Interviewee: Nan Phinney By: David Zierler

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ZIERLER: This is David Zierler, oral historian for the American Institute of Physics. It is July 16, 2020. I am so happy to be here with Dr. Nan Phinney. Nan, thank you so much for being with me today.

PHINNEY: Hi.

ZIERLER: Alright. So, to start, would you please tell me your title and institutional affiliation?

PHINNEY: Well, my last job title at SLAC was a Distinguished Staff Scientist, and I retired two or three years ago.

ZIERLER: And was your only appointment at SLAC, or did you have a joint appointment with Stanford in general?

PHINNEY: No. I was only a SLAC employee.

ZIERLER: Okay. So, let's start right at the beginning. Let me first ask you some questions about your parents. Let me know first — where are your parents from?

PHINNEY: My family was all from Chicago, and my parents both grew up in Chicago. My father lived in California briefly, I guess, around his early teens. But they lived in Chicago. Their parents were Chicagoans. And actually, an interesting thing about my family is I'm actually 11th generation American on both sides.

ZIERLER: Wow!

PHINNEY: The Phinney side of the family came over on one of the boats after the Mayflower, and of course, they married lots of other immigrants as time went on. So, it's not a strict [laughs] line by any means. The other side of my family is actually even more interesting. My mother's father was George R. Lawrence, who was a famous inventor and photographer. And he actually was often called the "father of flash photography." He took the famous pictures of the San Francisco earthquake from the kites.

ZIERLER: Oh, yeah. Yeah. I know those pictures. Did you get to know him?

PHINNEY: No. He was — my mother was the youngest daughter of his second marriage, and her mother was much younger, more than 30 years younger than he was. So, George died when my mother was 16, and long before she had thought of me.

ZIERLER: Yeah. Where did your parents meet?

PHINNEY: I don't actually know. Probably through friends.

ZIERLER: In Chicago, obviously.

PHINNEY: Yes. They were both in Chicago.

ZIERLER: And what were your parents' professions, and what level of education did they attain?

PHINNEY: Well, my father was not in the war, because he had a medical deferment. And so, he was at home, and my parents married very young. I think my mother was 19, and my father 20 or 21. And they would have gone to college, but after the Depression, they didn't have the money. So, my father did a lot of self-education with night school, and I think my mother had one semester of college, but that was really all she could afford. And then they had to go work.

ZIERLER: And what was their job? What did they do?

PHINNEY: Well, my mother was a homemaker, and my father was an electrical engineer who eventually went into sales. Interestingly enough, his mother actually had a master's degree, I think, in literature from Northwestern University. So, there was a tradition of education, even though when they became adults, they just simply didn't have the finances to go to school

ZIERLER: Right. But your sense is, is that by intellect and interest, they certainly would have pursued higher degrees if they had the opportunity.

PHINNEY: Yes. Yes, I think so.

ZIERLER: What level of technical education did your father have in engineering, or was he mostly self-taught?

PHINNEY: No, no. He went to night school for most of my childhood. But I don't think he ever really formally completed a degree. But it would have been sort of equivalent to a bachelor's degree.

ZIERLER: And did he involve you in engineering issues? Did he share with you aspects of his job? Was that fascinating to you as a girl growing up?

PHINNEY: Not — it's interesting. I have an older brother who's just a year and a half older than I am, so we were raised almost like twins. And sometimes people ask me when I decided to go to grad school, and my answer would be, "I don't know, 3? 4? 5?" [laughs] In my family, it was assumed that my older brother and I were going to grad school, and I never questioned it. It was always — it was just a total assumption from day 1.

ZIERLER: And what neighborhood did you grow up in?

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PHINNEY: I grew up on the north side of Chicago, right on the edge of a neighborhood called Rogers Park. I don't know what the neighborhood I was in was called— I'm sure it has some name, but we never used it. And Rogers Park was a largely Jewish area, and I grew up in a largely Catholic area. My family was Catholic. And very close to the lake, about a mile from the lake, so I used to walk over to the lake a lot as a child.

ZIERLER: And did you go to public school or private school?

PHINNEY: I went to Catholic school. I went to Catholic elementary school. We used to call it grammar school then. And Catholic girls' high school, and neither of those two educational experiences were particularly outstanding.

ZIERLER: And for high school?

PHINNEY: Catholic girls' high school.

ZIERLER: How was the science and math education at your school?

PHINNEY: Poor.

ZIERLER: It was poor?

PHINNEY: Yes. It was also — okay, it's a long time back, but it also was a school where not a large fraction of the graduates even went on to college.

ZIERLER: And was it poor in the sense that concepts, such as evolution or a…

PHINNEY: No.

ZIERLER: …14 billion year old universe was not taught, or it was just low level?

PHINNEY: There weren't all that many people interested in science, and so the science teaching was not — I would not say it was first rate.

ZIERLER: So, did you have, in terms of an interest in pursuing a career in science — did you have two strikes against you, so to speak, in terms of the quality of your science education and being a woman, would you say?

PHINNEY: Well, I'd like to come back to the second question separately, but no. Somehow, I was of the Sputnik generation. I was very good in math. And so, it was assumed I was going to graduate school, and it was assumed I was going to be a scientist. What exactly I was going to study was not so obvious, and in fact, I had an uncle who was a chemistry professor in Ohio, who was very influential to me. And so, when I first started college, where I went to Michigan State in the honors college, I started studying chemistry, probably under the influence of my uncle, who I admired a lot. But somewhere around my sophomore year, I decided I hated it, and so I switched to physics. Now, the question about being a woman — I don't want to tell you how many middle-aged white men felt compelled to explain to me why I shouldn't be a physicist.

ZIERLER: Yeah.

PHINNEY: But they were many. [laughs]

ZIERLER: In high school and in college?

PHINNEY: No. In high school, they were all — it was a Catholic girls' high school. I was taught by nuns. So, there weren't any men there, but by the time I got to college, and certainly by the time I went to grad school.

ZIERLER: What colleges did you apply to?

PHINNEY: Oh, my God. I have no idea. A lot. My older brother was just a year ahead of me in school, and he had gone to Notre Dame. He didn't have a scholarship. So, I applied all over the place trying to make sure that I had a scholarship to go to college. And I ended up with lots of choices. But in the end, I took a scholarship from Jewel Tea Company, which is like Kroger or A&P. It's a grocery store chain where I had worked. And interestingly enough, that scholarship was $1,500 a year, and it paid tuition, room, board, and books.

Now, when you got to Michigan State, was the plan to do physics, or that came later on?

PHINNEY: No. I mentioned that already. I started in chemistry, and then when I hit organic chemistry, I hated it. And so, I decided to switch to physics. And that was the period when particle physics was really kind of exciting. They were starting — accelerators were finding all these new particles. People were starting — they had just discovered that parity was actually violated. There was the proposal that the proton and neutron were made of quarks. And so, it was extremely exciting, and that's why I gravitated towards

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particle physics.

ZIERLER: And so, in terms of developing this unfortunate narrative of all of these men who told you you had no business — when did that start? Did that start even when you thought about transferring from chemistry to physics?

PHINNEY: No, it started probably when I started looking for grad schools.

ZIERLER: Okay. How did you do as a physics undergraduate?

PHINNEY: Fine.

ZIERLER: And what kind of physics did you gravitate towards.

PHINNEY: Particle physics.

ZIERLER: Because of all the exciting stuff that was going on?

PHINNEY: Frankly, I think if I were a student today, I would not go into particle physics. But I was very lucky. I was in the heyday of particle physics.

ZIERLER: Well, sure. Right.

PHINNEY: And my career was fulfilling. And lots of exciting, fun things happened.

ZIERLER: And were there any professors that became good mentors to you as an undergraduate?

PHINNEY: Not that I would single out, particularly. Not physics professors.

ZIERLER: Were there professors who discouraged you from pursuing graduate school?

PHINNEY: Well, like I said, these men would say, you know a woman can't really do science. Or, do you really think a woman can do science? Or, you won't be able to be a scientist and have a family. Or, you know all this — I mean, you can imagine them all. [laughs]

ZIERLER: Yeah. Yeah. What year did you graduate MSU?

PHINNEY: 1966.

ZIERLER: Okay. So, this is a little bit before — not like it made a huge difference in the minds of many of these people — but this was before the women's rights movement sort of hit campuses later in the decade.

PHINNEY: That's correct. I was just ahead of that. So, I went from undergraduate school at Michigan State to grad school at Stony Brook. And it was a great time to be at Stony Brook. It was the late '60s. There was lots of rock 'n roll, and drugs on campus. And by some miracle, actually, there was somebody at Stony Book who had some connection with Bill Graham. So, while I was a grad student at Stony Brook, I actually saw, live, Janis Joplin, Jimi Hendrix, Jim Morrison, Gracie Slick —

ZIERLER: Oh, man.

PHINNEY: All of these people, who all came and gave concerts at Stony Brook. It's amazing.

ZIERLER: Wow. Seriously. Now, was Brookhaven — was that part of the attraction at Stony Brook?

PHINNEY: It wasn't particularly when I first went, but in fact, I spent my last year of grad school — I got my degree in '72, and I spent most of the last half of '71 and all of '72 working at Brookhaven, because they had mainframe computers.

ZIERLER: Right. Who was your advisor at Stony Brook?

PHINNEY: Jack Smith. And Jack Smith actually was a student of Peter Higgs, so I'm a Peter Higgs grandchild.

ZIERLER: Yeah. And what was he working on during your time there?

PHINNEY: Well, my Ph.D. was in theoretical physics, and what I worked on with him was calculations of Z and W production for a proposed experiment at Fermilab, which was kind of ironic, because at my thesis defense, one of the faculty — Max Dresden — asked me if I wanted to see the Z discovered. And I remember at the time thinking: all I really want to do is [laughs] get my Ph.D. and get out of here. And then, ironically, I ended up spending almost 20 years of my life trying to make as many Z's as possible.

ZIERLER: Right. What did you want to do after you defended?

PHINNEY: Actually, I had gone to Europe for the first time in the summer of '72. And I just simply fell in love with it. And I wanted more than anything else to work at CERN. So, I found myself a position with École Polytechnique working at CERN.

ZIERLER: I heard from — Mary Gaillard shared with me that CERN was a very pleasant place for an American woman to work, because she felt much more equal there than she did in the States. Did you have a similar experience?

PHINNEY: Mary K. is a very, very good friend. I'm almost like her little sister. She kind of adopted me when I went to Geneva.

ZIERLER: Oh, really?

PHINNEY: Yes. And we've been very close friends ever since.

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We have actually had Thanksgiving together for more than 40 years.

ZIERLER: Really? Oh, wow. Oh, that's wonderful. So, was she both a mentor, or just mostly a friend when you were in Europe?

PHINNEY: Well, she was a friend, and somewhat really of an older sister. And when I took the job with École Polytechnique, I was actually working on an experiment at the PS doing data acquisition. But when the Psi was discovered at SLAC, we were all like, just crazy excited, and I remember working with Mary K. and some of the other theorists at CERN on calculations of what could it possibly be, things like that. So, we worked together just a little bit, but really my career path diverged from hers pretty quickly.

ZIERLER: Were there other women in your professional orbit who might have served as, I don't know, role models, mentors, people that you could turn to?

PHINNEY: No. Only Mary K.

ZIERLER: Only Mary K.

PHINNEY: There were very few women physicists at CERN and in those days, CERN did not treat them very well.

ZIERLER: Institutionally, you mean?

PHINNEY: Yes. In fact, the first woman physicist with a CERN staff position was Fabiola Gianotti, who is now the CERN director. That was long after Mary K had left. Back when Mary K. was there, there were a couple of other women physicists at CERN, but they all had husbands who had jobs, and it was assumed that they didn't need a job, because their husband had a job.

ZIERLER: And what about your experiences? Similar?

PHINNEY: Well, I wasn't married at the time, so [laughs] I didn't have a husband.

ZIERLER: No, but just in terms of how you were regarded generally.

PHINNEY: It's a kind of a difficult question. I'm pretty able to fend for myself, and I don't take a lot of crap from anyone. [laughs] And so, I think people backed off and let me alone.

ZIERLER: What were your research projects while you were there?

PHINNEY: Well, I started working on the hyperon experiment on the Proton Synchrotron, and then I fairly quickly switched over to working with the CERN, Columbia, Oxford, Rockefeller group, which was in IR-1 of the Intersecting Storage Rings. And that's where I worked. I worked primarily with that experiment the whole time I was there.

ZIERLER: And what were some of the big research questions that were motivating your work?

PHINNEY: That experiment was studying wide-angle scattering. It's infamous because we had a lot of background, [laughs] right around 3 GeV, and so the thresholds were set higher than the background, which was, of course, the Psi particle. And I was primarily doing data acquisition. I guess I had started doing a lot of computer programming in grad school, and so, that was mainly my focus. And of course, back then, we had these little tiny computers, PDP-11s, with 12 or 24 kilobytes of storage. Things that are unthinkable today.

ZIERLER: Did you have a set term there, or at a certain point, were you ready to move on and look for your next opportunity?

PHINNEY: After working for École Polytechnique, I had a number of short-term things, and then a research position at Oxford University for five years. And when I finished at Oxford, I went to South America for two months with a physicist friend who was finishing a postdoc at

SLAC. After that trip, I came back through California on my way back to CERN and stopped at SLAC. And I heard about this exciting new project to build the world's first ever linear collider. And I thought: wow, that really sounds like fun.

ZIERLER: Yeah.

PHINNEY: And of course, also, SLAC was, at that time — as you've heard from many people — one of our communications directors, Neil Calder, described it as the "shining lab on the hill that everyone wanted to go to." So, if there was anywhere — I mean, I loved living in Europe. I had a wonderful time there. But if there was anywhere I wanted to go back in the States, it was certainly SLAC. So, when the opportunity arose to work on the linear collider, I took it.

ZIERLER: Now, you thought that the linear collider was just more exciting — was that the basic draw?

PHINNEY: Yes. I guess another aspect of it for me was that CERN was getting ready to start the LEP experiments, and I know this sounds crazy, but they had 200 people on each of them, and I thought that was ridiculous and huge.

ZIERLER: Yeah.

PHINNEY: And so, I wanted to work on something with a smaller group of people The CCOR experiment I worked on had only about 25 people on it.

ZIERLER: Was part of the draw also — I mean, were you aware of Panofsky and Burt Richter? Were you aware of their reputation and their vision?

PHINNEY: Certainly — I mean, this was after the Psi discovery, so of course I knew who Burt was.

ZIERLER: Yeah.

PHINNEY: I didn't know of Pief as much. I thought the linear collider was exciting. I came to SLAC to work on the control system for the SLC, and that was a really fun experience. I mean, we were building a big database-driven control system for the first time. It was a very small group of people. It's hard to imagine, but ethernet did not exist. A company had been contracted to supply ethernet for the control system, and they failed. And so, a group of about 10 of us built a network in six months. It was quite exciting. Wild. It shows that a small group of very dedicated people can do what a very large group can't do.

ZIERLER: Right. What year did you arrive?

PHINNEY: '81. In the summer of '81.

ZIERLER: And what was the scene like when you were there? I mean, this is post- the mid-1970s. What was it like to be there at that time?

PHINNEY: Yes, it was after the Psi discovery

ZIERLER: Revolution.

PHINNEY: It was after the November Revolution for sure, and after the discovery of quarks by Taylor, Friedman, and Kendall. And it was a very exciting place to be. It was fairly open. There weren't a lot of other women, but there were a few women physicists. And I would say both Panofsky and Burt were quite — they were not in any way discriminatory against women. They were very supportive. It wasn't so much that they pushed you for being a woman as just that they took you for your talents and your abilities, and that was it. And you didn't have to fight to establish yourself.

ZIERLER: What other women were at SLAC at that time?

PHINNEY: Certainly Vera Lüth, who was on the Psi experiment at SPEAR. I don't remember exactly when Helen Quinn arrived. It was within a year or two. She might have been just before me or just after. Those are the two I think of most strongly.

ZIERLER: So certainly, there were women, but not many.

PHINNEY: And then in the controls group, there were other women, who also had Ph.D.'s, usually in some other field,

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astrophysics, or something else. So, there were women physicists working at SLAC at that time, but not many.

ZIERLER: What was the research culture like in terms of the extent to which you were free to work on the projects that you wanted to work on, versus being slotted into an extant group and contributing to that project?

PHINNEY: Well, since I came to work on controls, and there was an enormous amount to do, and we were always behind, basically that's what I worked on for the first few years. I was originally hired by Bob Mellon, who was head of the controls department at that time. And then eventually, after a couple of years, I led the software group for a while. I didn't have the same freedom of choosing what to work on, I think, as some of the other people you've interviewed.

ZIERLER: And what were some of the scientific and technical challenges when you joined the controls group?

PHINNEY: The network was one big one. The other thing is that most control systems people had built for accelerators or even experiments were fairly small-scale. And so, when we were building something with as many components as the SLC, they really didn't scale up. I mean, typically — I remember talking to people at Triumph at the time, for example, and basically they had — their control system was written such that every device was hard-coded into the program itself. And we wrote a control system where you just gave it a list of devices, and it knew how to do everything to all of them. So, it was much more generic and database-driven than anything that was really out there at the time. That was a very interesting challenge. And then, the network that we built actually was very forward-thinking. I mean, it's token-passing and message-based, and a lot of things that all the networks today have incorporated but were really new then. We weren't smart enough to patent them. [laughs]

ZIERLER: So, you were essentially making up your group, and you were essentially making up these rules as you were going along, because…

PHINNEY: We were.

ZIERLER: …there was no playbook.

PHINNEY: No, there wasn't a playbook.

ZIERLER: So, what was the process like of figuring out what worked and what didn't?

PHINNEY: I don't know how to describe that.

ZIERLER: I mean, is it like over a blackboard, and everybody is spitballing? Is it more in a technical sense — you're putting things together, you're taking them apart? Give me a sense of how you actually figured out what worked and what didn't.

PHINNEY: I don't know how to answer that one. Maybe I have to think about it and see if I can come back with an answer for it. It was very intense, and people working side-by-side. Yes, there was probably a certain amount of writing on the blackboard, and "Let's try this," and a lot of creative ideas from very many bright people. And then one thing we built in early was the ability to de-bug things. So, we had a network of microprocessors, and one of the things that we built right away was a remote debugger, so that you didn't have to go sit out in the gallery with a laptop, which of course, there were no laptops anyway. [laughs] But you could sit in your office in front of your terminal and de-bug something that was two miles away. And that was pretty novel. Another thing we built was basically the control consoles were all running the same code — literally the same code — with just their own datasets. The Digital Equipment Company (DEC) had a feature where you could make what they called shareables of code, and everybody ran the same actual code. And each console was separate, and you could actually also just run a console from your home computer and de-bug. When somebody called me at 3 in the morning, and there was a problem, I could just get on my computer and de-bug it from home, without having to go in.

ZIERLER: So, you were doing remote work way before the pandemic. [laughs]

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PHINNEY: [laughs] I was definitely doing remote work before the pandemic.

ZIERLER: Nan, just so I understand: were you working in your capacity as a physicist, or really was this computer engineering, and you were contributing in your capacity as a smart and resourceful person?

PHINNEY: It's actually some of both. It's probably more the computer and resourceful person. The interesting thing about a linear collider is that because you only get the beams to collide once, you have to make them very, very, very small. And there's a lot of ways to make them big, and it's very hard to keep them small. And so, you need a lot of fancy controls, and basically you need — physics informed the choices of how to do those controls. So, although it was implemented in software, thinking about what was needed required the physics background, and many of the other people who worked with me also had physics backgrounds, although in varying fields.

ZIERLER: And so, could you give a sense of how that interface played out — how you used the computer controls with the physics perspective to get what you wanted to happen?

PHINNEY: It's hard without getting too technical. One aspect of the SLC control system that was really interesting was feedback. And we built feedback to stabilize the beam orbit, and feedback that basically detected how well the beams were colliding with each other at the IP and brought them into collision more optimally. And even — there were various controls you had that you could tweak that would change the phase space of the beam so as to make it more tightly focused or less tightly focused. And we actually eventually incorporated all those into the feedback systems that ran autonomously without people having to intervene. It's hard to describe without being really technical.

ZIERLER: Right. Beyond the narrow technically challenges, what were some of the broader objectives of the control group, and how might they have related to the overall mission of SLAC at that time?

PHINNEY: SLAC at that time was building the Linear Collider, and what it needed to do was to produce Z particles — integrated luminosity. And when I first went, the person who was really leading the accelerator design effort was — I would call it — he had an Attila the Hun style of management. Burt was by this time lab director, and Burt wasn't paying as much attention as he should have. And this person who was leading the effort was rather abusive. We were all pretty unhappy working in that environment. And finally, there was a Department of Energy review which came and basically explained to Burt that he had a serious management problem. And so, this person was removed on August 1, 1988. I know the day. The SLAC accelerator group celebrated for years, on August 1 of every year, the departure of this particular person. [laughs]

This person left SLAC and went to the SSC. It's an excellent example of how the physics community failed to handle abusive behavior well. Most often the abusive person was just shuffled to another institution where the same behavior could continue. The focus was on preserving the career of the (almost always) male abuser and not on protecting the victims.

ZIERLER: Wow.

PHINNEY: Anyway, so after August 1, 1988, Burt took over managing the SLC effort— even though he was already

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lab director, he stepped in and took the leadership of the SLC project for a year. And then there was another transition to other management, and about a year later, it was kind of obvious that wasn't working really well. Around the end of 1990, Burt asked me to take over leading the project. and I led it for the rest of the time until we had our last run.

ZIERLER: What were the biggest successes and failures in your work with the group?

PHINNEY: I'd say my — what I succeeded in doing with the group was to take a lot of very talented people and make them slow down and think, so that they didn't go rushing around in all different directions, and then somehow harness their talents into actually improving the performance of the machine. You needed to have enough knowledge to know what was a good idea and what was a bad idea, and you had to somehow have good judgement.

ZIERLER: And how collaborative was the research culture at SLAC, beyond your individual group? In other words, would you rely on other groups? Would they come to you, or were you sort of insular in your own group?

PHINNEY: Oh, no. Absolutely not. The SLC was in enough of a crisis, and it was important for the lab that it succeed. Very early on, the experimental particle physicists realized that the machine really needed the work, not their experiment. And they rolled up their sleeves and they pitched in, and worked side-by-side with the accelerator physicists, engineers and controls group. Some of the more brilliant ideas came from some people who were really experimental particle physicists, not accelerator physicists at all, but just dug in, figured out how to address the problem, and solve it. One of the issues with the SLC was the terrain following arcs: it's probably the only machine that will ever be built that was not in a plane. And the problem is — I don't know if you are familiar with the SLC, but basically the two-mile linac accelerates electrons and positrons, and then the two come around two arcs, which would look like a tennis racket, to the interaction point. And those arcs had to go under the PEP ring and some water storage that's nearby SLAC. And so, they were not built in a plane. They went downhill and then back up. That was extremely challenging. People just didn't realize how small errors propagated into a big problem. One of the experimental physicists basically just used his physics analysis techniques to try and solve the transport matrix of the arc so that he could figure out how to introduce corrections. So, yes. We were not isolated at all. I mean, the group of people working on the machine was not small, but it was greatly augmented by people from the Mark-II experiment, from the SLD experiment, from the engineers in the controls groups, all over the place — people rolled up their sleeves and dug in and worked on it. And it took a lot of people a lot of work for a lot of years. So, when the steering committee and I started leading the project — when the Mark-II experiment was on the beam line — their best day, which was right about that time — was 15 Z events in one day, and that was a 25-hour day. [laughs]

My partner, Martin Breidenbach, was co-spokesperson of the SLD experiment with Charles Baltay, so there was very close communication between the accelerator physicists and the experiment. This also led to some amusing moments, as when the Chair of one review committee asked Marty "Do you feel confident that Nan will deliver what she promised? To maintain a happy home, he had little choice but to answer Yes.

ZIERLER: [laughs] Now, you mean when you became program coordinator?

PHINNEY: Yes.

ZIERLER: Now, you gave an example of outside people coming in to help on SLC. Were there any opportunities where you were able to use your expertise to help other projects at SLAC?

PHINNEY: Only after the SLC. Well, I mean, maybe that's not quite true. By the time the SLC shut down,

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the B factory was being built. Certainly, I worked with them on various things. I was on the Radiation Safety Committee for a while. I did some analysis of the safety systems, found some problems — so, things like that where my expertise was helpful for the new projects coming on.

ZIERLER: And to get a sense of the chronology, how long did it take for the SLC to be viable and to produce reliable physics?

PHINNEY: It was an uphill struggle. I think one of the differences during the time I was leading it was we tried to, every year, propose what we thought we were going to be able to do and then do it. So, one year we said we would get 10,000 events that year, and we did. And before that, with the SLC, people hadn't realized how hard it was going to be, and they kept making claims that they would get here, or they would get there, and it was crazy. They were miles away. They weren't ever going to be able to do it. So what we did differently — and I had a group of very good people working with me in the SLC steering committee — and we thought about what might be our stretch goals for a year, and we gave ourselves stretch goals, and we made them. But they weren't crazy goals. They were something that was believable. And then, that actually really helped, because — again, I said in the beginning that DOE was very different. DOE was actually more like our friends, and they were rooting for us to succeed. And they really kind of helped us. The DOE stood by us, and it was a much more collaborative arrangement than I think people find today.

ZIERLER: Now, when you emphasized the difficulties of SLC, is that to say that the difficulties weren't just in the development stage? It wasn't like, one day all of the development is done, and now this thing is working, and now you can just learn the science? There were difficulties throughout. There wasn't that clean break from development to a successful machine.

PHINNEY: No. No. It was a long, long slog.

ZIERLER: [laughs] I thought you were going to use the word "slog."

PHINNEY: [laughs] Nobody had ever done a linear collider before, and they didn't realize what it would take. And so, there were brilliant ideas that came in. I mean, one idea was called BNS damping, which is a technique that people use in all electron Linacs now, but it wasn't known before. It came from the Russians. And there were many other good ideas. At the time, I would talk with my colleagues at CERN who were working on LEP, and they too encountered things that they had not foreseen at all, like the energy dependence on the tides. But at SLC, it was one thing after another. We had a problem with the damping rings where every so often, there would be a pulse down the linac, which would just be wildly crazy in orbit. And it was eventually tracked down to a bunch-lengthening instability in the damping ring, where the bunch length would oscillate, and if it happened to extract the bunch just at the top of the oscillation, then the bunch would be wild going down the machine. At first, people called them flyer pulses. People called them lots of things. Finally, we figured out how to do an experiment to try

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and detect what was really going on. And once you found out what was going on, then you could think about how you were going to fix it. In this particular case, we actually rebuilt the vacuum chambers of the damping rings in order to fix it. Another example of a problem we had— the positrons went down the linac ahead of the electrons. There were two electron bunches. One went to make the positrons, so that was at the back. And we always noticed that the electrons were more unstable than the positrons. All of our theorists assured us that the positron bunch could not talk to the electron bunch, because they were too far apart. But eventually, we decided to do an experiment. We wiggled the positron bunch, and as we expected, the electrons wiggled. So, it wasn't true that they couldn't talk to each other. It's called long-range wakefields. And of course, as soon as we'd done the experiment, the theorists had revised their prediction to tell us that, yes, there were long-range wakefields in the linac, but of course, you had to learn it for yourself. Those are just two examples of how we solved one problem, and then went on to the next problem, and solved that problem.

ZIERLER: So, Nan, in terms of your emphasis of all of these troubles, all of these technical difficulties, what were the feedback mechanisms that you had to know that you were succeeding? How did you know when there was a good day, when good science had been achieved on a given day?

PHINNEY: The total number of events. That's really easy.

ZIERLER: That's an easy one.

PHINNEY: So, as I said, we started off with a 25-hour day that got 15 events, and in the end, we were doing more than 250 events an hour.

ZIERLER: And what is an event? And don't be scared to talk —

PHINNEY: A Z boson.

ZIERLER: Okay.

PHINNEY: You hit electrons and positrons together, and a certain fraction of them make Zs.

ZIERLER: I was going to say: don't be afraid to get too technical. That's what we're here for. How do you know when you've gotten a Z boson? What does that look like? What does that mean?

PHINNEY: Well, that is for the experiment to tell us. We, from the accelerator point of view, have no way of detecting whether they have had an event or not. They are the only ones — the experiment is the only one that can tell you they've had an event. Another thing that was really formational about the SLC, was it used to be that experiments — they wouldn't be up taking date all the time. They would have various issues. They would take down some part of the detector, or whatever. And probably if they were running 75 or 80 percent, they were happy. At SLC, since it was so difficult to get the events that the experimenters wanted, they learned how to be up more like, 98 percent of the time. And that's become the standard these days for other experiments, too.

ZIERLER: Right. There's a duality of the success, right, that there's an event you see — there's the Z boson — but there's another element of success, which is: this is how we're advancing physics, by accomplishing this. So, in what ways is creating this event and detecting the Z boson — in what ways, more broadly speaking, is this advancing physics?

PHINNEY: Okay. So, there's two sides to it. One is the accelerator side, and we proved that you can actually build a linear collider, and groups around the world spent the next many years — including me — designing the next generation linear collider. So, we advanced physics because we showed that this really wild idea of Maury Tigner and Burt Richter could actually be implemented and could be made to work. And what we learned in our long slog [laughs] informed what you had to build into the design for the next machine to make it plausible, that it would actually perform as needed. So, that's one side of it. The other side is that the SLD experiment at the SLC actually made the most precise measurement of an electroweak parameter, and they predicted that the Higgs had to be 125 GeV. And that's exactly where it was.

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So, that certainly advanced physics. They basically told the LHC people where to go look for it.

ZIERLER: Speaking about other major facilities: right at the time you took on the leadership position in 1990, this was right at the height of the excitement that the SSC was actually going to get built. I'm curious how closely you followed those developments, and the general tenor at SLAC of — during the time when the SSC seemed viable, what that might have meant for SLAC.

PHINNEY: Well, I don't know quite how to answer that. There certainly were a lot of SLAC people who went to the SSC. Vera Lüth, who I mentioned before, was one. Many others. There were a number of SLAC people that the SSC tried to recruit, but not necessarily successfully. The person who had been leading the SLC early on, who left, went to the SSC. So, I never considered going to the SSC myself, although I certainly went down to serve on review panels and things like that, and advise them.

ZIERLER: But was there a sense of excitement for what might have been achieved at SSC? Was there a sense of sort of concern that SSC might render some of what was going on at SLAC, you know, irrelevant or older?

PHINNEY: I mean, certainly everybody was excited about the SSC. Many people were also concerned about what were quite visible problems from a distance. And yes, whenever there's a big project, it takes away the money that's available for other projects, so of course, people had to have been worried about what the impact of a big, new laboratory would be on SLAC. But I was actually too busy with the SLC to really pay that much attention. I mean, as I said, I certainly participated in reviews and went and advised them in things where I had some expertise. But the SSC went down in '93, and we ran until '98. And SLAC has this tradition that when the accelerators are running, there is an 8:00 status meeting every morning. So, I spent most of 15 years going to 8:00 meetings, seven days a week, so — [laughs]

ZIERLER: Seven days a week.

PHINNEY: Oh, yes. When the machine runs, it doesn't run Monday through Friday. It runs around the clock.

ZIERLER: [laughs] The machine is not a government employee.

PHINNEY: No.

ZIERLER: Nan, can you give me a sense of your collaboration outside the SLAC world? In other words, were you working with people beyond SLAC? Were people coming to SLAC to work with you during these years? How did that work in terms of you specifically, and SLAC generally, its connections with physics departments and other laboratories?

PHINNEY: Well, as I said, there was a lot of collaboration with the experimenters, both on the Mark-II and on the SLD experiment, and those typically are from universities somewhere. So, that collaboration went on. Then when the SLC was first started, and the first damping ring had been built, CERN was just gearing up to build LEP — or maybe it was under construction — but there wasn't anything for the accelerator physicists to work on. So, a lot of CERN people came over and spent a year, or part of a year — a couple years — working on the SLC. So, in the early days, there was a lot of collaboration back and forth. In those days, actually, every good experimental physicist who got his degree at SLAC would be sent to spend at least a year at CERN. And there were a lot of exchanges back and forth.

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CERN people came to work on various aspects of the control system, or the accelerator. Later, it was somewhat less so, I think, partly because by then the CERN people were already working on LEP, and they had a lot of work to do on that themselves, so they didn't have as much time. But I think the SLC was widely seen in the community as a very, very challenging project. So, bright young people wanted to come work here. And then we got people from DESY and people from Frascati and people from almost all of the other accelerator labs. People from KEK, for sure. I should also say that because KEK had a real interest in linear colliders, that there's been a very strong collaboration back and forth for years. In particular, they built something called the Accelerator Test Facility at KEK, and essentially everybody — most of the accelerator physicists at SLAC from that generation — went over and worked on it. I mean, perhaps they stayed for three weeks, or a month, or a couple of months, or something like that. A lot of the equipment at the ATF was built by SLAC, so it was really a collaborative effort of the two labs. And then again, when the B factories came out, there was a lot of collaboration — I mean, there was obviously rivalry, because there were two competing B factories. But there was also collaboration, the people who were knowledgeable about our B factory served on their machine advisory committees, and vice versa.

ZIERLER: And in terms of your own scholarly output from a sort of PR or outreach perspective, were you writing papers? Were you presenting at conferences? Were you out there in that regard?

PHINNEY: I would say that we were guilty of probably writing fewer papers than we should have. But I think I gave my first SLC talk in 1990 at — well, first at SLAC, and that at a conference in Nice. And I think over the next 10 years, I probably gave 25 seminars or conference talks on the SLC, and another maybe 50 review talks at SLAC.

ZIERLER: That's a lot.

PHINNEY: [laughs] Yes.

ZIERLER: And what were some of the big ideas that you wanted to convey at those talks?

PHINNEY: What I think I did, that perhaps other people who had talked about the SLC did not do as thoroughly, was explain what the problems we were currently facing were, explain what ideas we had to fix them, and make the people who were listening feel like they were part of the team that was trying to solve the problem. I think that's one of the reasons that the DOE people continued to support us. They felt like they were part of our team. And so, that's an important thing to do. We were fighting a lot of new and interesting problems, and accelerator physicists love new and interesting problems. Why would you want to do this if it was the same every day?

ZIERLER: Right.

PHINNEY: I mean, the only point in doing it is it's different. Right? [laughs]

ZIERLER: And Nan, when you say that the DOE was invested in this, and that they were rooting for success — of course, the DOE is a very big agency. Did you have a sense, specifically, of individuals or at least offices who were right there?

PHINNEY: Oh, yes.

ZIERLER: Who was that, or what was that?

PHINNEY: Yes, it was — Bill Hess was leading what was — I've forgotten what the name was, but it basically included nuclear and particle physics. And John O'Fallon was the particle physics person. And we felt like they supported us. So actually, the year that we promised to get 10,000 events — which was, the first year that we were really moving — they bet me a dinner that we wouldn't succeed. But we did, so they had to pay. And we all went out for dinner. Burt and Laurose and Marty and I went out with them. [laughs] So, you can't imagine that today. Right? Would you imagine a DOE program officer would bet you…

ZIERLER: Right.

PHINNEY: …that you wouldn't get the number of events you said you were going to get?

ZIERLER: Yeah.

PHINNEY: That has to be, like, friends.

ZIERLER: It seems like a very long time ago, actually.

PHINNEY: [laughs] It does. Doesn't it?

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ZIERLER: When did you become involved in the NLC?

PHINNEY: When the SLC shut down, which was June of '98.

ZIERLER: Let's talk a little bit about that transition. Why and how did the SLC shut down?

PHINNEY: Well, the run was scheduled to end about two weeks later. I forget exactly, but something like that. But the positron target broke. The positron target was extremely radioactive, so there was no way that anyone was going to go anywhere near it and fix it.

ZIERLER: So, this is an unplanned event. There was no planned obsolescence of the SLC.

PHINNEY: Well, it was never clear from one year to the next whether we would have another run. There were many of us advocating for another run after the one that was scheduled to end in '98. But the B factory had been built, and there was clearly going to be an issue of resources between working on the B factory and on the SLC. Burt was always one who wanted to move on to the next new thing. So, it was not obvious that there would have been a further run of the SLC, although as I said, we were arguing for it, fighting for it. But when the positron target broke, that was it.

ZIERLER: And why did it break? What happened?

PHINNEY: There was a water-to-vacuum leak.

ZIERLER: So simply, it was a mechanical failure, essentially.

PHINNEY: Yes. It was the best week, the best day, the best shift we'd ever had, and I went in, and the people who were running the machine were just disconsolate. And I just said, "Hey, guys. We did great." "Two more weeks isn't going to change what we did. We did great."

ZIERLER: Was your sense immediately that this was the end of an era, that this would not be something that would be fixed, and that the SLC would keep humming?

PHINNEY: Well, as I said, we were planning to have a shutdown in any event. And that would have gone on for several months, typically.

ZIERLER: But with the idea that it would come back online.

PHINNEY: No. It was not — there was no commitment to have it come back online. Not only was this the last run, as I said — many of us were arguing that there should be another run — but it was not at all clear that the SLAC management was behind that.

ZIERLER: Were you among them, arguing for another run?

PHINNEY: Sure.

ZIERLER: Yeah.

PHINNEY: A glutton for punishment.

ZIERLER: [laughs] And so just so I understand, it's on the basis that the SLC is still producing exciting physics, that there's still new stuff to be learned with the SLC continuing operations?

PHINNEY: Yes. And there were particular things that the SLD experiment had just started to get enough data to get a handle on. B meson mixing is one of them. And had they had another run, at the kind of luminosity that we were then delivering, they would have had a lot of data and could actually have made some measurements on that quantity, which they could not do with the limited data they had. Anyway, so when SLC shut down, besides sleeping for probably [laughs] weeks, I looked around, and I decided to work on the NLC.

ZIERLER: Now, when you say you decided, did you feel like you had free run to pick your next project — it was entirely your call?

PHINNEY: Well, I think the two projects that I might have worked on, I might have worked on the B factory, or I might have worked on NLC. Those were the two big efforts that SLAC was engaged in. And I didn't feel like I should have gone off and done some weird thing that had no connection to the major efforts of the lab. So yes, I had some freedom. I mean, it was certainly my free choice, and I chose to work on NLC as opposed to the B factory.

ZIERLER: And why NLC over B factory?

PHINNEY: I wanted to see a future linear collider built and I felt more comfortable with the group. It was mostly a question of personalities.

ZIERLER: At what stage was the NLC when you joined?

PHINNEY: They had already published the Zeroth-order Design Report.

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So, I was not involved in that, I mean, except peripherally. I was still buried in the SLC when that came out, so I certainly collaborated a little bit, or kibitzed a bit with them, on some ideas, but I didn't really contribute to that design report.

ZIERLER: And what was your sense when you got more up to speed on these things — what was your sense of what this report conveyed in terms of the contributions so far of the NLC?

PHINNEY: I'm not sure I understood that question.

ZIERLER: Obviously, the report was a really big deal. I'm curious what your sense is, in terms of what that report conveyed, and perhaps if that influenced your desire to join the NLC group.

PHINNEY: Again, since the SLC was such a struggle, most of the people who were working on the NLC had actually also been working on the SLC in one capacity or another. They weren't disjoint groups. And the ZDR was actually an incredibly complete, thorough exploration of the issues and solutions to the issues for a linear collider. Years later, Tesla came out with a design report which was much less complete, than the ZDR had been.

ZIERLER: When you joined the NLC, what was your role?

PHINNEY: I think — I don't actually remember. I think I was deputy head of accelerator physics, or something like that.

ZIERLER: No, I don't mean —

PHINNEY: Tor Raubenheimer was leading the accelerator physics group, and we're very good friends. And so —

ZIERLER: Yeah, I didn't mean administratively. I meant substantively. What were you doing, day to day? What were your contributions, scientifically?

PHINNEY: I would have been working on the algorithms to tune the machine, which is kind of where I had contributed most on the SLC, in solving problems — is developing algorithms to tune the machine.

ZIERLER: And so, same kind of question: what were some of the technical and scientific challenges, in terms of developing those algorithms to tune the machine?

PHINNEY: I don't know. Actually, the biggest technical and design challenges for the NLC were really the RF development, which is not something I worked particularly on, and I think that's — in the end, the lack of actually demonstrating a viable RF system was what sunk the NLC and caused the international review panel eventually to choose superconducting RF. The other aspect of the NLC that I played an important role in was writing. I happen to be a fairly good editor, and so I think one of the first things we did was there was — not too long after I joined the NLC effort, there was a Snowmass meeting and we had to have a brief design report for the NLC to bring to Snowmass. And I either wrote or edited all of it. So, that was another area where I contributed right away.

ZIERLER: What were some of the big research questions that were relevant to the NLC?

PHINNEY: The important research questions really were developing the RF system. I mean, we had learned from the NLC that the short- and long-range wake fields in the linac structures were really an important issue to be solved, and the NLC design had gone to X-band, and it needed very high-powered klystrons, which had not yet been demonstrated. So, those were actually the biggest design challenges. I think for the rest, it was fleshing out what we already knew,

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improving on the design for the final focus, developing tuning algorithms, things like that. A lot of those things were reasonably understood, and more of what we were doing with the NLC for that part of it was documenting what we knew.

ZIERLER: When you mentioned that the RF system was central to what sunk the NLC, in what way exactly? That the NLC couldn't achieve what it was designed to do?

PHINNEY: We didn't have a working klystron to power the RF. And it wasn't convincing that we would be able to develop one.

ZIERLER: So, it wasn't a matter of budgetary issues or impatience that with just a little bit more work, you would get there. When the NLC shut down, it was because everyone was convinced that it simply wasn't viable.

PHINNEY: The NLC, per se, didn't shut down. There was the International Technology Review Panel. ITRP. The reason they chose superconducting — I mean, there were a number of reasons, but I think the strongest technical reason why they chose it was because NLC had really failed to demonstrate that the RF was viable.

ZIERLER: And is that synonymous with saying that nothing would demonstrate that the RF was viable? Is that just a basic statement of the science, or is that a statement more on the limitations of the NLC?

PHINNEY: Well, whenever you're doing a development, R&D program — which we were trying to develop the accelerator structures and the power sources— there's always a fight for resources, because resources are always needed for other things. And so, yes, had lots and lots of money been thrown at it, and the right talent dedicated to it, might we have made more progress? Perhaps. On the other hand, there were real issues with the X-band structures breaking down, and I think that the SLAC people led by Sami Tantawi, have finally dug into the basic issues and understood at some level why those structures break down. And it's not obvious that there was a solution, actually.

ZIERLER: And that's true then and now, or this is — we're talking about then?

PHINNEY: Well, Sami has a completely different design RF structure now, which gets around those problems, but it came from understanding. So, one of the problems is, these structures — Do you know what an RF structure looks like? It's a bunch of cells, strung together with an iris between each pair of cells. And typically, in an RF structure like the SLAC linac RF structures, you feed the RF in at one end, it travels through the cells to the other end, and it's dumped. So, the fact that you need to feed it from one cell to the next means that you need a lot of power in the beginning and less power in the end. And that power makes — the fact that it's coupled between the cells gives a path for it to break down, and the breakdowns actually just destroy the surface of the structure. And so, what Sami has is a new design for a structure which is very successful. It goes to high gradient, where basically each cell is decoupled from the other cells. The RF does not pass through the structure, but it feeds into each individual cell via a manifold. And there's a new manufacturing technique where you mill the cells and the structure to feed them from the copper block, so it's not very expensive to produce.

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And by not having an RF travel between the cells, not having to couple it between the cells, you can design each cell so that it's really optimized for high gradient and impedance and not to break down. And so, the SLAC linac gets to 25 MeV per meter, and the superconducting RF gets to 35 MeV per meter. And this goes up to 150 MeV per meter, perhaps practically more like 100. So, it's a different approach. Anyway, probably a huge digression that we shouldn't have gone into, but I think there were real issues with the X-band RF for the NLC, and that is really why it was not chosen.

ZIERLER: When did you become involved in the ILC?

PHINNEY: When the International Technology Review Panel, ITRP, decided that the world was going to go behind a superconducting linear collider.

ZIERLER: Yeah. Which was when, chronologically? When did that happen?

PHINNEY: I have to look it up. It's either 2004 or 2005.

ZIERLER: Okay. So, there's a break between — is it a pretty clean sequence from when you ended your work with the NLC? Did you switch right over? Was there overlap?

PHINNEY: Jonathan Dorfan was lab director at the time, and he had said that if they chose the superconducting, we would join the collaboration and be on board, because we were the ones with the expertise on linear colliders.

ZIERLER: Right.

PHINNEY: So now, when the ITRP said superconducting, we were all horrified, but we got on board.

ZIERLER: And why were you all horrified?

PHINNEY: Because we believed in our design. And I was afraid that they had just priced the machine out of being built. And I might have been correct. [laughs]

ZIERLER: Yeah. So, was this a matter —

PHINNEY: In fact, I was in Berkeley for a party right after the decision came out, and I was asked what I thought about it. And I said, "I think they've just made it unaffordable." That was my comment.

ZIERLER: Which would suggest that there was a choice in terms of how expensive they priced it. You're saying that it could have been done cheaper.

PHINNEY: Well, what I knew was that superconducting RF is very expensive, and the LCLS-2 project here at SLAC is showing that that it's a very challenging technology. Superconducting RF is difficult. Every time you try and do it, there are new problems. I'm familiar with that. We had new problems all the time on the SLC. But it's just by nature very expensive. And if you could build a normal conducting accelerator — and I think Sami's new design is really, really intriguing — it would be less expensive. At some level, the ITRP chose not to really consider cost, partly because they didn't believe anybody's costs. But the superconducting design was costed on a European cost basis, and that's not a U.S. cost basis. It's really different. It's closer to a third as much.

ZIERLER: Would it be fair to say that when this institutional decision was made at SLAC to join ILC that a certain amount of pride needed to be swallowed?

PHINNEY: Sure. Of course. But we did it. I mean, we had said that we would abide by the decision of the ITRP, and we did, and almost all of us joined immediately.

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And in fact, as part of the ILC, I was the lead editor for the reference design report, which was the main thrust for the next couple of years. It came out in 2008.

ZIERLER: And you mentioned your role was as editor, but of course, you needed to be involved in all kinds of discussions to know the literature that you were editing. So, can you give me a sense of the day-to-day? Who were the main stakeholders? Where were the meetings being held? What were the major considerations during the early part?

PHINNEY: Well, I think after the decision, there was a meeting at KEK. I think that was the first ILC collaboration meeting. The ILC has always been an international collaboration, so the meetings cycle around the world. They go Asia, Europe, U.S., Asia, Europe, U.S., alternating. So, the meetings are everywhere. I think one of the places that I contributed most on the design part was in the reliability analysis and in trying to specify what kind of tuning and other things you would need to be able to actually make the machine work. But for the reference design report itself, there were three of us, actually, who worked together. I was the lead, and Nick Walker of DESY and Nobu Toge of KEK, so it was nicknamed the 3N editorial team. People would be assigned to write a particular chapter, and then one of the three of us would edit it. In some cases, the chapters that came in were sufficiently bad that we rewrote them entirely. [laughs] And it was a lot of work. But one of the last meetings, I think, before the RDR was produced was in Beijing, and we were working not on the individual chapters on the parts of the machine, or the controls and things like that, but on the sort of overview, introduction part.

ZIERLER: To sort of foreshadow ahead a little bit, what did you see as some of the major challenges — technical, scientific, institutional — to the ILC, relative to best-case scenario versus what you were seeing day-to-day?

PHINNEY: I'm not sure how to answer that.

ZIERLER: I mean, in other words, did you see flaws inherent to the whole operation, or challenges that might have been insurmountable, even from the beginning?

PHINNEY: Well, the two things about the superconducting linear collider design that worried us the most — the initial design from DESY had what they called a dog-bone damping ring, which was in the linac tunnels. And most of us simply didn't believe that could be made to work. And eventually, it was replaced with a more conventional damping ring design. The other thing, which was a huge bone of contention — there was a very vocal group

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also from Europe — both from Germany and the U.K. — who felt that the positrons needed to be polarized. In order to make the positrons polarized, you have to use the high-energy electron beam to make the positrons, and that couples the two systems. At the SLC, we had used the second electron bunch to make the positrons, so the systems were coupled, and we were quite aware of all the problems that that can cause. It makes it very hard to bootstrap the machine. And so, we felt that trying to produce a polarized positron beam was too risky. But we lost that fight. I still think that it's pretty risky. But at least the design for the ILC the way it is right now has a polarized positron beam.

ZIERLER: How long did you stay involved with the ILC?

PHINNEY: I think until support for the ILC dried up in the US, which would have been — I don't know, around 2010 or '11. I'd have to go back and look carefully, because I don't remember.

ZIERLER: Sure. In terms of — and this is a highly subjective question — did you feel like you were contributing to the ILC as a member of SLAC, or were you a physicist that had an institutional affiliation, and you were sort of part of this worldwide collaboration?

PHINNEY: Very much the latter. And I think — also, the other SLAC people who worked on it — I mean, yes, we were SLAC people, but we were physicists working on an international collaboration with colleagues who were equals. And it was obvious that the machine was not going to be built at SLAC. I mean, SLAC might have had a role in some aspects of the machine, and hopefully, had it been built, or were it built, they would be involved in the experiments for the machine. We have had a very strong experimental involvement in the design for the machine. But the SLC shut down in '98. It's more than 20 years ago.

ZIERLER: Right.

PHINNEY: [laughs] The people who were young kids working on the SLC are now getting close to retirement age. I wonder if the Japanese were to decide to build the machine today, where is the expertise? I mean, most of us — I'm retired. And the people who aren't retired are not that far from retirement age, and you're talking about a time scale of 25 years. I mean, when I kind of gave up on the ILC was when I realized that either I'd be pushing 100 or pushing daisies [laughs] by the time it was commissioned. And it was like, no, this isn't going to work. [laughs]

ZIERLER: And so, just so I understand, administratively, you were an employee of SLAC. SLAC was giving you a paycheck.

PHINNEY: Right.

ZIERLER: But your work for ILC, you weren't necessarily representing SLAC's interests.

PHINNEY: Well, I mean, I think as a member of the SLAC team, I would always try and represent SLAC's interests. But that's not the way a collaboration works. A collaboration, if you want something to succeed with the RDR — I didn't say, "You got assigned that chapter. I'm not going to take a chapter until you give it to me," I mean, if the chapter was badly written, I just assigned it to somebody else, or I rewrote it myself. I mean, you just do what needs to be done to make the thing work. So, I don't know quite how to answer that. And yes, I was being paid by SLAC, but I was being paid by SLAC on funds that were allocated for ILC R&D by DOE.

ZIERLER: Right.

ZIERLER: And when did your involvement with CLIC start?

PHINNEY: Well, again, once I started working on the NLC particularly, but even before that, I mean, because I was an expert on linear colliders, when CLIC would have a meeting or something, it was a natural thing for me to attend some of those meetings. Obviously, not if I was in the middle of an SLC run probably, but we've always worked collaboratively between the various labs interested in linear colliders, and that's SLAC, KEK, CERN, and DESY, primarily. And so, we may have competed with our different designs, but we've also collaborated enormously and worked together and had meetings together, and had special topic meetings, like on final focus design or something like that, where everybody from all the groups would get together.

ZIERLER: When did you retire from SLAC?

PHINNEY: About three years ago.

ZIERLER: And was that sort of — did you see that as retiring from physics, or just from going in every day? Which is another way of asking: are you still up on the literature? Are you still connected with what's going on at SLAC, or did you really retire?

PHINNEY: No, I think — for so many people — as time went on, I was doing more administrative things and less actual physics myself. And so, it was kind of a natural transition. And the last couple years I was still working at SLAC, I was very involved with APS as the first speaker of the council. And so, I would say the last couple of years, I was putting a lot of energy into that. The Free Electron Laser has not ever been specifically my project, but that's the lab's main focus today. It was a good time to go do something else.

ZIERLER: Yeah. I want to ask some broadly retrospective questions. What do you see as — you can rank them, or if you just think of them as a group — what do you see as your most significant contributions as a physicist, over the course of your career?

PHINNEY: Getting the SLC to work. [laughs] That's a no-brainer. I mean, there's nothing even close, and nothing I spent anywhere near as much time on. I spent close to 20 years working on the SLC and another 15 years working on a future linear collider.

ZIERLER: [laughs] Right. So, it's the SLC and everything else. And in what ways did that — I mean, you mentioned it a little bit before, but in what ways did that achievement really move the dial in physics generally? What did the SLC do that would not have accomplished — another way of asking that is that — what do we know now that we wouldn't have known otherwise, absent the SLC?

PHINNEY: That's kind of a toughie, because what we know now is that it's possible to build a linear collider if it doesn't cost too much. And we haven't demonstrated that we can build a real linear collider. I mean, after all, the SLC was kind of a hodgepodge — using an existing linac and these arms to bend the beams around. And then I could go back to the SLD measurement, which basically nailed the Higgs mass, but since we've now seen the Higgs, the fact that they told you where it was doesn't make all that much difference. On the other hand, I think anybody involved in the Free Electron Laser would say that the Free Electron Laser would not even vaguely have been possible without the SLC. The problems that we slogged our way through were the things that they needed to know to be able to make the Free Electron Laser work.

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So, if you take a larger view and you step outside of particle physics, then we certainly enabled the Free Electron Laser.

ZIERLER: If we want to be optimists and say that when we're 100, pushing up daisies, or whatever, if at some point in the future this kind of project will come online, in what ways does the SLC serve as an effective blueprint for the next kind of project? And in what ways does it serve as a cautionary tale for things to avoid, or things to think through differently?

PHINNEY: The cautionary tale is kind of obvious. SLAC had been so successful with SPEAR, which they built with grad students on Saturday afternoons in the parking lot — that they rather arrogantly thought that the SLC would be just another piece of cake, and it was not at all. So, that was a profoundly deep mistake.

ZIERLER: And is the arrogance because they looked at the SLC and said, "This can be done, no problem"?

PHINNEY: I'm not sure I understand that question. I mean, did they look at the proposal for the SLC?

ZIERLER: Right.

PHINNEY: The problem was SPEAR was the most amazing, cheap success, and then Burt first went over to CERN and encouraged them to build LEP, and then came back and tried to make something quick and dirty that would get the physics out before LEP. And it just wasn't easy enough to do quick and dirty, like they had done SPEAR. It was way too hard.

ZIERLER: The opposite of my previous question: what were some of the greatest — I don't know what the right word is — frustrations or accomplishments that never came to fruition, that stick out in your career?

PHINNEY: Well, I don't really have an answer for that, except that, of course, when the SLC finished, we all thought that the world would build another linear collider…

ZIERLER: Right.

PHINNEY: …in 10 years or something like that. And it hasn't happened, and it's not obvious that it's even going to happen.

ZIERLER: And so, what is lost, that we don't have any prospects for the next generation linear collider? What has physics lost because of these developments?

PHINNEY: Ah. Well, that's kind of — that's a fundamental question which we maybe should have gone into further back. But electron-positron collisions are really nice because they're clean. They're two-point like particles. They hit, or they don't hit. And if they hit, and they break up and do something else, everything you see is interesting. When you have protons, they're composite particles, and one of the quarks in there hits another quark, but there's a lot of other stuff going on. And so, the events are very dirty. There are a lot of tracks, and there are a lot of background events. And it's really difficult. The LHC people have done a fantastic job, but it's really hard work to dig the physics out of there, and you can never see —you have to have signatures, like heavy quarks, or whatever, that you can dig out to see what's going on. If the Higgs decayed into an invisible particle — dark matter, something — you wouldn't see it, because you have no signature that you can separate out of that background. An electron-positron collider, you'd find it, because you know everything that happened. It's clean. So, I think now that we apparently don't have supersymmetry, and there doesn't seem to be really anything — I think, actually, the world would like to build a Higgs factory and look at clean events with electrons and positrons, and see if there's more to the Higgs than what we've been able to dig out of the LHC data.

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So, that's the real interest of an electron-positron collider. It's because the events are so clean.

ZIERLER: And to draw on your skills — your work for APS, or your work as an editor — when you say that the world wants this, if you are given the opportunity to testify before a congressional committee — why? Why should we spend the money on this? What's a compelling answer for people who are not physicists?

PHINNEY: So, I was wearing my physicist hat.

ZIERLER: Right. Of course. But society needs to support these things. So, what's the winning argument?

PHINNEY: I don't actually have one. When I look at — the European strategy group has just come out with an endorsement of building a big Higgs factory in a 100-kilometer tunnel. I don't know that I could argue to the world that that's really where it should be spending its money. I wouldn't be able to. So, yes. I said "the world wants this," but I meant the physics world.

ZIERLER: [laughs] Right.

PHINNEY: I'm not sure the *world* world wants it. I think the *world* world has many other things to spend money on.

ZIERLER: So, as an intermediate step between being a scientist and being a citizen of the world, if you had money to spend on other scientific endeavors, would you even go so far as to say that the money should be spent not on particle physics at this point?

PHINNEY: I don't want to say that. Particle physics is still my love. [laughs]

ZIERLER: Right. Of course. [laughs]

PHINNEY: But I would certainly understand someone making that choice.

ZIERLER: Nan, I want to ask you a sort of broader institutional history question of SLAC, given your long tenure there. So, to think back — you know, the easy bookends are the day that you arrived and the day that you retired. In what ways has SLAC, over the years, changed? And in what ways has it stayed the same?

PHINNEY: This could take hours. [laughs] One of the big changes is a thing I mentioned before, about free energy. In the original SLAC group structure, Pief had group leaders — Dick Taylor, Martin Perl, Burt Richter, and others — who had a vision of what they personally considered important physics, and what they wanted to pursue, and he backed them up and stood behind them. And the reason that SLAC had so many Nobel Prize winners is because these people were — they believed in a particular direction, like Martin Perl, who always wanted to find the other lepton. And nowadays, that isn't encouraged in the same way, that people have a vision and follow it. They are supposed to — there's a program for the United States physics community, and they're supposed to get on board that program. So, they're not encouraged to follow their dreams in the same way. What came along with that group structure was that basically they had some amount of money, and so if somebody came up with a wild idea that hadn't been on the list when they went to DOE the last time, but people had a little seed money that they could use to encourage someone to follow that idea. This is now impossible, when you need to go back to DOE to reassign a quarter of an FTE. It's just — there's no space in there. The other thing that these groups had is they had embedded engineering, that the engineers were part of the team with the physicists. What's happened now at SLAC is that the engineering is pulled out, and the engineers don't feel as connected to the experiments, or to the experimental work, as they did before. They don't feel as part of it. They're just like a rent-an-engineer, or something like that. And that's really changed how it feels

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for the engineering staff, and that's a big difference at SLAC. So there are obvious differences having to do with the fact that the focus of SLAC is primarily photon science now, instead of particle physics. But I think the bigger difference was moving away from the group structure, which had strengths, but was rather unusual among the national labs. And then the fact that DOE sees itself as micromanaging everything that is done at the lab now.

ZIERLER: Here's a more concrete question: to what extent was Pief's vision singular, and it's simply impossible that that could be kept up? And to what extent was his vision meant to be durable, that should have stayed true regardless of the changing times?

PHINNEY: I think Pief was singular, but I think Wilson, who built Fermilab, was cut from a similar mold — a somewhat similar mold. He didn't have the strong physicist model that Pief had. It's hard to say. I mean, things have — the whole atmosphere of doing science has changed so much. I believe the original proposal for the SLAC linac was perhaps 20 pages. Something like that.

ZIERLER: Yeah.

PHINNEY: And so, Pief had a vision with the strong group leaders, but he also was willing to stand up to DOE, and he got away with it.

ZIERLER: So what's the difference? Is it a matter of gravitas?

PHINNEY: I don't know. I'm actually not sure that some of the damage wasn't from the SSC. That project was so big that it attracted the defense contractors, and the physics community wasn't prepared for that at all. And they drowned in it.

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I don't really know what has caused the changes with DOE, but my interactions with Hess and O'Fallon when we were doing SLC, and the interactions with DOE now, are just light years different.

ZIERLER: And that's perhaps partly about the personalities and partly about some of the structural differences in the way DOE relates to the national labs generally.

PHINNEY: The thing is, both with Pief building the Linac here, and with Wilson building Fermilab, there was a tolerance for risk.

ZIERLER: Yeah.

PHINNEY: There's no risk tolerance out there anymore.

ZIERLER: Right.

Which is another way of saying, if there had been current levels of risk aversion in 1963, SLAC would have never gotten off the ground. Right?

PHINNEY: Never.

ZIERLER: Right.

PHINNEY: Never. Never. And the thing that I see, and that really frustrated me more and more the last few years, is it's all show. Sometimes, when you have too many reviews, nobody thinks anymore. It's easy to hide behind checking off the boxes. I've just seen it again, and again, and again, and I've been involved in building projects at SLAC, and now if you do a DOE project, you have to have reams, and reams, and reams of paperwork. But most of those reams don't have any content in them. It's just going through the motions. And it doesn't add value.

ZIERLER: Nan, I'm curious. In your later years, when you were eyeing retirement, that usually is often the time when you might feel more comfortable speaking your mind or shooting from the hip. Clearly, these were frustrations that you felt not after retirement, but while you were active. I'm curious if you ever felt it was appropriate or necessary to speak up about these issues.

PHINNEY: Well, I'm fairly outspoken. But we used to describe the SLC as trying to go through a brick wall with your head. [laughs] I'm willing to be outspoken about these issues if I think that I can have any effect. But if I think it's not going to have any effect, then I don't bother.

ZIERLER: Yeah. Yeah. Of course, that's a big difference between achieving success with the SLC as a scientific breakthrough — achieving success with the DOE would be a bureaucratic breakthrough, and perhaps that's a much bigger metaphorical brick wall.

ZIERLER: Well, Nan, I'm an optimist by definition, and so I always like to end these kinds of talks on a positive note, and I think there's plenty of opportunity to do that here, based on your long institutional memory. And that is in two regards: what is the most feasible way forward for SLAC to regain — what's the best word — the magic, the mojo, the frontierism, the ability to identify the fundamental physics and go for it. Right? What is — because —

PHINNEY: I don't know that there's a way. Again, the leadership has changed. The environment that SLAC works with, particularly with DOE has changed. I don't know that you can ever recapture the magic. I'm sorry. I like to be optimistic too, but —

ZIERLER: How much of that is a comment on sociological and structural issues, and how much of it is a comment on, you know, the idea that maybe there really isn't much fundamental work to be done in particle physics anymore — that that work has already been done?

PHINNEY: From my perspective,

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particle physics has priced itself to the point of diminishing returns. Back in the heyday, they could build PEP and PETRA, and neither of them really produced much because they weren't high enough energy. But it wasn't all that much money that you'd thrown away. And they all produced something, of course, because physicists being physicists, they'll find something to measure and produce a paper. But by the time you're talking about a huge accelerator, it's not obvious. So, you could imagine — and particle physics was an easier place for Pief to have that vision of strong leaders, because there weren't all that many hundreds of questions to ask that were really interesting. So, he could focus on a few of them.

ZIERLER: Well, Nan, let me try this way. Before you mentioned how your interest in going into particle physics was a matter of timing. Right? That the most exciting physics to do when you were a graduate student was, of course, particle physics. So, let's move away from accelerators, and SLAC, and DOE, and all of those things, and engage in a little alternate history from my last question, because I value your insight as a physicist generally. And that is: let's say, circa 2020, you're a graduate student now. Right? And just in the way that when you got to Stony Brook, you were looking around for the biggest and most fundamental work to do, and that answer was obvious — what would be the obvious answer to you now, if you were just starting to forge your career as a physicist?

PHINNEY: Well, I think I would not go into particle physics. Astrophysics has a lot of really interesting questions, and that might be — that's closer to my original interest. And also, I'd probably know more about it, because it's closer. But on the other hand, a lot of young people today are going into biology. And when I was growing up and going to school, biology was taxonomy, and it was boring as hell. But biotech and all the things — CRISPR, and all of these things that people are doing now in biology, are really exciting. So, I totally get it, why somebody might be much more interested in going there because new ideas, new techniques, new revelations. There's a lot going on. I'm not trying to denigrate other sciences, which I know less about. But I think I would not go straight into particle physics today, although it does give you the opportunity to go to CERN, and I loved living in Geneva. [laughs] I had a wonderful time.

ZIERLER: [laughs] Well, Nan, on that note, I want to thank you for the time you spent with me today. It's quite meaningful to get your perspective on all of these things, and I know that SLAC will appreciate, as a matter of adding to the historical record in as honest a way, and as fulsome a way, as possible, they will certainly appreciate your perspective on these things. So, I really want to thank you for spending this time with me.

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