SMB
Arthur Bienenstock
Remote Interview

by David Zierler
22 April 2020

DAVID ZIERLER: This is David Zierler, oral historian for the American Institute of Physics. It is April 22nd, 2020, and it is my great pleasure to be here virtually with Dr. Arthur Bienenstock. Dr. Bienenstock, thank you so much for being with me today.

ARTHUR BIENENSTOCK: Oh, it's a pleasure to do it.

ZIERLER: To start, can you tell me your current title and institutional affiliation?

BIENENSTOCK: Let's see. I am at Stanford University, semi-retired. So I'm a professor emeritus of photon science. I'm associate director of the Wallenberg Research Link. And I also have the title special assistant to the president for federal research policy. And then in addition, I'm a member of the National Science Board, the governing body of the National Science Foundation.

ZIERLER: Now, when did you take on the role as special assistant?

BIENENSTOCK: When I stepped down as vice provost and dean of research and graduate policy. As vice provost, I had responsibility for research at the university, for graduate policy, and also was playing a significant role in our interactions with Washington. We split the job into two and a half jobs. That is, we had a vice provost and dean of research, a vice provost and dean of graduate education, and I kept, for the time being, the federal policy part.

ZIERLER: I see. Let's go right back to the beginning. Tell us about your birthplace and your family and your early childhood.

BIENENSTOCK: I was born in New York City, and for the most part, grew up in the Bronx. My father, when I was born, was an electrician. I was born in 1935. At the start of the war, when I was six, I guess, he went to teacher training, because they needed teachers of—they needed electricians in the war. So we went up to my mother's family's house in Gloversville, New York, upper New York state, and I spent the year there with my mother and her family, and then returned to New York, where my father became a teacher. For most of my youth, he was a teacher in Harlem at New York Vocational High School, and very involved with his students, seeing education, in particular becoming an electrician, a means of furthering the livelihoods of these students. So in the family, it was a mixture—he was very interested in science, electronics, and the like. It was a mixture of that plus I'd say social concern. Often at dinner time, discussion would be on topics of the problems of his students. My mother, for the most part, was a homemaker. She was a really good pianist, and music was another big part of our family. It was a not religious family.

ZIERLER: Were your parents native New Yorkers?

BIENENSTOCK: No. My father was born in Pennsylvania, my mother up in Gloversville, and they both came to New York City.

ZIERLER: You were saying that your family was not particularly religious.

BIENENSTOCK: I started to study to be Bar Mitzvahed and really grew disinterested in it, expecting it to focus more on values and less on just reciting Hebrew. Let me see. I think my earliest strong memories are in junior high school, where I *really* became interested in algebra, mathematics. And that continued on. But I recall three other subjects captivating my interest—civics, learning how the government worked; grammar, working—learning how the language

was constructed; and believe it or not, typing. I was left-handed, and the typewriter freed me up in a marvelous way.

ZIERLER: [laugh]

BIENENSTOCK: Oh, and woodworking shop. I was a funny student.

ZIERLER: What kind of a high school did you go to? Was it public, private?

BIENENSTOCK: I went to the Bronx High School of Science.

ZIERLER: Oh, you did? OK.

BIENENSTOCK: Yes.

ZIERLER: Now was that selective, competitive admissions, to get in?

BIENENSTOCK: Yes, it was. And my classmates, the students that I know, have done an array of really good things. You know, that high school has produced I think eight Nobel laureates.

ZIERLER: Yeah.

BIENENSTOCK: It was a good experience in the sense that on the one hand, it was very academically oriented, and yet it was fun. Fun in a very clean fashion. I enjoyed—

ZIERLER: What years were you there?

BIENENSTOCK: Let's see. I graduated in January 1952, so I would have gone there, I guess, in 1949. In those days, the first year of high school was spent in junior high school. In

1949, I became interested in Zionism. I joined the Zionist movement. And that captivated a lot of my time. It was the early days of the state of Israel. One had high hopes that Israel would bring democracy to the Middle East. It was really a period of idealism. And I sort of split my time between high school and early college between school and the Zionist movement.

ZIERLER: Did you ever consider making aliyah?

BIENENSTOCK: I did. In fact, I went to the Polytechnic Institute of Brooklyn expecting to be the engineer on the kibbutz. And within the movement, I had a mixed role. On the one hand, I was Mr. Fix-It. The movement had a farm outside of Poughkeepsie, in New York. And the first day I went there, I picked tomatoes, but something was wrong, and I fixed that. My father had golden hands, and from the time I was very young, he taught me to fix plumbing, electricity. We worked on all sorts of things together. So on the farm, I was Mr. Fix-It. And it was clear that I had aptitude for engineering. But I also edited our literary magazine. So I had that mix.

ZIERLER: Did you get a sense, looking back at the Bronx High School of Science, that you were being exposed to math and science concepts at a higher level than had you gone to like a regular P.S.?

BIENENSTOCK: I don't know if it was a higher level, but we took four years of mathematics, four years of science, four years of English. I don't know if it was at a higher level, but since all the students were so bright, the level of discourse was very high. I can remember we'd sit around in a candy shop, ice cream shop, across the street from the school, and people would discuss the books they were reading and things of that sort. You know? And yet, I can also remember going to dances and then coming—they were typically on Friday night—and then

coming in on Saturday to clean up with my fellow students and having the gym to ourselves and waltzing away in this large gymnasium.

ZIERLER: Was the high school co-ed or was it boys only?

BIENENSTOCK: It was co-ed. A lot of my social life revolved around that. I still have—I'm still friendly with probably my first girlfriend. And she has won many awards.

ZIERLER: [laugh] What was the decision behind attending the Polytechnic Institute of Brooklyn? Were places like Harvard and Stanford, were they on your radar, or is that just too far away for you and your family?

BIENENSTOCK: Too far away. I never even considered that. Although in retrospect, I probably would have gotten financial aid.. I finished up at Bronx Science as president of the student organization, and given my student grades, I probably could have gone to Columbia or Harvard or a place like that. But my goal was to become an engineer. Certainly my father was very strongly supportive of that.

ZIERLER: Was the Polytechnic Institute—was that a CUNY school?

BIENENSTOCK: No, no. It was a private school. And for some reason, he wanted me to go to a private school. And I don't really know why, but he did. Although it was a stretch for the family financially.

ZIERLER: Now when you went there, did you declare the major in physics right away, or that was later on?

BIENENSTOCK: No. What happened—I expected to major in electrical engineering. The first year I took physics and calculus. We didn't have calculus in high school at that time. And I really got excited by, first of all, physics, and then by the ability to derive the equations using calculus. So I decided—it's a funny thing to say—you only go to college once. I wanted to have fun. So I was switched to physics as a major and took enough engineering so I could manage the kibbutz, but at least I'd have fun in college. And I also, throughout college, loved literature. I was at the same time editing our literary magazine for the movement. I took freshman English. They exempted me from sophomore English, which was a writing course, and it was clear I was—you know, Bronx Science expected you to write. The movement had very high standards for writing. And I joined the debate team and did that instead. But I always took—I typically took 21 or 22 units so I could get the literature courses in as well as the science and engineering courses.

ZIERLER: Given your father's background and your interest in engineering, when you developed an interest in physics, did you naturally gravitate towards the applied and the experimental side?

BIENENSTOCK: No. I was always mathematically oriented. So it was that combination. The experimental side came only in my senior year, and it's weird the way it happened. In my junior year, I met Roslyn, the woman who was to become my wife, and she was very involved with the folk music circles. Her boyfriend before me, who is still a friend, was Frank Hamilton. You probably don't know that name, but Frank replaced Pete Seeger in The Weavers.

ZIERLER: Oh!

BIENENSTOCK: He was of that quality, OK?

ZIERLER: Oh, wow. OK. [laugh]

BIENENSTOCK: Frank started a folk music school in Chicago and has recently started one in Atlanta, Georgia. At any rate, she traveled in those circles, and I decided I wanted to learn to play the guitar. I had always sung. I sung in high school. I sung in the movement. And I wanted to play the guitar. My wife pointed—then-girlfriend—pointed out that there was a graduate student at Brooklyn Poly who was an extremely good guitar, lute, and banjo player, and why didn't I go see him and see if he would teach me to play the guitar. This was in my junior year. He was in the x-ray lab, using x-rays—Poly had a magnificent array of faculty in x-ray crystallography. And he was, in that x-ray lab, a graduate student studying the structure of something; I can't remember what. It was in the days just before proteins could be—the structure of proteins could be determined. So that was a major part of the lab. At any rate, I studied guitar with him, primarily classical guitar but also folk. And he said one day, "Well, you're a physics major. Why don't you start learning some x-rays? You're around the lab all the time." And they liked me around the lab because they had a group, a folk music group, and I could sing. I could really belt out a song in those days. So I hung around the lab, and I started learning about x-rays. And in my senior year, I moved to Greenwich Village. I was working in a war surplus electronics and optics shop on Canal Street in New York to support myself living in Greenwich Village. And I walked into the lab one day in the start of my senior year, and a professor said, "My graduate student has gone off to Turkey. I need someone to work on a project." And so I agreed to that. And then I was captivated. I mean, when the answer wasn't known, I became enthralled, actually.

ZIERLER: What was the project?

BIENENSTOCK: It was studying the properties of titanium diboride. Titanium diboride—we did—my memory is we studied three things, and I only remember two of them. The thermal expansion, which was very unusual, up to high temperatures. So it was the first venture of the lab into high-temperature x-ray diffraction. Second, trying to understand the electronic structure. I think we made headway, but it gave me an understanding of the difficulties of understanding the electronic structure using x-ray diffraction, particularly when you were interested in the outer electrons and what they were doing.

ZIERLER: Did you find that you had a skill for tinkering in the lab, for putting instrumentation together?

BIENENSTOCK: Yes, yes. And that was clear. You know, a lot of the graduate students—in those days, you used x-ray generators with commercial equipment on top, so the equipment was made for you, but the equipment would break down from time to time. And I found my colleagues were afraid to get their hands dirty, whereas I would go in and deal with the generator itself, if there were problems, or the instrumentation. But that didn't happen, for the most part. I mean, for most of the time, I was just using commercial equipment. By that time, I had realized I was not going to go to Israel on a kibbutz. For several reasons, I realized that kibbutz life was not for me. I was prepared to be in certain circumstances a minority of one. That's fine when you're in a big city. It's fine in academia. It's not so great in a small community, very close. And my wife—my girlfriend, now my wife, she didn't want to go to Israel. So I didn't really know what to do, because I had never thought about physics as a profession. It was just, for me, fun. So I decided to stay on and use this project to get a master's degree. That was the limit of my interest. In my first year of graduate school, I took a course with a famous German theoretical physicist, Paul Ewald. Ewald was one of the two leading theoretical physicists involved with x-ray

diffraction in the early days, Hans Bethe's father-in-law, close to Sommerfeld and Debye. I took his course on advanced x-ray diffraction, and he talked one night of a problem that he was sure was soluble, but he had never been able to solve it. And I can recall that night—it was a night class. And after the class, I went over to Roslyn's family's house, not my apartment in Greenwich Village, and I said naively, "I'm going to solve that problem." So in addition to my experimental master's thesis, I started on a PhD. And the problem was there are 230 groups that describe the possible symmetries of crystals. Ewald was convinced that those 230 groups could be derived in so-called Fourier space, the space that describes x-ray scattering, rather than in real space. And that was the problem that I took on, in addition to my experimental master's thesis. So I would typically work in the lab—unless I needed to work overnight, I would work until about 10:00, go to my apartment in Greenwich Village, and work on this problem, probably until 2:00 in the morning. So I did that, and somewhere around the summer after my first year of graduate school, it was pretty clear that I had a solution to that problem, but that I could extend it as well to the so-called black-white groups. The 230 groups described the symmetries of a positive function, the electron density. But if you allow the function to be both positive and negative, then you have the so-called black-white groups.

ZIERLER: How do you allow that to happen? What do you have to do?

BIENENSTOCK: Oh! I don't remember the details, but you have to have operators now that—consider the possible symmetries of a positive function. You might have a mirror plane. So on one side of the plane, you have a certain electron density. You have its mirror image on the other side. When you go to the black-white groups, then you have the possibility of black on one side of the mirror, and white on the other, OK? So it's a whole new array of symmetry elements. Then there were color groups, and these were the possible complex functions' symmetries. So I

had found that in Fourier space, it was easier to derive all of those groups than in real space. And it was pretty clear that I had basically solved the problem. We had to wipe it up, and it took us another year or two to finish it off. So when I came in, in the fall of my second year, Ewald said, "You have to leave. You have to go to Harvard, Cornell, or MIT." He wanted me to go to Cornell to study with Hans Bethe, his son in law.

ZIERLER: Did Polytechnic offer a PhD? Could you have stayed on?

BIENENSTOCK: Oh, yes. That was my expectation—that this problem would be part of my PhD thesis. It was his expectation. I said, "OK" [laugh] since it was pretty clear that by the end of the second year, this problem was going to be solved. And then, my wife—my girlfriend, Roz; now my wife—she still had a year of college to do, and we wanted to get married, if I was going to leave New York. The only school that would admit her for her senior year was MIT. She was a chemistry major, leaning towards biochemistry. So I got into all three—MIT, Harvard, and Cornell—I think largely because of Ewald's influence. Since I had expected to go to Israel, I didn't care that much what grades I got.

ZIERLER: But what was the advice to leave and to go to one of these other schools? Was it that it was going to be better for your career? That you'd be able to do more as a graduate student? What was the basis for the advice to leave?

BIENENSTOCK: I think he wanted me to see a bigger world. After all, he had spent his early days with Debye and Sommerfeld and people like that. And a lot of the time that I spent with him those two years was spent describing the characteristics of Debye in particular, who he really admired, and Bethe, and Sommerfeld and people like that. I think he saw me as bright but provincial, and limited in my goals. And he wanted me to see what he considered the big league.

Although I have to say, Poly had a very big effect on me. In the x-ray lab, my advisor, Ben Post, spent a lot of time with me. It was funny; we were very close. He lived near my wife's family, and often on a Sunday, we would take my father-in-law's car, drive over to his house, and Ben and I would play ping-pong, and his wife and Roz would sit around talking. We remained good friends until his death, as I did with Ewald. They were very good to me. I was to Ewald's house, to Ben's house, to the houses of the other faculty members, and also to the house of every English faculty member that I had but one. They really treated me very, very well. So we got married. She did her senior year at MIT. I went off to Harvard. And that was a new world for me, in the sense that for the first time in my life, I was surrounded by people who I thought were better than me. I had never faced that before.

ZIERLER: You mean among your fellow graduate students?

BIENENSTOCK: Among my fellow graduate students. The group of graduate students with whom I was closest—Gordon Baym, now at—you know, retired at Illinois. Dave Mermin, who was at Cornell. David Adler, who was a professor at MIT. Pierre Hohenberg. I mean, it was really an outstanding group. And I thought they were all smarter than me, and I had never quite faced that before. So I went off to Harvard, and this time I decided I was going to be a theoretical physicist.

ZIERLER: Now, did Harvard accept your work as a master's student at Poly, or did they expect you to start all over again?

BIENENSTOCK: It was a mixed bag. I started over in courses that I didn't have at Poly. But I only took a year and a half of courses. It may have been only a year; I can't remember. So I started on a thesis with Harvey Brooks. Do you know that name?

ZIERLER: Sure.

BIENENSTOCK: And Harvey was working on something called the quantum defect method at the time, but he was also on Eisenhower's committee of advisors on science and technology. So there were those two worlds. For me, I was just working on my thesis. My thesis was to study the g shift in alkali metals. That is the shift of the energy it takes to overturn the spin in a magnetic field of an electron in an alkali metal compared to a free electron. After a couple years of calculating, I came right on for sodium and potassium, but believe it or not, I got the wrong sign— [phone ringing]

ZIERLER: Go ahead. Take that if you need to.

BIENENSTOCK: —no, my wife will take it—for rubidium and cesium. And I spent a year trying to figure out why the sign was wrong and went through every approximation that I had made, and I just couldn't come out with the right sign. In the meantime, I was writing papers on x-ray diffraction. I had spent two summers at the Bureau of Standards and gotten interested in the structure of glass, non-crystalline material. And throughout my graduate studies, as I learned more and more solid state theory, which was built around crystalline materials, I constantly asked, "How would this apply if they had glass and the lack of periodicity?" But it was in the background. But I did publish a couple of papers on the structure of glass and disordered systems along the way as a student. Not part of my thesis; just on the side. At any rate, I also published this work on the symmetry of Fourier space, and that hit big. It was an invited paper at an international crystallographic meeting. It was an invited paper at the American Physical Society. So it got me some attention. And also, my other faculty noted that I was publishing on the side in x-ray crystallography while working on my thesis. When I got to my oral exam, the PhD oral

exam, Nico Bloembergen was one of the people on the committee. And I went through the thing, and I explained how I had gone through every approximation that I made, and I just couldn't figure out why I had the wrong sign for rubidium and cesium. And Nico, at the end, said, "Artie, I think your theory is right and the experiments are wrong." He said, "It's almost impossible to get the oxygen out of rubidium and cesium, and I think the experiments are wrong." At any rate, Harvey and I published this 18-page paper that ends with a shrug of the shoulders. We just couldn't figure out why we had the wrong sign! Nico, meanwhile—

ZIERLER: Artie, let me just interject there—why did you go ahead and publish it, if that was your sort of conclusion? That it was inconclusive.

BIENENSTOCK: It was the honest thing to do. After all, people don't like to publish negative results, but negative results tell you something. They tell us something's not right, and that's important in physics. And I did that at other times in science. At any rate, Nico got two of his former students, Mickey Walsh and I forget who else—one was at Bell Labs, and one was at U.C. San Diego—to super-purify—one super-purified rubidium, and one super-purified cesium, and the results agreed with our calculation. So the experiments had been wrong. But all of that happened after we published this paper. At any rate, as I was finishing up, I wanted to stay on at Harvard as a postdoc, and I went to Harvey Brooks and told him that, and he said, "Let me think about that." And two weeks later, I went back, and he said, "We don't want you to be a postdoc; we want you to be an assistant professor. But we want you to go away for a year, and we've arranged for you to go to Harwell"—the atomic energy research lab in England. At the time—and I said, "OK." Not asking what the salary was or anything. Came home to my wife. We had a child, our first child at the time. I said, "Roz, I just accepted a job at Harvard as an assistant professor, except they expect me to go to England for a year, and I said yes." And she said,

"OK." That was that. The Harwell job only started in the fall. That summer, I went to the Bureau of Standards to work on negative thermal expansion. They had found that silver iodide, over an extended temperature range, contracted when you heated it rather than expanded. And I worked on that. Then we went off to England. And Harwell at that time had a number of really outstanding people. It was remarkable. John Hubbard, the famous Hubbard model. Walter Marshall. Just—it was fantastic, the Theoretical Physics Division. And we had a wonderful year. The other thing that was great is Harwell's about 15 miles south of Oxford, and the theatre would come to Oxford on its way to London, and it was really cheap. I mean, for something like I think a pound, you could sit in the best seats and see some of the most famous actors and actresses of England on their way to London in the theatre. So we went every week to the theatre and enjoyed it. We had a wonderful time. I worked there on phase transitions, but also theory of phase transitions, and also on negative thermal expansion, and then went back to Harvard.

ZIERLER: Now, Artie, I want to ask—when they said that they want to bring you on as an assistant professor but that you should go do a postdoc first, what do you think the message was there? What were they saying to you by encouraging you to go off but then come back?

BIENENSTOCK: I think they wanted to expand my horizons.

ZIERLER: This is a theme. From Poly, they wanted you to expand your horizons, and at Harvard, the same?

BIENENSTOCK: Yes. And when I look back, here's Ewald, you know, seeing me bright but not really knowing what I wanted to do, pushing me to go to Harvard. Similarly, I didn't really know—I had no sense of my own capabilities. I mean, here I was publishing—I think I published half a dozen papers as a graduate student. I gave two invited papers. And I was also helping out

other research groups when they needed to use x-rays. But I didn't have a sense of my own capabilities at all, and I didn't know how I was going to end up. I knew that the Bureau of Standards would hire me. I mean, I had spent a couple summers there. I had started a program there on the structure of amorphous materials.

ZIERLER: What was your initial connection to the Bureau of Standards? How did you get involved with them?

BIENENSTOCK: I finished my master's thesis in May or June at Poly, and Harvard didn't start until the fall, and I needed to earn some money. Ben Post arranged for me—he was my master's thesis advisor—to go to the Bureau of Standards for the summer, to work with a man named Aaron Posner, who was studying the structure of bone and teeth. When I got there, Aaron was interested in poorly crystallized bone, or young bone, and he wanted me to set up a system to do that. That was too complex. But then there was a man in the same division, Stanley Block, and he and another man had a theory that was related to the structure of glasses, complex glasses. And they wanted to study the structure of glasses, but no one at the Bureau had studied the structure of amorphous materials. Everyone worked on crystalline materials. So they asked me if I would set that up. And I had no experience there, so I started reading like crazy. But then at Naval Research Lab, there was a man named Jerry Karle, and Jerry had worked on gases and things of that sort where the techniques were related. So I went down and sought help from Jerry. Jerry was to later win the Nobel Prize, with Herb Hauptman. And I have to say, it was a really warm and welcoming atmosphere in Washington, between Naval Research Lab and National Bureau of Standards. So I set up this program and went back the second year. And we published one paper. I'm not particularly proud of that paper, but it got me started in the field. And that program went on for years. So I would go down to the Bureau and help them out with it. So they

knew me very well at the Bureau. But in spite of all of that, seeing how smart Gordie Baym and Dave Mermin and Pierre Hohenberg were, and some of the other people around, I didn't feel that I was as smart as them, to be honest. Now the Harvard faculty apparently felt differently, but I didn't see that at the time. So I think they wanted to broaden my horizons, and did. I mean, Hubbard was remarkable. Hubbard and I—it was funny—at Harwell, we would have tea or coffee twice a day, in the morning and the afternoon. Invariably, Hubbard and I would end up at a blackboard talking. Then the theoretical physics group would go out for lunch. And Hubbard was pretty conservative, and I was a New York Jew, liberal. And we would sit opposite each other and argue politics, day after day. And it's funny because I went back five years later, and they said no American had ever enjoyed it as much as Roz and I. We had a really good time. In fact, Harwell offered me a job, too. And sure enough, Hubbard and I end up at a blackboard at coffee, and then we all go out to lunch, five years later, sit opposite each other, and start arguing about politics.

ZIERLER: [laugh]

BIENENSTOCK: [laugh] At any rate, the Harwell experience is very—it did broaden me. Because between—I had spent my PhD thesis studying one-electron phenomena in metals. Then I go to the Bureau of Standards and want to understand negative thermal expansion, and that gets me into statistical mechanics, which I had never really mastered in my mind. I go off to Harwell, and again, I work on negative thermal expansion, because the really weird thermal expansion of germanium had come out early on when I arrived at Harwell, and I wanted to explain that. But I was working on phase transitions, more statistical mechanics. So it really broadened me. The negative thermal expansion got me both into statistical mechanics and lattice dynamics, which was very popular at the time. The other work on phase transitions got me more interested in

statistical mechanics. So it did broaden my palate markedly. And that was that. I went back to Harvard. And there, I don't know if I should keep talking, or you should—

ZIERLER: No, keep going!

BIENENSTOCK: Well, I went back to Harvard, and I continued working on statistical mechanics. And Dave Turnbull had come to Harvard while I was away at Harwell, and Dave was really interested in glassy metals. Now, all this work on the side on structure of amorphous materials became useful for him. Dave was also a brilliant thermodynamicist and understood statistical mechanics at a level that I didn't. Plus, in general, a materials scientist. So I started up, and my first student—I was interested in order-disorder phenomena. So my first student was Duk Yong Yoon. No, it was Hin-Chiu Poon. And we worked on the lattice dynamics of systems with impurities in them. Poon was to go on to Bell Labs and to become very well-known for models simulating integrated circuits. And he was a brilliant graduate student. My second graduate student was Duk Yong Yoon. I co-supervised him with Bill Paul, a high-pressure solid state physicist. And Yoon studied the pressure dependence of the order-disorder phase transition in beta brass. Because I had a theory about how that related to thermal expansion if the ordering interactions were only near-neighbor. And it became natural for Dave and I to have our two research groups meet together. So we would meet together for coffee in the morning, coffee in the afternoon, and Dave and I would have lunch together maybe three times a week. He had an enormous influence on me, and again, broadened my interest in statistical mechanics, in amorphous materials, while—oh, the third problem that I took up—I became interested in these phenomena where the temperature dependence is the opposite of what you expect. Things contracting instead of expanding when you heat them. Bill Paul was intrigued by the problem of lead telluride, selenide and sulfide. In most semiconductors, the band gap decreases with

increasing temperature. In those three compounds, the gap increases with increasing temperature. It was one of the major unknown problems in semiconductor theory. Harvey Brooks had this theory that you could calculate the temperature dependence of the band structure with pseudopotentials and applying temperature dependent Debye-Waller factors to the pseudopotentials. So I sent one of my graduate students, Charles Keffer to measure the Debye-Waller factors in lead telluride as a function of temperature with x-rays. So that got me back into x-rays. And in the meantime, he did a pseudopotential calculation. Ultimately, that worked out. We were able to explain it. But then Dave wanted help setting up an x-ray lab to study the structures of his glassy metals. So I was working on that, too. Somewhere along the line, I went to a Gordon Conference on disordered materials, and I met a man named Stan Ovshinsky. You may know that name because the American Physical Society has that Ovshinsky Lectureship.

ZIERLER: Right.

BIENENSTOCK: We were sitting around, and Stan told me that he had these fascinating materials that he could switch back and forth, he believed, between amorphous and crystalline, and he also had a threshold material that would switch from insulating to conducting depending on the current density, and would I be interested in consulting with him. And I was. Because in the meantime, we had a daughter—my wife and I—who had cystic fibrosis. I was very uncertain about whether I could afford to be a faculty member. So I went to Stan's lab outside of Detroit, in Troy, Michigan, and it was clear that he could electrically switch materials repeatedly from a high-conducting state to a low-conducting state, which he assumed the high-conducting state was crystalline and the low-conducting state was amorphous, although he had no way to prove that. So I got interested in that on the side. I came back and told Turnbull about that, and Turnbull said, "Artie, you gotta work on that. That's really fascinating."

ZIERLER: What was so fascinating, do you think? What was the connection?

BIENENSTOCK: The ability to switch reversibly on short time scales from crystalline to amorphous; that's what fascinated Dave. Dave had theories of the glass transition. He was very interested in how you form amorphous materials and things of that sort. And here was someone who could go back and forth between crystalline and amorphous on a millisecond time scale, then. So I did that, and I worked on several other problems. And in my fourth year at—Harvard gave assistant professors a sabbatical. I had wanted to go back to Harwell to work with John Hubbard, but because our daughter, Amy, had cystic fibrosis, England was not an appropriate place. Tony Siegman was on sabbatical from Stanford to Harvard, and Tony said, "Why don't you come to Stanford on sabbatical? We've got people doing experiments related to your theoretical work on lattice dynamics of impurities in disordered systems." So I went to Stanford.

ZIERLER: I just want to ask a quick question, before we move on to Stanford. I'm curious—your transition from a graduate student at Harvard to a professor at Harvard, was it difficult in terms of your own identity? Given all of the luminaries and giants on the faculty at Harvard, was it hard for you to make that transition where you were able to think of yourself as a faculty member, given the fact that you were a graduate students there? Or was that an easy transition for you?

BIENENSTOCK: I never thought about it. You know, it was my job. [laugh] I thought more about how do I make up good courses. At Harvard, I was teaching statistical mechanics, and that's when I really learned statistical mechanics. I was teaching x-ray diffraction. I taught a senior course in materials. And I was just busy, trying to get tenure, and—

ZIERLER: So you never thought, like, "What am I—Schwinger teaches here. What am I doing here?" You never had those kinds of thoughts?

BIENENSTOCK: Well, I thought they've made a mistake, but I liked it.

ZIERLER: [laugh]

BIENENSTOCK: I didn't think of myself in their class. Though as the time went on, I was giving more and more invited papers. Even in England. I had a funny experience. I came up with a calculation that matched the thermal expansion of germanium, but I did it in a way that I wanted to entitle the paper, "A Black Magic Calculation of the Thermal Expansion of Germanium." But they wouldn't let it out of Harwell with that title, so we just entitled it "A Calculation of Thermal Expansion of Germanium." And Walter Marshall and John Hubbard arranged for me to give a talk at a meeting in London on anharmonic effects in lattice dynamics. And I sat there as people went through long Green's function calculations. And I'm someone they had never heard of. I was the last speaker. And I got up and I gave this talk that was based on this calculation that I had wanted to appropriately entitle "A Black Magic Calculation." People started laughing. And I wasn't trying to be funny, but I gave my talk, and I couldn't figure out, why were people laughing? And I figured maybe I made a fool of myself. At the end, the two most famous people in the room, Blackman [?] and Hugh Barron [?], came up to me and said, "Would you like to join us for dinner?" [laugh]

ZIERLER: [laugh]

BIENENSTOCK: So I knew it wasn't foolish, my talk. At any rate, at Harvard, I was giving a lot of invited talks. I still thought they had made a mistake. I went off to Stanford and really

had a delightful six months there. In Massachusetts, when I left, I really had to dig myself out of the snow to get to the airport, with the family. We had two children at the time. We get to Stanford. They've arranged for us to stay in graduate housing. And we sort of had a attached little house. And Amy, who had cystic fibrosis, and who was really limited in her activities in Massachusetts, could just go outside and play. It was wonderful for her. Went back to Harvard, didn't get tenure, and Stanford offered me a job. A number of other places—Bell, and Illinois, Buffalo offered me jobs. And we went back to Stanford.

ZIERLER: Did you give Bell a hard consideration? Was that an attractive option?

BIENENSTOCK: Yes, it was really attractive, but I loved academia. And I liked the variety of functions. I liked both teaching and doing research. And for me, teaching was a way to learn. I mean, I became a much better physicist as a consequence of teaching than I would have been just from research. And I liked working with students. Students were fun.

ZIERLER: And Artie, you mean undergraduates and graduates? You enjoyed both?

BIENENSTOCK: Primarily graduates. I enjoyed my graduates. I enjoyed undergraduates, too. The most memorable undergraduate with whom I worked was Zach Fisk, who joined my group—

ZIERLER: I talked to Zach last week, actually.

BIENENSTOCK: Ah! Yeah. And Zach was wonderful. He was so smart. And so unusual, in a lot of ways. But my graduate students were fantastic. They've gone on to do such good things. So I enjoyed the graduate students.

ZIERLER: Artie, I have to tell you a funny story about Zach.

BIENENSTOCK: [laugh]

ZIERLER: And you know, I take a look at this guy and he doesn't look Jewish—

BIENENSTOCK: No.

ZIERLER: —and I ask, you know, "What's the deal with the email?" And he said he had a Vietnamese graduate student with a sense of humor that gave him that email.

BIENENSTOCK: [laugh]

ZIERLER: So if you ever get an email from him and you're wondering what the deal is, that's why his email address is chutzpah@gmail.com. [laugh] Anyway.

BIENENSTOCK: We were just together for some event, and I can't remember what it was, and rode on the bus together. It was good seeing him.

ZIERLER: So what was the message behind the title "visiting assistant professor"? Were you on a short leash? Were they really testing you out before they wanted to commit to the tenure line?

BIENENSTOCK: No, that's the standard Stanford—I mean, visiting professor is the standard title for someone who is based someplace else and comes on sabbatical.

ZIERLER: Oh, oh. So at that point, you were not really seriously considered for a tenure track line at Stanford? That came the year after?

BIENENSTOCK: I don't know. Who knows? It may have been. I just don't know. But it was clear once I was there that they were considering me, because they asked me to give several talks. And I gave talks on the theoretical side, but also on the structure side. I covered the gamut.

ZIERLER: What was your impression—coming from Harvard to Stanford, were different fields being emphasized? Were there different trends that were being pursued?

BIENENSTOCK: Yes. My closest link was to Walt Harrison, who had come. And Walt was heavily involved in pseudopotential. And I was involved with pseudopotential on this lead telluride problem. Then, there was a group studying the infrared spectra of systems with impurities in disorder, and I was very close to them and started doing experiments with them. That was in electrical engineering. And then I met the materials scientists, so I was interacting with materials scientists. There was a wonderful kineticist, Marsh Pound, and Marsh and I became friendly. Bill Tiller and I became friendly. Because my interest in glass necessarily got me interested not only in statistical mechanics, but the kinetics of processes. So I was close to the materials scientists. On the other hand, the electrical engineers were interested in impurity, so I was close to them. And Walt was doing the pseudopotential stuff, and I was doing that, too. So I was involved with all three. It was just—I didn't realize at the time just how nice Stanford is. And I'll get back to that later. But I was intent on getting tenure at Harvard. I mean, that was my goal.

ZIERLER: What was your reaction to being denied tenure? Were you crushed? Did you take it in stride? How did you feel at the time?

BIENENSTOCK: I was disappointed, but I didn't have time to be disappointed. I remember distinctly I had been invited to Illinois to give talks, one theoretical and one experimental, on two

successive days. And I learned that I didn't get tenure the morning that I was to fly to Urbana, But I didn't have time to think about it. I was disappointed. Roz was disappointed. I can't remember who was taking care of the children, probably my folks or her folks. But the two of us flew off to Illinois. We were disappointed. But we didn't have time to think about it. And what's more, clearly Illinois was interested in me, and Illinois had Bardeen and Pines and Kadanoff and Baym on the theoretical side. It had Lazarus on the experimental side. And it was pretty clear that I would fit in, in the middle, to talk to the theoreticians and to the experimentalists. Illinois was the best probably solid state physics department in the world at that time, so I wanted to make a good impression. I gave my two talks, and I remember Gordy and Nina Baym throwing a party for us, and chatting with Bardeen and really enjoying it. So I was really disappointed, but I didn't have time to think about it. The next thing was, where was I going to go next? And then Buffalo offered me a job. A man, Karl Willenbrock, who had been associate dean at Harvard, had gone to be I think provost at Buffalo. He wanted me to chair a materials science department. Illinois offered me a job, Bell offered me a job, and Stanford offered me a job. And my wife was very clear; she wanted to go to Stanford. And for good reason, you know? I mean, for Amy, compared to the other places, it was a difference. And in the end, it was I think the luckiest thing to happen to me, that I didn't get tenure at Harvard, and that I followed my wife's urging and went to Stanford. The problem with Illinois was that the medical school was in Chicago, and with Amy having cystic fibrosis, we needed a good medical school. And Stanford had a center with very good people in cystic fibrosis.

ZIERLER: Was it ongoing care, or you needed to be close in case of an emergency type of situation?

BIENENSTOCK: Oh, it was ongoing care. We did therapy twice a day for Amy. It was percussion therapy. I would do it in the morning, my wife would do it in the evening, or vice versa. She needed a lot of care. Took a lot of medications. I mean, she was the first person I knew who could swallow five pills at once, big pills. She took a lot of care. She was also a genius. To give a sense of it, before she was three, she could talk on the telephone. You could talk to her in rhymed sentences; she would respond in rhymed sentences. She graduated from high school with 32 units of advanced credit in the sciences and mathematics, although she spent a quarter of her junior and senior year in the hospital. And she only lived to go to Stanford one year, but she was a remarkable person. Trained a cheap horse to be a ribbon winner, and would go into the ring, win a ribbon jumping the horse, fall asleep on her horse, get up for the next event, drink a can of Coke, and go in and win another ribbon. The last time she could ski, she won a bronze medal in downhill ski racing. She only weighed about 75 pounds when she went to Stanford. Two weeks into the first quarter, a young woman approached her and said, "We need a cockswain for women's crew." So she would get up 5:00 in the morning, go out and crew, then go to the hospital for therapy, go to class, go to the hospital for therapy, go to the dorm. At her memorial service, we learned she was considered the humorist in the dormitory. I mean, she was a remarkable young woman, fun for the time that she lived. At any rate, for other reasons, Stanford turned out to be probably the best place in America for me to be. So I'm really happy to be here.

ZIERLER: What was the culture among the faculty at Stanford versus Harvard, socially, collaboratively?

BIENENSTOCK: I would say that within materials science and applied physics at Harvard, we collaborated as much as I collaborated at Stanford initially. I enjoyed Harvard very much. I

enjoyed Turnbull and Bill Paul and Peter Pershan who had been a close friend who was there. Harvey was there. Vic Jones. Nico. All of those people, I liked them very much. Cliff Frondel was in geology, and Bill Lipscomb was in chemistry. I was friendly with them. So I enjoyed Harvard. What I didn't see at first at Stanford—oh, let me tell you the other side. Growing up in New York City, you read a lot, and in my adolescence or late adolescence, David Riesman was a popular hero. He had published *The Lonely Crowd* and another book that I've forgotten. And I had read them while I was in college. Well, Dave Riesman lived near me, and we often walked the same way while I was an assistant professor. But I had the sense that I was never going to meet him, And it wasn't just me, because Turnbull was as learned in the intersection between history and economics as he was in materials science, but he told me late in his life that he only spent one evening, I think, or it may have been an afternoon, with the people in history at Harvard. I get to Stanford, and now Stanford has a different characteristic from Harvard in the following sense: at the time I was at Harvard, the school system in Cambridge wasn't that great. So either you were well-to-do and you sent your kids to private school, or you went off to Belmont, Lexington, Newton, the surroundings, Brookline, when you had children. So the faculty dispersed. At Stanford, about half the faculty lived right on campus. And the first year that we were there, there was a lottery for building lots for houses, and we got a housing lot, and we spent the first year building a house on campus. Stanford made a very advantageous loan, so we were able to afford a nicer house than we had ever had, and I'm speaking in it now. But there was also a faculty swimming pool, and like Harvard, a faculty club. Well, you would go to the swimming pool on the weekends with your children, and you'd spend a lot of time there because the children wanted to spend a lot of time there, and you were meeting faculty from all over the university. I remember probably at the start of my second year at Stanford, I got a call from the

sociology department, saying, “Artie, we have an Israeli sociologist as a visitor. Would you like to come to lunch with the faculty to meet him?” So you know, it was a completely different world of—here, I had these broad interests, and people were welcoming. They knew I didn't know sociology on the scale that they did, but they were pleased that I was interested in their field. And that characterizes Stanford so much. We have friends now, close friends, in history, in English, in music, in dance, in the medical school, in the law school, throughout the institution, and it enriches our life enormously. And if you want to take up something new, you don’t hesitate to call a faculty member and say, “I want to learn about this. What should I read?” And almost invariably, you'll get a suggestion for one or two things to get started, and things of that sort. Or, my wife and I, we met each other as teenagers at the New York City Ballet, and ballet has been a continuing interest. We are friendly with a dance historian. I will typically have lunch with her once or twice a year at the faculty club. When a star ballerina from the San Francisco Ballet retired and came to Stanford to teach ballet, she became a good friend, remains a good friend, although she’s back in France at the moment. It’s that kind of institution where people like each other across the institution, and we interact so much in an informal way. If you've got a lot of interests, it makes it fun to be here.

ZIERLER: Artie, in the late 1960s, how big were protests on campus? Civil Rights, anti-war, women’s protests—how big did that impact the Stanford campus?

BIENENSTOCK: Oh, enormously! And it influenced my *life* enormously. We had the Vietnam conflict. Now, when I was leaving Harvard, it began to become serious in Vietnam, and I had joined others in a back page ad on the *New York Times* protesting the war. As the protests went on at Stanford, I was clearly opposed to the war, and yet I vocally opposed efforts to rid the

campus of Department of Defense research. So here I was on the one hand opposing the war, and on the other hand, opposing what a lot of the protesters wanted.

ZIERLER: Did that even include research that was being done explicitly for offensive military purposes?

BIENENSTOCK: I was not opposed to that. What I was opposed to was doing any classified research. You have to know that in my background, Amy, my daughter, slept in a mist tent. It was a tent in which ultrasonically water vapor was made into very small particles, but of a particle size where they were absorbed on the lungs, and that helped keep her healthy. The knowledge of the particle size and how to make it was the result of gas warfare experiments. So I was well aware that—of—what do they call it?—dual-use technology. And I thought the research should be judged on the merits. The other thing that I felt at the time was even though I had strong feelings about the war, the war itself was controversial, and I knew that my colleagues, in particular Marsh Pound, with whom I was close, who was very conservative, was in favor of the war. And I didn't believe it was appropriate for the university to take a stand on something that wasn't directly relevant to the functioning of the university. I mean, this was a controversial matter. The university should be a place where we should be discussing these things and pursuing them intellectually, not reaching decisions on matters where there was controversy. Sometimes you have to.make decisions to run a university. That was one thing. The second thing was the very first year that I arrived at Stanford, they appointed me to the financial aids committee. And on the aids committee, I realized that needs-blind admissions was a very really nice feature of well-to-do universities. But Stanford had to raise money. Stanford didn't have a major endowment at the time, and it had to raise money for financial aid. So I helped the financial aids office in fundraising. With corporations. They would ask me to come have lunch

with donors and things of that sort. The second year, they made me chair of that financial aids committee, and I didn't realize it at the time, but I was the first faculty chair of that committee. It had previously always been chaired by an administrator. That year, we went from an academic council of all the faculty of the university to an academic senate, which was representative. And that senate decided to merge financial aids with undergraduate admissions in a single committee, and I was made the chair of that committee in 1969, two years after I came to Stanford. The first year, it dealt with some matters that aren't worth going into. But at the start of the second year, the Black Student Union sent the president ten demands related to undergraduate admissions and the admissions of Black students. I was a faculty power person, so I sent the president a note saying, "Hey, that's my committee's purview. It wasn't your place to make those decisions."

ZIERLER: Wow.

BIENENSTOCK: [laugh] I think the president was overjoyed to get that memo.

ZIERLER: [laugh] Right. "Take it." [laugh]

BIENENSTOCK: Yeah. [laugh] We spent a year meeting with representatives of the Black Students Union, and what was then not the Latino students, but the Chicano students, and they wanted separate minority admissions committees. They wanted a retreat from elitism in the admissions. Now I have to say that the two representatives of the Black Student Union, one went on and became a famous law professor and civil rights professor at Harvard, the other a theoretical physicist at Boston University. [laugh] So in the end, we said no to every Black Student Union demand. On the other hand, I saw the capability of the minority students that we were admitting, and we decided to go aggressively for minority students of very high quality.

ZIERLER: And recruitment was California mostly, or it was national?

BIENENSTOCK: That was another thing. When I was made chair in ’69, we had a new dean of admissions, Fred Hargedon. Fred had come from the East, from Swarthmore. He noted that the previous dean of admissions had recruited primarily from—I'll label it, for convenience, white Protestant communities. And Fred had come from a situation where New York in particular provided some of the very best students, and also where there was a lot more diversity. Fred broadened our recruiting markedly. Fred and the committee saw eye to eye on where to go. When I look at the quality of the minority students that Stanford has graduated, I think it’s well worth it. You know, I'm always proud of Cory Booker. Cory came as a football player. He was very strongly influenced by Don Kennedy, who passed away yesterday, our eighth president. I still admire Cory very much. But we had several—including a young woman who worked for me who clearly came from a very limited economic background and who has gone on to be a doctor, and also working in public health. I think Stanford has done well, that way. We needed very good babysitters for Amy, so we depended on Stanford undergraduates. I was taking one of them home probably at the end of my second year as chair of this committee, and she said, “Artie, you know, the ratio of men to women at Stanford”—there were many fewer women than men—“is not good for the men or the women. You're chair of undergraduate admissions. You should do something about it.” So in the fall of my third year as chair, I set up a committee. It turned out that Mrs. Stanford had put a limit on the number of women that could be students at Stanford after Governor Stanford died. And it was in our founding grant, so it was a legal document. I set up a committee to study that. They came back with two options. One, either we mandate 50/50, or we just do away with that restriction. The committee decided to do away with the restriction. We didn't want to mandate a quota. And so I had to handle these fairly

controversial things as the chair of the committee and became fairly well-known at the institution.

ZIERLER: And I take it that this experience led pretty smoothly into you being named vice provost for faculty affairs.

BIENENSTOCK: Well, there was an interlude. Stanford was going through budget problems, and it appeared that we would not be able to do what we had done in engineering routinely, and that is when we wanted to go into a new field, if we didn't have someone in that field, we'd hire someone from the outside. We weren't going to expand the faculty. So I applied to be a University Fellow to study, what does an institution do to stay current in engineering when 80% of its faculty are tenured, and it's not expanding? What steps could it take? I got release time from teaching to study that problem. But when it was announced, the provost, Bill Miller, called me in, and he said, "Artie, you can't just study that for engineering. You have to study it for the whole school." And he said, "Besides that, we've gotten these executive orders on affirmative action. Could you take a look at them? Read them and come back and tell me what you think we should do." So I read them for a week, went back and met with him, and I said, "Bill, there are two things. One, it looks like it's aimed at minorities, but the initial big impact is going to be women." And I said, "While other institutions are floundering, we should go out and actively recruit the very best women and minority faculty that we can find. If we do this well, it will enhance the institution enormously. If we do it poorly, it will not only hurt the institution but it will hurt the minority and women students. You want to get the very best people you can, and while others are floundering, we should just go out there." So he said, "You want to run the program?" And now this was '72? The bottom had dropped out in the funding in physics. My students were having a hard time getting a job. I was down to one student. And I said, "Yes."

OK? So he made me first faculty affirmative action officer. And I started out on that, and then six months later, I was made vice provost of faculty affairs, but I had this previous history of dealing with minority and women's issues. At the same time, I was working actively on the structure of amorphous materials, particularly amorphous semiconductors, and we had an international meeting, and I was chairing a session. A young graduate student by the name of Dale Sayers got up and spoke about studying the structure of amorphous germanium using not x-ray diffraction but x-ray absorption spectroscopy, something called extended x-ray absorption fine structure. He had a terrible time doing it with x-ray tubes, because it required a continuous spectrum. You wanted to measure the x-ray absorption coefficient as a function of photon energy. And x-ray tubes are very intense at individual photon energies, but they give off a very weak continuous spectrum. In the meantime, we saw the possibility of getting synchrotron radiation from the storage ring that was under construction up at SLAC. My colleagues, Seb Doniach and Bill Spicer, were actively promoting *that*. And I saw we could measure EXAFS so much more effectively with synchrotron radiation than with x-ray tubes, so I joined in with the two of them to develop what was then the Stanford Synchrotron Radiation Project. Seb and Bill had the primary responsibility because I was vice provost for faculty affairs. I think I was named an associate director of the project.

ZIERLER: Were you ever concerned that all of this administrative work was going to excessively—it was going to take you too far away from your students and your research? Or that was a balance you were willing to strike?

BIENENSTOCK: It was a balance. You know, my background—it was—I'll put it simply as social justice and science and technology. That's what I grew up with. Plus music, and later dance. But that was my background. And the university was giving me the opportunity to do

both. Throughout that period, I was giving invited papers at the international meetings on structure of amorphous materials, among other things. But I didn't worry about that so much. Because we had to figure out, how do you do affirmative action well? And that means how do you get the faculty behind you? Because in the end, they have to do it. How do you do it? At first, I didn't have the trust of the faculty or the minority community. I said, "We are going to go after the very best people." Yeah? Now the white male faculty didn't take me seriously. They didn't think I was honest about it. The minority community thought I was going to use that as a way of not getting minority faculty. But when they saw the people who came to Stanford—they were so good—that turned everything around. Now, the people who came are still friends, some of them, and they've contributed so much to the institution. But at first, that was the situation. So you had to build up trust. You had to get the faculty to really believe in it, go out and seek the very best people. We couldn't identify them; they had to identify them. You had to get the minority faculty to work on it too, and they were enthusiastic. I mean, once they realized it was serious, we all worked closely together.

ZIERLER: Now when your time ended as vice provost in 1977, was that your own decision, or is that a standard sort of five-year terms before you rotate out?

BIENENSTOCK: It was mixed in the following sense. I was good at administration in the part where you're trying to figure out what to do. Once it starts to get routine, it's less interesting to me. So that was happening. I mean, we had everything in place. I have to say, we formalized all our appointment procedures, and as a consequence of formalizing those procedures, I think Stanford's faculty rose in quality generally in that period afterwards. So the job of starting it was interesting to me. Second of all, the synchrotron, with which I was heavily involved—one of my graduate students helped us to set up the first beam line—was getting really interesting. I saw,

"Boy, can we do things!" And Seb was stepping down as director, so the vice provost then for research who had the office opposite me, came to me and said, "Artie, you have to go and take over. Seb is stepping down and we need someone to lead the SSRP." So it was a mixture of the two—my interest in synchrotron radiation and my declining interest in administering something that was getting routine.

ZIERLER: Now what is the relationship between the synchrotron radiation laboratory and SLAC? Is it within SLAC? Are they adjacent institutions? How does that work?

BIENENSTOCK: At the time, it was completely separate and yet dependent. The synchrotron radiation project until 1982 was funded by the National Science Foundation, not the Department of Energy. It was administratively separate. I didn't report to the SLAC director, although in practice I had to. We were parasitic. SPEAR was run for high-energy physics, and we just took the radiation out on one and then two beam lines.

ZIERLER: And the people that were in the lab, were they mostly faculty members, or they were specific employees of the laboratory?

BIENENSTOCK: It was a mix. At that time, every faculty member—we brought in Herman Winick as deputy director, and Herman had a joint appointment in Applied Physics and at the synchrotron lab. And that's the way we did it, and I wanted it that way. That is, I wanted the faculty to have a link to campus. We had hired Keith Hodgson while I was vice provost. We were competing with Harvard for who would get the project. Herman was at Harvard at the time. I told the NSF that we would hire someone in structural biology if we got the project, and we brought in Keith Hodgson. So Keith was in the Chemistry Department, but his research was largely largely in synchrotron radiation although he had a campus lab. But we had a big staff—

not so big; we had a staff that was scientists and technicians who were hired by the synchrotron project itself. When I took over, it became a laboratory. That is, the NSF finally decided we were here, sort of, to stay. They weren't fully committed, but—

ZIERLER: And what were some of the big projects that were going on at the lab? What were some of the big research questions that you were looking for answers for?

BIENENSTOCK: Ah. We had one beam line with five experimental stations. So one of them was devoted to small-angle x-ray scattering, and perhaps the most interesting thing that I remember at the time was a Caltech project on how the atomic arrangements in muscle change as frog muscles contract and expand. Then we had the end station, which was hard x-rays, also, and there, there were two types of projects that I remember very well. First of all, we were exploring extended x-ray absorption fine structure. My group was looking at glass. People were looking at catalysts. Other people were looking at glass. All sorts of disordered systems. I remember there was a project on hemoglobin. What happens to hemoglobin when it takes on oxygen to the environment around the iron atom, and what happens to the environment around the iron atom when it gives off the oxygen. We could attack metalloproteins functioning at an atomic level. Also, we were starting to explore how you use so-called anomalous or resonant x-ray scattering as a structural tool both for amorphous materials and for biological materials. Then there were the twp soft X-ray station. Perhaps the most interesting thing there that I remember—there was a lot going on—a lot of it on the electronic structure of solids, but Bill Spicer and others were studying the nature of the semiconductor metal interface. That was extremely important for transistors. And remember, it was the beginning of integrated circuits, and you wanted to understand what happened when you deposited a metal onto a semiconductor, to the surface states, and things of that sort. So Bill was exploring those sorts of things. Bill was also working

with me on the electronic structure of amorphous materials, or rather Bill was taking the lead on that. I was looking at the structural part; Bill was looking at the electronics things. Those are what I remember at the time. We had strong groups from Bell Labs, from Xerox, and from the University of Washington.

ZIERLER: Was this a place that attracted top postdocs?

BIENENSTOCK: It attracted top postdocs, but it attracted some of the most famous people in the field—Britton Chance, Peter Eisenberger, Farrel Lytle, Ed Stern, David Monckton, Bob Birgeneau. We were attracting some of the really outstanding people in the field. I remember my graduate student—her name was Sally Hunter —she was going out to dinner with some of the most famous people in the field. She was brilliant. Really good. I don't think she realized how good she was, particularly because, you know, they were so much more experienced than her. But they realized how smart she was.

ZIERLER: So you were the director of that until 1997, but then you had a lot of overlap at SLAC. How did that work out?

BIENENSTOCK: Oh. Well, there was a progression. In '82, NSF got from DOE an accelerator at Michigan State, and we went to DOE. So we were DOE, but we were still a separate lab. And I became, in a sense, a DOE national laboratory director. The other thing that you have to take into account was for us, the apparent disaster of Richter and friends discovering the psi/J particle. Because that meant they were running SPEAR at lower energies than the energies that yield copious hard X-rays. That meant that we had to come to terms with the fact that the ring was going to be run at low energies. And I remember Seb went on sabbatical while he was still director, and I was made—I don't know if I was made acting director or I was

associate director. So with Herman Winick, we were the two highest ranking people. And Herman was over at my house, we were sitting in the living room one night, and he said, "We have to introduce wigglers." And we just made a decision between the two of us. I don't know how we got the money, and how Burt Richter agreed to let us put a wiggler into the storage ring. By that time, I had become aware to how sensitive storage rings are to the ultra-high vacuum. You really have to keep that vacuum in place. And every time you install something around the ring, you're jeopardizing that. And what's more, you were jeopardizing the beam properties that they needed to collide to get to luminosity. But Burt agreed to it, and it turned out that it increased the luminosity rather than decreased it. But it was all risky at the time. I have to admire Burt in that first of all, he took a chance on our setting up any beam line, knowing the risk of our puncturing a window and letting air in was much greater than anything else he had on the ring. And then taking a chance on the wiggler too. And even though Burt and I fought pretty hard over the years, I really admired him. Burt once said that no one in his professional life had given him a harder time than me, and yet we ended up really close friends.

ZIERLER: In what way did you give him such a hard time, as far as he was concerned?

BIENENSTOCK: Oh, the hardest came when they were introducing the SLC. Now the SLC, you have to understand, the SLC, you'd use the two mile long linear accelerator to produce electron and positron beams of the thickness of a hair. They then had to circulate in opposite directions and meet after following a path that's maybe another mile or so. That meant you really had to control the magnets carefully. And that meant you wanted to run a stable linac at 50 GeV. We wanted to at two and a half GeV. That meant reducing the energy in the linac, reconfiguring the magnets. I mean, it screwed up everything for SLC. So for both of us,

maintaining our programs was life and death for the lab, and we battled like crazy. Burt needed his staff to focus on SLC, and I wasn't satisfied with the operation of SPEAR over which he had control. And at one point, I cancelled a million-dollar run because I didn't have assurance that our users would get good beam, and instead used the money to send our users to other synchrotron facilities. It created a furor. Out of that, Helmut Wiedemann on the SSRL staff proposed that we build the booster synchrotron and do the injection all on our own, and not depend on the linear accelerator. Burt was wedded to linear accelerators, but that was too expensive for us. So we built the booster synchrotron. So those were the battles. But in '92, the DOE and Burt decided that they had enough of separate DOE laboratories, one for synchrotron radiation and one for high-energy physics, and they made SSRL a division of SLAC. That meant that Burt was my director, and I was an associate director of SLAC. Both of us said, "We have to make this work." Previously, Burt was in a compromised position because his total responsibility was high-energy physics. He had to make high-energy physics work effectively; he had no responsibilities for synchrotron radiation. I was a synchrotron radiation director; I had no responsibility for high-energy physics. It changed everything, and it was Burt's and my nature that I perceived that I suddenly had a responsibility towards high-energy physics, and he had a responsibility towards synchrotron radiation. And we just dug in. We cooperated very closely. And it mattered in a number of ways. We were still operating SPEAR half-time. We only had budget to operate SPEAR half-time. By that time, SPEAR was fully dedicated to synchrotron radiation. But we only had budget to operate it half the year. In '94, I took a sabbatical to go to the European Synchrotron Radiation Facility, actually to a CNRS lab. I was on ESRF's Science Advisory Committee. And I saw how wonderful ESRF was, and I knew the Advanced Photon

Source was coming on, and I thought to myself, "If we only operate half the year, we're going to be closed down. We've got to operate full-time."

ZIERLER: Why were you only operating half a year? Was it just about the budget?

BIENENSTOCK: Yes. So I came back, and I went to DOE, and I said, "We just need more money. I have to operate full time." And besides that, our beam lines were in extraordinary demand. We had users who really needed more beam time. So I went to Iran Thomas, the head of DOE's materials science division at the time and I said, "We have to have more money." I had gone whining to him for years, wanting to get more money. But that time, he said to me, "Artie, I'm worried about all the materials science facilities. "The funding has declined to the point where I'm worried about safety of the facilities." So he said, "I agree with you. We need more money for the facilities." Well, I was driving downtown to meet with the head of the Office of Science, Martha Krebs, that afternoon. And it's about an hour and a quarter drive down from Germantown to downtown, to the Forrestal Building. And I thought to myself, "I'll never get the $3 million that I need to run SSRL full-time." Because the way appropriations go, that would come out of some other lab's budget, and that's not going to work. I said to myself, "But if I went after $50 million, that would have to be incremental, and I could help solve all of these problems for the materials science facilities." These were the synchrotron labs, the neutron facilities, and the like. So, I get down to Martha's office, and as I recall it was not too long after SSC had folded, and Martha listens to me and she said, "Artie, that's a really good idea, but you have to organize all of the facility directors." So I go back and I start to organize all the facility directors, and we've never met before, and we put together a $50 million package.

ZIERLER: As a proposal, you mean?

BIENENSTOCK: As a proposal. Well, we put it together as a package. We didn't have it quite as a proposal yet, but we had a package of things that we could do for $50 million. Some operating money, but some upgrading of the facilities. And for some reason, I was going to OMB to meet with our examiner. Emily Pelton was her name. A young woman at the time, very capable. And I go to OMB, and before I could get to her office, I'm stopped in the hallway by her boss, Gary Benethum, and Gary said to me, "Artie, you know, I'm worried about the operating budgets of all the DOE facilities." He said, "The high-energy physics labs, they're so expensive to build, and then we only operate them part-time because of the operating budgets." And I turned to him and I said, "Gary, have I got an idea for you!"

ZIERLER: Oh, boy.

BIENENSTOCK: And by the time we got to Emily's office, $50 million had gone to $100 million. So I had OMB on my side. I had never met OSTP at all. I go back and tell Burt about this, and Burt's really pleased, and we decide the two of us are going to work on this together. So this is '94. And we get all the labs involved, and then OSTP' associate director for science, M.R.C. Greenwood, gets involved. And she said I have to go to Congress. And I don't go alone; I bring other lab directors, and we go to Congress. And we sell this on the idea that—it was, as I recall, a Republican Congress at the time—you know, "This doesn't make sense. You make this big capital investment. You want to use it effectively, and the operating budget should be commensurate with the capital investment."

ZIERLER: So Artie, how are you making that point that it's not being used effectively? How do you make that argument?

BIENENSTOCK: We had statistics. I had statistics from all the labs. I had details on what each lab would do with money that it got. So we had a detailed plan by that time. Not that we parceled out individual—I thought it wasn't our place to parcel the money. That was DOE's place. Mine was to raise the money. But we had to have substance to show to Congress and to OSTP. And we went to Congress, and it passed! So that was that. But that gives you a sense of Burt and I had a working relationship by that time where we worked together. The next thing that really mattered was the x-ray free electron laser. Because there was enormous skepticism within the synchrotron radiation community, but Burt was really excited about that.

ZIERLER: What was the skepticism?

BIENENSTOCK: Well, everyone was wedded to circular rings for synchrotron radiation, and the free electron laser concept was unproven. We didn't even know if the concept would work at high energies. The SASE concept. So even within SSRL, there was disagreement. Should we upgrade SSRL to get lower emittance, or should we build the free electron laser? And I said, "We'll do both."

ZIERLER: [laugh] What were the budgetary implications of you saying, "We'll do both"? Is that twice as expensive, essentially?

BIENENSTOCK: Oh, yeah. Oh, much more than twice as expensive!

ZIERLER: Much more—OK, OK.

BIENENSTOCK: And Burt was behind me. Burt was there, even helping me with my own staff. And what's more, we saw that the same sort of accelerator studies that you would perform to get a free electron laser functioning were also valuable for Next Linear Collider. So Burt could

devote some accelerator physics to those parts of the studies that were appropriate for both fields, and it became a sort of really close working relationship. I don't think we saw at the time that—or maybe we did—that SLAC wasn't big enough for the next high energy physics accelerator, although we still felt, and I felt, that SLAC should build the next linear collider out—with Livermore, out in the desert someplace. So I believed in that strongly, probably still do. So that gives you a sense of the working relation. And you know, it has been marvelous. By coming into SLAC, we had as colleagues and coworkers people with enormous skills in accelerators, in particular. I don't think we could have pulled off the free electron laser if we didn't have the skills that had been built up for the SLC. So the two were intimately related. By that time, I understood that, and I was as much a proponent for high-energy physics particularly accelerator physics as I was for synchrotron radiation.

ZIERLER: Now obviously this work directly led into you going on sabbatical and being the associate director at OSTP in '97.

BIENENSTOCK: That's right. There was one interim phase where I had to organize the community again. I got a call from OSTP—I can't say from whom—saying that the budget was threatened, and everything that we had achieved in '94 might be turned around. So I had to reorganize the user community, and we got that turned around. So by that time, I was really a known quantity at OSTP.

ZIERLER: And was one of your motivations in taking this sabbatical—was that you could advance your work at Stanford from the inside, so to speak? Was that part of it?

BIENENSTOCK: No. Let me say—I had been director of SSRL for 19 years. I had decided already that I was going to step down as SSRL director. That was the longest I had done

anything in my life, except being married! And I thought, "I will go back to teaching, research and skiing." I had a sabbatical due to me. I wanted to ski. I had a house up in the mountains. So I wanted to do that. And so I had already planned to leave. Now, it wasn't a sabbatical; it was a leave of absence. Because it ended up being three and a quarter years. But my motivation was twofold. When you looked at the Clinton first five years, there had been considerable increases for NIH but not for the rest of science. And I wanted to turn it around. It wasn't just for SSRL and Stanford; it was for the whole field of all the other sciences, including the social sciences. Remember, I said way back, my interest in sociology? Well, I wanted to help them all. So it wasn't just Stanford by any means. The other was I wanted to—it was an opportunity to do something completely new. And I liked that opportunity.

ZIERLER: Was there an allure to being in the White House, to working at that level of government?

BIENENSTOCK: I don't think I thought of it as that so much as the opportunity to fight for budgets. That was probably my primary goal.

ZIERLER: In terms of managing the flow of information in OSTP, what were—and obviously you were working on things besides your own parochial interests at Stanford, right? You were working on matters of national science policy. What kinds of information were coming in to your office, and what was your role in moving them up the chain? And where did they go from your office?

BIENENSTOCK: So first of all, immediately—my immediate predecessor in the job was Ernie Moniz, and Ernie had assembled a really good staff. So I had a staff that could cover the social sciences, the biomedical sciences, and the physical sciences, of very high quality. I

inherited that. And I inherited two very important projects. One was on the government-university partnership, and the other one was on research misconduct. The different agencies had different definitions of research misconduct, and they had to be harmonized. Ernie had started those, and I inherited that. The other thing that was very important was that Duncan Moore came in with me as associate director for technology. And I think it's no secret that in the first five years of the administration, there had been tension between the technology division and the science division of OSTP. Duncan and I knew of each other, we respected each other, and even before we were confirmed, we worked together. And we brought our two divisions together. On top of that, we had Kerri-Ann Jones as head of the international division, and Rosina Bierbaum as head of the environment division, and the four of us became very close. We started really working as a team. And we reported first to Jack Gibbons, but he stayed only for a few months, and then Neal Lane came in. So by and large, I reported to Neal Lane, and Neal reported to the president and to the chief of staff. For the most part, that was John Podesta. However, as I said, I had a lot of interests. Remember affirmative action, minorities. There was a group working on education in the White House with whom OSTP had not really linked, but I got to know that group, and they realized that I was interested, among other things, in improving the situation of minorities and women partly via science and technology. So I met regularly with that group. Then I got to know some of the civil rights people directly. Clinton had a One America office, largely peopled by lawyers. And as time went on, I worked to convince them that a lot of the good jobs were in science and technology and it was important to increase the participation of women and minorities in science and technology. Somehow I got invited to the meetings on the H-1B quota, where Labor and Commerce would argue over what the quota should be. And there, I would attend those meetings, and I one day piped up, "No matter what you guys decide, it's not

going to help academia, because no matter what you decide, by the end of the calendar year, all the visas are going to be taken up, and we do our hiring in the spring." Gene Sperling listened to me, and the next meeting, he came back with the idea of a cutout for academia. So I was interacting around the White House with different people, not reporting to them so much as just working with them, and working with the agencies. Became very close to Harold Varmus, who was head of NIH. Harold lived four blocks from me. I was with Duncan, a strong advocate for increasing the budgets of the DOE Office of Science and the National Science Foundation. And I argued that even if you were only concerned about health, so many of the diagnostic and therapeutic procedures have their basis in the physical sciences and engineering. Harold agreed. So we worked together on that. We would meet often, not too often but regularly, at a bakery on Connecticut Avenue in Washington, Firehook Bakery. Sometimes Rita Colwell would join us and discuss policy. And I would use my experience in synchrotron radiation and x-rays in my talks. Harold really followed the same line using magnetic resonance imaging as the basis of his talk. And we developed a close relationship there. Similarly Mike Smith was deputy at the Department of Education, and we had a big initiative on research in education and worked with Mike. So we worked around the White House but also with the agencies.

ZIERLER: Did you ever think that you would stay in government, or was the plan always to return back to Stanford?

BIENENSTOCK: The plan was always to return back to Stanford, and that had an enormous advantage, and that is, I was never looking at what my next job would be, in government.

ZIERLER: Right. So you were probably a little more fearless than you otherwise would have been.

BIENENSTOCK: Exactly! And it's sort of interesting because when we arrived, Duncan and I arrived, Bill Clinton was envisioning the 21st century as the century of the biomedical sciences. And Duncan and I, I think, linked with Tom Kalil and others—by the time the first budget appeared in January or February—I can't remember which—Bill was talking about the interdependency of the sciences, and he proposed a big increase for not only NIH but DOE and NSF. So we influenced the thinking in the White House. And I think that was in a sense—both Duncan and I, we were fearless. We both knew we had tenured positions to go back to at the end of the administration.

ZIERLER: So was the 2001 end date, was that just a function of the end of the Clinton administration?

BIENENSTOCK: Quite definitely.

ZIERLER: And was that self-consciously on your part because you saw where science policy was heading under the W. Bush administration?

BIENENSTOCK: No. I never intended to stay. My wife didn't come with me to Washington. Two things happened. One, she was on a path to become chair of the board of the American Lung Association of Northern California. Given the history of a daughter with cystic fibrosis, that was important to the two of us. And the second thing is the day after I was confirmed, our first grandchild was born in California. So she stayed behind until the last six months and then came to Washington, but our intent was always to go back at the end of the Clinton administration. I don't know what would have happened had Gore been elected, because to be honest, as much as I have really enjoyed Stanford, OSTP was the most exciting job I ever had.

ZIERLER: Really!

BIENENSTOCK: Yeah.

ZIERLER: So you're suggesting that possibly in the event of a Gore presidency, maybe you would have stayed on?

BIENENSTOCK: That's correct. I mean, certainly, Roz came to Washington to work for Gore. She was down there at DNC very often, working away as a volunteer. I might have stayed on, and indeed have worked on science and technology committees in the campaigns ever since. But when Jack Marburgerwas head of OSTP in the Bush administration I went in regularly to meet with him—I have strong loyalties to OSTP no matter who's there, as I do now with Kelvin there, and Michael Kratsios. I retain strong.

ZIERLER: What do you see as your legacy at OSTP?

BIENENSTOCK: First of all, the research-government partnership, that was improved markedly. It ended—well, in the interim, I opposed a heavy dependence on cost-sharing because cost-sharing was undermining peer review. When people start to bid with money rather than scientific expertise, that undermines peer review. And indeed, Rita heard it, and others, and cost-sharing got cut back markedly. Second, resolving issues related to indirect costs, although I never brought that to satisfaction. Unreimbursed, reimbursed indirect costs still cost universities a great deal. Money that could be used for financial aid and other matters. But the things we achieved were marked and are still recognized as such. Harmonizing research misconduct—in retrospect, I think it was one of the most important things that we did. There, the lead person was Sybil Francis. Sybil went on to be the wife of Michael Crow at Arizona State. She did a superb

job there. She was heavily involved, while she was there, with the research-government, university partnership, and then Anne-Marie Mazza took it over. Anne-Marie Mazza is playing a major role at the National Academies as a staff member. Let's see. A funny thing that I couldn't deal with directly because I had a conflict of interest—when I got there, DOE came to me and said, "Artie, we are funding the synchrotron radiation facilities. A third of our users are biomedical people funded for the most part by NIH. Our budgets have been flat while NIH's budget has been going up steadily and markedly. It's not fair."

ZIERLER: [laugh]

BIENENSTOCK: Marvin Cassman, Director of the National Institute of General Medical Sciences at NIH came to me and said, "Artie, when the genome is mapped, we are going to want to do about 10,000 protein structures." The only way that would be possible is with synchrotron radiation. DOE and NSF run these facilities. We have no say in their operation, and yet we're critically dependent on them.

ZIERLER: Can you explain the science behind synchrotron radiation and its relevance to gene mapping?

BIENENSTOCK: Yes. The relevance is to the determination of protein structures so you understand the function of the proteins. Synchrotron radiation, you produce x-rays—well, at that time, they were about ten million times as intense as an x-ray tube. So it meant you could do protein structures with a speed and accuracy that you just couldn't do with an x-ray tube. So they wanted to solve these protein structures. Does that help?

ZIERLER: Yeah, yeah.

BIENENSTOCK: So I set up a committee that was run by Bob Marianelli. Bob Marianelli had been at DOE, and he had been my program officer when I was SSRL director. I came to, while I was SSRL director, respect him enormously for both his fairness and his understanding of science. So he came in as assistant director for the physical sciences. He ran this committee. I had to stay out of it completely because I expected to go back to SSRL. They came up with a solution that satisfied everyone. NIH contributed to the upgrading of both SSRL and the National Synchrotron Light Source at Brookhaven, as well as to the funding of new beam lines and the funding of an operation of beam lines at the various synchrotron radiation facilities. DOE and NSF retained responsibility for the basic operation of their facilities. I liked that very much. Oh—we got the Clinton administration to focus on women and minorities in science and technology. With the One America office, we got 25 companies to pledge a million dollars a year for ten years, each, to encourage women and minorities in science and technology, and indeed that program went on. One of them went bankrupt in 2008. But I think that focus on women and minorities was appropriate in science and technology, and I still believe in it. And again, just wandering the halls of what was then the Old Executive Office Building—and meeting the people in One America, and getting to work with them—so much of this is just informal. But in a White House that is so committed to the advancement of women and minorities, when a scientist comes in who clearly has that high on the agenda, that helps an awful lot to build relationships. One of the assignments I got from TJ Glauthier when he was at OMB was to broker the environmental standards of Yucca Mountain, and between EPA and the Department of Energy. And that was fascinating. I loved doing that.

ZIERLER: Was Harry Reid involved in any of that?

BIENENSTOCK: Indirectly. You know, we came out with good standards that weren't enacted in our administration, and I would say the Bush administration improved on them. Bush came out with what I would have wanted but couldn't have achieved in my administration, although I was satisfied with what we achieved. Clearly, Harry Reid wasn't going to allow a repository at Yucca Mountain. Me, I think long-term. Let me get back to women and minorities. What got me going on women and minorities was the following question. When I arrived at the White House, there had been court decisions and referenda against affirmative action. And here I had spent a lot of my life on affirmative action. So I asked the question, what would happen to the science and technology workforce if we failed to increase the participation of women and minorities in science and technology and the nation went through the demographic changes that we anticipate for the next 50 years? The Bureau of the Census was anticipating that the country would become increasingly Hispanic. The Hispanic and the African American community had lower participation in science and technology than white males or white females. And the one group that had much greater participation were Asian Americans. So we just examined that. We just took the numbers. What were their participation rates then? Suppose it stayed the same for each group and yet the demographics of the country changed. What we showed was that the fraction of the workforce involved with science and technology would decrease rather than increase, as it has since after the Civil War, rather steadily, if that were the case. We had to both increase the participation of women and minorities and also keep the country really attractive for immigration. And I think successive administrations have almost followed through, although recently there have been other priorities.

ZIERLER: Yeah, yeah.

BIENENSTOCK: There were other things that I was pleased with. I mean, it went on and on. We had this initiative based on a PCAST study, of doing large-scale studies of new techniques in education. Mike Smith over at Ed was enthusiastic about it, but the Department of Education didn't feel that it had the capabilities to assess the proposals to the same degree that NSF had. And neither agency felt it had he competence to manage large-scale studies of this sort that NIH had. So we put together an interagency initiative involving all three agencies. My memory is the initial budget was something like 15 or 20 million dollars. After we put it together, I went to a party where Bill Frist was getting an award from the American Chemical Society. Bill had put out a report where the first half of the report was strongly in favor of educational options that I opposed—vouchers and things of that sort. But the second part called for a study just like what we were proposing. So I was at this meeting and I met Elizabeth Prostic, a young woman who had a very big effect on me—who had led me through confirmation. She was on John McCain's staff. I told her I had read Bill's report and I disagreed completely with the first half, but we had an initiative that was going to come out from the president that was just like the second half. So she said, "You have to tell the senator." And she sort of grabbed me by the arm and pulled me over to meet Bill Frist. Now, I knew of Bill Frist because he had worked as a medical student or a young doctor with Norm Shumway at Stanford, a famous cardiovascular surgeon. And Norm was a friend. So first we talked about Norm, and then I just said, frankly, "I read this report of yours. Don't agree with the first half, but the second half, we've got this initiative coming out that's just what you are calling for, and I'd really like your support." And he said to me, "Two different staff members wrote that report. Why don't you work with the second one, and we will get that thing funded?" And Bill remained through my three years someone with whom I liked to work. The last year the project, the initiative was up to $50 million in the president's budget. The

appropriations committee cut it back by $10 million. I called Bill's office, and Bill introduced an amendment on the floor when the appropriation reached the Senate floor, and we got our ten million back. So it was a nice period. When I look back Bill was actually the closest colleague I had in the Senate. My closest colleague in the house was Vern Ehlers, another Republican. It was that kind of era. Vern was wonderful—

ZIERLER: That sounds like a very long time ago.

BIENENSTOCK: It was a long time ago.

ZIERLER: [laugh]

BIENENSTOCK: I liked and admired both of them very much. But Jack Marburger and I stayed friends when he was head of OSTP, and sometimes we would appear on the same stage. I was APS president, and he would be speaking in support of President George W. Bush's budget. He had to be. I would be urging more money for the physical sciences and engineering than the President had proposed. And then the two of us would go out for dinner together, even though it looked like we were speaking in different ways. I feel the same way now about Kelvin Droegemeier and Michael Kratsios at OSTP. I may disagree with them from time to time, but I really admire and respect them, like them very much.

ZIERLER: Now when you got back to Stanford and you became—there was an interlude of a year, it looks like, before you became director of the Geballe Laboratory.

BIENENSTOCK: Right.

ZIERLER: Were you in touch with Ted when you were at OSTP, that this was something that you were going to do? Or this was put together by the time you already got back to Stanford?

BIENENSTOCK: I don't think I stayed in touch with Ted. You know, Ted and I had been good friends. We came together to Stanford. We shared two positions.

ZIERLER: Yeah, but he's got, what, 15 years on you?

BIENENSTOCK: Yes, but we came at the same time, and we conferred even before the two of us went to Stanford. He was two thirds in applied physics, one third in materials science. I was two thirds in materials science, one third in applied physics. We were very friendly throughout the period. I think while I was at OSTP, I focused on OSTP. I couldn't get involved with a conflict of interest at all. I mean, I couldn't even accept a dinner from Stan Ovshinsky. It was that clear. I did go to dinner with him, but I paid for it myself. Which bothered him, but that was the way it was. When I came back, I said, "I don't want an administrative position."

ZIERLER: That's enough administration, right? [laugh]

BIENENSTOCK: Right. So the first year, I had nothing.

ZIERLER: You just went back to teaching?

BIENENSTOCK: No, I think I had a sabbatical the first six months. Then I went back to teaching. And then, the second year, I didn't become head of the Geballe Laboratory. They made me head of the Materials Science Advisory Board, I think it was called. And there we made several recommendations that as I recall we should set up a nanocharacterization facility and a

nanofabrication facility. I may be wrong about nanofab, but I remember nanocharacterization very well. And then, the third year, that—sometime in the second year, Sandy Fetter who had been Geballe Lab director, wanted to step down. Sandy lived across the street from me. We had been friends ever since I had been at Stanford, good friends. And I took over the Geballe Lab with the request that Sandy agreed that he would stay on as associate director, thinking I needed his experience. And we are in many ways complementary in the way we approach things. So I took that over for a year, and that was in a sense, a tumultuous year, because the Department of Energy, it was funding SLAC, and a lot of materials research at SLAC, and separately, through campus, a lot of materials research on campus. It decided that it wanted to fund all of the materials research through the SLAC contract. Now that changed everything. You can't imagine how big an effect that was, because of two things. One, it meant that rather than complying with A-21, which governs reimbursement and all sorts of things by the government, we were under a SLAC operating contract which has different rules. And that meant setting up different administrative procedures. So at Geballe Laboratory, we would have two different sets of administrative processes depending on who was funding the research. Second, it opened up opportunities, because with an operating contract, you can change the funding upward, easily, and that meant opportunities for our faculty. And those proved to be important. So it was a period of adjustment. Then I was made vice provost and dean of research and graduate policy.

ZIERLER: So a one-year directorship at the Geballe Laboratory—that's a relatively short tenure. Were you thinking of staying on longer, or did you really only sign up for a year?

BIENENSTOCK: I was thinking of staying on longer, but then the dean of research—it came along. By that time, I was an unusual beast in the sense that I had experience as a faculty member. I had experience running an interdisciplinary laboratory. I had experience running a

DOE laboratory and a national facility. And I had the government experience. So it was sort of natural that I go into the Dean of Research office.

ZIERLER: And was this an opportunity for you to sort of, at a higher level, see how well some of the things that you had done at OSTP were playing out at the university level?

BIENENSTOCK: Yes. And it also meant that I could go back to functioning officially in the policy realm.

ZIERLER: You really caught the policy bug.

BIENENSTOCK: I did. And also the sense—I saw the quality of the young faculty that Stanford had hired, and I thought, the best contribution I could make is to ensure that they had good situations to perform their research. That was a greater contribution to science than I personally could make at that point.

ZIERLER: And when you became the special assistant to the president for federal research policy, was that a very natural transition? Was that really essentially a continuation of the work you were doing as vice provost?

BIENENSTOCK: Yes, exactly. What happened is I got elected to the American Physical Society presidential line and I realized I just couldn't possibly do both. And to be honest, I was exhausted. I was trying to do too many things as vice provost and dean. And as I said, they split it up into two and a half jobs when I stepped down. But I was truly exhausted from the job, and I wanted to keep the policy part. I had very good relations with our government affairs people. And what's more, we shared common values, and therefore I liked working with them very much.

ZIERLER: And has your—are you special assistant—I don't have in front of me the administration, but how many presidents have you served?

BIENENSTOCK: Just Hennessey and Marc Tessier-Lavigne. But in reality, I worked much more closely with our government affairs people, and I meet with the president when need be. And we have a very good head of government affairs, Ryan Adesnik, and he has very strong fundamental values that I respect. You know, Stanford will not use its government affairs office to raise money for specific projects. We lobby on principle. We may lobby for the Department of Energy's budget and things of that sort, but not for—we won't go to Washington for an individual project.

ZIERLER: So this has been a pretty long—you've been in this position for quite a while. Over the past 14 years, what have been—I guess particularly with the Trump administration, what are some of the trends that have changed over the course during your tenure, in federal research policy?

BIENENSTOCK: Well, I've watched OSTP carefully, Harold Varmus and I worked closely together on stem cell policy. So I thought that stem cell policy was carefully thought out, and I believed in it. Then Bush reversed that, limiting the research to only existing stem cell lines. So that trend in stem cells has changed. I haven't followed it closely, to be honest. Budgets. We worked hard to get the Clinton budgets up and passed for the relevant agencies, all the relevant agencies. I think Jack Marburger had to work harder to achieve that. In this administration, President Trump has proposed decreases for the relevant agencies. Fortunately, Congress has not gone along with that. That's another trend. The trend that worries me the most is an increasing—let me step back again. One of the things I've really come to respect is President Reagan's

National Security Decision Directive 189. That directive distinguishes between openly publishable fundamental research on which no restrictions of collaborators or who can work on it can be made, and classified research. It says the only restriction is, if you've got to put restrictions on, is classification. And remember that Condi Rice reaffirmed NSDD 189 either when she was national security director or when she was secretary of state; I forget which. At any rate, the virtue of that is that you have all the benefits of openness and free interchange of ideas that benefit science so much and the advancement of science. But the other benefit of it is that when you want to secure stuff, you carefully vet the people involved before you give them clearance, and by and large, you perform that research behind gates. Now people have wanted to muddy the waters and perform what I'll label sensitive but unclassified research at universities. And I think that's a potential disaster. Either you lock down laboratories so you lose the advantages of openness in research, or you will fail to keep control of the things that you really want to control. I worry about that more than any other single thing.

ZIERLER: If you look at the headlines over the past three years under the Trump administration, it certainly seems that science is under attack in a way that it never has before. But I wonder from your vantage point if that's underblown or overblown or that the news media basically has that story right?

BIENENSTOCK: I think that certainly in dealing with the pandemic—the science advice was not taken into account in the way it should have been in the early days when we had lots of warning and things probably would have been quite different. A lot of people would not have lost their lives, and we wouldn't be in this economic situation had we been more aggressive at the very beginning. And in that sense, most of the administration is not taking science into account in policy in the way I believe it should. On the other hand, when I work with Kelvin

Droegemeier or Mike Kratsios or the staff at OSTP, I see them committed to providing the administration with the best scientific advice that they can, and as advocates for good science.

ZIERLER: What is the Wallenberg Research Link? What is that?

BIENENSTOCK: The Wallenberg Research Link is funded by a couple of Wallenberg foundations in Sweden.

ZIERLER: This is Raul Wallenberg, or a different Wallenberg?

BIENENSTOCK: His family. Not Raul himself, but that extended family. These foundations are the primary private funders of research by far in Sweden, and the third-largest private funder of research in Europe. And when you consider the small population of Sweden compared to the rest of Europe, that's a significant thing. I have to say I got involved first because I had been a close friend of the person who started it, a man named Stig Hagstrom, who was both a Stanford faculty member and a major figure in Swedish higher education. We had been friends since he was a postdoc at MIT in 1964. And as vice provost and dean of research, I oversaw the program, and then when I stepped down, they asked me to help out because he had Parkinson's and they needed help. He needed help. The program initially funded joint research between Stanford faculty and Swedish faculty on technology in education. As the years went on, it broadened it to other projects—projects in the digital humanities, and projects in what I'll label applied neurosciences. So these were broad collaborations between Stanford faculty and Swedish faculty and students and postdocs and all that. When Stig passed away, they instituted four postdoctoral fellowships at Stanford to be held by people who got their doctorates at Swedish universities, at Stanford. And they were in the area of energy, synchrotron radiation, and things of that sort. The foundation came to see that that might be the most valuable contribution that they could make.

So now, the program is dominated by the postdoc program. We select ten postdocs a year to come to Stanford, and they're selected by a committee at Stanford, and they're broadly based in the university. Since they come for two years to Stanford, we typically have about 20 postdocs. Now in addition, the collaborations continue between Swedish faculty and Stanford faculty. And what's more, when the postdocs go back to Sweden, they often continue to collaborate, and the foundation helps those collaborations as well. The goal of the foundations in supporting this program is to improve Swedish higher education and research. Over the past couple of years, they've established a similar program at MIT. And I can't help but remarking that Maria Zuber, my colleague on the National Science Board, is the vice president for research there, overseeing that program. So we will definitely collaborate with them. And they've also established a program in Singapore. Those programs are smaller than the Stanford program. I look forward, when things ease up with the pandemic, to collaborating with people in Singapore as well, so that we share experiences and best practices and the like. But that gives you a gist of it.

ZIERLER: Yeah. Well, Artie, I think at this point in our conversation, I want to switch a little bit to sort of broader questions to ask you to assess sort of your career as a whole. And I think the first and most obvious question is, you have this remarkably unique interplay between your legacy in physics as a scientist, but also your legacy in policy as an educator and as an academic. So in thinking about your contributions, do you see them as two sides of the same coin, or are these two separate tracks, in terms of the contributions you've made in physics and the contributions you've made in science policy?

BIENENSTOCK: I see them really entwined. I don't think I could have made the contributions in science policy had I not previously both been a faculty scientist, a leader of a national laboratory, but also I consulted for Stan Ovshinsky for 40 years, and got a sense of what

a small to medium business based on technology is like. So all those things contributed. But also, I wanted to be an educator, and the social justice sense—I was brought up on that. I mean, that was dinnertime conversation. So to me, they're all mixed together, with part of being a human being. I'm an honorary fellow of the Stanford Humanities Center. I think it's all part of living and enjoying life.

ZIERLER: What are your proudest moments in both your contributions to science and science policy?

BIENENSTOCK: I think the best thing I did was the interplay between synchrotron radiation and the structure of amorphous materials. There, scientifically, my students really contributed to the development of new techniques that allow us to determine atomic arrangements in amorphous materials in ways that were just not possible before. And I must say it was largely my students. That's scientifically. Policy wise, [pause]—

ZIERLER: One theme, if I could just help you along—clearly, you gave opportunity to countless students who otherwise would have never had the opportunities if not for your commitments to their cause.

BIENENSTOCK: I don't know if I can claim that. I had such good students myself that they would have thrived no matter what.

ZIERLER: No, no. I mean your interest in promoting underrepresented groups.

BIENENSTOCK: Yes, yes. I agree there. When I look at my colleagues at Stanford, those who—women and African Americans and Hispanics—boy, they have contributed so much. And you get a sense of it. I mean, Condi was provost. Several of them have been vice provosts and

deans. They have taken leadership positions. And what's more, by taking leadership positions, they've not only inspired the women and minority students at Stanford, but they've gained the respect of white male students as well. So I like that part. The constant support for and advocacy of fundamental research and scholarship across a very broad range of endeavors, from the physical sciences to the social sciences to the humanities and to the arts—I've just—it has manifested itself many different ways, but that's where I've made my contribution.

ZIERLER: What do you see as the future for science policy in education? Where are trends headed?

BIENENSTOCK: I think a lot depends on the next election. I wonder if I really want to go into that. I think it's vital that the nation have policy that is well informed by science. But I always recognize that in making policy decisions, you have to take into account more than science, and that a good politician has to balance many competing goals and values in a diverse democracy. And a good politician thinks carefully about that balance. So science is just one part of it. I often say that in some ways I learn more about humanity by reading novels than other ways.

ZIERLER: [laugh]

BIENENSTOCK: So I have a very broad respect for good novels that teach me things. So it's the whole spectrum of trying to understand this universe and the people in it, and then using it to enhance the world.

ZIERLER: Well, Artie, we're at the three-hour mark, and I think for my last question, I want to ask you something that is sort of personal and also forward-looking, and that is—you

are clearly—you could be retired and disengaged from everything right now if you want to be, and yet you're so active on so many different fronts. I guess my question is, what are your motivations to stay active? Why do you still want to be engaged? What are you working towards for the future? And how are those commitments representative of all the things you've been working for throughout your career?

BIENENSTOCK: A large part—I have three areas of focus. I've described the Wallenberg Research Link. On the National Science Board, I see myself, with my colleagues, as an advocate for the broader participation in science in technology at every level and always as an advocate for the Science Foundation funding basic research, fundamental research, in a context where we have outstanding mission agencies also funding research. America is unique in that sense. And the Foundation benefits from the existence of the other agencies funding research. Then, in this world, I spend a lot of my time on international scientific cooperation. That's why I'm co-chairing this study for the American Academy of Arts and Sciences. It's vital that this nation take into account the fact that it now only funds about a quarter of the world's R&D, that we have bright, capable colleagues around the world, and that we can learn from them, and that we can accomplish more for our nation and for the world by cooperating than by simply nationalistic competition.

ZIERLER: Well, Dr. Bienenstock, it has been an absolute delight spending time with you today. I really appreciate your perspective on so many aspects of not just your career, but really modern American history. It has been a real privilege to hear your perspective on all of these things. This oral history will be a tremendous record for researchers for a long time to come. And I know the folks at SLAC are going to be quite happy that we were able to do this. So I really appreciate your time today.

BIENENSTOCK: Well, thank you for doing it.

[End]