn, who probably was a homosexual because he always had his boys over, but he was a very good teacher, very enthusiastic. Physiology was a disaster. I don't know how I ever got through physiology after that introduction. And biochemistry was... I had people like Wittenberg, David Shannon. They were very good. But I was most impressed with the people in medicine.

Summers: And what would you say was, thinking about the time at Columbia, when you were there, what was the mix of research and practice that the faculty did?

Berliner: Oh, I would say 90 percent clinical. There were people in the department, like Michael Heidelberger, who didn't do any clinical medicine at all. So there was lots of research going on. But the people who you thought of as the department of medicine were not really doing very much.

Summers: Did you interact with Heidelberger?

Berliner: Well, I was in the lab next door to him during the time I was working after I had finished my internship, Michael Heidelberger and Elvin Kabat. They were in one lab and I was in the next one.

Summers: Well, I've known Heidelberger because his son was my teacher, and so I met him on several occasions.

Berliner: That was in the Midwest somewhere. Was it Minnesota?

Summers: Wisconsin.

Berliner: Wisconsin, yeah.

Summers: When I went to NYU to interview for an internship, the first person I ran into was Michael Heidelberger and he was very old.

Berliner: Well, he didn't go to NYU until after he had to retire from Columbia.

Summers: I know and the fact that I wanted to go and talk to him sort of embarrassed Chandler Stetson, who was the chair, like, “Well, he’s this old fossil. You don’t want to really talk to him.” But, you know, he was the most famous that they had there. So, what advice did you get besides going into practice to make money?

Berliner: Well, Loeb was the one who suggested that I might go to work with Jim Shannon. When I finished my internship, I got a fellowship and spent working -- I was working with Bob Loeb on this, and that’s when I did this thing I told you about, the blood volume and . . . And for some reason -- I don’t remember what it was -- I cut that short and started with Jim Shannon. But, anyway, the idea of my going to work with Jim Shannon was Bob Loeb suggested it. He thought he was the up-and-coming guy in studying the kidney, and Loeb was particularly interested in fluid and kidneys. He was the one who actually showed that the big problem with that as a disease was loss of salt in urine. So that was how I got there.

Summers: Why the focus on the kidney?

Berliner: I got interested in it. It was one of the few . . . It was probably the first... When I was in my fourth year, I developed pneumonia, what was then called viral pneumonia. Anyway, and my brother was a medical student at NYU at that point, and he came home one day and he had brought a book with him, and the book was Homer Smith’s book on physiology, the first small one, the 1936 book maybe. And I read it. I found it fascinating. I had never read a textbook before that I enjoyed. And so I got very much interested in it. It was probably the first part of medicine that became semi-quantitative, you know, being able to measure filtration rate, blood flow, and so on. I found this very interesting. So that’s how I got interested in the kidneys, and that was why I ended up going to see Shannon. And Shannon had just been made head of a research service. He had just been made head of a thing called the Research Service, a new hospital on Welfare Island. Welfare Island, you’ve probably never . . .

Summers: Roosevelt Island.

Berliner: It’s now Roosevelt Island. First it was Blackwell’s Island, then it became Welfare Island, and it’s now Roosevelt Island. Anyway, Goldwater Memorial Hospital, which had its name changed to Goldwater Memorial just about that time, was a brand new place, and Shannon . . . There were two services that were -- one with Columbia and one at NYU, and each of them had one ward that was devoted to research. And Jim Shannon was the head of NYU, and he had -- he was more noted as a basketball player

than anything as a student before he got into NYU. But he did very well in medical school and then went on and got a Ph.D. in physiology with Homer Smith, during which he did some very nice work indeed. So he was really a very up-and-coming bright mammal physiologist. I was scheduled to start there the 1st of January 1942, which you may know was about three weeks after Pearl Harbor and about 10 days after I got married. Anyway, so although the first six months or so we got ready to do some renal function studies, the problem of the Japanese-captured malaria soon thereafter occurred and the supply of quinine disappeared. So we switched over to work on malaria almost immediately.

Summers: Let me back up a little bit and find out more about your internship, which was at . . .

Berliner: At Presbyterian.

Summers: Presbyterian Hospital, yes. What went on . . . That was a two-year internship?

Berliner: Yes, two years.

Summers: And you chose medicine? Was that just to follow up your interest in kidney disease, or was that just . . .

Berliner: Oh, I never did consider anything but medicine. That was what I was determined to do anyway because I really loved medicine, and I've had a great time. I enjoyed clinical medicine. But, behind it all, I'd like to go and at least be a full-time member of the department of medicine.

Summers: Now, was the internship -- what do you call it, straight medicine internship now, or was it a rotating?

Berliner: No. It was straight medicine. You spent -- you had, let's see. I guess you had eight quarters in the two years, and you spent one quarter on surgery and one quarter on admitting, but otherwise it was all medicine.

Summers: And that experience... Who were your main mentors there?

Berliner: Well, whoever happened to be attending on your ward, and, of course, Loeb was everybody's favorite so he was one. I have trouble remembering . . .

Summers: So this was strictly Columbia Hospital or . . .

Berliner: Oh, yeah.

Summers: I didn't know.

Berliner: Columbia Presbyterian Medical Center. At that point it was 68th Street. And it was -- the staffing was all Columbia. But a lot of the attendings were part-time people. Some of them were very good: Dana Atchley, who had originally worked with Loeb; Franklin Tyner [sp.]; Randolph West. It was a very good experience. I don't know if there's much more to say about it.

Summers: Well, I mean, was the internship one of these formative experiences in making you a real doctor or what they talk about, on call all the time?

Berliner: Well, in those days it wasn't nearly as rigorous as it is now. We were on every other night and every other weekend. And on the week when we were on for the weekend, we had an afternoon off. But we usually got to sleep when we were on. You'd get calls maybe an average of once a night for something or other. But they had a very good night supervisor, nurse, and all she wanted is you to say okay, you can do that. But, you know, other than getting up maybe to type a pneumococcus or some acute emergency, it was not very often that you had to get up, unlike, I know now, when they're on, they don't get any sleep at all. That wasn't the way it was.

Summers: You mentioned that you got married along the line here, just before you started your fellowship.

Berliner: Yeah that that had been planned for a long time.

Summers: Tell me about that. Where did you meet your wife?

Berliner: Well, the first time I ever met her, she was at a day camp, and her counselor was the girl I was going out with. And, actually, she went out with my brother. She's three and a half years younger than I am. When you're 16 or 18, that's a big difference, you know. So she went out with my younger brother. And then we were at a mutual friend's engagement party and we got to talking to each other. And it was interesting because the mother of her friend, of a friend of hers was at this thing, and the mother said, "That's the boy she's going to marry," which was really unusually perspicacious seeing it was the first time I'd ever even spoken with her.

Summers: And where was she from?

Berliner: She was from a town, a small town two towns away or maybe a couple of miles from mine. She was a student at Adelphi at that point. She was a day student. She had been at Skidmore, but her father had financial problems and she switched schools.

Summers: And did you then move to the city? Or did you continue to commute from Long Island?

Berliner: Well, I was in medical school.

Summers: Oh. You got married while you were in medical school?

Berliner: No, no. Didn't get married, but we started going together. So I essentially lived in New York from the time I started medical school till I went to NIH. I mean, my home was in Long Island, but I was rarely there.

Summers: And your first child is Robert?

Berliner: Yeah.

Summers: When was he born?

Berliner: Nineteen forty-five.

Summers: Oh, I see. And what did your wife do?

Berliner: She was a social worker.

Summers: Social worker.

Berliner: She was just out of the school of social work when we got married.

Summers: So, let's move on to your work with Goldwater Hospital. How did you do that? Was this just something you were going to try out, or was this the beginning of a career in research.

Berliner: Well, that's what I had hoped it was. I had, as I say, I went there to work on renal physiology, and we hadn't been at it very long before we switched over to work on malaria.

Summers: What was the source of your support there?

Berliner: With the malaria?

Summers: No, I mean . . .

Berliner: I was a resident. The research service had two residents. One was John Taggart, who was later chairman of physiology at BNS, and the other was me. We both started at the same time. He came from California. We became very good friends. And so we were paid by the City of New York. There was no city hospital.

Summers: This was a chronic-disease hospital?

Berliner: Yeah.

Summers: And so, were there a lot of patients with renal disease?

Berliner: No.

Summers: I mean, renal?

Berliner: No, no. Most of the patients had tabes dorsalis or Friedreich's ataxia or other things.

Summers: Mostly elderly?

Berliner: Not necessarily, no. And I have to say that in those days, we weren't very careful about the justification for using inpatients for studies. You know, the whole business about informed consent didn't exist.

Summers: I was reading one of your papers where there were a number of medical student volunteers, and you thanked someone for providing the volunteers, you know, sort of like they had this cohort of captive . . .

Berliner: Yeah. Well, there were some. There were some that really were volunteers. During the war, I did a study on some, a group of anti-malaria, possible new anti-malaria drugs in a prison in New Jersey, a reformatory, they called it. And they were volunteers in the sense that nobody was coercive. I think they hoped they might get something out of it but . . . And then we also had volunteers in the conscientious objectors for the malaria program.

Summers: So, why did you choose malaria?

Berliner: Well, I didn't. Shannon chose it.

Summers: Yeah. Well, how did he go from working on kidney physiology to working on . . .

Berliner: Well, E.K. Marshall, who was chairman of pharmacology at Hopkins, was a very good friend of his, and I think that he was probably the one who -- he was going to get involved and then decided that he ought to see if he couldn't get Jim Shannon too. Anyway, they were some of the, a couple of the key people in the whole program that was developed during the war. We didn't have much -- the rest of us didn't have much choice. But, actually, shortly after we started, then we began to get army and navy officers assigned to work with us, and we had some pretty good people working with us on that basis. We were the only ones who weren't in uniform.

Summers: And so, the goal of this program was to find quinine . . .

Berliner: Substitute. Yeah. People, you know, atabrine was available. Atabrine had two problems. First of all, nobody knew how it should be used, because when you gave atabrine most of it was taken up in the tissues, and the concentration in the blood built up extremely slowly if you didn't, if you gave it the way you gave quinine, which was the way everybody did. And everybody thought it was too slow because if you didn't begin to get better for three or four days after you started. And it might not be enough for the cerebral malaria. As a matter of fact, it was this, the first study we did was on the physiological distribution of quinine, and atabrine. I found out about this, and obviously what you needed to do was give a big dose at the beginning, and when you do that, it's much better than quinine. So I think that was probably, as far as the effort during the war was concerned, one thing we did, because by the time the new drugs came along, the war was almost over. But we found out if you used atabrine properly, it worked very well indeed. The other problem with atabrine is it made you turn yellow. It went away eventually, but it took a long time.

Summers: Where did you get your malaria to test it on?

Berliner: Oh, you know, malaria was . . . The Nobel Prize was awarded to someone by the name of Julius Wagner-Jauregg, in Vienna for discovering that, for some reason, malaria had a very beneficial effect on patients with paresis. So that was the reason for giving malaria. So you gave malaria to people who had central nervous system syphilis, and then you tested directly.

Summers: So you were doing this sort of as a byproduct of this kind of therapy.

Berliner: Yeah. So when we got inpatients with central nervous system problems we gave them malaria, and then treated it.

Summers: Where did you get the malaria to give them?

Berliner: Oh, there was a thing that had been used in, down in Florida called the McCoy strain, and it was transmitted by blood from one patient . . . We eventually got to doing some mosquito-transmitted malaria. Actually, the McCoy strain of malaria was the main thing we did.

Summers: And so, your work sort of seemed to have been building on what you already used in terms of techniques for studying blood distribution of dyes and . . .

Berliner: Well, Steve Brody was the key person in this because he developed methods for measuring all these things in the blood, and I don't think we ever gave any that we were not able to follow the concentration in the blood.

Summers: I guess what I meant was that your studies on the plasma concentrations, the idea of pharmacokinetics, so to speak, seems to be a common theme here, even though it's kidney on one hand and maybe ions, but here you have drug distribution.

Berliner: That's right. Now, this, of course, was Jim Shannon's idea.

Summers: Were you involved in testing other drugs or . . .

Berliner: Oh, yeah.

Summers: How did one go about finding new drugs for malaria?

Berliner: Well, it was a big program started at the most elementary chemical level, through pharmacology, and then we would get the ones that looked big from this program. And we must have tested 15 or so different new kinds...three or four years we were doing this.

Summers: So it was sort of a clinical physiology.

Berliner: Yeah. And we were the first ones ever to give anybody chloroquine best of all the possibilities that we had.

Summers: Was all the research done in Goldwater, or did you do field studies?

Berliner: Well, I was sent -- I spent four or five months in Costa Rica. I was sent down there to study malaria. This was 1943. Somebody had made it and they found this nice little hospital on the Pacific coast in Costa Rica, and they said, "This is a great place," so I went down there.

Berliner: There were two of us. It was me and a fellow by the name of John Meyer, who was an army officer, and he eventually ended up working for the Rockefeller Foundation, and a technician. And we set up a laboratory to study patients who were going to come in, but there were no patients. There was no malaria. Maybe the whole time we were there, maybe three patients came in with malaria. By the time we got around to doing anything, they'd all spontaneously disappear. They were people with chronic malaria who would have a flare-up and it would disappear in no time at all. So I had a good time. I spent every morning birding. And it rained every afternoon. So I had a good time birding, but I didn't see any malaria. John Taggart and Bud Earl, who subsequently was chairman of medicine at Northwestern. They were sent to Panama for the same purpose, and their experience was just about the same. They didn't see any malaria either. So that was a flop. Actually, the real studies were all done in induced malaria.

Summers: How big was the lab in Shannon's operation?

Berliner: How many square feet do I think we had?

Summers: No. I meant, how many people were involved?

Berliner: Well, let me see. There was Jim Shannon and his sort of deputy with Bud Earl, David Earl. Then there were John Taggart and I. We were the senior junior people. There was Steve Brody and Sidney Udenfriend, who were the chemical people... methodology. And then we had a series of army and navy officers assigned to work with us. Irvin London was one of them. Let's see, who else? Fred Bang [sp.]. I don't know. Over the course of years, we had maybe eight or 10 people and they were there for, maybe for six months or so. And one of the last of them was Tom Kennedy. Tom stayed on after the program, after he got out of the army, and worked with me and moved over to the Columbia service after the war.

Summers: And so, after the war, you continued to work there for a while?

Berliner: Yeah. For a brief period, I continued to work on malaria. I was working with a system for growing parasites *in vitro*, testing drugs *in vitro*. It didn't work out very well. But I had a good time doing it. And then we got back to working on the kidney, and it was shortly after I moved back over to the Columbia service that I got invited as assistant professor at Columbia. And Tom and I were at work on the kidney, and that was when we discovered potassium secretion.

Summers: Your CV says you were instructor in medicine at NYU. Was that just a . . .

Berliner: Just a title.

Summers: A title while you . . .

Berliner: I never saw a student, though.

Summers: And you were in research all that time, until '47, till you went to Columbia, was at Goldwater Hospital.

Berliner: Yeah.

Summers: Shannon's group?

Berliner: No, not Shannon. Shannon left shortly after the war. Shannon, during the war, had been appointed, had been designated to become chairman of the department of pharmacology at NYU. When the time came for him to

move over there, they had a dispute about space, and he said, “The hell with this. I’m not coming,” and he accepted a job at Squibb. He was director of research at Squibb then for three or four years. I think that must have been about 1946. So he left. And I continued working there for another maybe a year, and then I moved over to be in the Columbia service, and that’s what really got working on the kidney stuff.

- Summers: So, when you say you got back to the Columbia service, where did you physically locate your work then?
- Berliner: Across the hall.
- Summers: So it was...
- Berliner: Yeah.
- Summers: I see. Change the label on the door. Did you have students from Columbia or residents?
- Berliner: Yeah, we did. Students would come not very many, but a few. And I was supported by a grant, Bob Loeb’s grant.
- Summers: A grant from?
- Berliner: From NIH.
- Summers: From NIH.
- Berliner: I didn’t even know...
- Summers: The good old days. We didn’t worry about that. So this group, Kennedy was there?
- Berliner: Tom Kennedy and I. As I say, the most important one was when we found evidence of secretion of potassium by the two groups.
- Summers: Were you seeing patients then, too?
- Berliner: Well, you know a hospital. There wasn’t much, just keeping things under control. Admissions were very few. We saw patients, but it didn’t amount to very much. I saw more patients at NIH. My first couple of years as director of intramural research.
- Summers: So, this hospital didn’t have . . . I mean, it wasn’t strictly the research people and the clinical people.
- Berliner: Oh, there was, the research service took care of their own patients.

Summers: I see.

Berliner: But the rest . . . The Columbia service, there were -- I can't remember -- three or four floors. The lowest, first floor, was the research service, and the rest of it was regular inpatient service.

Summers: So, did you use the patients for research or . . .

Berliner: Yeah, mostly, although there were far more patients than we needed for the research that we were doing. So Columbia had one stack of services. The lowest floor was the research service, and NYU had similar. And although there was somebody in charge of the whole thing, the research service was sort of independent.

Summers: What was it like to work at NIH in those early days?

Berliner: It was wonderful.

Summers: Feel like you were in a pioneering venture?

Berliner: No, not particularly. I think it was, we felt pretty much like you might at any laboratory. There were no distractions. It was all whatever you were doing. It was a great place to work. It probably still is.

Summers: Did you have that -- how should I put it -- over-dedicated, full-time, 100-hour-a-week sort of thing, or did people have academic time for reflection?

Berliner: Everybody -- it was very serious, hard work.

Summers: I think there's a sort of image in that I can work longer. If you're not in all day on weekends, you're not serious.

Berliner: Yeah. Well, you know, I remember going back to the time I was in medical school, the fifth floor of the medical school. This was Columbia. Always the lights were on when you went by, no matter when it was. That was biochemistry. Nobody else felt that way, but they were always on. I guess that has always been true. But, no, NIH was pretty much 8 to 5. You had something that had to be done but most people when they were working, they were working.

Summers: What do you think of the attitude toward minorities?

Berliner: Well, there weren't any. There were no minorities. Black technicians weren't any anywhere.

Summers: What about foreign visitors?

Berliner: Well, as NIH became better established in the early days. I remember Jim Shannon had in his lab a postdoc. And he came for a couple of weeks. We had a very good time. And he's the only foreigner that I can remember.

Summers: NIH and academic institutions and programs.

Berliner: Oh, yeah. Somewhere in the mid '50s, we started an educational program, actually, which became the foundation for advanced education.

Summers: What was the motivation?

Berliner: For one thing, there was the idea that we may eventually -- as a matter of fact we did. A number of people were getting candidates-- Julie Axelrod Steve Brody... now most labs...I don't know how... demand for it to teach.

Summers: Did you feel that the ones who were coming were well trained?

Berliner: Most of them were not particularly trained in science. They were highly motivated.. We were getting a new chairman of the department and fixing up lab space.

Summers: So when you decided to go into renal physiology or kidney function, do you remember the motivations for your research, or were you setting off on some program to find out everything about the kidney, or did Loeb sort of partition out projects?

Berliner: Well, what I was really interested in was trying to find out how the kidney handles electrolytes, particularly sodium. As a matter of fact, we discovered that potassium...we were studying the effect of diuretics on sodium secretion. The potassium photometer was a new thing in those days. As a matter of fact, we made our own. And if you were going to measure sodium, you might as well measure potassium, too, because it sort of fell out of it, you know. And we never did find out much about ...we know diuretics increased sodium. That was much more interesting than potassium.

Summers: Can you remember back to those very experiments? Were you surprised or . . .

Berliner: Oh, yeah. Yes, indeed. I remember we gave it to dogs. And then we examined potassium excretions. Potassium excretion was high to begin with. It went down. It was low and went up. Then it might stay constant, in spite of the fact that the filtration frequently dehydrated. Excretion of potassium filtration rate. So we decided this must be the potassium.

Summers: Did you think that right away?

Berliner: Well, after we'd done a couple of experiments and seen the same thing several times, yeah. So we decided we're going to have to find out, demonstrate that potassium is secreted by showing that you could get more out of the urine. So we started giving dogs infusions of potassium and we found they got quite high potassium. The potassium was getting awful high and we were careful putting more. And then I happened to read a paper. A couple of guys whose names never appeared again anywhere as far as I know, and what they had found was that if they gave rats large doses of potassium over a week or so, they became tolerant of much larger doses. Doses of potassium that would kill a normal rat. So we said, "We'll try that." And so we gave our dogs five to 10 grams of potassium a week or so and then tried again, and measured potassium secretion.

Summers: Where did you get the dogs?

Berliner: Oh. They were cheap, though. I don't know where they got them. They were not stolen. They were not city type dogs.

Summers: And so this set you off on potassium for a while?

Berliner: Yeah. The rest of the time I was at Goldwater working on potassium, and how or where in the tubules it was secreted and how it behaved under various circumstances. Bob Goldman, who had joined the NYU service, he had been in the army during the war, and when he came back, he had been a student at NYU and he was very much interested in instrumentation. He and I then made this very ingenious...

Summers: So, did you see these studies on potassium having clinical relevance or did you begin to think about pathology?

Berliner: The sodium part obviously had clinical relevance.

Summers: How was this work taken up or not taken up by other physiologists?

Berliner: Oh, well, immediately probably not very much.

Summers: When was that, though?

Berliner: Let's see. I got a letter from Gerhardt in Milwaukee. It must have been for a job. I think he went to work for...

Summers: The fluid and electrolyte balance in pediatrics was important to know.

Berliner: Oh, yeah, I know. But whether it's secreted or whether it's just not reabsorbed doesn't really make much difference.

Summers: In sort of immediate postwar academic medicine, what was the establishment like? You talked about presenting at the Young Turks and so on.

Berliner: Well, that was pre-war.

Summers: That was pre-war, right. But was there a big change from pre-war to postwar in the establishment?

Berliner: Actually, the big change came in the middle '50s.

Summers: Yeah.

Berliner: When Jim Shannon became director.

Summers: I think that's my impression, too. But I was wondering, before that, what was it like?

Berliner: Well, it was just more of the same as far as I know. There were things like fellowships for \$5,000 a year, and that was supposed to pay their salary. So that became a group that used to meet regularly, and they became some of the old-boys' club.

Summers: Were you a Markel fellow?

Berliner: No. Marston was. He had a lot of money then. But I can't say that there was much change, and we had a lot of people coming back from the services.

Summers: And it was a small group of dedicated people working here and there?

Berliner: Yeah, until Shannon... people could afford.

Summers: If you think back to that time, not knowing what was coming in the future; did this seem like a satisfying career laboratory research?

Berliner: Oh, yeah. Shannon had gone to Squibb in about 1948 or '49.

Summers: What were the negative feelings about going to work for the government?

Berliner: Oh, just that lazy bum type. And so, the only one that really took the attitude that you write it off was Tom Kennedy and he went there about a year before I did. We ended up in the people like Steve Brody, Julie Axelrod, and Anfinson.

Summers: What had Axelrod done at Goldwater?

Berliner: Oh, he had come after the war. He was not in malaria. He had been in medical school and he had got a job working for some commercial outfit. Steve Brody, he found that this was something he liked doing, and so he didn't go back. And then when Steve decided to go to NIH...

Summers: Did Axelrod ever get his degree?

Berliner: Yes, he did, afterwards. He got his degree at George Washington. But that was later.

Summers: So you were recruited to go in the crest of the wave in a way. Shannon had one or two people or other places to go?

Berliner: Most of us started in the late summer of 1950. Chris Anfinsen, Julie Axelrod, Jim Davis. These are all people who are now members of the Academy. And then not long after we started, he began recruiting because they were digging a hole to become the Clinical Center we were going to have to have sort of house staff. So he began recruiting clinical associates, and two future directors of NIH, Wyngaarden and Varmus and did a remarkable job recruiting.

Summers: So, what was NIH like? What got Shannon to go there?

Berliner: He was already in industry, which was looked down upon, so it wasn't the same kind of change as if he'd gone directly.

Summers: But, I mean, did they promise him a lot or . . .

Berliner: No, not personally a lot and of course, the NIH budget. That was for everything.

Summers: But, I mean, to recruit all those people. Obviously, that was a big expansion. Or were you replacing people that were being shunted . . .

Berliner: No, no, no. This was new. We occupied essentially maybe two floors.

Summers: Had a decision been made before Shannon got there to make this expansion, or did he argue for it?

Berliner: I think that was the Intramural Program.

Summers: So was the Intramural Program sort of a new idea at this point?

Berliner: Oh, NIH was an intramural program entirely up to that point. I mean, up to after the war, there was a very small grant program in the Cancer

Institute, going way back. But NIH was essentially a local operation. It expanded greatly after the war because they took over the things that had been supported by the Office of [unintelligible] contracts and that kind of thing. There was a continuation of malaria support after the war, but it was through NIH now.

Summers: Did Shannon's recruitment have a sense that, do you think that here was a guy who had been successful in an extramural program and he had that knowledge to be inside and . . . Do you think that people saw the future for NIH in a way that it did develop?

Berliner: Well, I think that people saw Jim Shannon was [unintelligible]. Now, he knew all the right people, more senior than those of us, and that made a big difference. But I think it was Jim Shannon more than anything else. But somebody had already argued to Congress.

Summers: Oh, to make this.

Berliner: C.J. Van Slyke was the director. And he left everything. The director of intramural research nominally was responsible.

Summers: Van Slyke?

Summers: So, there was the Cancer Institute before.

Berliner: The Cancer Institute was established around 1937 or something like that. There were two, the Cancer Institute and the NIH. These were two things. And then after the war, they began adding on other institutes. Anyway, it became the NIAID, and another one that became NIAND. Then more Institutes were added neurology and some of the others later. And they did reasonably well on recruiting.

Summers: So you had the Heart Institute when Shannon went there?

Berliner: Yeah. It was established before.

Summers: Did you get the sense that time that the Heart Institute or that Shannon was going to take a leadership role in the overall show?

Berliner: No. I had no idea at all like that. I can't say I was surprised. In 1952 or '53, what happened was that the guy who was the director of NIH when I went there retired, and the choice was who was going to be made director. And the two candidates were William Sebrell, who was selected, and Norman Topping, who had been the deputy director for Intramural Research, when Topping didn't get the job, he quit, he resigned. As a matter of fact, he became president of the University of Southern

California. So Jim was invited to take the associate director of NIH, which he did. That left the NIH with the Heart Institute without a director of Intramural Research. And they started looking for one, and while they were looking, they decided that to select their own acting director. Anyway, they elected me, and that's how I got in. And after they had tried a couple of times to get, looking for somebody on the outside, they gave up.

Summers: So, the relative position of the Heart Institute and the other institutes, was it small, large? Was Shannon the first one?

Berliner: Well, we didn't have nearly as much space as many of the others. What became the Arthritis Institute in Building 3, we had Arthur Kornberg and Bernie Horecker. They occupied one floor. On another floor we had mosquitoes. We didn't have a hell of a lot of space. But the Clinical Center was under construction. We were at that stage. And most of the other institutes at that stage of the game were staffed largely by the public health service. That began to change.

Summers: And so by commissioned corps.

Berliner: Yeah.

Summers: Were you expected to join the Public Health Service?

Berliner: Well, a lot of us did because we needed to have [unintelligible] during the Korean War. And although I went, it was not -- I joined shortly after I got to NIH. I went there as a GS-15, and shortly after that in the Public Health Service for two years. And there was one uniform that they all passed around to get our picture taken. But there were sort of people shuttling in and out of the commissioned corps throughout was there, for one reason or another. Sometimes it was more financially favorable to be in. But I was on the intramural side.

Summers: Well, why don't we -- I've taken enough time. I think this is a natural breaking point. I like to sort of talk about the years at NIH next time. I want to see how well I can cover what you were doing there. I guess I was interested in something of how the lab worked, you know, the style, the projects, who picked what projects.

Berliner: People picked their own project pretty much as the situation arose. There was no really long-term planning of anything. That was one of the nice things, as a matter of fact, about working at NIH. You didn't have to figure out in advance what you were going to do the next couple of years. You could just start doing something, and if it worked, you kept doing it;

if it didn't, you just stopped and did something else. So you didn't have to convince any other group of people of the usefulness of what you proposed to do. The one thing you asked me about the other day, I just was thinking about it. You asked me about women. And there were two women in the Heart Institute in rather prominent places, one, Terry Stadtman and the other was Martha Vaughan.

Summers: Well, Martha was Orloff's wife.

Berliner: That's right. There was quite a fuss at one time at NIH because all the prominent women were the wives of a scientist. Terry Stadtman was Earl's wife, and Martha, Jack's wife. Well, you know, as far as that goes, they're both members of the Academy, so that there's little doubt about their competence. It wasn't just nepotism that got them where they were. But, nevertheless, as I say, there had been that fuss about it. There must have been other women at some time or other, but I don't remember any during the time I was scientific director.

Summers: What about -- I noticed most of your papers were single-author papers. This reflected the way you worked, or was it . . .

Berliner: Well, many of those papers were presentations, you know, papers I wrote for something or other. Most of my work, I don't think I ever had a totally independent project entirely my own, nobody else working with me. There was usually at least one junior person.

Summers: Was there an annual review by the director or any sort of general sense of where it was going?

Berliner: Oh, scientific directors, as a group, used to review promotions or came up. And we had a group called the Board of Scientific Counselors, who were people from outside who came a couple times a year, and each time they came, reviewed the work in one laboratory, one or two laboratories. So there was lots of review of the work that was going on

Summers: Well, you mentioned collaboration. Who were some of the people you collaborated with both within the Heart Institute and outside?

Berliner: With whom I collaborated?

Summers: Yes, or your laboratory.

Berliner: Well. Bob Bowman was technical development. As a matter of fact, Bob Bowman and I had also worked together at Goldwater. He wasn't involved in the malaria program, and he came after the war. But I think I said he and I made my first photometer, and he made a fusion

[unintelligible], and then he developed all kinds of things later on, including the spectrofluorometer that Steve Brody used in many of their, much of their work. And he developed instruments and for freezing-points, which we used in concentrations.

Summers: Was he viewed as a full-fledged scientist or support system?

Berliner: Oh, yeah, yeah. He was an M.D. He had spent the war, in the war, they decided he was a psychiatrist, and when he came back he worked on nerve transmission for a brief period, but he found he was much more interested in physical. And he was a very ingenious guy. We were very good friends personally.

Summers: So, for your biochemical collaborations, you had Brody in-house, more or less.

Berliner: Yes.

Summers: What about the whole area of clinical chemistry? I mean, was that a growing discipline at the time? Similar to pathology...

Berliner: Yeah. Well, actually,[unintelligible], who started out in laboratories, came over and worked in clinical chemistry specifically and the Clinical Center. And he developed a way to measure fluoride concentrations and became very popular in our lab. Unfortunately, he committed suicide. He had been depressed and had been admitted to the Naval Medical Center, and they decided he was in good enough shape to get out over a weekend. But his wife was away that weekend. She called and came home and found Ernie in the refrigerator.

Summers: That's wild. So, other aspects of clinical chemistry related to the kidney?

Berliner: Not particularly. I don't remember anything.

Summers: I mean, urine tests have been pretty standard for a long time.

Berliner: Oh, yeah.

Summers: The gravity.

Berliner: The measuring of proteins. We did one study one time, a question of how much protein was in the kidney, but we were not successful.

Summers: And the role of the kidney and hypertension was known.

Berliner: Yeah.

Summers: But was the chemical control the hormonal regulation of blood pressure?

Berliner: Well, a lot of that was developing during the time that we were working on it.

Summers: Well, when was the angiotensin system started?

Berliner: Well, the details were worked out, oh, probably in the late '50s, early '60s. Angiotensin got its name, of course, by [unintelligible] and the group at the Cleveland Clinic kidney extracts could raise blood pressure. I think they called it angiotensin. So when they got together, they decided on angiotensin. I guess it was known that we could do something which caused angiotensin, but I don't know what it was. The precursor came later. Then the whole idea of converting came much later, of course.

Summers: But in terms of the role of the kidney in heart disease, that was an important understanding.

Berliner: Yeah, oh sure. The Goldblatt business really directed our attention to the kidney as a major problem related to hypertension. Just how it worked and was understood.

Summers: And then the endocrinology, the control of the kidney through aldosterone.

Berliner: Well, Jim Davis did a lot of that work. Fred did a lot of work in that general area, too. It was a difficult problem because they didn't get along at all. Anyway, Jim did some very nice work with aldosterone.

Summers: And then why did the, where was the focus on the adrenal glands, the aldosterone ?

Berliner: Well, we'd known for a long time that the adrenals, of course, had a lot to do with excretion.

Summers: With adrenalectomized animals or patients . . .

Berliner: Well, originally, Addison's disease caused a drop in sodium and the drop in blood pressure and so on. So it was well known that control of sodium excretion had an effect. And Jim Davis did a lot of nice work with animals.

Summers: Was your lab involved in the chemistry?

Berliner: Oh, yeah.

Summers: Well, thinking back, you mentioned micro-puncture as one of the sort of techniques that helped ? Were there other techniques?

Berliner: Yeah. Verney came with Jack Orloff. They isolated, dissected out of a kidney *in vitro* to control both external and internal contents.

Summers: And wasn't needed to make this preparation work.

Berliner: Well, first of all, people had tried to isolate the tubules by treating the kidney. You did that, you took out a tubule and they blew up like a balloon. You couldn't get it because it lost the membrane, which held the thing in place. And he found that if you dissected it then he, along with Bob Bowman, devised this double-barreled pipette, the outside barrel to hold the tubule in place around the inner barrel along with a lot of micro method.

Summers: And so you dissected the tubule?

Berliner: That's right. And you could do it in separate segments, segments that you couldn't possibly dissect the kidney. Micro puncture was limited to the surface.

Summers: And this technique a whole set of advances?

Berliner: That's right. It made it possible, for example, to study directly the big descending [unintelligible], which turns out to be a very important site of electrolyte transfer.

Summers: When did it become clear that there was a hormone controlling water?

Berliner: Oh at least 1920 or so. And it was first thought to be a diuretic because they gave enormous doses. But the key work was done by Verney at Cambridge at that point in the late '20s, early '30s, showing that, if you perfused you got secretion that showed that it was the high pressure feeding the brain.

Summers: But then the target . . .

Berliner: Well, that was the work of Bliss who was largely responsible for work on the frog skin that showed permeability of the frog skin to water increased vasopressin. We always thought about it as ADH isolated and structure determined by what's his name, Dube [? Unintelligible]

Summers: I knew the story from Bob.

Berliner: That's right. I think Simmons worked on that.

Summers: So, how did the clinical nephrologists come and work with you? How did you interact with developing specialties?

Berliner: I'm just trying to think if we ever really had anybody who was a clinical nephrologist in at all. We had a lot of people who ended up in departments of medicine, and I guess they ended up doing some

nephrology when they did. But they were interested in kidney from a physiological view, pathological. They were interested in controlling electrolyte balance and worried about things like that, but not particularly about nephritis. I don't think we ever had anybody who was a purely interested in nephrology primarily. And most of them ended up in departments of medicine because they were financially much better off in medicine than they were in physiology.

Summers: So your professional identity was as physiologists, if what you talk about professional identity. What about the overlap or possible overlap of your work with people and other problems of electrolytes in nerve cells?

Berliner: Well, we were interested in it.

Summers: Was there shared technology?

Berliner: Not till much later. We really started out in nerve muscle, and that's been taken over by the membrane transport kidney people. This happened to be a very useful technique.

Summers: Were there people at NIH who were doing that kind of work?

Berliner: Oh, there were a lot of people. Casey Cole and his group were doing a lot. They were interested in . . . I think, as a matter of fact, Casey Cole developed a technique that was eventually used by, in Britain . . .

Summers: Hodgkin?

Berliner: Hodgkin originally done by Cole and whoever back when they were still in New York and the Naval Medical Center and NIH. So there was lots of that kind of activity going on at NIH.

Summers: So, some people refer to this whole field as membrane biophysics. Did you think of yourselves as biophysicists?

Berliner: Not then.

Summers: Not then.

Berliner: Joe Hoffman was also involved. He worked with red cells, and he considered himself a biophysicist. I don't think anybody else in all that were biophysicists.

Summers: Why do you think that was? Can you think back of leads which you regret not having followed up or things that, in retrospect, you'd say those were good ideas but we didn't follow up, regret? I don't want to say regret.

Berliner: I understand what you mean. No, not really as a matter of fact, quite the reverse. A couple of our best experiments were picking up on things that somebody else tried to do and had not got it right.

Summers: Give me some examples.

Berliner: I told you the other day about Winkler having given lots of [unintelligible]. I don't remember whether he killed them or what, but it never got any significant results. And we figured out how to make it work. The other one was an experiment. We were able to show that to make urine get concentrated in the absence of lowering filtration rate sufficiently. This had been done by somebody else, too. We went with one kidney as the control. It produced a very small volume of urine. As I say, these are a couple of things that other people have thought they had proved you couldn't do.

Summers: What would you say were major problems you worked on for a long time.

Berliner: Well, figuring out what controlled sodium. It's a very, very complicated process, and we never really did. I don't think anybody still knows how all the various things. We know a lot of things that affect it, but how they are integrated, no.

Summers: How were you attacking that? Did you have a sort of experiment you did?

Berliner: We started out with studies and then we went to micropuncture.

Summers: Did your work depend a lot on having gone through modify things?

Berliner: Quite a bit. I think I told you that our idea that potassium might be secreted was stimulated by working with a diuretic. And then I researched the relationship between potassium [unintelligible] only drug that I remember specifically working with. Of course, when you think of what stuff we did related to clinical medicine, it's much more about controlling excretion rather than the kidney itself. Those are the only ones that I remember specifically. We collaborated with [unintelligible] We did a study of crystalline excretion.

Summers: What is that?

Berliner: It was something used to measure lower blood pressure. It turned out that it not only was cleared completely from plasma, but the red cells gave up a lot of it, too, and higher than PAH, the thing that you used to measure blood flow. But that was just a curiosity, really.

Summers: Did you ever study the role of the kidney in detoxification or clearance of poisons or noxious things?

Berliner: No. we never actually did it ourselves. The effective acidification on excretion. But we never actually did the experiment.

Summers: Was the military ever interested in your research?

Berliner: They were.

Summers: Okay. What was the relationship with the military medical people at Walter Reed, and the Naval Hospital?

Berliner: Well, we inherited a couple of people from, one fellow who came from Walter Reed to work with us. And Mike Walser came over and worked with Jack Orloff. But we never actually did any work in collaboration with them.

Summers: Were you involved in the controversy at the time?

Berliner: Which controversy?

Summers: I was thinking about scientific controversies at this point. That is, did you disagree with the other labs for your beliefs or research?

Berliner: No, not really.

Summers: It was all harmonious?

Berliner: We were pretty harmonious, In kidney physiology, were there groups of opposing camps or national groups that were competing. Well, they were competing in the sense of who's going to get ahead of whom, but not in the sense of trying to prove somebody else was wrong and you were right. I would say that our lab was one of the two main labs, and there were others. Schwartz and Bowman and Maury Strauss. There were other groups, but these were primary.

Summers: Did you assist exchange students or postdocs?

Berliner: No. As a matter of fact, by the time anybody left a lab, they were ready to go out on their own anyway.

Summers: What was the average sort of training experience of a postdoc? How long did they stay on average?

Berliner: At least two years. Many stayed three or more. On the clinical associates, they signed up for two years, and most of them stayed on at least longer in the laboratory and they were clinical associates.

Summers: And then the usual career path was to go be a professor somewhere or . . .

Berliner: Yeah.

Summers: And at that time there were jobs for almost everyone?

Berliner: Yeah. I don't remember anybody who left our lab had trouble getting a job. I know I considered Jim Shannon largely responsible for the fact that you could do work in the lab and find someplace.

Summers: Yeah. Well, certainly that's one topic I want to come back to, all of NIH and the whole medical research field. Tell me a bit about the colleagues you had. You mentioned Kennedy, Orloff as some of your major collaborators. You said a little bit about each one of them, but maybe you could expand a little on how they developed and what their role was.

Berliner: Well, Tom Kennedy originally came to work at Goldwater in the malaria program. He was in the army, his work was on malaria. And he came along -- I guess it must have been just before the war ended when the malaria program broke up. And he stayed on and worked with me, and we got the idea we might be able to measure blood flow the A-B difference for function. And we were catheterizing and A-B differences. And that didn't work very well, and we only did a few, gave that up and started working on [unintelligible]. He and I were working on acid. And we continued working _____ till 1949, and then Tom went to NIH _____. Jim Shannon had talked to both of us about possibly _____. Tom came from Washington. He came _____. His family lived in Silver Spring _____. And then when I moved, when I was -- about that time, Jack Orloff, who had been working here _____, came to see me about coming to work _____, and _____ that he would, and then I just _____ NIH _____. And we collaborated very closely for the next 10 years _____. He eventually succeeded me as _____ lab, and then subsequently as _____. _____ John Fredrickson _____ succeeded me as director of Intramural Research for the Heart Institute when I moved _____. But anyway, he didn't stay at that very long, and Jack Orloff came _____ director of Intramural Research _____. And we were very close collaborators and close friends.

Summers: Was his wife in the Heart Institute too?

Berliner: Yeah. She started -- she was originally in Chris Anfinsen's lab.

Summers: Their son was a medical student _____.

Berliner: Yeah, that's right. _____. Their second son _____ and then he went to NYU.

Summers: I knew their son was a medical student. _____.

And so Orloff didn't . . . When he came from Yale and _____ working with Peter _____ medical student or a house officer.

Berliner: House officer. He had been _____ went to medical school at _____. He was an undergraduate at Harvard. _____. I guess he went to NYU _____ and interned at Mt. Sinai, and then he came _____ lab _____.

Summers: And Kennedy _____?

Berliner: Tom worked with us for a while, and when micropuncture started up, he took a shot at it. It didn't work out very well. Meanwhile, Jim Shannon was interested in _____ Building 1, so he gave up laboratory work and _____ he became the guy who put together the papers for Jim, testimony and stuff like that. _____.

Summers: And looking at your CV, there were a couple of names that appeared quite often. One was Levinsky

Berliner: Yeah. Norm Levinsky [sp.] had come from Boston, and had worked with Bill Sawyer, who later worked on anti-diuretic hormone _____, and then came as a clinical associate _____. And we worked together for _____ but the clinical associate _____ couple of years after that. And _____. And then he left and went up to BU, where he was -- Bud Rowman [sp.] was then the chairman of the Department of _____, and he worked with Bud for a while and then became, subsequently became chairman of the _____ medicine.

TAPE 4, SIDE B

Summers: You mentioned Barry Brenner [sp.].

Berliner: Barry Brenner came as a postdoctoral fellow and worked with me for several years, and then went out to San Francisco, where he was at the VA Hospital, I guess. And actually, when I came here, Sam _____ tried to recruit him _____, and at the same time, he was offered a name professorship at Harvard and he took it. So he's been head of _____ Sam Levine professor of _____.

Summers: There was a _____ named Clapp [sp.] who . . .

Berliner: Jim Clapp [sp].

Summers: Jim Clapp [sp].

Berliner: Jim Clapp [sp.] came from North Carolina, and I think he spent a year or two in Seldon's [sp.] lab at Texas before he came to work in our lab. And he was the one who really got micropuncture working in the laboratory. He did a very good job _____ wanted to go back to _____, which he did.

Summers: Other people _____ remember from this time? _____ CV, so I . . .

Berliner: Well, those were . . . Rex Jamison [sp.] was another. Rex Jamison [sp.] worked, came from _____. And he was a clinical associate who worked with me on problems related to the concentration in the urine. He did some very nice work _____ in rats and _____ was made possible by a technique developed by a Japanese fellow _____. And then he left and went out to Stanford, where he was _____, was appointed as chairman of medicine at Rochester, didn't get along with the dean, and he left and went back to Stanford, where he is now.

Summers: Were all of your people successful, or were there _____?

Berliner: I would say pretty much so. I _____. There was one fellow, _____ Davidson, who was a very good worker and _____ very long, but turned out for some reason, he made up stories unrelated to the work, fortunately. But he was caught up in making some ridiculous claims of things and finally left in sort of a cloud. We had to repeat some of the things he had done to make sure that that _____, but they were okay _____. But he's the only _____. Then he went back out to Oregon, where he came from, and I don't know what became of him. But he's the only one who I can think of that _____.

Summers: What were the -- were there procedures at NIH for dealing with this at that time? How was this handled?

Berliner: It was just handled by us. As far as I know, there was never any question _____ any of this work. It would have been very difficult, of course. There were too many looking over your shoulder all the time. As a matter of fact, though, there was several instances of scientific misconduct in the Heart Institute during the time I was scientific director, and nobody ever heard of it ever, settled without a big fuss, a great contrast to what's happened since.

Summers: Did you have technicians who would _____ with you for a long time?

Berliner: Oh, yeah. In _____, there were two very good _____ technicians who _____. One came a year after I did, a very nice black woman, a very smart girl, and she retired _____, with whom she ended up working, left to go to Hopkins. She _____, and she just retired a year or two ago, so she _____ 25 years _____. Another one originally came to work with Adrian Hartman, and _____ about 1952. She retired a few years ago, too. Those were the only two that I remember that were really long term. One who initially worked with Tom Kennedy when he started _____ micropunctures and then worked with Barry Brenner [sp.] when he came and left when Barry Brenner [sp.] did, and as far as I know she's still working with _____. Those are the only ones I can think of _____ long-term people.

Summers: What about administrators? Did the lab chief have administrative staff or secretaries?

Berliner: They had secretaries, but _____ people who were technically administrative _____ the Office of the Scientific Director, which wasn't a very big group either. I had a secretary and there was a business manager for the Heart Institute Intramural Program, and she had a couple people _____. That was it.

Summers: So for a postdoc to get a paper typed, did they just type it themselves or . . .

Berliner: Oh, no. There were secretaries. The labs had secretaries _____, usually two. One was more or less the secretary _____ head of the lab and one was _____. I don't think anybody _____ so much easier _____.

Summers: Were there physiologists in other institutes who you felt represented your discipline across NIH?

Berliner: Well, of course, there were the neurophysiologists. _____. There were endocrinologists _____ considered themselves physiologists.

Summers: So what was physiology _____ like _____? When you'd go to the meetings and so on, what _____?

Berliner: Well, there were the cardiovascular physiologists. They were probably the biggest. Then there were renal physiologists. Those were the ones that I _____. The neurophysiologists _____. They had not organized their own organization in that time _____. As a matter of fact, the presidents of the Physiological Society were often neurophysiologists. They were big meetings. Physiology was the biggest group in the federation at that time.

It was before the biochemists decided that they were going _____. And . . .

Summers: And what about things like digestive physiology, gastroenterology?

Berliner: Oh, yeah. They were all in there.

Summers: _____ physiology? Lung physiology?

Berliner: Yeah. The biggest ones, I guess, were -- cardiovascular was probably the biggest group. Neuro and kidney and _____. Pulmonary, gastrointestinal, reproduction _____ handbooks, which came out while I was on the Publications Committee.

Summers: So it was organized by basically body part?

Berliner: Yeah, by the systems.

Summers: Physiological systems. When did it start to be organized by process, you know, membrane, transport, hormone receptor targets, or whatever? I mean, _____.

Berliner: Well, _____.

Summers: _____ physiology became a discipline sometime.

Berliner: Well, it was after I got out of the _____, I'd say in the '70s and after that.

Summers: Now, would you . . . I don't want to force you into a choice here. The work you were doing, was it cellular physiology or organ physiology or tissue physiology?

Berliner: It was organ, I would say.

Summers: What was the role of NIH in this discipline? Were you encouraged to be, participate in the national organizations?

Berliner: Oh, yeah.

Summers: Be leaders, if you could?

Berliner: Yeah. I was president of the Young Turks; I was president of the Physiological Society.

Summers: And this helped at NIH, getting promoted or . . .

Berliner: Oh, no. It didn't hurt, because the group that decided on promotions were the scientific directors, and they didn't . . . I guess if you were elected

president of something, they'd think, well, at least a physiologist or whoever it is _____ highly of him, and that's a plus. As a matter of fact, by the time I was president of _____, I couldn't be promoted further anyway.

Summers: That happens to a lot of us. _____.

Things like journal editorships and so on were not treated as a distraction from your work or . . .

Berliner: No. The problem with journal editorship was one of space. But there were a lot of people who were editors at NIH _____. As a matter of fact, it was looked on positively.

Summers: I remember reading a speech by the president of the American Physiological Society. This was in the late '40s, early '50s. It was sort of lamented that physiology was sort of losing some things to various disciplines. I think _____ genetics and biochemistry and radiation biology and these sort of things all developing at their own . . . The _____ physiology and physiology were being defined by what was left as opposed to positive things. Is it something you call or _____ the discipline?

Berliner: Well, it came a little bit later, the _____. Well, I mean, the . . . It's even more striking, I think, in the science of clinical investigation _____ discipline. It's a parallel, _____ where it used to be *the* big meeting. Now it isn't. Attendance is dropping. People _____ everybody goes to their own specialty instead. The same thing happens _____. First the neurophysiologists _____. All the others _____ their own _____. But the core physiology _____.

Summers: If you look at a physiology textbook, say, the ones that are used for medical students, it doesn't seem to reflect what physiology departments do for research.

Berliner: That's right.

Summers: _____ there's more and more.

Berliner: Well, _____ a lot of the organ system physiology has mostly been pretty well worked out. _____. _____.

Summers: What would you say were your major discoveries _____ if you were going to point to things you're proud of or . . .

Berliner: The whole lab?

Summers: Yeah.

Berliner: Oh, I'd say our work on potassium, Davis's work on _____ formation, Goldberg's _____, our work on the concentration in the urine. And, of course, there's been quite a bit since I left. _____ still doing _____.

Summers: Was -- most of the people you mentioned have, seem to have medical backgrounds. Was that sort of _____ thought of as a prerequisite for the kind of work _____?

Berliner: No, not particularly. Well, one of the reasons was, of course, the thing that brought a lot of people to NIH was the draft. Many of the clinical associates were commissioned officers as a substitute for, early on, for Korea and later for Vietnam. And that was one of the reasons why many of them _____. There were no particular _____.

Summers: Was there a tension or a competition between _____?

Berliner: No. I was trying to remember whether there was a difference in _____. I don't think there were many of _____. It just happened that a lot of the people were interested in kidney _____.

Summers: Were there many PhDs in physiology being _____ at this time?

Berliner: I'm sure there were plenty of them.

Summers: Because this was before training grants, that sort of thing.

Berliner: Well, training grants really got going in the middle '50s _____ Shannon so that they were . . . There were several departments of physiology _____ a lot of PhDs. Some of them turned out very few. Places like Illinois and _____ turned out a lot of PhDs. _____ turned out very few.

Summers: Well, why don't we finish off here. They're probably going to start sawing in earnest here shortly.

I guess next time I'd like to talk a little bit more about, if you can think about _____ the lab _____. But I know some _____ more about the way NIH worked in those days and the programs that developed. I mean the training program, for example, _____ training grants and so on, which was really important.

TAPE 5, SIDE A

Summers: Well, last time we were talking about the scientific work in the Heart Institute and various aspects of _____ physiology. I was wondering what problems _____ larger world. Was NIH considered an equivalent of

academia or considered a more prestigious place at that time, or was there a _____ to establish your professional credibility?

Berliner: Well, I think by the middle '50s _____, NIH was considered the equivalent of _____ as far as the quality of the work was concerned, and a lot bigger than almost any equivalent.

Summers: Did you have the facilities to do _____ science _____ staff or . . .

Berliner: Oh, yeah.

Summers: _____ postdoc if you wanted it?

Berliner: Absolutely. Well, you know how appropriations work. _____ run out at a certain time. Frequently there was money left over. If you did not want to get that returned to the treasury, which we did, they'd likely not get that much _____. So there was always a rush to spend the money at the end of the year. If something came up during the year that you needed money for, you used it. You didn't have to have that problem at the end of the year. But resources were not limited _____.

Summers: _____?

Berliner: Yes. We were assigned a certain amount of space, and _____ the time. _____ till I left. _____ structure _____. _____ ambulatory care, which freed up space _____. _____ probably recognize _____.

Summers: What about the personnel? Were you able to hire as many as you _____?

Berliner: That's something else. _____ so that that was always a problem, slots _____ what level you were _____ what you paid _____ number of slots, which made no sense at all. _____ contracting out _____.

Summers: _____.

Berliner: Well, you know, _____ things were mostly _____. _____ investigation _____ couple of years. But _____ didn't do much else except _____. So that was the most difficult thing _____ council _____ society to do. _____. And the Physiological Society _____ also _____ bunch of journals.

Summers: _____ problematic issues over _____?

Berliner: Oh, yes.

Summers: Arguments over the direction the journal should go?

Berliner: Well, _____ before I got _____. _____. They ran _____. They were in charge. Not that they were getting _____ out of it. But _____ a big _____. _____. _____. And that system was abolished and _____ over to a completely _____. But, as I say, _____. The only _____. So I got out _____. _____. I got out because _____ physiology _____ Washington _____.

Summers: So did the society _____ active role _____ policy matters _____?

Berliner: Not really. Of course, you know, the Physiological Society was part of the Federation, and they used to lobby for NIH _____ as part of the federation, never separately _____. But they were never very much involved _____. _____.

Summers: Did you get involved in _____ government employees _____?

Berliner: That's right. I guess I worked for the _____ group. _____ part of my job.

Summers: What was the relationship _____ NIH _____ AMA?

Can you hear what I'm saying now?

So let's pick up on some of the questions about NIH in the discussion.

I asked you before about the relationship of NIH to organized medicine, the AMA, various other . . .

Berliner: Well, as far as I know, there was really no formal relationship at all.

Summers: Were they suspicious of what NIH was doing?

Berliner: Well, I'm sure they must have been initially. By the time I was much aware of what was going on, I'm sure they had gotten over it, because they really didn't have much to do with anything they were doing.

Summers: What about other agencies that supported research, like the Cancer Society, the Rockefeller Foundation?

Berliner: Well, they all got in on -- they were all very much involved in lobbying for NIH appropriations, and there was a certain amount of movement of people from NIH to those organizations. For example, Dick Rousha [sp.], who at one point was the director of the Cancer Institute, became head of the National Cancer Society, whatever it's called.

Summers: American Cancer Society.

Berliner: American Cancer Society. But as far as the research activities are concerned, there really wasn't very much relationship.

Summers: What about the pharmaceutical industry and their interest in research vis-a-vis NIH?

Berliner: In those days you didn't have nearly the amount of involvement, that, you know, patenting this and patenting that, which has become such a big deal these days, and people moving from academia to the pharmaceutical industry. There wasn't very much of that. The only important big thing I can remember is that Sid Utterfriend [sp.], who was head of one of the laboratories in the Heart Institute, _____ very capable, left to become head of the Rouch [sp.] Institute and took with him several people, _____ of NIH scientists.

Summers: Was there a concern in NIH about the whole issue of public versus private information and confidentiality and . . .

Berliner: I don't remember any. A certain number of people would give talks at pharmaceutical places and so on, and . . . But other than that, I don't remember any particular collaboration.

Summers: And what about the relationship of NIH to other government health programs; for example, other health initiatives; for example, the national health insurance ideas?

Berliner: No relationship.

Summers: The smoking campaigns or . . .

Berliner: Well, now, the smoking campaign, I guess the original statistical things relating cigarette smoking to lung cancer was done by people at NIH -- well, partly, not entirely. There was one also at Sloan-Kettering. I can't remember -- I've forgotten all the names. But some of the NIH statisticians and people were involved and provided the data which led to a certain general statement, which was on all the cigarette packages.

Summers: Was that discussed or debated within NIH?

Berliner: Not that I remember.

Summers: I think the big study that Harvard, the Framingham, must have started a long time ago.

Berliner: That was a Heart Institute program originally. That was an Intramural . . . Well, not intramural in the sense that I, as director of Intramural Research,

had nothing to do with it. It was sort of run out of the office of the director of the Heart Institute, and I have to admit that neither Jim Shannon nor I had a very high opinion of the Framingham thing. What in a sense they did was collecting numbers to prove things everybody was convinced about in the first place.

- Summers: Who was the director? I mean _____?
- Berliner: I can't remember. Over the years, there were several different ones. One was Dawber, D-a-w-b-e-r. I never really had much of an idea what exactly went on.
- Summers: When did it start?
- Berliner: Oh, it started in probably 1948 or something like that.
- Summers: And was it different than other programs or . . .
- Berliner: Well, it was a long-term longitudinal program, and they examined a lot of people and then followed them for a number of years. And the _____ measuring. They measured their cholesterol and their blood pressure and so on, things that people were pretty well convinced were important related to heart disease. And they got some good numbers, no question. The data they came out with had served a lot of purposes because they're solid numbers. But I don't think they discovered anything new.
- Summers: So, was that motivated by the need to convince the larger medical community perhaps?
- Berliner: I'm not quite sure just what the original idea had been, I mean, why they decided to do it. But they did a very thorough study and . . .
- Summers: _____ there were other programs that the government was involved in, the beginning of OSHA and so on. Was NIH consulted or involved?
- Berliner: Well, the growth of OSHA was after I left NIH, and I don't know whether they had any relationship at all.
- Summers: Another one that was postwar was the so-called Atoms for Peace program, the whole use of peaceful medical uses of atomic energy. Did NIH get involved in that?
- Berliner: Not that I know of. Not as far as I know.
- Summers: You had talked before about the relationship of NIH as a unit of the Public Health Service with the other Public Health Service divisions. Could you say a little bit more about that?

Berliner: Well, the original . . . The NIH, up to the time around 1948 or 1950, was really very much a Public Health Service . . . The people were public, almost all Public Health Service people, many of them commissioned officers. And with the growth, commissioned officers became important because a lot of the younger people came in as commissioned officers during the Korean War and during Vietnam and so on. And in between, the doctor draft continued after the Korean War, went right through the time of Vietnam, so they didn't necessarily avoid going to Vietnam by being in the Public Health Service because it was just part of reserves.

Anyway, as the NIH became . . . As appropriation for the NIH increased and so on, NIH became more and more independent in a sense. The rest of the Public Health Service was very envious, I would say, initially and resented the NIH because they looked on NIH as a privileged part of the organization _____ most of the functions. As a matter of fact, many of the other functions of the Public Health Service withered away that the hospital . . . The original Public Health Service was the hospital function, with the Marine hospitals. Staffing the Marine hospitals was the original Public Health Service, and one by one, Marine hospitals were closed up over the years. I don't know if there are any left now at all.

Summers: What about -- was the Indian Health Service, prisons, federal prisons, under NIH?

Berliner: No. They were not yet under the Public Health Service. They were not NIH.

Summers: Did these _____ places or were they places where NIH could do clinical studies or . . .

Berliner: Let me see.

Summers: Because I recall some tests on various vaccines during the war in prisons, you know, _____.

Berliner: Yeah. But they were not particularly Public health Service. I don't think - - no, I don't believe there was any particular relationship.

Summers: What about the relationship to the federal, the Food and Drug and the Bureau of Biologics?

Berliner: Well, the Bureau of Biologics Standards was _____ was an NIH function until about 1970, roughly, somewhere around there, when it was transferred from NIH to the FDA. I guess the director of NIH interacted with the director of FDA through the surgeon general's office and so on,

the secretary's office and so on, but I don't believe there was any formal connection.

Summers: Postwar, there seemed to be a development of biology at the former, at the national labs like Argonne and _____ Ridge and Brookhaven in quasi biological and medical sciences. Did NIH have a relationship or was instrumental in that?

Berliner: Not in any formal way, no.

Summers: Did this mean that NIH, in terms of what kind of research it did, would avoid the kinds of things that were going on at these institutes?

Berliner: No, not necessarily. I don't think there was much overlap between the kinds of things that were going on in the national labs and the NIH. As far as I know, there was no particular coordinating mechanism to make sure there wasn't or that there, or to make sure that there was _____.

Summers: Was there things in place to facilitate space sharing of facilities or interagency agreement kind of things or . . .

Berliner: Not that I'm aware of.

Summers: What was the relationship of NIH to the CDC? I guess it was called the Communicable Disease Center.

Berliner: Communicable Disease Center, yeah. Again, well, _____ I'm sure there was considerable discussion of ideas and so on, but I don't think there was any actual physical relation between them.

Summers: Do you recall that Congress used the NIH as a resource, or did congressmen come out to find out what you were doing that would be of interest to their various constituents or . . .

Berliner: Well, I think most of that occurred in hearings on NIH budget plans. The NIH encouraged people to _____ get care, although we didn't get anything like the numbers that went to Walter Reed or the Navy or something like that. But we did, as I mentioned, have a number of key players.

Summers: Why did they go to Walter Reed or the Naval Hospital?

Berliner: I think that was a sort of a formal _____ usual route. They were eligible for care there for some reason, and I never went into it. But they _____. I think I mentioned it. Shannon saw a couple of very important people from the NIH appropriations were the ones who came to NIH.

Summers: What was the relationship with Walter Reed, the Naval Hospital, and NIH? Were they _____?

Berliner: None in particular.

Summers: _____.

Berliner: No. There were some people . . . Well, Joe Slidell [sp.], for example, who took over the job that Jim Shannon had before he became director, had come from Walter Reed. There was John Seal [sp.], who was a scientific director of the _____ infectious diseases, who had been a naval officer. So there was some movement of people. I believe Casey Cole and his whole group had been at the Naval Research, and I think Manuel Morales as well, and they all moved over to NIH at one point. The people -- it was not NIH and they were collaborating, but just a matter of people.

Summers: And eventually the military medical school, the Uniformed Health Services, whatever they call it, was formed. Was that an NIH -- was NIH involved in that or . . .

Berliner: No. As a matter of fact, the guy who had been my instructor in surgery when I was a medical student, Pete Rousinov [sp.], was very much behind that, and I had a lot of discussions, and one very close friend and classmate of mine, Spider _____ web, was sort of his -- was helping Pete Rousinov [sp.] in his _____. Pete was something in the Office of the Surgeon General of the Navy, I guess. And _____ I don't think we thought much of the idea of establishing this new medical school. That was just about -- that was shortly before I left NIH. The medical school really was just getting started, hadn't actually opened its doors.

Summers: And why were these guys pushing for this medical school?

Berliner: It was supposedly to supply officers for the armed forces. How successful that had been, I don't know, but I would guess at least they got a few years out of each of them because that was part of their commitment for being there. I have not really followed it very closely, so I can't tell you much about it, how successful that has been.

Summers: Was it assumed that the faculty and the students would have any relationship with NIH in research or . . .

Berliner: No. _____. Subsequently, I don't know.

Summers: When -- right now the National Library of Medicine is a beautiful building there. That must have been a relative _____ in the '40s or '50s.

Berliner: That was, became an NIH function in, somewhere around 1967 or '68.

Summers: What was there before that?

Berliner: Well, it was called the Surgeon General's Library. I'm not sure just where it was located, but it was not . . . And then they built that building and it was transferred. The NIH acquired two functions. One was the library and the other was the support of medical education, which had never been a function of NIH before. That was the capitation business. And that was just about the time that I moved over to Building 1 shortly before ____.

Summers: Did you help ____ director's office?

Berliner: No.

Summers: We talked a little bit before about Shannon and his plans and vision. But let's go a little more into that.

He presided over this great increase in federal support. Do you think that he envisioned that the support was going to have to be federal support forever or that there would be other sources of support or . . .

Berliner: I think he looked on the NIH as the major source of support for research. I don't think he particularly looked on ____ thought that the federal government was going to be a major supporter of medical education or of the pharmaceutical industry or whatever. But research, yes.

Summers: Did he see that this expansion was going to come to a plateau at some point? Was there discussion of the ____?

Berliner: Everybody ____ going to continue to increase at a rate of 15 percent a year, but I don't think he ever thought it would level off altogether.

Summers: Was there a discussion about what the goals should be, that is, what the desirable ratio of investment and research?

Berliner: Not as far as I know, no.

Summers: And what about the possibility of federal money, meaning federal control? Was there debates about . . .

Berliner: Of course, that was the idea everybody had. It was one of the reasons why ____ people were against ____ like the NIH. But I don't think NIH ever looked on it that way itself. It was a concern ____ academic immunity and AMA ____.

Summers: Was there a sense that . . . I mean, were there academic leaders in opposition to the NIH's budget?

Berliner: I don't believe so. No, no. I think it started _____ really became, almost from the start, a great, great source of support for much of academic research. I don't think anybody would have looked at kind of a gift horse in the mouth.

Summers: When did the strings start becoming attached. I mean, we now are aware that if you take federal money, you have to meet compliance for certain demography, for equal opportunity, for all these . . . I mean, they're using the federal funding as a way to coerce _____.

Berliner: Yeah. I think that's . . . As far as I know, that's a more recent phenomenon. I mean, it had gotten to be a lot of _____ things that go with the overhead that didn't exist in the earlier days. As a matter of fact, initially NIH paid 8 percent overhead, period. That was the standard. And I guess it didn't really matter much when NIH research was only a small part of what any institution was doing, but as they grew, it became to be a very substantial expense. And, fortunately, the idea of paying full cost finally got solved in the middle '60s, I guess it was.

Summers: When did training grants start? Was that _____?

Berliner: I'm not quite sure. I really don't know. I'm sure that there were training grants before Jim, but nothing like on the scale that he managed to get. And I think most of the training grants initially were not intended to support research, but for the training of specialists.

Summers: But the training of specialists came first.

Berliner: I think so. _____.

Summers: How did they decide which specialists _____?

Berliner: The Heart Institute, it was cardiologists.

Summers: I know, but I mean, was there a feeling of we needed more in certain areas, and other areas were already filled? Was there a manpower survey _____?

Berliner: No, hell no.

Summers: So these training grants were given to departments or to universities to . . .

Berliner: No. They were to individuals.

Summers: They were individual.

Berliner: Yeah. There was a principal investigator just as there is on a research grant. I think I mentioned the other day that we were looking over at so many areas where they were training grants. The guy who was allegedly responsible for the training had never done any research himself _____.

Summers: And so these were like individual postdoc or graduate student fellowship training?

Berliner: Well, _____ was that training grants frequently had both postdoctoral and pre-doctoral _____ and a certain number of slots, a certain number of funds to support a certain number of people, and they were the equivalent of fellowships essentially.

Summers: But then the PI or the mentor was the one who selected the candidate.

Berliner: That's right. Of course, NIH had direct fellowship support in addition to the training grants. I mean, there was your NIH fellowships, which were awarded directly to individuals.

Summers: And there was preference for, well, certainly in the clinical area, for M.D.'s, but was there an intent to attract M.D.'s into research by putting more money in this direction, or did that come later _____?

Berliner: I don't think anybody at that time was, had the same kind of concerns that you hear these days about not enough M.D.'s doing research. In those days it was mostly M.D.'s.

Summers: Was there, were there many visitors that came to NIH for like sabbaticals or sort of short-term collaborators?

Berliner: Oh, yeah, oh, quite a number. But that grew over the years and it became quite a -- there were large numbers of people from outside, with outside support of one sort or another.

Summers: And the balance between applied, basic, and clinical, how was that adjusted or defended?

Berliner: It was -- I don't think anybody really often thought of it in those terms except when somebody asked, and then you tried to figure out what do they want _____ to classify your research in these categories. _____ how much of it is basic. And you try to guess as to what their motivation was, which they'd like to see higher, and that would have a very strong influence on how it broke down. _____ sometimes it's pretty hard to say _____.

Summers: You mentioned your collaborator, I believe, Bob Bowman . . .

Berliner: Yeah.

Summers: . . . who became involved in sort of support of instrumentation. Were there other examples of support kind of groups at NIH where there's, for example, statistical people who would basically not do so much their own research but just _____?

Berliner: Oh, yeah. There were mathematicians, there were statisticians, a lot of -- there were statisticians at least in the Heart and in the Cancer Institute. A number of methods were developed at NIH.

Summers: Can you think of some that you recall?

Berliner: Well, let's see. There was one Herb Silver [sp.] was involved in. I can't remember.

Summers: Well, was there a concern that . . . I mean, heavy instrumentation, that is, developing large machines or complex things that would be one of a kind or would have to be in the center, did NIH see itself as a place for that, say, like the National _____ Lab.

Berliner: No, no. I don't think at that -- well, at least when I was there, the NIH was _____ instrument center for anything. People came from all around to use it because that was at NIH. NIH developed a number of such places extramurally, but not anything _____ as far as I can remember in the Intramural Program.

Summers: One of the things that I understand Shannon was concerned about was the cost of research was going up. It was no longer something that a little bit of money could supply because of the instrumentation and because of the expense of, well, reagents or whatever. Did NIH -- was this rule applied internally too, that is, that the money was available for development of new things?

Berliner: Oh, yeah, yeah. It was pretty much the same. NIH intramurally ran pretty well parallel to things in the Extramural Program.

Summers: So, what about the relationship with commercial firms from, say, Beckman, for example, or, well, outfits that made equipment? Did they have a big presence at NIH in deciding where science was going or . . .

Berliner: Not really. I can't remember anything that I would classify that way.

Summers: I'm just wondering where these companies got their ideas for what to build for scientists. You know, needle micro holders or new _____ devices or, you know, the kind of instrumentation that becomes very expensive and highly sophisticated.

Berliner: I don't really know. I don't know.

Summers: Was NIH conducive or opposed to the idea of people taking their ideas and inventions and making small companies? Did people do that?

Berliner: No. _____ their own. No, as far as I know, that didn't exist at all. I'm not sure whether it does today either. I suspect . . . I'm not sure government employment is compatible with running your own business.

Summers: I see.

Berliner: Certainly I've seen any number of NIH people have gone off and joined industry, but not while remaining part of NIH, as far as I know..

Summers: You mentioned that the Cancer Institute solved some of their space problems by subcontracting.

Berliner: _____ a space problem, a slot problem.

Summers: A slot problem. By subcontracting off campus. The Cancer Institute was the major one that did this?

Berliner: As far as I know. I don't remember any others while I was around.

Summers: Did this get _____?

Berliner: I remember that Bob Gallo had a big outfit somewhere that he sort of ran on a contract.

Summers: Well, this he had sort of inherited from Bob Huebner [sp.] _____.

Berliner: I guess so. I'm not sure. I don't remember any big contract operations in the Allergy and Infectious Disease Institute.

Summers: Well, maybe it came -- my recollection was when Gallo sort of took over Huebner's [sp.] mantle _____.

Berliner: I think so.

Summers: Was there concern about conflict of interest in these cases?

Berliner: In which cases?

Summers: Offshore or off-site contracts.

Berliner: No. I mean, they were essentially intramural operations. There wasn't any conflict.

Summers: I remember visiting _____ and you couldn't tell the difference, you know, _____ labs and a couple of others. These guys just sort of ran or their wives ran or something like that.

Berliner: Yeah, that's right.

Summers: In the grants program, how did the NIH decide between what NIH thought science should be doing and what the investigators thought it should be doing. That is, did you just rely on investigator-initiated ideas, or were there . . .

Berliner: Well, for a long time it was almost entirely -- there was money available in certain areas, and if the applications came in that fit in that area, they went _____ where the funds came from. The idea of requests for proposals was something which arose much later. It must have been -- NIH must have been making grants for 15 years or so before that became a practice. So the internal logic of the field was largely what determined . . .

Summers: How did -- you say the money was available for certain areas. How was that apportioned?

Berliner: Well, it was mostly _____ what the institutes asked for in their appropriations.

Summers: So the institutes, through the scientific director, for example, would decide . . .

Berliner: Not particularly the scientific director. No. That was the institute directors, extramural, intramural. The scientific directors had very little to do with the Extramural Program. So what was _____ Office of the Director of NIH and the institutes' administration.

Summers: And so they would decide that kidney needs 10 percent of our budget and heart valves need 10 percent and . . .

Berliner: _____ it wasn't quite -- it wasn't all divided up that way.

Summers: I see. Well, what were the categories? Were they disease categories or problem categories?

Berliner: Yeah. Almost entirely disease categories.

Summers: Not mechanism, not membranes ____.

Berliner: No. I don't think the word membrane ever got into an NIH ____.

Summers: And this was because that's the way people thought about it or because . . .

Berliner: Well, that's the way they thought about it and the public represented it in Congress.

Summers: What did NIH do as far as attempts to communicate with scientists on the outside? I know, for example, the Cancer Institute published a journal. Did the other institutes publish journals or ____?

Berliner: As far as I know, the Cancer Institute was the only one that had a journal of its own. Well, I mean, there was lots of communication because, first of all, people in many areas came to NIH to try and push their ideas and see if they couldn't persuade the director of NIH or somebody to do more in some area or other. And, of course, there were plenty of scientists in and out of NIH in the study sections.

TAPE 5, SIDE B

Berliner: So there was plenty of advice and persuasion from outside as well as . . .

Summers: So the NIH did sponsor meetings, too.

Berliner: Oh, yeah.

Summers: When did this whole notion of consensus conferences start?

Berliner: Oh, that was long after I left NIH.

Summers: Was there an attempt to or did NIH see itself as a public education, outreach kind of organization, campaigns, for example, about smoking or health or diet?

Berliner: No, not at all.

Summers: Was there attempts to push NIH in that direction?

Berliner: Not as far as I know. The surgeon general's office did a lot of that sort of thing, of course. ____ smoking was a major one. But I don't think any of the working arms of the Public Health Service ____.

Summers: How was the Intramural and Extramural Program balanced? It started out to be mostly intramural and then developed this live grant program. Was this pretty much Shannon's doing?

Berliner: Well, it hadn't started. I mean, that the extramural program grew to the size that it did was largely Shannon's doing. But the growth of the intramural, I think the growth of the extramural program really came with the transfer of those wartime grants to NIH.

Summers: And the study sections got started just about that time.

Berliner: About then, yeah.

Summers: And how were they picked? Was there an old-boy system who the section, the executive secretaries knew?

Berliner: Not -- I don't think _____ particularly . . . Well, I don't really know about how they decided things. But, you know, when it got started, the number of possible candidates was rather limited anyway because there weren't that many people active in the various fields. So it probably was relatively easy to find the right people.

Summers: When did site visits get started?

Berliner: Oh, not long after the study section system was established, although not on the scale that came later. I never quite understood why it was necessary to have all these site visits. I don't know what . . . I think it would have been a lot cheaper to bring the investigators to NIH than to send everybody out to visit this place _____.

Summers: Was this a sense that one had to be accountable to Congress and so on, and have _____ paper trail?

Berliner: No, I don't think so. No. I think that it started out, I think, that we really ought to know more about what's going on in this place in order to be able to decide whether this is a good thing to do. But, as I say, I'm not sure that the site itself is a very important part of what the study section learns from making _____ site visits.

Summers: Did various people on the outside try to _____ around NIH and the grant system to get special favors from Congress at that time?

Berliner: Well, there were always a few special things like building a building or something somewhere that short-circuited the regular process. And there was always somebody who had a friend in a place of influence who might get some pressure on to fund a particular thing. But that was relatively minor. It was not a big deal.

Summers: Was there interest by other government units to get involved in the health-care field?

Berliner: Not as far as I know.

Summers: I'm thinking now of the Army Breast Cancer Initiative.

Berliner: Oh. Well, that kind of, you know . . . Walter Reed, for example, did research. I guess there was a research unit at Naval Medical Center, but nothing like this breast cancer thing that's occurred recently. It was an extramural program essentially for, in the Army or whatever it is. That sort of thing didn't exist at all as far as I know.

Summers: What about with the Veterans Hospital system and research? Did that sit within NIH?

Berliner: Well, the VA hospitals became eligible for NIH research grants; that is, the people in the VA hospitals. But as far as I know, there was no particular relationship with the VA.

Summers: So it was just another independent organization.

Berliner: Yeah, like a medical school or a university.

Summers: We talked a little bit before about the expansion in the number of institutes, and I was wondering what the, how the motivations changed over time. They seemed to start with a disease orientation or an organ orientation and move to a population orientation, that is, childhood or aging, that sort of thing.

Berliner: Well, the general feeling has been that unless there, if you're only part of another institute, your particular interest is not going to get the same kind of attention it would get as if it had an institute devoted to it. An example _____ the Eye Institute, which was, originally had been part of the Neurological Diseases Institute. They weren't happy with the fact that they were only part of something. They wanted to be . . . And I guess to a certain extent there's some validity to that view. Whether it's a good thing or not is another question. But the NIH usually was opposed because it multiplied the administrative side of things without really providing . . . It was a toss-up as to whether the increased funding for the research would be worth the administrative costs of setting it up. I don't know that . . . There had been sort of a . . . Some of the new institutes were devoted to organs or, and some more broadly, like the child health thing. But I don't know that there was any particular trend in one direction or another.

Summers: I mean, the first institutes were the Mental Health and the Dental and the Heart Institutes, after Cancer, and then it seemed like they became more and more, well, as you say, visibility for various people's interests. Did the motivations come from within NIH?

Berliner: No, no. They were entirely from outside. The NIH did not propose any new institutes as far as I know. The only one was General Medical, which was essentially something which existed and which wasn't called an institute before. But as far as I know, that's the only one that was generated by NIH.

Summers: Why was that generated? What were the arguments and the discussion inside NIH for that one?

Berliner: Well, just to get it the same kind of status as the rest of the NIH functions.

Summers: Was this an attempt to make sure that fundamental, basic research was recognized or . . .

Berliner: Yeah, I guess so, because that was sort of . . . You didn't have to have a categorical label for things that went to that institute.

Summers: What are your recollections about the surgeon general at the time? I guess his name was Sheely [sp.]?

Berliner: Sheely [sp.] was the surgeon general when I . . .

Summers: Was he a strong supporter of the NIH?

Berliner: Yeah. Well, you know, I guess you might say he didn't interfere. That was a general -- I think generally that was the attitude of the surgeon generals. The surgeon general was sort of neutral. Of course, you know, no -- I can't think of the word -- administrator or whatever is ever opposed to having more resources under his aegis, so the surgeon generals in general were _____ an increase in the appropriation for any part of that of their domain.

Summers: It seems, by and large, surgeon generals have a shorter tenure than the directors of NIH.

Berliner: Well, they certainly were during the period of 1948 or '50 to 1970 anyway. See, there were two directors of NIH between 1950 and Jim Shannon. When I got there -- what the, I can't remember the name -- Henry Sebrille [sp.] preceded Jim, but Sebrille [sp.] had only become, was only a director for a couple of years. My guess, there were about four or five surgeon generals during that time.

Summers: And what about the relationship of the secretary of HEW? I think _____ Folsom [sp.] was the . . .

Berliner: Folsom [sp.]. That's the name I couldn't remember. He's the one that was from Rochester. He was very favorable to . . . And it was under his watch that Jim Shannon was able to get the NIH, really begin to expand on _____. There was very strong support from Folsom [sp.].

Summers: Why was he so interested in NIH?

Berliner: I don't know. I guess Jim Shannon just sold him on it. As far as I know, he didn't _____ have any reason for coming but _____.

Summers: It seemed like there was a long period from the time NIH started to support research till the time they really started to support education, medical schools. What accounted for that delay in getting into the educational aspect of . . .

Berliner: I don't know. A lot of it I'm sure had to do with the suspicion of government taking over _____. When they got their foot in the door, they would be, try to run _____. I'm sure that was a part of the atmosphere, but I really don't know the answer specifically to your question.

Summers: During this time, do you think that there was a skewing of the medical schools as researchers got richer and richer and the teachers got nothing?

Berliner: Well, I guess at some schools that was probably true. But I don't know that anybody ever thought that that was necessarily a bad thing, although there were some people who weren't doing any research.

Summers: But do you recall any, in the discussions at NIH, about how we can get more money to education, or this was not something _____?

Berliner: No, not at all. I mean, I think I said the reasons for NIH getting involved at all was not as a general support of medical education, but in order to facilitate the expansion of the numbers of people being turned out by medical schools. It was strictly based on expansion rather than allegedly paying for the expansion.

Summers: And they did that by this capitulation.

Berliner: Right.

Summers: Do you think that was successful?

Berliner: It may have been. Well, certainly it did get the schools to expand. In that sense it was certainly successful. Whether it became a major part of support of schools is a different question. It was a pretty trivial amount when I got _____. At one point they decided that in order to qualify, you

were going to have to take transfers of Americans from foreign medical schools, you know, _____ West Indies or whatever, and I was very negative about this. I'm not at NIH now; I'm at Yale. And I said, if that's the rules, we aren't going to take any capitation. That's because I didn't think they ought to be telling us what we had to take. As a matter of fact, we did take a couple just to show that it was not because we wouldn't take good people, but we weren't going to have them telling us who we had to take in.

Summers: So the fears about control were realized.

Berliner: Yeah. That's true, to a certain extent anyway. This was based on the lobby of the families. At this time, the number of applicants to medical school was something like four times the number of slots available, and a lot of people were going outside the United States to go to school. Some of them were pretty good, obviously, and some of them were not very good. But with that kind of demand, these families had lobbied strongly to get something done so their kids could get back into _____. That's how it happened. And I thought the AAMC was, really laid down. They didn't stand up for what they should have.

Summers: NIH was supporting construction grants, too, during the '50s and '60s.

Berliner: Yeah.

Summers: Now, was this related to the research infrastructure, or was the construction . . .

Berliner: Oh, yeah. It was entirely for research.

Summers: Hospital construction was a different _____.

Berliner: Hospital construction. That was _____. That was a separate _____ had nothing to do with NIH.

Summers: And was this easy money to get? Did Congress appropriate that?

Berliner: I think that construction funds were often the hardest to convince Congress _____ to do. It never really got to be a very big part of the NIH appropriation. I can't really say from personal knowledge, but _____ the situation.

Summers: What about construction on the NIH campus itself? Buildings seemed to sprout like _____.

Berliner: During the time I was there, they got up to 31. That's a pretty good expansion. Of course, now, some of them were not very big buildings. Almost any shack would get a number. But they didn't nearly fill up the way it has since. I can't remember how the construction of the intramural buildings on the NIH campus were supported. I'll have to pass on that one now.

Summers: Was there a concern about the way medicine and research was going in terms of which schools were developing? Was there an attempt to use government, NIH policy to do things they thought should be done?

Berliner: Not as far as I know.

Summers: According to different geographic areas or . . .

Berliner: Well, you know, that's always an issue. And it was no real coincidence that the University of Washington did very well while Magnuson was head of the committee that dealt with appropriations. And I don't know, you know, some interest in Congress in getting something in their own district or their own state. But it didn't, on the whole, have a major effect on where things went.

Summers: What about which people were funded? I don't mean individuals, but I mean, was there a concern that, about more women or more minorities, doctors or researchers or . . .

Berliner: Not in the earlier days. That really came later.

Summers: I read in some of Shannon's writings, he made a big point of a meeting of the NIH institute directors with President Johnson in 1966 about the future of NIH. Do you remember that time, or was this _____ to pick out?

Berliner: Well, I remember that situation. I don't remember the details. It's been a long time. But that had a major influence. That was the point at which NIH stopped growing so fast. And it was not long before Jim retired. That was a very important juncture, but I can't remember exactly how it played out.

Summers: Do you remember what was the motivation for having the meeting? Was this at Shannon's request or . . .

Berliner: No, I don't think so. I think that came from upstairs. But I can't really say. I wasn't directly involved. But I do know that it was an important juncture, and I can't say exactly how it played out.

Summers: When you said Shannon had mentioned that it was, that Johnson was concerned over -- we had to worry about rapid translation of research results into patient cures. And do you think that was a -- he was getting political pressure to show some results or . . .

Berliner: Not as far as I know. If he did, it didn't have much influence on what happened.

Summers: Or was this just a, do you think, a matter of concern over budgetary issues?

Berliner: I think it was more, it was a main issue with budget. It was at the height of the Vietnam War and the war was getting very expensive. There were budgetary problems throughout the government beginning to show up, and I think that may be what prompted that particular ____.

Summers: Were there other meetings with presidents that you can remember that went on while you were at NIH?

Berliner: Well, I'm sure there must have been, but I'm not aware of them.

Summers: Was NIH a place where, you know, for ceremonies, the president or vice president would come to cut ribbons or . . .

Berliner: Well, I remember the first one, when Truman came out to cut the ribbon for one of them. That was just shortly after I got there. I remember the cabinet people being there, but I'm trying to remember if I ever saw another president out there. I don't think so.

Summers: When Nixon, I guess it was, declared the war on cancer, was this something that NIH wanted or . . .

Berliner: Hell no! No. As a matter of fact, that was _____. That was when I was a deputy director for science. And one day Mary Lasker called Bob Marston [sp.], who was then director, and they invited Bob and me to come and have lunch with her at Mrs. Harrison Williams' house on Foxhall Road. I don't know if you . . . Mrs. Harrison Williams was _____ considered the best-dressed woman in the country or some such thing. Anyway, she had a very, very fancy house. And we had lunch with Mary Lasker. And I think Florence Mahoney was there, and Mrs. Williams at Mrs. Williams' house. And it wasn't quite clear what the hell we were doing there, but finally she got -- Mary Lasker got around to saying, "You wouldn't mind if I got you a lot more money." I said, "Of course we wouldn't mind." But we didn't know what she had in mind. It turned out this was -- she then came up with a . . . There was a senator from Texas . . .

Summers: Tower?

Berliner: No, no. He didn't get reelected in the subsequent election.

Anyway, he had introduced, was about to introduce this bill for the war on cancer, and this was the start. But the NIH, the director's office, did not particularly support the idea of this so-called war on cancer.

Summers: And why was that?

Berliner: Well, we thought the Cancer Institute was reasonably well funded and they were over-promising. They weren't going to cure cancer in the next five years, which was more or less what was being promised. And we thought it would be oversold and that it would have, in the long run, a negative effect because . . . It didn't turn out to do that, to have that negative effect. It turned out not to -- some good stuff has come out of the additional support. But it hasn't cured cancer by a long shot.

Summers: And so, was the Cancer Institute in on this?

Berliner: Oh, yeah, of course.

Summers: But, I mean, were they in on it before the director of NIH was in on it?

Berliner: Well, it's hard to tell. They might have been -- not publicly.

Summers: _____.

Berliner: No, not publicly. Of course, I'm sure they must have known about what was happening somehow through the grapevine, but . . .

Summers: And who was the director of the Cancer Institute back then?

Berliner: I think it was Dick -- well, Dick Rousher [sp.] certainly was by the time anything happened, but I'm not sure whether he was director at that time or whether it was . . . Oh, what was his name? I'm terrible at names. you know. It's not just that I can't remember them, but I can't remember the ones I spoke to yesterday. I'd have to refresh my memory. I can't remember _____. I think Dick Rousher [sp.] was, and he was a great buddy of a guy who had -- he had been one of the major pharmaceutical . . . He was very close to Nixon. Bokst [sp.]. And he was very much involved in this, and he was a good friend of Dick Rousher's [sp.] and also was very, somehow had very close relations with Nixon. He was one of the -- I can't remember which pharmaceutical outfit he was head of at that time. It doesn't matter.

Summers: And so this was -- was this the first sort of targeted kind of program publicly announced at NIH, or was there another one?

Berliner: The first kind of what?

Summers: The war on cancer.

Berliner: Oh. The first war on anything?

Summers: War on anything, yeah.

Berliner: As far as I know, yes.

Summers: The war on polio, if you will, was not quite promoted the same way.

Berliner: No.

Summers: That was a . . .

Berliner: Yeah. And that was not an NIH thing at all, really. That was largely supported by the Polio Foundation.

Summers: And what about the later war on smallpox? Was NIH involved in that?

Berliner: Not really. I guess some of the people in Allergy and Infectious Diseases were involved, but not -- if that was done. I'm not sure just what part of the Public Health Service was. Probably CDC.

Summers: So did NIH see itself as sort of the research as opposed to the implementation end of things?

Berliner: Yeah, definitely. It was not, on the whole, involved in implementation. I would say that _____.

Summers: Because I recall with the whole AIDS business, one of the criticisms was that the NIH was sort of an ivory tower and should be doing more to, you know, whatever one does.

One of the things that comes about this time, as I recall, was the whole issue of accountability to Congress and the Fountain Committee investigation and that sort of thing. What do you recall of that?

Berliner: Not very much. I don't really remember much about it.

Summers: Because it had to do with some accusation of wrongdoing, as I recall, and a congressional investigation of accountability, or some money was spent for something. I don't _____.

Berliner: You've probably got that right, but I just don't remember it at all. In the intramural program, we were pretty much isolated from all the stuff on _____, so that we frequently really didn't know too much about what NIH was doing extramurally and so on.

Summers: What were some of the issues that were going on intramurally in the '60s?

Berliner: I don't think anybody ever thought of it as issues. It was just research.

Summers: The good old days.

So, do you remember major projects you worked on in the director's office or . . .

Berliner: Not really, no. The scientific directors used to talk about promotions.

Summers: Parking?

Berliner: Ah, yes, parking.

Summers: When I interviewed at NIH, that was the question I asked. _____ 8,000 people drive into the parking lot at the same time. I said, my God, how do you do that?

Berliner: One of the great things about retiring from the dean's job here, because after 33 years, I no longer had to worry about anybody's parking but my own.

Summers: So, NIH administratively ran reasonably smoothly?

Berliner: Oh, yeah. There were really no big fusses about with anything.

Summers: What about the technical staff and the whole supply of assistants? Was there a personnel department, training groups, and all that sort of . . . How did you get staff?

Berliner: Well, there was a personnel department at NIH, you know, that classified people and classified their jobs and so on, because they were paid by, you know, on a scale of the federal . . .

Summers: So this was a standard federal employee.

Berliner: Yeah.

Summers: All the employee issues had already been worked out.

Berliner: Yeah. The only issues would be what level do they qualify for. Most of the training was on _____. We were not very heavily into technical

support. Many, many people preferred to do their own stuff and had only minimal, if any, technical help. And technicians filled slots. A slot could be a technician or a slot could be a GS-15. So he was very careful about _____ people.

Summers: Was there concern about hiring foreigners, or how did you do . . .

Berliner: Well, foreigners were not eligible for regular civil service, and most of the foreigners who came were supported by outside fellowships or something from their own . . .

Summers: What about security? Was there any concern about national security?

Berliner: In the early days, there was lots of -- anybody who, at the professional level certainly, had to be okayed by the civil service and frequently required FBI investigation and all that nonsense. That was very unpleasant in the early years, in the middle '50s. And occasionally somebody whose . . . One guy was said to be ineligible for a commission in the Public Health Service, and I raised hell about it and fought it and finally he got a commission. The reason why he didn't was because his father subscribed to the *Daily Worker*.

Summers: This was the legacy of the McCarthy worries?

Berliner: Well, yeah. But McCarthy was only part of the, one of the symptoms. The whole business of anti-communism, the House unAmerican Activities Committee and all that, that was all very active at that time. It was very pleasant.

Summers: I mean, this was the _____ presumably someone ran into trouble. What was their . . .

Berliner: Oh, there were others, I'm sure. This is the only one that I personally came up against _____.

Summers: And was there a sense that what people were doing was classified _____?

Berliner: Oh hell, no. They were just working for the government. You couldn't work for the government _____.

Summers: Did NIH have a policy about doing classified research?

Berliner: No. The _____ of scientific directors had secret clearance. I don't think we ever _____ had any secrets fed to us.

Summers: And so, what was the relationship, for example, to the germ-warfare programs, the nerve gases, and Fort Detrick and . . .

Berliner: As far as I know . . .

Summers: You can't tell.

Berliner: I didn't know anything. If there were any, I didn't know about it. If there were, it would have been through the Allergy and Infectious Disease Institute.

Summers: Do you ever recall being approached by people like that from Fort Detrick or stuff on . . .

Berliner: No.

Summers: . . . cholinesterase inhibitors or nerve gases, organophosphates or anything?

Berliner: _____.

Summers: Well, why don't we stop now.

TAPE 6, SIDE A

Summers: So, it's May 8th; sorry, May 9th.

So, Shannon retired in what, '67 or '68?

Berliner: Sixty-seven or '68, I guess. It was probably '68, but I can't remember.

Summers: Was he just _____ ready to retire or was he . . .

Berliner: Well, I think he threatened to resign if he didn't get something from the secretary, and the secretary said, "Sorry." At least there was some rumor about the thing. I don't really know.

Summers: You don't remember what _____?

Berliner: I don't know.

Summers: And what did he do when he retired?

Berliner: Well, he spent a year at the National Academy as sort of a _____, and then he went to Rockefeller, where he was assistant to _____, some kind of _____. I think _____ office of the president _____.

Summers: What did he do from there on?

Berliner: Well, he did that for eight or 10 years, I guess. I don't remember the figures. And then he moved out to Oregon, where his daughter was at the

University of Oregon, and he didn't like that very much. He moved back to Bethesda and finally was in a retirement home _____.

Summers: And did he, in retirement, do things with science or . . .

Berliner: Not in the . . . I think . . . Well, certainly what he did was science, but I don't know how _____.

Summers: How was his replacement decided upon?

Berliner: Well, there was a committee that reviewed a bunch of people passed their names to the secretary, John Gardner. I was one of them, one of the names. And the rumor was that I was the leading candidate. But Mary Lasker didn't think that was a good idea. This is all maybe true or maybe not, maybe _____. Anyway, they ended up picking Bob Marston [sp.]. And certainly _____ was correct _____ the story _____ mythical. I can only be thankful to Mary Lasker _____ I would have been a very unhappy _____.

Summers: And where did Bob Marston [sp.] come from?

Berliner: Well, Bob Marston [sp.] had been dean at the University of Mississippi, where he had integrated the school and very proud of his role in integrating. And he was in -- I don't remember. I don't know if you remember the -- what was it called? It was trying to involve various key medical schools with community hospital _____ in medical programs, and he was put in charge of the regional medical programs. And that was at the NIH, and he was doing that. He did that for a year or two, and then was made the director of NIH.

Summers: I see. So he had come to NIH for other reasons.

Berliner: That's right, yeah.

Summers: So his background was in education. Was he a scientist?

Berliner: Well, he had -- he had been a Rhodes Scholar and had worked at Oxford with -- I'm _____ coming up with names.

Summers: What sort of field? Do you remember?

Berliner: Well, it was related to _____ immunology. And then he had been in the army and at Walter Reed and had been assigned to something at NIH, where he was also doing some sort of immunology stuff. But he was not particularly strong on science. That was why he was persuaded to appoint

me as the deputy director for science when I was supposed to be in charge of the sciences.

Summers: And so you were appointed, according to your records, director for laboratories and clinics?

Berliner: Yeah. Well, that was sort of the title that was given to, I guess, Bill Myer, who was my immediate predecessor in that job, in charge of intramural research _____. He had been called associate, previously associate director for intramural research or something like that, and it was _____ the job was changed. I stayed, essentially stayed in the same job, but my duties were expanded to include some supervision of the institute's general programs in science.

Summers: And so, then when it got changed to deputy director for sciences, that was the same job?

Berliner: It was essentially.

Summers: And so, what was your role and vis-a-vis . . . You'd gone in the Heart Institute to the director's office. Did you have . . . What did you do in the director's office?

Berliner: Well, I reviewed the programs that were all intramural _____ scientific directors. We used to meet. The scientific directors met regularly, and I was sort of the chairman of that group. And we discussed various scientific problems and suggestions for changes.

Summers: When you said scientific problems, you mean areas of the science that . . .

Berliner: Well, Bob Marston [sp.], John Sherman, and I worked very closely together with the institute directors and scientific directors.

Summers: So that if some new technique came along or some new problem presented itself because of the science, this is what you discussed? Or should we do this or should we put _____?

Berliner: _____ promotions and appointments.

Summers: I'm just trying to get a sense of what kinds of discussions went around the table. Was it like a journal club or like a _____ meeting?

Berliner: _____.

Summers: Okay. What did you see as the difference between Shannon's NIH and post-Shannon NIH? Was there a big difference that you _____?

Berliner: Yeah, there really was. When Jim was there, it was really a one-man show. And after Jim left, it was -- it spread out a lot more, and nobody had the same kind of influence in the secretary's office and the surgeon general's office that Jim had. And it became more of a cooperative leadership.

Summers: Did Marston [sp.] try to develop this kind of liaison or . . .

Berliner: Well, that's pretty difficult. It was shortly after Bob was appointed, Nixon was elected. They had a bunch of people in the secretary's office who were not very interested or effective. Finch's background was in California, as the governor, and he was not very much _____. _____ a very nice guy, wonderful guy, but not very sharp.

Summers: And so, you say it became a collective sort of leadership?

Berliner: Yeah.

Summers: Was there sort of jockeying for power or influence?

Berliner: I wouldn't say that particularly, although Charlie Edwards started out as head of FDA and then became assistant secretary for health of the then-HEW, and he was a lightweight too. It was pretty tough to get things done in those days.

Summers: Was this because of budgetary constraints or just . . .

Berliner: No. The budget continued to grow. And, of course, just about the time I left, we got the war on cancer. No. It wasn't the budget so much as initiatives were hard to get _____.

Summers: How much was NIH independent of the secretary's office in terms of making decisions about what could be done?

Berliner: When Jim Shannon was . . . Well, no. You _____. Jim Shannon gives Folsom [sp.] a lot of credit. He was able to sell Folsom [sp.] on his ideas. These were all _____ from Jim. But a cooperative, interested secretary can make a very big difference, and _____. The secretary's office could put . . . I mean, eventually it got that Congress would run around the secretary's office anyway. But the secretary's office would decide how much NIH _____ request in its various appropriations and so on, and these were always far lower than what NIH ended up with for many years. Of course, Congress didn't pay any attention.

I remember one, after I had left NIH, one of the scientific advisors to the president _____ a group of us -- I can't remember in what context --

that the administration gave NIH much less than they thought it should get because they knew that Congress would _____ it anyway. And this was _____.

Summers: And so, when you talk about it would be difficult to get new initiatives, that would be initiatives that the secretary would have to sign off on.

Berliner: Yeah.

Summers: Starting a new laboratory or . . .

Berliner: A lot of this was actually the assistant secretary for help, who represented the secretary's office _____.

Summers: And then, what was Marston's [sp.] tenure at NIH? How long was he there?

Berliner: Oh, he left shortly before I did. Let's put it this way. When Nixon was reelected, he asked for Jim's resignation. That's when I started looking for a different job.

Summers: I see. So, till the resignation . . . Was this just a pro forma thing that always happened when the administration changed?

Berliner: Well, with a new administration, everybody who has some kind of a secretary appointment formally offers their resignation, and usually if it's a political type of role, then the resignation is accepted, and usually it's like, oh, the FBI or whatever, they're not. And Jim Shannon had been through a number of different presidencies, and this was never -- nobody really ever thought that this was a politically sensitive job, but Nixon decided to make it one.

Summers: So, what was the politics? I mean, what was _____ Republican and the Democrat _____?

Berliner: That's a good question. But you noticed in the paper of yesterday, it said that the administration has stopped looking -- I guess it's in the science -- has decided they're not going to appoint a new director of NIH this year. _____ the administration beginning in January, and they'll leave it till then, which essentially says _____ carries political as well as scientific _____.

Summers: How far down did the political, did those appointments go, the ranks?

Berliner: With the passage of the War on Cancer bill, the director of the Cancer Institute and the director of NIH were made presidential appointments

requiring the consent of the Senate. The original intention was to get the directorship of the Cancer Institute up to that level, but then they decided they couldn't do that unless they also did the director of NIH. So that was the basis on which the director of NIH resigned with the change in administration. Of course, it wasn't really a change in administration, because Nixon had been president. This was his reelection.

I don't think that the political _____ beyond the director's offices, the director himself. I can't remember anybody being . . . Oh, sometimes you'd get someone in the administration that wants somebody in some job and puts some pressure on the NIH director, but otherwise _____.

Summers: Was there something that happened in the health research field that Nixon felt needed more political . . .

Berliner: Not as far as I know. I don't know. But Bob Marston [sp.] might have said things that were interpreted as too liberal or something.

Summers: Well, the Regional Medical Program, was that something that Marston [sp.] continued to push after he was director?

Berliner: No. I think that the Regional Medical Program never really amounted to anything. I don't know why it was established in the first place.

Summers: Wasn't there concern about the maldistribution of physicians _____?

Berliner: Oh, yeah. Well, that certainly _____.

Summers: _____?

Berliner: Well, with that, I don't think that was the basis for which, on which the Regional Medical Program was established. You'd have to go back and read the history. I didn't pay much attention in those days.

Summers: It seems like an odd thing for NIH to be in charge of in that it's . . .

Berliner: Yes. Well, the NIH got _____ that. They got -- the library was added to NIH, the education program _____ in the sense of the capitation and all that stuff _____. Whether or not these were good things _____.

Summers: How was the library program handled? That was the surgeon general's library.

Berliner: Yeah. Well, they built the building on the NIH campus at the direction of the _____ the library became part of the NIH _____. It didn't really change anything at NIH.

Summers: Was the library always nearby before?

Berliner: No. It had been downtown.

Summers: Downtown. So in that sense, it must have been a big help to have a good library _____.

Berliner: The NIH had its own library, which was quite adequate for most of the things you'd want to do.

Summers: Well, now it's become sort of the national repository, sort of the excuse for other libraries not to do anything, I think.

Berliner: Yeah. Electronic things have changed all that. You don't really need to have the book itself.

Summers: If you do need to have somebody worry about scanning it or _____ having it there.

So, when Marston [sp.] left, who succeeded Marston [sp.]?

Berliner: Bob Stone [sp.]. He was quite _____ for the job.

Summers: Where did he come from?

Berliner: I've forgotten _____ I believe was dean somewhere in the Southwest. He had taken a course in management at MIT, I think it was, and that impressed Charlie Edwards, who was then, had then become assistant secretary.

Summers: So, and _____ Stone _____, or was he _____?

Berliner: He didn't stay very long. And he was really not well suited to the job, but I think he realized that.

Summers: And did you work under him too?

Berliner: Briefly.

Summers: With him?

Berliner: I was already on my way out when he got there.

Summers: So, you imply that you were, as you say, looking for other things. This must have been some . . . I mean, given the fact that you had apparently such a successful career in NIH, it must have taken quite something to make you think that this is the time to leave.

Berliner: Yeah. Well, if I had still been somewhere in the _____ institute intramural program, then it probably wouldn't have made a difference, but I got a position _____ was exposed to what, where things that happened downtown made a difference, so I decided it was time to get out.

I don't know that I told you I had been appointed _____ Brewster had been a member of this committee that had reviewed various possible candidates for the directorship.

Summers: _____.

Berliner: And when I came -- I was up here because I had been made chairman of the University Council Committee for the Biological Sciences, and I came up to get indoctrinated or whatever, and Mae and I spent the night at _____, and Brewster told me _____ coming out here the next day to tell the faculty that _____ resigned. _____. And he _____. This had happened the previous fall. He said, "If you decide you want to leave NIH, let me know." That was one of things I had in mind as a possibility.

The other thing that I was being heavily recruited to be director of the Cardiovascular Research Institute at the University of California-San Francisco, where I had a number of good friends, and where Julius Conroe [sp.], who had been the director. But I decided Julius was too _____.

Summers: What was?

Berliner: Julius was too hard an act to follow. I think that was really a backwater place until _____ somewhere around 1955 or '60, when Julius Conroe [sp.] and Holly Smith really set that place up _____ one of the best places in the country. Julius really was a great organizer. So _____.

Summers: Yeah. I remember when I first came here, they attracted one of our young pathologists away who was one of the few pathologists who seemed to relate to the students. That was Charlie -- I forget his name, too. He had been a student of Lee _____ tradition _____.

So your recruitment was sort of the happenstance of being in the Brewster's house at the time when _____.

Berliner: No. _____ between that time and the time I _____. There was a _____ search committee. I don't know how . . . I think they had offered the job to a guy -- what was his name? -- Hickham [sp.], John Hickham [sp.], and John Hickham [sp.] got sick or something and couldn't take it. So there was quite an interval between _____.

Summers: Why do you think Hollis didn't take the job?

Berliner: Didn't stay.

Summers: Didn't stay.

Berliner: He never stayed at any job.

When I first knew Lou, he was a resident in neurology at the Neurological Institute in New York and I was an intern at Presbyterian. Then he became a cardiologist and _____. Then he was at Tulane. I forget what his job was at Tulane. Anyway, he kept changing jobs. A very bright guy. But I think he probably was wise in not sticking with anything too long because somebody else _____ clean up after him.

Summers: So when you decided to leave NIH, was there an attempt to try to keep you there?

Berliner: Yeah, a mild attempt, but _____. They can't raise your salary because you're already getting the maximum salary. They can't offer you more lab space _____ lab space. I had a great house on the NIH campus.

Summers: So you gave up lab space and lab work when you moved.

Berliner: When I moved over.

Summers: Moved over to Building 1, yeah. Was there ever any . . .

Berliner: I still had several fellows working for me in the lab _____. But I decided it was time to quit when I found _____ worrying about the lab _____ and other problems on my mind. If you don't spend your spare time thinking about your work, you'd better do something different.

Summers: Did you ever think about going back to laboratory research after _____?

Berliner: _____.

Summers: So, when you came to UO, what was the -- was there some overriding issues that you were sort of given a brief to deal with or . . .

Berliner: No. _____ job _____. There were some problems. There was a problem. The anatomy department was down to virtually nobody, and the microbiology department had been gutted by people moving over to human genetics, and there were only one or two people left there. And for better or worse, we decided to _____ some departments and _____ their function in other departments. Then there were some appointments to be made. Dan Cook had been told _____ that he was not going to be reappointed as chairman of pediatrics. Likewise Jack Cole. He wasn't going to be reappointed as _____ surgery, but he wasn't _____ chairman

of pathology. Still, there were lots of things to deal with. And Lou Thomas _____, who had been an old friend for many years, a very good friend, died about two months after I got here, so we had to find a chairman of medicine.

Summers: So, what was the reputation of Yale when you decided to come? Was this a school which . . . Well, where did it stand?

Berliner: Well, I don't know. I've always had some attachment to Yale myself _____ high regard. But I didn't, you know, hadn't been in the milieu of the deans and so on of the schools to have any strong feeling about how they would view the school. I subsequently learned that Yale had _____ very high among the schools that _____ support from NIH, so _____ about it.

Summers: So, when you got here, you had these appointments to worry about. What about the educational aspect?

Berliner: Well, at that time they had an office called _____ been in effect for a year or two, a new curriculum, which resembles very closely the one they're talking about now, and it sounded to me like it was a disaster. The students were expected to do some clinical things in their summer between their first and second year, and they cut back on the amount of basic science with the idea that in the fourth year the students were going to come back and fill up with basic sciences, which they now knew was so important. So two things happened. First, the students spent all their time in their first year practicing physical diagnosis, going around and looking at, seeing patients and so on instead of paying attention to what they were supposed to be doing. And they were doing miserably on the international boards at the end of the second year. And, of course, nobody came back to any of the basic sciences in that fourth year that they were . . . So I used to meet regularly with the curriculum committee, and we decided to abolish those programs and go back to something more like a _____. And I think it's crazy that they're now talking about doing the same damn thing again. That was tried at Duke with the idea that reduce -- I think even before Yale did -- of reducing the amount of basic science at the beginning of the school with the expectation that students would learn how important certain basic sciences were to the biochemistry _____ come back in their fourth year and do it. None of them did. They all ended up in _____.

Summers: What do you think was the driving force for this new curriculum, as they called it?

Berliner: Oh, I don't know. The idea was _____ feels they have to have a new curriculum periodically, and the new curriculum is usually the old curriculum _____.

Summers: The _____ do you think driven by the clinicians or the basic scientists?

Berliner: That one may have been driven by the students, actually. That was during the period of student unrest in the late '60s.

Summers: The drive for relevance?

Berliner: Yeah, that's right. I think that that probably was an important factor.

Summers: Were there student groups that lobbied you or . . .

Berliner: No. By the time I got in, the students had quieted down quite a bit.

Summers: What was your view of the student body and the admissions process Yale had at that point?

Berliner: I'm not sure I know what you mean.

Summers: Well, I mean, were the students -- you know, there's the Yale system, and we like to think we attract certain kinds of students. You know, the mechanisms we have may be a little different.

Berliner: Well, I think that -- I guess there are a certain number of students who are particularly attracted by the Yale system. I mean, that's a main reason for selecting it. But the candidates for admission to medical school _____ was very good . . .

Summers: Far more than you could take.

Berliner: I've never been a great admirer of the so-called Yale system, but it's a sacred cow. I don't think anybody can ever get rid of it. I think it's very good for very good students, and I think it's a disaster for marginal students.

Summers: Was this about the time when . . . It seems to my recollection this was about the time when we admitted more women and more minorities.

Berliner: Yeah. We began to, yes. Of course, it never was anything like the number of women there are now.

Summers: Is this something that you were involved in, or was the admissions _____?

Berliner: The policy was more or less established. I can't take any credit for it.

Summers: You mentioned the board scores. What did you think should be done other than abolishing the new curricula?

Berliner: Well, I thought that was one thing, and the other thing we introduced was examinations, which -- the idea was that they were to be taken anonymously, but if anybody . . . At least we'd find out. People would have a chance to find out how well they were doing. And I think that made _____. Theoretically, if those who didn't do well then identified themselves, the code was supposed to be broken, and I don't know that it - - it certainly never was while I was dean, and I don't know if it ever has been since.

Summers: Were you the holder of the code or . . .

Berliner: No. The dean of students was, had the code.

Summers: Do you recall -- were there . . . Do you think that the student activism was pretty much . . .

Berliner: It had died down. It had almost disappeared by the time I got here.

Summers: What did you see in the school as far as the weaknesses or strengths in the research area that you needed to work on?

Berliner: Oh, I don't know. I think it was fairly strong _____. I don't think any major weaknesses . . .

Summers: You mentioned microbiology.

Berliner: Yeah. Well, there wasn't a great deal of microbiology at that point. We made a couple of attempts to recruit people to strengthen that area without too much success. We almost got, what's his name from NIH. He would have been very good.

Summers: Krausy [sp.]? Was it Bob Krausy [sp.]?

Berliner: No, no. No, this was someone from the laboratory, a virologist. _____.

Summers: Later, I think Bob Gallo talked about coming here, but that was . . .

Berliner: Yeah. That was long _____.

Summers: I don't think you would say the same thing about _____.

Berliner: No. He was all set to come and he decided -- there had been some tragedy in their family. A son had died and his wife didn't want to leave. What

the hell was the guy's name? A very nice guy. I knew him very well. Oh, well. It'll come back to me.

Summers: The biochemistry department was -- the problem was solved more or less by the time you got here.

Berliner: Yeah. It was _____.

Summers: Was the cell biology, anatomy _____? _____ George Pelotti [sp.]?

Berliner: George Pelotti [sp.] got here just about the same time I did. He had been recruited by Fritz. And I'm going to find out who I was talking about. Mark Channick [sp.].

Summers: Oh, yes.

Berliner: Where were we?

Summers: We were talking about strengths and weaknesses of various research programs and departments, what you thought needed attention here. This is about the time the cancer center was formed. There was a push to develop cancer research here. What were your views on that?

Berliner: Well, it was an opportunity to get resources, including a building, that we couldn't very well pass up. I _____ with great enthusiasm _____ on cancer from my previous incarnation, but I think we gave it reasonable _____. But there was always the debate about whether the cancer centers could make its own appointments or whether all the appointments ought to be in the departments, and we ended up with requiring that they be in the departments. I don't know whether that's still true.

Summers: Yes, it is.

Berliner: I suspect that some of the outfits in the central and the Cancer Institute itself _____ are not particularly enthusiastic about that _____ the cancer center enough authority _____ services, but I don't know that it's made any difference in the long run.

Summers: What was the relationship or had been the relationship between the public health department and the medical school, nursing department, these other sort of health-related things? Did you, as dean, have -- what was your relationship?

Berliner: Well, the School of Public Health, of course, is part of the medical school. Again, this is something that the public health community is not very enthusiastic about. This is one of the few instances where this is still the

case, as it was in most places originally. So they were treated more or less as a department of the medical school, and I think it did reasonably well.

The nursing school, there's not much relationship.

Summers: So did the commonality of teaching or possibly research areas was -- the dean of the medical school and the dean of the nursing school, they routinely . . .

Berliner: No. They both went to the same sessions on the other side of town, but that was about it.

Summers: And what about the relationships with the university then? I mean, Kingman Brewster [sp.] was the one who recruited you, and he must have stepped down soon after you got here. Was that . . .

Berliner: Yeah, a year or two after I got here, yeah. But I got along very well with Marge Amante [sp.]. We were very good friends. During the interval after Kingmans resigned and Connie Gray [sp.] was sort of acting president, she had a _____ together to talk about what the university ought to be doing and planning various things, in which I was a member, and Marge Amante [sp.] was _____, and Rudy Rogers [sp.], who was the librarian. And they were very good, got to be very friendly. And then _____ became president and _____, so they all worked out very nicely. So I got along very well. I always got along very well with everybody who was provost _____. It was Ed Cooper when I first came. He left shortly afterwards to go to Washington. And then Hanna.

Summers: _____? Did you deal with -- the main campus gave you enough support, or was there a degree of tension there?

Berliner: Yeah. Well, this I think is one of the things that got us to straighten out. When I got here, the university got -- there was a . . . The medical school paid something like 35 percent of my central expenses of the university. And we decided that that didn't make any sense at all. It was based on some arbitrary formula that the medical school ought to get the indirect costs that were generated _____ grants from NIH, and then we would pay for the things that the university supported. There was no reason why we should pay 35 percent of the bursar's office, which is 90 percent undergraduate students and there was no reason why we should pay 35 percent of the president's office. He didn't spend 35 percent of his time worrying about what went on in the medical school, and so on. And we tried to work out for each of the functions that were being . . . Mike Coleman [sp.] was really very, very good in this. So we ended up greatly reducing the amount the medical school was expected to pay to the

university. And whereas they had thought that the medical school was a drain on the university, it turned out quite the reverse, that the medical school actually was very much putting them in the black.

I gather to some extent that that has fallen apart since I left.

Summers: You mean the agreement of the formula _____?

Berliner: Yeah, with the way it's handled. I don't know the details anymore, but from things I have heard, it sounds like it's not, isn't being done the same way. And I think that was one of the major accomplishments during the time I was _____. It made the finances of the medical school very positive.

Summers: What about faculty tenure slots and so on? How were they handled?

Berliner: We had an agreement that the medical school would get a certain number of tenure slots each year, which was based on a projection of how many people would be retiring when. When somebody retired, that slot was lost, so essentially the 25 or whatever it was per year that we were allowed was based on the assumption these would occur.

Summers: Was that based on assumptions for growth, or was that based on . . .

Berliner: Not rate of growth. That _____. _____ to allow for some growth. I don't know what the situation is now, but . . .

Summers: Well, from what I understand, we get very few tenure slots because the position -- most people go into the non-tenured ranks now.

Berliner: And, of course, the other thing is that the people don't retire when they were expected to.

Summers: What about the problem area of the hospital?

Berliner: Well, you know, the president of the hospital and the dean of the medical school are negotiating all the time as to who's going to pay for what and how much of this who's going to . . . We managed to get along in that sense. It was never a very cordial relationship.

Then there was the business about the suit brought by _____.

Summers: And what was the major issue?

Berliner: Well, the major issue was who's going to -- who's in charge. Is the chairman of the department? Particularly surgery. There were some other things like the cath lab and medicine and to some extent maybe some

gynecology, but it was mainly the surgeons. And unlike almost any other medical school where the chairman of the department of surgery is more or less in control, that's not the situation here. It certainly isn't after the suit was brought and really wasn't much that way before. And the board of the hospital is a community board; it's unlike many . . . And their sympathies lie with -- they don't really understand what a good teaching hospital should be, and their sympathies are more like the community hospital side of it. So they folded and gave the people who were suing them pretty much everything they wanted.

Summers: And the lawsuit was against the medical school or . . .

Berliner: No. It was against the hospital.

Summers: It was against the hospital.

Berliner: Oh, yeah.

Summers: And the medical school just went along with whatever _____.

Berliner: Well, we had no . . . All we could do was cheer up, cheer one side along, but we had no leverage _____. We could certainly move somewhere else.

Summers: And, so what -- did the relationship of the medical school dean and the director of the hospital, the president of the hospital, was what, one of a client or a . . .

Berliner: No, colleagues, although there was a lot of tension between them over distribution of resources, who was going to pay for what.

Summers: And the clinical departments, of course, had their hospital wings. To what extent does the medical school or the dean get involved in that sort of operation, I mean resident training, patient care, things like that?

Berliner: Well, the chief of staff, at that time the chief of staff of the hospital, Larry Pickett [sp.], was an associate dean, and he was pretty much in charge of those things from the hospital standpoint, and _____ the dean's office, but there were no issues that required the dean to get involved in it.

Summers: _____ things immediately come to mind like the size of the residency program and the question of _____.

Berliner: Yeah. Well, that was negotiated between the chairman of the departments and the chiefs of the hospital departments as well and the hospital.

Summers: So the medical school didn't have a direct involvement in that kind of education. So, as an aside, it's always seemed strange to me that the

medical school has to pay for the teachers of this educational program over which you have no control. We have 10 professors of radiotherapy, and teach 30 minutes in medical school for a year, and it's all driven by the needs of the hospital, and yet the university has asked us to _____.

Berliner: Yeah.

Summers: I mean, there are other aspects of the medical schools _____ with things like YPI and the child study center and _____/

Berliner: Well, YPI had been here _____, and it was about to be thrown out to make room for the cancer center in the genetics program. And it had been _____ decided, well, let's get rid of the YPI, and the psychiatrists had a fit, and _____ persuaded me that we were to find a place for them, and so we finally ended up with their going up to Albertus Magnus, where there was a dormitory building available for it, and then finally _____. That's what I think one of the mistakes I made. Of course we didn't foresee the financial problems that were going to arise because of the managed-care business because their patients used to be in there for a year or so, and nobody would pay for it anymore. And I think it probably would have been wise to just let it go because it didn't make very much of an academic contribution. It had earlier on. I mean, there were people like Ted Liss [sp.], Steve _____, who had done academic stuff using that material. But as far as I know, nothing came out of YPI in the way of academic _____, and we probably would have been more astute to let it wither away _____.

Summers: And the relationship with CMHC was, again, another sort of _____?

Berliner: We had very little to do with it. It was a state-supported thing, and we had a certain number of people there. We never had that much . . .

Summers: Just thinking back to the lawsuit, Larry _____ Arthur _____ was involved in that as one of the principals. Had you recruited Arthur?

Berliner: Yeah, I had recruited Arthur, and I think Art was not exactly the most diplomatic person. But he's a good fellow. I like Art personally a great deal _____. But he was kind of a bull in a china shop, unfortunately.

Summers: Well, why don't we -- we've gone over an hour here now. Why don't we break off here. I've got a _____ somewhere at 11. But I'd like to finish out with another session on Yale things. Would that be all right?

Berliner: Sure.

Summers: Next Thursday, this week, okay?

Berliner: As far as I know. I'll let you know if there are any problems, but I don't know of any.

TAPE 7, SIDE A

Summers: Tuesday, April 26, I guess. And we just left off . . .

Berliner: Tuesday the 25th.

Summers: The 25th, right. We just left off, you were leaving Goldwater Hospital and going to be recruited by Shannon. That was about 1950?

Berliner: It was 1950, August 1950. I remember because it was Alice's birthday, August 1950. She was three years old.

Summers: Tell me about your children a little bit. The oldest is . . .

Berliner: Well, this is Bob Jr., who lives in Chicago, is divorced from his first wife _____ one of his three children. He's married to a very nice gal now, who is one of the vice presidents of the Tribune Company.

Summers: What does he do?

Berliner: He's a lawyer, although he's not typical. He was a partner in a law firm but he's now mainly doing entrepreneurial real-estate business. Alice is . . . He was born in 1945.

Alice was born in 1947, and she lives here in New Haven. She is married to Jim Handler [sp.], who has an appointment at CPH and who is the epidemiologist for the state health department. They have three kids, the oldest of whom graduated from Yale last year and is now in Chicago. One is a sophomore, freshman, I guess he's a freshman at Wesleyan, and the little girl is in high school.

And Henry, you may know, was born in 1950, and he is _____ bookstore here _____. They have one. His wife works for the city school system. They have one daughter who's a sophomore at Grinnell.

Summers: Cornell?

Berliner: Grinnell in Iowa.

And then Nancy, whom I'm sure you know, who's married to Alan Fierst [sp.], who is associate dean at the school of architecture, and they have two kids, one of whom you see quite _____.

Summers: Actually, I see most of the children frequently. I first met your two grandsons and Alice's daughters when they were in China, and they were little boys crawling around in the dirt under the table _____.

Berliner: Right.

Summers: And I met Henry's daughter a couple of times _____.

So in 1950 or '51, you were recruited to go to NIH. Did you have any concerns about going?

Berliner: Oh, yeah. Everybody -- I think I said last time, everybody -- the attitude towards government employment was very negative, and . . . As a matter of fact, of course, it turned out to be a great place, I mean, the Heart Institute particularly at that time. Jack Orloff, with whom I worked for many years, had been planning to come to work with me at Goldwater, and he came along to NIH.

Tom Kennedy, who had worked with me back at Goldwater, was already there, so the three of us got together and worked as a team.

Summers: Was the NIH competing with people who would have normally gone to academia, or were they trying to raid academia?

Berliner: Well, I don't think so really. About the time I got there, they -- I can't remember the guy's name who they recruited to be director of intramural research for what became the Arthritis Institute -- it wasn't called the Arthritis Institute; it was called Experimental Biology and Medicine Institute at that time -- and he was a guy from Mayo. I can't remember what his name was, a well-known guy, and about retirement age. And he didn't last very long at NIH. I don't know what the story was.

Anyway, the Korean War was on at that time, so it wasn't very difficult to recruit young people because it gave them a chance to get into the Public Health Service and do something useful, and that was the basis on which the first group of clinical associates was recruited.

I think in general, except for a very few people, most of the NIH people were home-grown. They started off with this group _____ in the Heart Institute. I don't think we ever recruited a senior person after that.

Summers: What was the conditions of the recruitment? Did they offer you a lot of stuff or was . . .

Berliner: Well, not really, because there was plenty available, but I don't think any of us had any big demands because when we moved in there, the clinical

center was being planned and we were supposed to . . . Tom Kennedy, I remember, was largely responsible for ordering equipment for the whole intramural Heart Institute, sort of a blank check. That was a very strange procedure. But anyway, it worked out reasonably well. But, so NIH became a feeder for academia rather than a drain on it.

Summers: What was -- were the salaries better or worse or . . .

Berliner: The salaries were better. My salary -- I'd been getting \$7,200 when I left Goldwater, and I got \$10,000. That was the highest regular beginning salary, GS-15, which was the highest regular rank at that time _____. They created supergrades shortly thereafter, but _____.

[knock on door, tape recorder turned off]

Berliner: Where were we?

Summers: We were just talking about the salary conditions and _____.

Berliner: Oh, yeah. The salaries were a little higher, but that wasn't a big thing. It was a nice place, you know. Bethesda looked very different then than it does now. There were still trolley tracks on Wisconsin Avenue. It was a two-lane road out through Bethesda. There were trolley tracks on Connecticut Avenue. All these things have been greatly widened. Now you, if I go to Bethesda now, I can't even recognize it.

Summers: And most of the NIH grounds were -- was it just a park or space or . . .

Berliner: Well, it was a big space. One part of it was a golf course. Well, _____ the Clinical Center was Building 10, and we were up in the fifties, I think.

Summers: And they were numbered in the order in which they were put up.

Berliner: That's right.

Summers: Okay. But Shannon had gone there just before you.

Berliner: Yeah. He was there about a year, a year and a half when I got there.

Summers: So tell me a little bit more about him. I mean, clearly he was a figure in _____.

Berliner: Well, Jim had -- Jim went to Holy Cross, but he was a best star basketball player and not much of a student. But Wykoff [sp.] at NYU took a liking to him and he was _____, where he did very well. And he then worked with Homer Smith and went on to get a Ph.D. in physiology. And when I went to work with him, he was a promising young physiologist, had done

some very nice work. Then during the war, he got over into malaria, and I don't think he ever touched anything in the kidney again. I think I said he was -- they had lined him up to be chairman of pharmacology at NYU and he got into some kind of dispute over space with them, and rather than take up the job, he took a job at Squibb. And he was there for, it must have been maybe three years. He was _____ development.

Summers: For malaria?

Berliner: No, no. His malaria business was done by then. We thought we had malaria cured, of course, but . . .

Summers: And what kind of things did he do at Squibb? Do you remember?

Berliner: I don't know specifically.

And then when he got to the Heart Institute, he was organizing, was his main thing, recruiting and organizing. And he was only in the, had been in the Heart Institute maybe, when I was in that, after I got there, maybe a year and a half or something like that, and at that time he was made director of Intramural Research at NIH. He was deputy director or associate director for intramural affairs, something like that. And that left the Heart Institute without a scientific director, which is where I . . .

Summers: So he didn't maintain a laboratory there?

Berliner: No, no. He didn't have a laboratory at any time.

And not long after he was in the director's office, the business about polio vaccine came up. You may or may not remember that there was one of the batches of polio vaccine caused a number of cases, and Jim was responsible for tracking down and finding out what it was all about and getting it straightened out, and that's what brought him to the attention of the secretary's office. And when Henry Sebrill [sp.], a year or two later, retired, Jim was made director of NIH.

And Jim had big ideas about how the NIH, about what American medicine ought to be, and . . .

Summers: Were these big ideas, ideas that he had espoused ever since he was . . .

Berliner: Not that I was ever aware of. I don't think so. But he went about it very shrewdly. He made very good friends with the chairmen of the committees in Congress. Fogarty, he took, and Lewisdale [sp.], took care of Fogarty as a patient. Fogarty had diabetes and didn't do anything much to take care of it, and I've forgotten how he . . . Well, in Fields [sp.] case,

he, _____ if I remember correctly, courted his chief of staff, who was an S.O.B. and a real _____. But anyway, Jim had him eating out of his hand, and that's really what made the NIH _____. _____ had a secretary successive to Hobby [sp.], who was the first one. This guy came from -- I have somehow associated him with Rochester, Rochester, New York. I can't remember. He was _____ and was very supportive of the growth of NIH. And that's when the NIH really took off. When we got there, I think the budget was something like \$48 or \$49 million for the whole NIH _____ Jim. When Jim gave up the directorship, it was close to a billion, over a billion.

Summers: _____.

Berliner: And he got _____ training program. That was one of the things he really pushed to expand the number of people who could do research.

Summers: What do you think this came from? Was he predominantly a clinician or a lab man or . . .

Berliner: He was not _____.

Summers: Did you see the laboratory as more and more relevant to medicine?

Berliner: Yeah, I would say yes. He was a laboratory scientist, really. _____. He never went back to anything he . . . When he left something, he left it. And Tom Kennedy -- they had a celebration of the 50th anniversary of our laboratory last year, and Tom Kennedy wrote a piece about it. And he made the point that he had worked very closely with Jim Shannon, because after . . . Some years later, he left the Heart Institute and went to work directly for Jim in Building 1. And the point he made was that Jim had been outstanding in a number of fields, but he never went back to one when he left it. That was it and he was on to something else, which was very true.

Summers: What was his administrative style, if you could characterize that? Hands-on or a delegator or . . .

Berliner: No. He was a pretty good delegator. He was very careful about who he picked to do things, and then he let them do it. He was never micro-managing, you might say. He just let people go their own way as long as he thought they were going in the right direction.

He fired a couple of institute directors. I don't think the director of NIH could do that these days. As a matter of fact, I think Varmus commented on the fact that the institute directors are _____ these days. That certainly wasn't true when Jim was director. When Jim was director

of NIH, Jim ran the NIH. The directors' jobs were not really _____ very important _____.

Summers: Well, tell me about the lab that you set up then.

Berliner: Well, the initial lab, we had a very limited amount of space, and when I got there, there were two people already there. One was Tom Kennedy, who _____, and one was Jim Davis. Jim Davis had been working over at Baltimore City Hospital as a _____. _____ the Baltimore Marine Hospital, which was the Public Health Service. Jim -- he had approached Jim Shannon and Jim Shannon hired him, and he was there when we got there.

And subsequently, there were two guys who joined us before we moved over to Building, before Building 10 became available and we had much more space. They were very _____, and Adrian Hartman would later train in physiology at the University of Iowa. He was the son of a very well-known figure at that time, _____ Hartman, who had written a couple of books for the general public on science.

And Tom Kennedy and Jack Orloff and I worked together here for 16 years. And then after we moved over to Building 10, we _____ to get people added, Joe Handler, who's a professor at Hopkins now, and Moe Berg who's currently the head of the laboratory. And we worked on the kidney, mainly on electrolyte transport.

Summers: And what were the other groups in the Heart Institute? Were there . . .

Berliner: Well, there was Steve Brody, who's pharmacology, and Sid Youngfriend [sp.] was part of his group, and so was Julie Axelrod, and they -- Julie Axelrod left the Heart Institute a few years later and was in the Institute of Mental Health. Sid Youngfriend [sp.] branched off and had his own laboratory. Steve worked on, well, it all started, I guess, with reserpine and the effects on the central nervous system, and he got very much interested in biogenic amines, and actually the neuropharmacology was _____ mainly involved with. And actually, I think if he hadn't -- he had some very good ideas and was very productive. He also had some very poor ideas, which he often pushed _____, and he would have gotten the Nobel Prize rather than Julie Axelrod if he hadn't created so many enemies by some of his wilder things.

Summers: So the research at the Heart Institute was just a rather small component, I mean, with two or three laboratories?

Berliner: Oh, no. There was Chris Anthonsen's [sp.] laboratory, which did biochemical things, and he got particularly interested in protein structure. And although he was not in the Heart Institute at the time he got the Nobel Prize, the work that he eventually got the Nobel Prize for was started at NIH.

Earl Statman [sp.] was part of Chris's lab and subsequently became an independent and separate laboratory.

Then there was a laboratory of . . . Jim had the idea that _____ have organic chemists who were going to synthesize and isolate compounds, and the pharmacology was going to be done by Steve Brody, and then they were going to be passed on and tested in patients by the people in the clinical area.

In the clinical area, there was Fred Barter [sp.], an endocrinologist, and Luther Terry was an internist. Luther was named after Senator Hill's father. Luther _____ Hill was Senator Lister Hill's father, and Luther was named after Mr. Hill's father. That's how Jim got, Luther got to be surgeon general.

Anyway . . .

Summers: So there was this broad, this notion of broad . . .

Berliner: Yeah. And he was active in drug development, which was way up the line. Again, it never worked because people couldn't sit around waiting for what the organic chemists were going to eventually come up with, and everybody got in doing their own thing.

Summers: Do you think this might have come from his work in Squibb?

Berliner: Oh, yeah. _____. _____ from the malaria program, which he more or less continued at Squibb, though I don't know that anything ever came out of it _____ from Squibb.

Summers: And was there an attempt to keep people on track? Or once they made discoveries, they more or less _____.

Berliner: Everybody pretty much was -- and that was the policy that I kind of followed when I became director of intramural research. People were doing good work.

Summers: What was your role as lab chief?

Berliner: Oh, the lab chief more or less approved purchasing and hiring, particularly, and delegating, you know, who got what space, who got what resources. And in some labs, the director of the laboratory ran everything. Steve Brody, for example. He had a lot of people working with him, but they were all working on things Steve Brody wanted to do, and I didn't approach it that way _____. Jim Davis did his thing, Tom, Jack, and I did ours, and Hartman did his, and so on. So it varied greatly depending who it was.

Summers: Did you meet regularly with the director?

Berliner: Yeah. We used to have meetings. I guess every other week, all the lab chiefs would get together as a group, and then, whenever you had a problem, you could go talk to the director. Jim wasn't around very long, so _____.

Summers: What was the relationship with other groups outside the Heart Institute? Were the institutes fluid in terms of the boundaries or rather rigid or competitive?

Berliner: Well, each of the institutes had its own space, budget, that sort of thing. There was a fair amount of collaboration between various institutes. The scientific directors used to have weekly meetings with the director of intramural _____ NIH _____. But other than that, everybody pretty much did their own thing. But there was -- most folks were fairly friendly and cooperative.

When Chris **Anthony**son [sp.] left the Heart Institute to become chairman of the Department of Chemistry at Harvard, not long after he got there, Harvard decided they were going to have rotating chairmanships, and Chris didn't like that. And I was the one that persuaded Chris to come back to NIH. But by the time he came back, the space that he had occupied in the Heart Institute was no longer available, and so I recruited him for the Arthritis Institute. And then there was Marshall **Nei**renberg [sp.], who was in the Arthritis Institute, and although he had discovered the key to the genetic code, the Arthritis Institute didn't want to give him any resources. So I hired him _____.

Summers: So it sounds like by that time you were the scientific director?

Berliner: Yeah. I had been scientific director before we moved into Building 10. Let's see. I don't know whether I was actually, had been officially appointed as scientific director or whether I was still chairman of the _____ laboratory.

I think I may have told you last time that when Jim left, somebody had to be in charge, and the lab chiefs elected me to be in charge, and so I was sort of acting scientific director for a year or two, before they gave up looking for an outside candidate _____. But at least by the time we moved into the Clinical Center, I was scientific director. I remained as head of the laboratory and scientific director for some years after that.

Summers: So, when you became officially the scientific director, did you still maintain the laboratory?

Berliner: Yeah. I think -- I remember _____ the laboratory for another four or five years, and then Jack Orloff took over.

You know, for a while I was interested in doing hands-on stuff. I liked it _____. But after a while, I had too many other things, too many interruptions to _____.

Summers: Did you have many colleagues that came and stayed, or were they mostly just clinical associates who came through?

Berliner: After -- I'm trying to remember. Of course, I did have some postdoctoral fellows who came and worked with me. Some of them were, came originally as clinical associates and some did not. Jack and I worked together for a good many years, and then Jack sort of branched off on his own, and I had a bunch of other young _____ people working with me. Barry Brenner, who's a professor in medicine at Harvard; Norman Bevinsky [sp.], who's chairman of the Department of Medicine at BU; Irv Davidson, who did some very nice work _____. I don't know what happened to him. He disappeared from my view. So that's the string of younger people who came through the lab. Rex Jamison [sp.] . . .

Summers: This is making noise. I want to just check to make . . . [recorder turned off, then back on]

Berliner: Originally we were doing mostly clearance stuff. I would get in the lab and do a fair amount of that myself. But when they into doing micropuncture and stuff, I never put my hand on that one.

Summers: Did you have skilled technicians that were available _____ or . . .

Berliner: Yeah.

Summers: Or unskilled ones.

Berliner: The technicians did the more routine things usually, and the actual experimental work was always done by the junior people.

Summers: How would you describe NIH as an environment for a young person at that time?

Berliner: Oh, it was a great place. For a number of years, almost, of course, because of the draft situation, and we were able to get very promising young people. But we certainly didn't do them any harm. They all came, most of them came out and got very good positions afterward. It was a very stimulating place and a chance for young people to really get into the lab and learn something about how to do research. And at one point, all of the leaders of the Young Turks and the Association of American _____ had been through NIH.

Summers: You mentioned before that you more or less serendipitously found the secretion of potassium.

Berliner: Mm-hmm.

Summers: And it seems that looking at your work from the time, that potassium and acid really formed the basis of a lot of research. What was the state of knowledge? I mean, I don't know physiology well enough to know what the issues were.

Berliner: Well, the techniques for measuring sodium and potassium, before there _____ photometer, were exceedingly laborious, and people didn't do it if they could avoid it. Sodium was a gravimetric procedure; potassium was a very complicated colorimetric one. I never actually did any of them myself, but I saw people trying to do them. So that the amount -- what was known about the excretion of sodium and potassium was not very much until the _____ photometer became available. That was probably 1945 or so. So it was more or less a blank field, certainly as far as the kidney. The only previous work _____ potassium excretion _____ what was _____. And he had given the animal large amounts of potassium _____ hadn't seen anything very much _____ potassium _____ animals were _____ urine. And I think I told you we had _____ you had to do it to really be able to get _____ secretion _____ a week or two _____ it. _____ really never been _____. _____ adrenal _____. Anyway, that was, there was, as I say, very little known about it _____ virgin territory.

And then we happened to get this _____. Anyway, we had very interesting _____ not only _____ urine _____ gave us enormous _____ potassium _____.

Summers: How did you get a hold of that or why did you want to study that?

Berliner: Because it was supposed to be a diuretic. Sulfanilamide [sp.] _____ get _____, and _____ turned out to be _____. So the idea came along that maybe we _____ diuretic. That stuff was made by American Cyanamid. _____. I guess probably it was Jim Shannon _____. _____ gave it to us to see what it did, and a fair amount of people were interested. First of all, it increased bicarbonate excretion _____ showed that bicarbonate reabsorption _____ tubules could not be adjusted _____. _____ tubules _____, and we got out more bicarbonate than _____. And it also had this very interesting _____ potassium. So we played around with the idea that there was some kind of competition _____ secretion of _____. It turned out that _____ quite as simple as we thought _____. So that was the basis on which that work was done.

Summers: _____ bicarbonate _____.

Berliner: _____ I couldn't. I've gotten to the point where names escape me. When we got _____.

Summers: And then, did this play out in any sort of clinical study?

Berliner: Oh, yeah. _____ turned out to be an interesting drug. _____ actually for treating glaucoma. Bicarbonate secretion _____. And as far as human physiology, kidney physiology, exploratory excretions, not really. It partly explains the fact that large doses of steroid _____. I don't think it was a clinical breakthrough.

Summers: Were you able or did you conduct some of these studies _____ voluntary?

Berliner: I don't think we ever did any _____.

Summers: How did you, when you did do studies on patients, how did you _____?

Berliner: Oh, at NIH, _____ were recruited for various studies _____. You had to be known _____ hypertension _____ studying that _____. There were volunteers as well _____ spend the night. But none of the work that I did at NIH _____.

Summers: _____.

Berliner: Well, it is related to _____ acid-base balance, and I had the idea -- it turned out not to be quite _____ going back to some work we did on antimalarial _____ basis, excretions increase _____ urine acid. And I thought _____ use _____ bases like quinine as a standard _____ changes. And so we _____. It turns out that excretions _____, but it was certainly _____ excretions _____. That _____ sort of _____. And if you want to get rid of that organic base _____, organic acids _____.

Summers: So, presumably you reported these results _____ various research meetings and so on. How was the sort of _____? Was there getting to be a large research community of kidney physiologists?

Berliner: Oh, yeah.

Summers: _____?

Berliner: Well, yeah. As I think I mentioned, we were one of the first _____. So really some of the brightest young people were going into that _____ molecular biology _____.

Summers: It seemed to me that when I was a student _____ endocrinology, the other science _____.

Berliner: Yeah. The two were very closely related, but _____ electrolyte _____. So there was quite a large group. As a matter of fact, our papers were on the general programs and _____. So there wasn't any problem of finding enough people _____.

Summers: What are the _____. Did your work have an immediate relevance to that?

Berliner: Well, not the work we did in the lab, but we were certainly very critical of what some people were doing _____. Surgeons were drowning people, pouring enough fluids into them _____. Then there was a thing called low-salt syndrome because some patients with massive edema _____, and they were going _____ sodium _____ far more _____. So this was _____ things we were trying to _____. _____ give them any salt _____, a few things like that. But actually, as I say, the stuff we were doing in the lab didn't _____.

Summers: What were the other parallel things that were going on _____?

Berliner: Well, up to the time that we got into microcomputers, pretty much this was sort of the core thing, and Bob _____ lab and _____ our lab _____ major _____.

Summers: And what was _____ working _____?

Berliner: Several things _____.

Summers: When did kidney pathology _____?

Berliner: I guess the biopsies _____. You know, if you waited to get end-stage kidneys, they all look the same. So I guess really the biopsies _____, which was also _____. _____. By that time I had sort of . . . The clinical

side of pathology became either dialysis or transplantation ____ kidney _____. I sort of ____ pathology _____.

Summers: Did you ever work for ____? Were you actually seeing patients ____?

Berliner: Well, the first four or five years as scientific director, I used to make rounds once a week of whatever patients they happened to have in. Some of the ____ would have _____. So I never _____.

Summers: What about dialysis in ____? Was there an official view that ____ not the way to go?

Berliner: No, there was no official view ____ way to go ____ science involved, and if we occasionally had a patient who needed dialysis, _____.

Summers: Did NIH get involved in issues about ____ take a role ____?

Berliner: Well, not really. In the late -- I guess it was the middle '60s, when, you know, there was a big business about ____ with _____, and they had _____. The guy who was the head of the Bureau of Budget at that time started _____, and _____ appointed _____ the chairman. _____ made up _____. I was _____. So we looked into the whole question of _____. And the committee ended up recommending that _____, which was a recommendation _____ budget. _____ got a negative view at NIH. But this finally did end up in _____. _____ never really _____. _____ paid for _____ set up a dialysis _____. _____.

Summers: When did micropuncture become ____? Who developed it?

Berliner: Ann Richards at Penn. ____ laboratory started doing it back in the '20s _____. And just before the _____, they did _____. And then _____. There was nobody doing _____, and so _____ the late '50s _____ tried it again. _____ very good technique in the early days _____. Even _____. And _____. He was _____ by _____, so he started it. The first study _____ Walker and _____. _____ Women's Medical College _____, and she was the only one who had had _____ still in the laboratory _____ two people. John _____ did it on _____ Gerhardt [sp.] _____. Both went to _____. So _____ everybody _____.

Summers: _____?

Berliner: Well, because you could really _____. I have to say that the first few years were really _____. John Gotshall [sp.] never had any _____.

Summers: Where was _____?

Berliner: _____ completely changed. I mean, people _____ became focused.

Summers: Was there some technical innovation that made it possible for _____?

Berliner: Well, the after _____ were much better _____. _____ technology for doing the _____. _____.

Summers: And your lab got _____ this technique along with everything else. And you never tried it yourself?

Berliner: Me personally, no. I watched _____.

Summers: One thing that _____ that kidney physiological studies tell us a lot about membranes in general. Was this the view then, to study membrane biology or kidney _____?

Berliner: Well, the people who were studying membranes _____. And the people who were interested in membranes were _____, so there was a lot of _____ the lab. _____.

Summers: What's special about _____?

Berliner: Well, to transport sodium _____.

Summers: _____ urinary _____?

Berliner: _____ chamber _____ two chambers. This _____. _____ separated the two chambers _____. _____.

Summers: Is this because the _____?

Berliner: No. It's because _____. _____ end of the bladder and _____.

Summers: _____ auxiliary kidney _____. Are _____ unique in that respect?

Berliner: Well, I suppose _____.

Summers: _____.

Berliner: Originally _____ technique _____. They're very similar.

Summers: I guess the problem I have _____ environment _____. One of the things that _____ modern _____ fair amount of _____ fair amount of _____. What was the balance between theory and _____?

Berliner: Well, we got very much interested in the theory _____ concentration _____. And he did a lot of theoretical _____. As a matter of fact, _____. _____ We didn't do _____.

TAPE 8, SIDE A

Summers: . . . time we left off, you were talking about things at the medical school here, medical school issues. And I thought of a few more topics that I think will be of interest.

I think when you were starting at the deanship was the time when the relationship of medical school to the postgraduate years was a big issue, was it not?

Berliner: I'm not sure I know what you mean.

Summers: Well, the whole issue of most students didn't take a rotating internship anymore. They went to specialty internships, and what they did during the fourth year to get ready for this . . .

Berliner: Oh, yeah. There's no doubt that was . . . As a matter of fact, this is quite characteristic of everything about medical education, about the student attitude, and they always want to be doing now what they're going to have to do next year. I've often said that the only reason I _____ for a fourth year of medical school is because if you didn't have a fourth year, they'd do in the third year what they now do in the fourth, because they want to be interns, but the year before they're interns, they want to be _____ and so on right down the line.

Summers: So, did this, I mean, did the medical school have any discussion about this, or was there any _____ could do?

Berliner: No. We talked about it, but there's not much you can do about it. We tried to persuade the students not to spend all their time taking some internships in their fourth year, not taking it, not necessarily, take all the biochemistry they can in college and not have to take it over again, but I don't think it has much effect.

Summers: Yeah. I remember students who took five rotations in ophthalmology at different places just because they felt that the departments wouldn't take them seriously unless they'd seen them.

Berliner: Exactly. And that was clearly difficult for them. And it was encouraged, unfortunately, by some of the faculty. It certainly was true in medicine. They wanted to see what kind of interns they were before they _____.

Summers: Somewhere along the line, though, the fourth year changed. I mean, I suspect when you were a student and when I was a student, the fourth year was fairly didactic. There was a structure to it which . . .

Berliner: Oh, yeah. Yeah. We didn't have an awful lot of free time for electives. The fourth year is almost all elective, and while students signed up for electives, nobody kept track of whether they actually did anything.

Summers: Do you think that this was driven by the lengthening of the residency program or the fact that more and more, what they really need to know is going to be deferred, something like that?

Berliner: Well, you know, the residency is not greatly prolonged for anything but surgery, but I don't think that was the reason.

Summers: I was thinking back to my own experience, where I would say half of my classmates, after one year of rotating internships, went to practice family practice, and so the medical schools had an obligation, and we all had to deliver six babies to graduate. There was a sense that you had to know how to do certain things because in another year, that was the end of your training. So when three years of internal medicine or four years of family practice _____ became the standard, then the medical schools didn't _____.

Berliner: Well, there's a hell of lot more to learn in medical school than there was when we went. That's certainly _____. It should make it even more important that you not take any time away from it, because . . . But, you know, getting your hands into what you really want to do is a very powerful driving force _____.

Summers: This brings up another issue of the constant balance between basic versus clinical education. You mentioned before this new curriculum that you found in place, which was forcing the students in the clinical experience very early. Did you have any trouble -- I mean, did the faculty immediately recognize that this was not working, or did you have _____?

Berliner: Oh, I think so. I think there was . . . I wasn't the only one who thought it was a disaster, too. It was a pretty general feeling that it was not working out very well.

Summers: And how did you go about making a new curriculum?

Berliner: Well, _____ we had a _____ curriculum committee _____ used to meet _____ for a while and decided to do away with the things that were _____, the major distractions, particularly the so-called -- it was _____ kind of experience that _____ to have the first summer. So _____ much more clinical . . . What's the word I'm thinking of?

Summers: Sort of traditional?

Berliner: Yeah, traditional. _____ was no difficulty persuading people they should go that way.

Summers: You mentioned the anatomy, a problem, I guess you'd call it, or need to do something with the anatomy department. What were the issues there?

Berliner: Well, the issues were that the people who had been largely responsible for the anatomy department had all retired, except one or two, and it was very difficult to get people who were interested in, who could, who had done, who were here doing enough, not much research left to be done that involved people who were interested in teaching gross anatomy. And so we decided, well, we'll find the people who teach gross anatomy, but we won't have a separate department, so we'll have to go looking and recruiting people for that purpose. So what was left of the anatomy department was incorporated into surgery. A lot of the teaching was done by the one or two people who were left plus people from other, orthopedic surgery and stuff like that.

Summers: And what about other laboratory hands-on kind of experience? Was that still going when you came, or was that . . .

Berliner: It had almost died. People were staying away from the laboratory part of their courses in droves, except in pathology, I think, _____. The physiology department had taken over the student laboratory space and converted it into research space, and this was pretty much, pretty _____. There was no biochemical laboratory, no physiology laboratory, all the things that we used to do when I was an intern. I have to say that the physiology that I learned in medical school, the laboratory was completely useless, but I did it just the same.

Summers: Was there discussion in the medical school about residency and specialty training, or was that pretty much left to . . .

Berliner: I don't remember any _____.

Summers: So that was sort of an autonomous thing at the school?

Berliner: Yeah. Well, actually, you know, that's pretty much determined by the varied specialty boards _____, although there was, off and on, a lot of talk about general so-called family practice. We had one of the few schools that's resisted having a department devoted to that purpose. But other than that, I don't think there was any . . .

Summers: Was there pressure or discussion about having a family practice, community medicine or . . .

Berliner: Well, there was a lot of talk about it from the outside, but nothing -- there was no internal movement in that direction at all.

Summers: What was the relationship -- what's your views on the AAMC as an organization _____ medical education _____? Were you involved?

Berliner: Well, I used to go to the AAMC meetings and to the council of dean meetings. I think I mentioned one that I was rather unhappy with their stance on this business about taking students from offshore medical schools. I never really had the feeling of AAMC. I mean, it was just a place to get together and talk about problems. I don't think it ever accomplished very much.

Summers: What about the other sort of organizations having to do with accreditation? I guess the LCME or -- what do they call that?

Berliner: Yeah. Well, I guess during the time I was dean, we had one review by the LCME _____. And it took a lot of work, but it really didn't have any great influence on anything. I guess more marginal schools, it might have, but it didn't seem to have _____. _____ never put a lot of effort into it.

Summers: What kind of . . .

Berliner: Well, you had to come up with all kinds of statistics and do this and that and prepare data and tables and so forth.

Summers: And this is to maintain accreditation in some sense. Right?

Berliner: Yeah. _____ very unlikely that any of the major medical schools would have any problem with their accreditation.

Summers: What was the relationship of the medical school to Yale College when you were dean?

Berliner: None really.

Summers: Well, I mean, you had some departments like MB&B which were . . .

Berliner: MB&B was shared, and there were certain members of the medical school faculty who taught undergraduate courses or gave seminars or college seminars and so on, but there was no -- I didn't deal with the dean of Yale College except that we sat on a lot of committees together. But we didn't make any arrangements about one thing or another with _____.

Summers: Was there ever discussion about having a combined bachelor's from the medical program or anything like this?

Berliner: No. There was way back. That's how _____. When I was an undergraduate, there was a thing called -- I forget what the title was. But there was a formal program where you could be admitted to medical school after three years of undergraduate work, and that's what I did, although I didn't come here.

Summers: Did they count the first year of your medical school as sort of the equivalent of your _____?

Berliner: You got a degree at the end of your first year of medical, you got a bachelor's degree.

Summers: That's what I did, too, which I regret to this day.

Berliner: Well, it was the middle of the Depression. It was a good idea. Medical school was very expensive. The tuition was \$500.

Summers: As I recall, that was what was driving me, too, was my parents remembering the Depression.

So, and what about the relationship with the graduate school and the MBPC program, development of that kind of work?

Berliner: Well, the . . .

Summers: We had an MFPP, a grant _____.

Berliner: Yeah. That was run by -- who was in charge of it? It _____ of some of it, but really the graduate teaching was largely a departmental problem, and the medical school as a -- the school's function was mainly a matter of trying to raise support out of the graduate school for the teaching effort. There was a formula by which the medical school received a certain amount of money for graduate teaching. I can't remember how it worked.

Summers: What about the research by the medical students and the MB thesis requirement? Did that ever -- the issue of maintaining the thesis requirement ever come up?

Berliner: Oh, yeah. I suppose every once in a while, some of the students would object to it, but it's one of the sacred cows. I think this one's a little better than the other sacred cow, which is the no-examination sacred cow. But student research day, which was the day before yesterday, was very good, a lot of very good stuff done. I think on the whole it is a good thing, and the students seem to be enthusiastic about it _____. I think it's somewhat better organized now than it was back when I was a dean. And I think that

poster sessions added a lot. Nobody ever thought of poster sessions back in the early '70s.

Summers: That was just beginning.

Berliner: Yeah, beginning to come about in some of the societies that were getting - - the number of submissions were too big to get, put them all on a program. Up to that time, though, there wasn't any such thing and nobody ever thought of doing it. And I think it's added quite a bit, too.

Summers: Was the _____ of Hope Building part of the work?

Berliner: I think that was -- I think it was a very good _____. I don't really remember what the Hope Building was like. It had a great big central cavity, which took about two thirds of the volume of the building, the interior volume, because it had this big skylight, which was the main source of light when that was built. When that was built, the place was lit by gaslight, and to get some daylight in was a great achievement, and that went all the way from roof all the way down almost to the basement. And it turned out, I think, to be a very, to have been a very good job, and I think it was a great addition of -- teaching facilities were rather poor up to that point _____. It was a great improvement.

Summers: Yeah. I think when you mentioned poster sessions, I think of that building because the space around the sides there are such great spaces for poster sessions. I remember talking to the _____ we'll just have it more or less that way because the size of _____ and the size of the building meant that they had to put more space than they would have normally, but it just turned out to be good for that kind of . . .

Berliner: Yeah. You know, I think the buildings were turned out remarkably well. The only thing anybody ever complains about is they were keeping that old amphitheater _____, which makes you feel like you're going to fall into _____.

Summers: Yes.

On the medical school, the number of programs that were developed with outside funding, such as the Robert Wood Johnson Foundation support and the HHMI, did you have involvement with those?

Berliner: Well, I did a lot of dealing with HHMI. The Robert Wood Johnson thing was much later. And I think on the whole, I had a rather negative view of that program. But the HHMI, they had, you know, they changed quite a bit. When I became dean, the program was much, much more limited than it has subsequently become. The institute owned the Hughes Aircraft

Company, and it was run largely by the engineers and the original associates of Howard Hughes. And they put out only a very minimal amount of money, and they supported a few people at Yale, a few people at Harvard, and so on, particularly those who George _____ wanted to support. And it was only later, when the aircraft company was sold to General Motors, that they got a, they were required to get a new board, trustees or whatever they were called, and to behave like a, much more like a foundation than they ever had in the past. And _____ that's expanded enormously. We did have a number of people who were supported. Dick Gershon [sp.] was one, and there were several in the Department of Medicine, but it was short-term support a few years at a time. Then they began to branch out just about the time of my last few years as dean, where they wanted their own facilities and so on, which they paid for very well. I think the Howard Hughes thing is really a great contribution. And they don't -- the string is attached, but not nearly like they were _____. That building out there is largely Howard Hughes _____.

Summers: Was there resistance on the part of the central administration to taking that money or . . .

Berliner: No, no. They nominally set, for example, the salaries of the people who were supported by _____. We told them what -- I was not allowed to put into writing anywhere, but they used to call them on the telephone and tell them what their salaries were supposed to be, and then they would fix their salaries until _____.

Summers: The other, of course, major support comes from the federal government, and was there -- what were the issues around soft-money positions at the time? I remember Brewster, in the early '60s, seemed to be rather opposed to this and was warning about the problems of soft money in medical schools.

Berliner: Well, by the time I got here, it was, you know, you have to realize that the medical school had about one third, a little more than a third of the total operating budget of the university. I guess it's gotten to be more than that. They had about 5 percent of the endowment of the university, and you got no support out of the general endowment. Only the medical school got it when it came to the medical school, so that the medical school endowment was paying something like 4 or 5 percent of the operating budget of the medical school and other hard-money tuition and whatever other small amounts of . . . But they don't come near to running what the medical school . . . It's not only . . . So that between the income generated by the clinical faculty and the income generated by research grants was _____

made up 90+ percent of _____. That's all soft money, if you want to call it soft money. And so there wasn't any way of running the medical school that could stand up with other schools around the country _____ do it that way.

Many of the other leading medical schools had much greater endowments than _____. Harvard, Columbia, Hopkins all had much, much greater medical school endowments. I don't know how it got to be that way, but it was true.

Summers: Were there attempts to think about increasing the medical school endowment substantially?

Berliner: Fundraising efforts, but they never really _____. I don't know that the medical school endowment _____ is relatively much better now than it was then. I mean, of course it's _____ a lot more than it was then, but as a fraction of the university's endowment, I doubt that it's changed much.

Summers: So, did problems arise because of this? I mean, was funding . . .

Berliner: Not really, because we were pretty much in the black from the time we, as soon as we straightened out what the medical school's contribution to the university functions should be, and when the university medical school was able to get its indirect costs generated by grants, it is true, I think, that the medical school's real indirect costs are lower than the rate that the university had because the university had a much _____ cost per grant _____ the university, the rest of the university, are much higher than here. So we had two-thirds _____ research support, we kept the university's indirect cost rate down, which was to the interest of the university.

Summers: And were there kind of programs that people wanted to start that caused the school difficulties, that is, for policy reasons, or were there any difficult _____?

Berliner: Not that I remember.

Summers: What about commercial ventures? _____ or _____ companies or . . .

Berliner: Well, that just was beginning to get started about the time I left the deanship. It was beginning to look like a problem. At least it was happening a lot elsewhere, and the tendency to think in that direction here began to . . . There was one company that started _____, as far as I remember the only one, involved Litz [sp.], Marquesy [sp.], _____ -- what's-his-name in chemistry?

Summers: Don Carruthers [sp.].

Berliner: Don Carruthers [sp.], and one other guy. I don't remember who that was. Frank Reynolds maybe.

Summers: Frank Reynolds, right.

Berliner: And that was the only one that came up during the time I was dean. I can see why it happened, but I'm not at all . . . I think it's an unfortunate trend in American medicine _____ these days. There's so much -- you know, the first opportunity that somebody gets, they start a company and do something.

Summers: Were there approaches from various local organizations to collaborate with the medical school in commercial ways or companies that wanted to buy into us?

Berliner: Not . . . I think, again, that most of that came a bit later. There wasn't very much of that kind of thing _____.

Summers: Was there a patent policy, a patent _____?

Berliner: The patent policy was developed in the last few years of my deanship. Of course, there always had been one, but the one that I think which is more or less in effect now is, worked out I guess when Bill Brady was still dean, and we had a lot of meetings to discuss _____. I think the present policy was worked out then. I don't know whether any changes have been made to it.

Summers: How integrated was the medical school with the main campus in terms of things like grants and contracts and that kind of thing? I mean, we now have our grants and contracts office as _____ separate.

Berliner: During the time I was dean of the medical school, the university established the office over there that dealt with collaborative research, which was supposed to be involved in development of contracts. What was the name? Bob _____.

Summers: Bob Bickerton [sp.].

Berliner: Bickerman?

Summers: Bickerton [sp.].

Berliner: I don't know that it had much influence on what went on over here.

Summers: Another thing that sometimes happens to courses in medical schools is the departments need to be trimmed down or new departments needs to be

formed. What kind of things like that happened while you were dean? You mentioned microbiology fading away, more or less.

Berliner: Well, what later became the department of neuroscience, what's _____, anyway, that was -- a new unit was established. It was originally thought of as neuroanatomy. That was one new function. One other one was -- I'm not sure what immunology was, what its status was. I guess it was still part of pathology _____.

Summers: Then it became a section of immunology, didn't it?

Berliner: Yeah. It was a section of immunology within -- I guess it was separate.

Orthopedics. I think orthopedics -- I'm not sure whether orthopedics split off while I was still dean or just after.

Summers: What was the motivation of that sort of thing?

Berliner: Damned if I know, really.

Summers: _____ subdividing _____.

Berliner: Yeah. Well, that's not long before I came, ophthalmology had split off surgery. Neurology had split off medicine. To some extent, the establishment of institutes with those functions had very strong influences in creating separate departments for _____ activities.

Summers: Did you preside over the splitting of radiology into diagnostic . . .

Berliner: No. That had happened before I got here. I guess it was the guy who left and went to San Francisco.

Summers: Dick Greenspan.

Berliner: Well, Dick Greenspan came back from San Francisco.

Summers: Right. He had gone.

Berliner: He had gone out there when the previous guy, who was a radiotherapist . . .

Summers: Oh, that's Mark Kreegerman [sp.].

Berliner: Kreegerman [sp.], yeah, right. And he went down somewhere in Arizona.

Summers: To Los Alamos.

Berliner: And I don't know why they decided to split the department then. That had all happened before I got there. And, you know, they're not really very _____ machines _____.

Summers: _____ screwdriver to be in that specialty, but medically and scientifically _____.

Berliner: _____ very little _____. It made some sense _____.

Summers: So, what were some of the problems you saw when you were dean that stand out in your mind now?

Berliner: I didn't have any very big problems. I'm glad I'm not dean now, but in those days it was a lot of fun. There were no really serious problems. Money was the biggest.

Summers: Deans were turning over at a rapid rate at that time, and you seemed to be one of the ones that stayed for the longest _____. What was your colleagues _____?

Berliner: You mean other deans elsewhere?

Summers: Deans elsewhere. Why were they getting out of the job? Do you know?

Berliner: Well, a lot of it was the student unrest in the period, which was dying down when I became dean. I think the turnover dropped off quite a bit at the beginning of the '70s. So I was dean for 11 years, which was long enough.

Summers: When I was in medical school, I had eight deans in which I was a student under.

Berliner: Really?

Summers: Yeah. I mean, some of them lasted only six months, you know.

Berliner: Really?

Summers: One was the famous dean, John Bowers [sp.], who got sent out of town tarred and feathered because he was trying to _____ as chair of surgery at Wisconsin.

Berliner: Oh, that guy.

Summers: _____. Anyhow, there was a whole series of things that . . . I think that's probably what we went through.

When you came to Yale, did you feel that there was something you wanted to do or accomplish or change or build _____?

Berliner: Not really.

Summers: You didn't see it as having an agenda or a mandate, or did Brewster say we want this done or that done?

Berliner: No. No. I think his attitude was he wanted a good school. I tried to see that he got one. The university -- it's taken a long time for the university to become a university _____ Yale College and appendages. Yale College was the center of all thinking, at least up to Kingman, I think, and he _____ began to think that there really was more to worry about than Yale College. _____ name of medical school as the medical branch of Yale College.

Summers: What was your relationship with the deans of the other professional schools?

Berliner: Cordial, friendly, not in any business sense.

Summers: Were there attempts to have cross-school programs much, M.D./J.D. degree or something like this?

Berliner: No. There were a few people who, in the M.D./Ph.D. program, would do their Ph.D. in some subject unrelated to medicine. There was nothing _____.

Summers: And what about the school of organization and management vis-a-vis the medical school, _____? That seems to almost be _____ these days.

Berliner: Yeah. That's right now. It was just getting started when I got here. I think it was brand new when I got here, maybe a year, one year old. And we used to have lunch with the deans. The president and provost would meet with all the deans maybe once a month or so, and that's when I'd see them, but that was it.

Summers: There was a group of people in DPH who had interests that seemed to me to be appropriate for within the school.

Berliner: Oh, yeah. John Thompson and . . . They were responsible. He and one guy, whose name I can't remember now who headed up an organization, and I'm not sure whether he was originally, came up with the system by which Medicare -- what's it called?

Summers: Diagnostic related groups.

Berliner: Yeah, DRG.

Summers: _____.

Berliner: They were the ones who invented that.

Summers: And what was your view of that? Was that a positive?

Berliner: Well, it didn't have much effect on anything I was involved in at that time.

Summers: Why did you decide to retire when you did?

Berliner: Because I got to be 70 and had to.

Summers: Okay.

Berliner: You know, when I came, the original idea was that nobody had any administrative roles beyond 65, so there was an exception made for Rudy Rogers [sp.], who was _____ the university library, and then me, and we were both allowed to continue beyond. So I was 69 when I left _____ and I took my last official year as sabbatical.

And it was the stupidest things that the National Academy of Sciences ever did was to come out with a view that the exception for universities to have a required retirement at age 70 was unnecessary because everybody wanted to retire anyway.

Summers: Would you have stayed on had that rule not been in place?

Berliner: I suppose I would have. I don't know.

Summers: That's a _____ question.

Have subsequent deans consulted you for advice on their problems?

Berliner: Not very much. Lee and I were, _____ were pretty good friends, and I can't remember his ever actually consulting me a little. He used to talk about things periodically.

Jerry Brenner [sp.] put me in charge of a committee that was supposed to have something to do with space, and we met and talked about space for a long time, but nothing ever came of it. I think he maybe wanted to get that out of his, _____ say somebody was working on it. And Kessler and I have said hello to each other a number of times _____. And the only thing he's ever talked about is the fact that he and I went to the same small high school.

Summers: There was a one-time attempt to figure out ways to get the faculty to retire gracefully around here. I remember Howard Levitan [sp.] was very active a few years ago in that. Did you have any discussions with those folks?

Berliner: No. I don't know much about it. I know it hasn't worked very well. I have lunch quite regularly with three or four people, all of whom are well beyond 70, and only one of them is finally retired.

Summers: Who is that? Who did retire?

Berliner: Jack Cooper.

Summers: Oh, okay.

Berliner: The school doesn't lose anything by having these people, except that down the road I think you have to worry about make a place for young people.

Summers: Oh, you mentioned your relationship with Lee Rosenberg, which brought to mind . . . Well, I understand that you and he played poker together for a long time.

Berliner: That's quite true.

Summers: So, tell me a little bit about this poker club.

Berliner: The poker game? Well, the poker game predated me by a long time. It had involved _____, I think, and -- I'm not sure whether . . . Oh, the former chairman of _____ whose name I know so well.

Summers: Was it Sam _____?

Berliner: No, no, long before I knew Sam.

Summers: Phil _____?

Berliner: No. Think back further. He went to England and became Regis professor at Oxford.

Summers: Paul _____.

Berliner: Paul _____. He _____. And Dick Greenspan and Nick Green. _____ remember. There's this problem with names.

Summers: Where was Phil from? What did he do?

Berliner: A surgeon.

Summers: Bill Black?

Berliner: Bill Black, that's right. And Joe Hoffman. I don't know. Anyway, Sam Broder and Lee Rosenberg and I. We played, we used to play, we were supposed to play once a month, and we did for quite a while, but it got to the point where it got so difficult to find a day when everybody was available, they always wanted to play only on Friday nights, and to find a Friday night when nobody was in Europe or Australia or whatever got to be very difficult. So the game has kind of petered out in the last couple of years, but it was a pretty good game -- not a high-stakes game by any means.

Summers: This was a Yale thing. Was there an equivalent one at NIH when you were there?

Berliner: Yes, there was.

Summers: So, who were the players at NIH again?

Berliner: The players at NIH were Chris Anfinsen, Steve Brody, Sid Udenfriend, Dan Steinberg, Al Rabson. There must have been one or two others, and I can't come up with them right now. But those are the ones that I remember most clearly.

Summers: You played once a month with this group also or . . .

Berliner: I think we played more often than that, maybe a couple of times a month.

Summers: And did this date back to the '50s, when you first came there?

Berliner: I think it started somewhere in the late '50s. I can't remember exactly how it started and when. Steve Brody always claimed to have paid his way through college by playing poker. He may have been the guy who kind of started it.

Summers: How did you take in new members of the group when they came.

Berliner: Well, I don't know that we took in very many new ones as long as the original group held together. After that group had broken up because various people had left and moved off to other jobs, there was another game, which actually ran, a table-stakes game. Ours was much more limited. And I can't remember who they were, and I only played with them once or twice.

Summers: What does table stakes mean?

Berliner: Well, table stakes means you've got to set them out over here. You can bet anything up to the amount of chips you have on the table.

Summers: Okay.

Berliner: Whereas our other game was something like a 25-cent or 50-cent limit, and you'll have a limit of three raises or something like that. The other one could run quite a bit of money.

Summers: Well, let's finish here.

END OF INTERVIEW