NBWM
Persis Drell
Stanford University

by David Zierler
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DAVID ZIERLER: OK. This is David Zierler, oral historian for the American Institute of Physics. It is May 4th, 2020. It is my great pleasure to be here virtually with Dr. Persis Drell. Dr. Drell, thank you so much for being with me today.

PERSIS DRELL: It's a pleasure.

ZIERLER: OK. So to start, would you please tell me—and I know it's going to take a little—it's a long answer, but would you please tell me your title and institutional affiliation?

DRELL: OK. So I am a Professor of Materials Science and Engineering and a Professor of Physics at Stanford University. I am the James and Anna Marie Spilker Professor in the School of Engineering. I am the Provost of Stanford, Vice President for SLAC and former Director of SLAC.

ZIERLER: OK. And now let's start right at the beginning. Tell me about your birthplace and your family background and your early childhood.

DRELL: I was born in Boston when my father was on the faculty at MIT. He moved to Stanford when I was 6 months old, and bought a house, sight unseen, that is now a university building, but right then was pretty much as close as you could be living to the center of campus. It was an old wood-framed house that had been one of the original houses built for the faculty at Stanford.

My earliest memories are of my father in the—at the physics department. But when I was quite young, the idea of SLAC started to develop. There were some Quonset huts on campus that my father moved his office to while SLAC was being designed, and then ultimately he moved up to SLAC when the construction was complete.

There was a bitter divorce between SLAC and the physics department at that time, and my father and Panofsky actually quit the physics department. And the then provost created a new faculty up at SLAC, which my father was part of. It was very complicated. I had a hard time figuring it out as a kid.

ZIERLER: Now I should say at the outset the context for this discussion is that I'm doing a broader oral history of SLAC. And in talking to everybody from BJ Bjorken to Mary Beth Beerbohm and everyone in between, you know, the dominant theme of all of these discussions is that SLAC really is a family, you know, that it's about the people. And it's so unique in that regard, even among other national laboratories or physics departments.

And, you know, that's one of the reasons why so many people are so happy to have your participation in this project. So I just wanted to say that at the outset. Now, in terms of growing up on the Stanford campus and, you know, being Sid Drell's daughter, how involved were you with science in general and physics in particular at a much younger age than other people might have been?

DRELL: There were some just fascinating people who came by the house. Because of my father's role, and because of the science that SLAC was doing in the late '60s, mid-late '60s, and '70s, pretty much anyone who was anyone in the field of high-energy physics came through

SLAC at that time. And my parents were very warm, and a lot of those people came through the house.

So I grew up with the likes of Feynman and Gell-Mann and TD Lee and Freeman Dyson in the living room. And of course Burt Richter and Panofsky and BJ as well. I had zero interest in the physics. I didn't actually even like math until middle school.

And my interest in math only blossomed because I was tracked low in math in 7th grade, and I took that as a personal insult. So I started working very hard in math, and then really—started to really enjoy math. I had appallingly bad high school physics, and swore I was never going to take another physics course after that. I went off to college headed towards math.

ZIERLER: Now, in your decision to go to Wellesley, was there a bit of a rebellion not in sticking around in Stanford in particular, and Northern California in general?

DRELL: None of the three of us—I have two siblings—went to Stanford. All of us went east to college. Both my parents had gone—had been on the East Coast for college: my father at Princeton; my mother at Wellesley. We were probably subliminally encouraged that going away to college to the east was a good thing.

My decision came down to Princeton versus Wellesley. My father would've loved to have me go to his alma mater, Princeton. And I turned down Princeton for Wellesley, and that probably was a little bit of an act of rebellion, and it's probably the smartest decision I made in my educational journey.

ZIERLER: Why do you say that?

DRELL: Because spending four years at a woman's college was transformational. I graduated from Wellesley, and there was a superb teacher at Wellesley who was the person who turned me into a physicist. And I then went to Berkeley for graduate school, and I was the only woman in my class entering that year at Berkeley. And I just am not sure I would've survived if I hadn't had the four years at Wellesley. It was fantastic.

ZIERLER: I can't help but ask when you talk about this cast of characters that you grew up with at Stanford, of course they were all men. And so I wonder if, you know, your identity wanting nothing to do with physics really just had to do with the fact that you were a young woman growing up, and this just seemed like an alien field to you?

DRELL: Actually, I don't think that's right. I really think I didn't want to have anything to do with physics because I had this appallingly bad physics teacher. What was absolutely fascinating as a kid were the personalities that I met. They were fascinating personalities, absolutely fascinating. So, for me, it wasn't physics. It was these really unique individuals that were showing up in the living room.

ZIERLER: So what changed at Wellesley? How did you turn the corner on physics?

DRELL: I had a superb teacher. My father had suggested that if I was going to be a mathematician, which is where I was heading, that I would be a better mathematician if I took one more physics course. And so I took a course that was not required for the major but everybody said that it was taught by a great teacher.

And I took Modern Physics taught by this legendary teacher—her name was Phyllis Fleming. She was famous at Wellesley. She was dean of the college. And I essentially took every course

she offered, which meant I took about half the physics major backwards, and then added the rest of the courses. And then by the time I graduated, I was a physicist.

ZIERLER: So in terms of saying that you were a physicist, in terms of your own identity within physics, is that the point—just within physics I'm talking about now. Was that the point when you stopped being Sid Drell's daughter, and you became your own person as a physicist, or did that come later on?

DRELL: Well, there was this wonderful discovery I made, I think it was my junior year. There was an exchange program between Wellesley and MIT and I took a famous lab course (that's still a famous lab course at MIT for physics majors) called 8.13/8.14 Junior Lab.

And at MIT I was getting a lot of, "Oh, Drell, are you related to Bjorken and Drell?" And I don't quite know how I stumbled on it, but I discovered that my mother, it turns out, or through my mother, I'm very distantly related to Josiah Willard Gibbs, who is **the** great American physicist. And so it was about junior year in college when somebody would ask me about Drell, I'd say, "Huh, Drell? Who's that?"

ZIERLER: [laugh]

DRELL: "I'm related to Josiah Willard Gibbs." So I had developed a fair number of defense mechanisms fairly early.

ZIERLER: And was the idea that you were going to go to Berkeley right away? Did you want to take some time off, or did you want to pursue a graduate program in physics right out of Wellesley?

DRELL: By the time I was a junior or senior at Wellesley, there was no question I wanted to go straight onto graduate school. And I was in experimental physics at that point.

ZIERLER: So you had been exposed enough between the theory and the experimental that you were set on the experimental?

DRELL: I don't know if it was exposed or not, it was just that's what I'd fallen in love with. It wasn't like I spent a lot of time thinking about it. It was the—it was what I was really enjoying doing.

ZIERLER: Yeah, and so why Berkeley?

DRELL: I am—and was very, very close to my family, so the West Coast had a lot of attraction. I realized as a graduate student I wasn't going to get summers off. I was going to be working a lot. So I wanted to go to a place that was close but not too close to my parents. And then the other thing was I wasn't really sure what kind of physics I wanted to go into, except I was pretty sure I didn't want to go into particle physics, which is kind of funny because I ended up in that.

And Berkeley just had a lot of variety. It was a very big department. There were many different kinds of physics represented. I was worried about funding. But then I got a fellowship, so I figured I could go to Berkeley and work with whomever I wanted.

ZIERLER: Now did you feel fortified having gone to Wellesley that you were ready for a place like Berkeley in a way that you might not have been had you gone to Princeton as an undergraduate?

DRELL: I came out of Wellesley with a tremendous amount of confidence in how to learn, and that I knew when I knew something, and I knew when I didn't know something. And I knew that if I didn't know something, I could learn it.

Berkeley had at that time, I think they still have, a very rigorous exam system where you have to pass—oh, my goodness, it was 12 hours of written exams, and 3 hours of oral exams in order to be advanced to candidacy. And I took those exams very seriously. I studied for them, and I passed all of them in my first year.

And I observed many students from more noted institutions for producing physicists, like Harvard and MIT. And the students came in and they sort of assumed they knew everything, and then they didn't pass the exams on the first time. And at that point, I had total confidence in my abilities, and just dealt with whatever came along.

ZIERLER: And who were some of the professors at Berkeley that you became close with?

DRELL: Well, Gene Commins was my advisor. He was wonderful. Dave Jackson was my mentor my first year. He and Barbara were wonderful. And then George Trilling was a mentor, and he sadly passed away just a few days ago, those three, Commins, Jackson, and Trilling, were the three that had enormous influence on me.

ZIERLER: Now, coming into Berkeley, did you know that you wanted to work with Gene, or that only developed later on?

DRELL: No, The summer before I went to Berkeley, Bob Cahn had suggested, "Oh, when you're thinking of, you know, talking to faculty, you should talk to Gene Commins." I talked to

Gene and then I wanted to work for him just because I loved the way he thought about physics, and I wanted to learn to think about physics that way.

ZIERLER: How did he think about physics? What were you drawn to?

DRELL: He—so first Gene was an incredible teacher, and so he would always explain the physics of what was going on. He loved deep questions. He was somebody who was motivated by the physics questions he was trying to answer, not by being an expert in a particular technique.

He was very—just very facile. He jumped around from different areas of physics, following the question that he thought was the most profound. And so I just thought that was a fabulous way to think about physics.

ZIERLER: And what were his research projects at that time? What was he working on?

DRELL: It was parity violation in atomic thallium.

ZIERLER: Now, in terms of developing your own dissertation, did he hand you a research problem and you went with that, or did you mostly come up with it on your own?

DRELL: So he had a group, and we were sort of focused on various ways of measuring parity violation in atomic thallium. There was one idea that he came up with, and I started working on it. And I spent a year in the machine shop, building the equipment. And then we discovered it had a fatal flaw, which we could've known in advance if we'd thought about it a little longer. So I took that experiment apart in a night, and felt really sorry for myself for two weeks. And then we just got back to finding the right way to measure what we were trying to measure.

ZIERLER: What was the flaw?

DRELL: It was actually a fundamental symmetry that we were violating in the approach to the measurement. So at that point, I think I was Gene's only student, so we worked fairly closely….we came up with another approach. I mean, it wasn't that I went off and independently came up with some brilliant idea. But they were really hard experiments. But it was a great experience to totally fail at something.

ZIERLER: And I know, you know, in dissertation research, you're very narrowly focused. But I wonder if at the time or later on if you reflected on what larger contribution you felt like your dissertation made to the field?

DRELL: Well, so, first, we have to separate my dissertation from the successful experiment that actually, took me to my next step in the field. First there was the failure of the first version of the experiment. And then I started building a new experiment, and a critical piece that the glass blower was making failed.

So I ended up writing my thesis on a failed experiment. It was a very in-depth analysis of all the systematic errors that could cause false asymmetries. That was my PhD. And then I stayed on as a cheap postdoc for a year to actually do the successful experiment. It was life on the edge. No guarantees.

ZIERLER: [laugh] And was your sense that concentrating on a failed experiment was a very unique kind of dissertation to do?

DRELL: I didn't care. I wanted to get it out of the way so I could get onto the experiment. And I had two months to kill, and that was the best use of the two months.

ZIERLER: Now, was it common for Berkeley graduate students to take a postdoc at Lawrence Berkeley National Laboratory? Was that a common path?

DRELL: Well, so, first, there was the first year of a postdoc that I stayed just working with Gene to finish the experiment. And then I went on the job market, and I actually ended up switching fields to particle physics. And that search was more complicated because, at that point, I had a two-body problem.

And my husband or soon-to-be husband and I had to do the usual thing of looking at a bunch of different places, and finding out the best combination. It came down to a decision between Cornell and Berkeley, and we stayed at Berkeley. Three and a half years later, the decision was to go to Cornell.

ZIERLER: And what kind of work were you engaged in at Berkeley National Laboratory?

DRELL: I switched into high-energy experimental physics, and I was actually building an experiment at SLAC.

ZIERLER: Oh, you—at that time? Early then, you were? Huh.

DRELL: Yeah, I was building a detector that was going to run actually on the SLC, the Stanford Linear Collider, which at a certain point was clearly not going to deliver what I had hoped. And that's when I went to Cornell.

ZIERLER: So you had a very unique vantage point in terms of toggling between Lawrence Berkeley National Laboratory and SLAC. What were your impressions as you inevitably compared the experiences?

DRELL: So I had issues with the SLAC of the mid-1980s. They were issues that several decades later, I would have the opportunity to fix.

ZIERLER: So these were the Burt Richter years, I assume?

DRELL: Burt—was Burt director? He may—I forget the exact time.

ZIERLER: In '84. 1984 is the transition.

DRELL: Right, that was the transition between Panofsky and Burt. So Burt had just started up, yeah. And Burt has been a—was a lifelong supporter of mine. I mean, I owe him a lot. But the laboratory of that time—well, it was the 1980s. Physics of the 1980s had a lot that needed to be fixed too. [laugh]

ZIERLER: So perhaps you could speak generally if not specifically at SLAC about the kinds of issues in the 1980s that needed to be fixed?

DRELL: The issue at SLAC was that it had a fairly feudal culture at that time. And it was a time when—

ZIERLER: That would be F-E-U-D-A-L, feudal culture?

DRELL: Yeah.

ZIERLER: OK. [laugh]

DRELL: And that culture stuck on at SLAC too long, and was one of the challenges I had to come back and deal with when I returned in the 2000s that it was—

ZIERLER: Do you mean the hierarchy was too rigid?

DRELL: It—how to describe this best? There was a structure that had served SLAC incredibly well in its founding of very strong groups. These were the SLAC letter groups: Group A, Group B, Group C. There would be a PI, you would call them now, but then they were called the group leader, a Burt Richter or a Martin Perl. Those group leaders would have the resources to hire staff scientists, postdocs and graduate students, and the groups got funding from the lab director, and they got to do what they wanted in terms of the science.

The world changed in the '80s and '90s. It was no longer, you know, the government just sends money, and we'll do good things with it. There became issues of accountability and alignment with the priorities of the national program. There became different ways of doing business. SLAC didn't get the memo. And those—that group structure challenged SLAC's evolution. And it's one of the things that made it painful to evolve later on.

ZIERLER: So how might it have—would—how could it have changed that would've, you know, prevented these issues?

DRELL: Rewriting history is dangerous business. I would say that SLAC was so successful in the '60s and in the '70s and on into the '80s because of the group system that existed. But there was perhaps an arrogance that comes with tremendous success. And then when the SLC turned on, the turn on did not go all that well. It was an extraordinarily challenging machine to operate. It was breakthrough new accelerator physics and technology. And it was hard to be competitive in producing science with the more traditional LEP machine at CERN running in the same energy range.

DRELL: There are a few precision measurements that SLC made that are great measurements that will stand in the history books. But the—

ZIERLER: Did SLAC not see what LEP represented?

DRELL: No.

ZIERLER: Why not? Arrogance? Resting on laurels?

DRELL: It is easy to say that arrogance might be part of it. But in reality, the hardest thing to do is to evolve forward from success that is so dramatic as it was in the early days of SLAC. But the world changed. And it took SLAC a long time to realize that the frontiers of the field were ultimately going to move elsewhere.

ZIERLER: How much of this had to do with the fact that SLAC had been so single-focused on one area for so long? Was that part of it, or was it really more cultural?

DRELL: I actually think they're both parts. I give Burt enormous credit for recognizing that the future of the lab was not going to depend on one single HEP program and he made the synchrotron light source part of the lab instead of having it be a separate entity. Burt is the one who pushed for getting the Linac Coherent Light Source because he knew that on-site particle physics accelerators would not remain forefront machines at SLAC forever. I give Burt great credit for seeing the future with a clarity that almost no one else did at that time.

But in the 1970s and 80s, the heart and soul of the lab was high-energy physics. There was going to be an ILC some day in the central valley. They were going to—you know, they were SLAC. They didn't have to play the game by everybody else's rules.

ZIERLER: Right. And just to orient ourselves in terms of the narrative, these are sentiments that you're thinking about prior to you joining Cornell, or did you sort of stay on the radar in terms of what was going on at SLAC even when you were in Ithaca?

DRELL: Oh, I stayed on the radar or perhaps I should say, SLAC stayed on my radar. I talked to my father, right. And in that period when I was at Cornell, there was a bitter, bitter battle between Cornell and SLAC over the siting of a U.S. B-Factory. That was really painful because it was clear that the DOE would never build a B Factory at Cornell. And yet we were being told to compete, so you compete. It was a lose-lose proposition.

ZIERLER: Right. And in terms of your hire at Cornell, what did that represent in terms of what the department wanted to emphasize where it wanted to go? How did you understand that?

DRELL: When I was hired at Cornell?

ZIERLER: Yeah.

DRELL: What did it represent? They had a—pretty much everybody at Cornell worked on CLEO and CESR, the machine at Cornell. I was going to work on CLEO and CESR, which was fine. I was eager to get some data because there hadn't been data at SLC before I left. And I was a hardware jock at that point, so I was going to work on hardware, although I actually didn't end up working on hardware. I ended up doing mostly data analysis.

ZIERLER: Did you come in tenured to Cornell?

DRELL: No, this was my first assistant professor appointment. I'd been a postdoc for three years—three and a half years.

ZIERLER: Did you start taking on graduate students of your own right away?

DRELL: Yep, I interviewed at Cornell with a 6-week-old under one arm so—

ZIERLER: [laugh]

DRELL: They were really, really supportive about that. I got there. I started teaching. I started taking on graduate students. I didn't like to travel. And then kid number two came along, and ultimately kid number three came along. And I discovered I could not only supervise my graduate students, I could supervise other people's graduate students because I was always around when people were out traveling. I had quite a wonderful group of students and postdocs there.

ZIERLER: And how did you split your time in terms of graduate teaching, undergraduate teaching, lab work, and, you know, family whenever you could fit that in?

DRELL: The honest answer is I was really busy, and I don't remember those years very well.

ZIERLER: [laugh]

DRELL: But Cornell helped made it work for me. In my offer letter, and you have to remember this is before family leave and policies like that, they offered that I could go to part-time any semester I wanted, and not teach. And so I taught whatever needed teaching when it needed teaching, because I was so grateful for that degree of flexibility, and every fourth semester or so I took advantage of the part time offer. And I don't know how I split my time. I just did it.

ZIERLER: Yeah. And in terms of your research, what were you working on in those years?

DRELL: Well, we turned on—CLEO II was a fantastic detector, and I was doing measurements of the CKM Matrix, V_{cb}, precision measurements of quantities like that. I did some measurements of the tau lepton branching ratios. It was really fun because in the 1990s we had almost an order of magnitude more data than anybody else, and anything you look at is fun with that much data.

ZIERLER: Yeah. And, again, the comparison question because you have such a unique vantage point. When you got to Cornell, I mean, obviously Cornell has its own major history, right, what were the differences in terms of how physics was done at Cornell versus Berkeley? The kinds of things that were emphasized, the kinds of things that got funded?

DRELL: Cornell's a really interesting place. It was quite unique in that high-energy physics was almost half the department between experiment and theory. And pretty much at that time all the experimentalists worked on the machine there.

And what I always really liked at Cornell was the way they treated you—you come in day one as an assistant professor, and they treat you like you're the future of the department, and they actually listen to you. I can remember an early faculty meeting talking about the need to replace Maury Tigner, who was retiring, with a faculty hire. And it's like, "Oh, that's a really good idea. We better go take that seriously."

And I was like, wow, they're going to take me really seriously. It was really good. A group of young faculty were hired in around the time I was. We worked together. In the end we felt like we could just own the future, and change the future, and we did—it was a good feeling.

ZIERLER: So between that sentiment of being valued as an assistant professor, and also your unique arrangement of them being very generous in terms of your family obligations, were there any administrators or faculty members who you felt were really your champions and greatest supporters during the Cornell years?

DRELL: Supporters? I always felt everyone was supportive. There were people—I mean, some of the members of the department I've stayed in close contact with. I've always said I left part of my heart in Upstate New York. And there were people I learned a lot from. Top of mind is Peter Lepage --- I learned so much physics from Peter. He stays—is a great friend. But it wasn't like I had a mentor or anything like that. I'm not sure I ever wanted a mentor.

ZIERLER: So what did you learn from Peter?

DRELL: Oh, lots of physics. There was a new theoretical construct, heavy quark effective theory, and I can remember—I don't know how I talked him into doing this. But we had Peter give us a series of lectures to the experimental group, and we set aside a week or two weeks, and for several days, and for several hours each day he helped us understand the new theoretical framework. And by the end of that, I felt like we, the experimentalists, understood the framework of the theory really deeply and really well. So that was the sort of thing you could do there.

And then—towards the end of my time at Cornell, we had to really reframe the future of the physics program there to focus on charm physics because we couldn't compete with the B Factories at SLAC and KEK. And, again, with Peter and some of the other theorists but really led by Peter, we came up with an experimental program that would use the experimental data we could get from CLEO, and pair it with the innovations in lattice gauge theory to make precision

measurements, and test those precision measurements in ways that no one had thought of. We were able to come up with a program of experimental measurements and theoretical calculations that you could then test theory with experiments to great precision. It was very deeply satisfying.

ZIERLER: Did you enjoy undergraduate teaching during your Cornell days?

DRELL: I loved it, yeah.

ZIERLER: What kind of courses did you teach?

DRELL: What did I not teach? I taught the big engineering service courses in E&M and waves many times. I taught the undergraduate particle physics course. I taught advanced undergraduate mechanics.

That was a lot of fun. I really learned advanced mechanics at that time by teaching it. And then I think at the end, my last couple of years, I was teaching first-year graduate quantum mechanics. And there were other courses in there too.

ZIERLER: And in terms of your graduate students, are there any that particularly stand out as among your best?

DRELL: I had some excellent students. Ken Bloom who is faculty at Nebraska now and Andy Foland who is at L3 technologies were two standouts. Adrian Cho is a writer for Science magazine. My students have gone on to a variety of careers. They're almost all in physics but most are no longer in academia.

ZIERLER: And what was the decision process that led you back to Stanford in 2002?

DRELL: Oh, that was really, really hard. Any decision that has two careers, three kids, two sets of aging parents, is hard. I think that—

ZIERLER: So it was a matrix of decisions? It wasn't any one thing?

DRELL: No, it wasn't any one thing. But I think that professionally maybe the future at Cornell was a little too clear. And coming back to SLAC, it was clear that the lab didn't quite know what its future was.

ZIERLER: So it sounds like there was something that was problematic about how clear your future at Cornell would have been?

DRELL: Well, it was problematic in the sense that I like uncertainty. So, you know, just being able to see the next steps a little too clearly is unsettling.

ZIERLER: Did you see joining the Stanford faculty as coming home?

DRELL: No, not really.

ZIERLER: It was a new experience for you?

DRELL: Yeah, yeah.

ZIERLER: And you came on as director of research at SLAC. Was that a package deal that you joined the physics department and SLAC at the same time?

DRELL: No, I wasn't in the physics department. I was just on the SLAC faculty.

ZIERLER: But you had the title of professor, but professor at SLAC?

DRELL: Professor at SLAC, right.

ZIERLER: OK. Did you have any institutional affiliation with the physics department?

DRELL: No. I mean, I knew people and it was—we had a joint department meeting with the SLAC faculty and the physics department faculty, which was I think the first time that had happened since my father had quit the physics department. That was kind of fun. So, I mean, I knew people. But, no, I did not have any affiliation with the physics department. That only happened when I stepped down from being lab director in 2012.

ZIERLER: Right. So now we can get to really the meat of the interview, which is, you know, you have as entering as director of research in 2002, you have all of these—you know, from 15 years prior, all of these pretty strongly held ideas about the way SLAC should be going about its business, and the way it shouldn't. And so now you have an opportunity to act on these ideas. So I'm curious—you know, I guess at the beginning, did you put together a game plan? Did you sort of take it more day by day? What was your plan going in?

DRELL: So it's kind of interesting. I certainly didn't have a plan going in because I just knew that the lab was evolving, and that was an exciting opportunity to be part of the laboratory evolving. I did not enter with a clear opinion about the older culture needing to evolve. That became apparent after I arrived.

ZIERLER: And how was it evolving?

DRELL: Well, it was moving into astrophysics. The Gamma-ray Large-Area Space Telescope was being constructed. (Once launched it was named the Fermi Gamma-Ray Space Telescope)

They'd had a fairly important review just before I showed up that had not gone all that well. So that was a major challenge.

The SLAC B Factory was—it was in tight competition with KEK. But it wasn't quite clear what the next steps were going to be. The ILC people were hoping for the linear collider. And then the LCLS was starting to ramp up.

What was fascinating is I think back at this—I don't have it exactly at the tip of my tongue. But I actually gave an all-hands when I showed up sort of to introduce myself to the research division, as it was called. And it's fun to go back and read because it actually is a bit prophetic in what was about to happen to the lab that the lab didn't know. [laugh]

ZIERLER: What was about to happen to the lab that the lab didn't know?

DRELL: Well, where were we going to go with the astrophysics after GLAST? What was the future of the accelerator-based particle physics program? Were we always going to keep doing the things the way we'd always done them, or were we going to change? That's the theme that goes all the way through it. And the answer is we were going to change, and it was sort of figuring out how to have that—help that happen.

ZIERLER: Now, in terms of conveying this message, if you can give a sense of the hierarchy, where is the director of research in the scheme of things at SLAC?

DRELL: It's an associate director. It's an associate lab director. Now they're called associate lab directors. But SLAC had its own title for everything.

ZIERLER: Right. [laugh]

DRELL: And one of the things which irritated the Department of Energy to no end was that I was called the research director. But that was really only for particle physics research, and totally ignored all the photon science research, which, you know, was under the SSRL director and he didn't get the fancy title of 'research director'. So it was very much of a class system with the particle physicists and the photon scientists.

ZIERLER: So in terms of the lab being in a period of evolution at this point, did you see it as sufficiently dynamic that you coming in at that time had the opportunity to really shape how the lab evolved? Or you had inherited evolving projects already underway, and the best you could do was guide them the best that you could?

DRELL: So, first, I wasn't that thoughtful or self-aware or anything. It just it seemed like an exciting time. I really liked the idea of the particle astrophysics because I had taken a sabbatical and spent a year doing particle astrophysics, and was thinking of transitioning my own research interests. I didn't think that the B Factory was as much of a future as the rest of the lab thought. I thought it had a finite runway.

ZIERLER: And you were correct in that, more or less?

DRELL: And I ended up being correct in that.

ZIERLER: Yeah. Why so? Why do you think you got that one so right?

DRELL: Why did I get that one so right? Because I'd been on some national committees at that point, and I could see that the rest of the field didn't think the B-factory physics was all that important or exciting. I mean, it was really important to measure the CP violation, and that had been done. But I just had—maybe it was part of—being part of that bigger ecosystem.

I just saw where the field was going. And I knew there were people at SLAC who thought there should be a Super B Factory and super luminosity machine. And I just didn't think the field would support it. And I also felt that by the time we were in the 2000s, SLAC couldn't just decide to do something on its own. The whole field needed to be involved.

ZIERLER: So, coming in with these ideas, I wonder if you can contrast your experience at Cornell? Obviously you're more senior when you get to SLAC. But did you feel like—I mean, how were you regarded? Did you feel like your ideas were taken seriously from the get-go and that they were acted upon, or there was some transition in that regard?

DRELL: I have no idea. I mean, it's very difficult to say what people would have thought when I came in as the research director. What—I mean, time gets compressed. I showed up in the spring of 200…

ZIERLER: '2?

DRELL: …2. By 2004, the Gamma-ray Space Telescope project is in deep trouble, and I've fired the project manager, brought in a new project manager. He is struggling, and I've gone into the project as the deputy project manager to get the project on track.

ZIERLER: What's the problem? What's going on? Why all the trouble?

DRELL: They can't make flight hardware.

ZIERLER: This is a technical problem?

DRELL: Yeah, it was all—it was—we were supposed to make 16 towers, and they couldn't get the engineering models to work. And whereas part of the lab had the idea or the attitude that you

could—well, we never really wanted that project anyway, so let it fail. I guess I knew enough to know that, first, if we, SLAC, let a project fail, we would never get another project for a very long time. Secondly that it would damage the department---the Office of Science and the DOE, and you don't damage the department.

And so I had to fix that problem. Some in the lab just didn't quite get it. And I didn't come in with any brilliant technical ideas. But I could use my position as director of research, and because I knew everybody at that point, to be able to pluck people out of what they were doing, and say, "Look, I really need you for the sake of the lab to come over and spend the next six months of your life trying to figure out why these PC boards are breaking down or something."

ZIERLER: So how did those efforts turn out?

DRELL: The Fermi Gamma-ray Space Telescope is still up there in low Earth orbit—

ZIERLER: [laugh]

DRELL: —and it has operated flawlessly since it launched.

ZIERLER: And would you be so bold as to take most of the credit for that?

DRELL: Absolutely not. Absolutely not.

ZIERLER: Where does that credit go?

DRELL: Credit goes to the technical people who had some brilliant ideas, and a whole host of project people who worked tirelessly to get it done.

ZIERLER: Now, broader question: what do you think it said about SLAC and the field in general that they adopted this interest in astrophysics? What was that about?

DRELL: What was that about? Well, to some degree, there's—what I thought it was about is that by the time you get to 2001, 2002, you've had 30-plus years of the standard model being absolutely perfectly right in everything it's predicted. And then there's this huge surprise that the Standard Model is only describing 4% or 5% of the universe because we missed all the dark matter and dark energy. And we're not going to learn about the dark matter and the dark energy just from accelerator based experiments.

At that point, we could say we might learn about it at the LHC. But it was a great unknown to explore, and we were going to have to use tools in space and underground. And so it was time to branch out into new approaches—there's two ways people pursue physics. Some of them pursue a certain kind of technology like accelerator-based physics. And others will pursue the science problem.

And I'll go back to what I said about Gene Commins. What was inspiring about him is he went after the science problem. And the science problem for high-energy physics was not just exclusively going to get solved with big accelerators. You had to go into space. You had to go underground. And so that—I thought that was super exciting.

ZIERLER: So SLAC embracing astrophysics, did you see that as a departure from SLAC's initial mission, or was that a natural progression of where things had to go?

DRELL: Oh, I thought it was a totally natural progression. And both Burt and Jonathan Dorfan get credit for moving in that direction. One of the first things I did when I came to SLAC was to

help recruit Roger Blandford and Steve Kahn, and get the Kavli Institute gift to set up the Kavli Institute. It was a joint institute between the physics department and SLAC, and it was a huge step forward for Stanford. That was Jonathan's idea along with the chair of the physics department, Steve Chu.

And then, secondly, with GLAST, Blandford, Kahn, the Kavli Institute and projects like LSST that followed on, it established us as a center for particle astrophysics. Which, I mean, we'd had individually good people, but we'd not had a coherent program. And that recruitment really changed that.

ZIERLER: And in terms of building that identity, was that an inward-looking process, or were you and your colleagues looking at other institutions, other labs, as a model to emulate?

DRELL: Hmm, I can't answer that because it was underway when I got there. But you will note that Fermilab has a big particle astrophysics program. Berkeley has a big particle astrophysics program. So I don't think it was unique at Stanford.

ZIERLER: And when you're named deputy director, are you made to understand that this was a stepping stone to assume the directorship?

DRELL: No, I have always said, and so I'll repeat it for you, that you need to understand [laugh]—if I had known I was going to be a lab director, I would never have left Cornell.

ZIERLER: [laugh]

DRELL: OK? I had—already at that point, people had talked to me about lab director jobs, and I knew it was a job I didn't want.

ZIERLER: Because you're no longer going to be a physicist?

DRELL: Actually, it's not just that, it's that my—I had this conversation with a senior DOE official when I was being asked to be lab director. It's that I felt that lab directors in the DOE system seemed to have a mission to preserve their labs. And I didn't accept that mission.

If I felt that it was in the best interests of the science and of the nation, I would shut a lab down. And I would be willing to do it. Now, that's all one thing to say. It's harder to do. But I did not want to be boxed into a job where my main mission was to preserve the lab. So when I—

ZIERLER: So did you use your time as deputy director—I mean, obviously you didn't know you were going to become director, but you accepted it as a possibility. Going in with that, did you sort of pave the way to not being boxed in when you did become director?

DRELL: I became a deputy lab director when Jonathan Dorfan restructured the lab management—he appointed two deputy directors. There was me and Keith Hodgson, and a chief operating officer. And I think that was after the—we'd had this terrible, terrible arc flash accident. A contractor was almost killed.

And at that point, the LCLS project was in trouble. So there's just lots going on. I became deputy and I was just trying to help. It was—in no way was it a step towards a director. It was just a reorganization of the laboratory to try and align the resources so we could get the LCLS project going. And I had—

ZIERLER: But realistically, your career is ascendant. You're on the way up, and you're named deputy director, right. Where else—what else are you doing beyond that? You're not capping your career as deputy director either.

DRELL: I was deputy director as a service to the lab, and then I told—in fact it would've be the spring of '17—no, excuse me, spring of 2007—'7? Yeah, spring of 2007, I told Jonathan that I was going to step down from being at that point the associate lab director, and that I was—I would stay in the deputy role until the new director was on board to do a transition because Jonathan had announced he was going to step down. And then I was going back to science because, at that point, Fermi was going to launch. And I had done five years in administration, and it had been fun, and I felt I'd accomplished a lot. And I was ready to go back to science. So I was not just saying I didn't want to be lab director. I was making moves to ease the transition for the lab and then I was going back to science.

So that was the plan, and I think it was the same day or the day after we officially named a new associate lab director so I've gotten rid of those responsibilities but I'm still carrying the deputy title, and it's probably August of 2007. And Jonathan walks into my office, and he says, "Look, I think I should step down sooner, and you should step in as acting director." I was chairing the search committee at that point. And then, you know, the search failed, and then I ended up being director.

ZIERLER: So, in retrospect, right, best-laid plans, like of course you became director, right?

DRELL: So I really didn't want to be director. It was not a job I had ever wanted, and then I especially didn't want to be director of SLAC because in 2007, I knew just how hard it was going to be. Because at that point, the lab had really run out of time to evolve to our future. And so it was going to be more revolutionary change than evolutionary change.

ZIERLER: Why? What was the time line that you had run out of? How does that work?

DRELL: Well, the Department of Energy is really mad at us in 2007. They are threatening to rebid the contract for the lab which is a signal that Stanford is not doing its job as the contractor. The LCLS project is a half a billion or 400 million and something project. It is not going well. There's a second project, the LCLS scientific instruments. And there is no good agreement between DOE and the lab about what the scope of the instrument project is going to be.

There's terrible communication problems between SLAC and DoE. And we're going to have to turn off the B Factory. It's absolutely obvious that the B Factory has to come to an end. And so I could see that all those things are going to happen. It is clear we will have to turn off the B Factory or we will not deliver the LCLS. And—I mean, it's just going to be really miserable and hard. So --- I felt the walls closing in around me, I explored a few other options and -

ZIERLER: Sincerely explored?

DRELL: Yeah, yeah, and then—

ZIERLER: Can you share what those other options were?

DRELL: Probably not.

ZIERLER: OK.

DRELL: But the—in the end, it felt like running away. You know, the lab—here I've been at the lab for five years. I've gone into people's offices and looked them in the eye and said, "Look, I really need you to give up what you're doing and what you want to do, and come over here and help." And at that point, it felt—I couldn't stay and not do the job. And then if I left, it just felt

like running away. So, I agreed to be the lab director. I also—just to point this out, I had three teenagers at home at the time—

ZIERLER: [laugh]

DRELL: —just, you know, it wasn't as though life wasn't busy.

ZIERLER: Right.

DRELL: So I agreed to do it.

ZIERLER: So that's a very fulsome answer in terms of all of the problems that you were facing. But I wonder, you know, what strengths at SLAC could you draw on to get through this or to make—to create success?

DRELL: So in the end, the technical strength of the accelerator group was outstanding. But I didn't know we could deliver the technical success of the LCLS when I started as lab director. You have to remember my first all-hands to the staff as Director is the largest layoff in the history of the lab, and it was—yeah, it was just really tough.

ZIERLER: Did you have another all-hands when you became director? How did you get your message out what your agenda and your plan was?

DRELL: Well, there was the—there was an all-hands when I was named director. That was very nice. And then the first week of January 2008, I have an all-hands, and I announce this major layoff. And I announce why there's going to be a major layoff, and I announce we're going to turn the B Factory off early. And, you know, it's not—that's not a really good agenda. [laugh]

ZIERLER: [laugh]

DRELL: It's a—I mean, it was really painful to go—I mean, I did it in phases. I went to the 8 a.m. accelerator meeting to tell them in person because this was their life.

ZIERLER: Did you have a kitchen cabinet that you relied on to bounce ideas off because these are momentous decisions? Or did you sort of make all of your decisions in official channels?

DRELL: No, I had a—I mean, I had my associate lab directors. I didn't have a deputy. I had people like Bill Madia, Pat Dehmer who was in the Office of Science. So I had—

ZIERLER: But these are all within SLAC. I'm talking about your peers in other physics departments, at other national laboratories. This was all internal in terms of your decision-making and conversations?

DRELL: Well, DOE is not so internal. But, the other lab directors --- eventually I developed some great relationships with the other lab directors. But I don't think I was really particularly close to any of them at this time.

ZIERLER: Now, you referenced a few times already your communications with DOE officials prior to you becoming director. How had your communication with DOE changed when you became director? Were you talking to different people? Were you talking about different things?

DRELL: I didn't know a lot of people at the DOE and actually, Bill Madia, Stanford Vice President for SLAC, did a great service to me by taking me to Washington a bunch of times, and introducing me to people so that when there was a problem, it wouldn't be the first time I was meeting them, because there were problems.

ZIERLER: So what kinds of people were you meeting? At what level? In what positions?

DRELL: So the Office of Science hierarchy, Ray Orbach down. And I knew Ray pretty well.

ZIERLER: I'm speaking to Ray tomorrow, as it happens.

DRELL: And then Bill introduced me to some of the people involved in security and safety oversight. And that was really important because we had issues [laugh] that needed to be dealt with. So it was not just the Office of Science, but it was the other parts of the DOE that have to work well with Office of Science that Bill was introducing me to. And then I—

ZIERLER: Now, you were director during the Bush-Obama transition. I wonder if you were attuned to any of the changes that might've affected your relations with a new crowd of DOE people when the Obama administration came in?

DRELL: Well, Steve Chu was the secretary in Obama's first term, and I'd worked for Steve my first summer in graduate school, and we've been friends forever, so that was fun.

ZIERLER: Was it more than fun? Were there material differences as a result of him being Secretary of Energy?

DRELL: Well, I just felt much more comfortable with Steve than I did with Sam Bodman. I mean, Bodman was very nice. But Steve it was a friend. So that was great actually; very helpful. And actually by that time things at the lab are starting to go well. First, the LCLS turns on and lases. That's a miracle, right? In every story, there's the "and a miracle occurred" moment. That miracle moment in my time as director is the turn of the LCLS.

And then—but then there were various budget things. There were three other lab directors and I. We would go to Washington. We would walk the halls of Congress. We would see Steve. We would, you know, figure out how best to help the department. At that point, I sort of knew what I was doing a little better.

ZIERLER: Yeah. Now, I've heard a variety of opinions about the relationship between SLAC and the Stanford campus proper, right. So I wonder what your feeling was about those relations, and how you may have wanted them to change during your tenure as director?

DRELL: So, I think they have changed over time. I actually give the greatest credit to the Kavli Institute, and that got people talking together. And then as we became more of a multi-program lab, it was no longer just that the only place SLAC plugged in was the physics department. It was chemistry. It was biology. It was structural biology.

And then actually later on when I became dean of engineering, there was this huge opportunity of a relationship between engineering and SLAC. And that's kind of taken off. So I would say that it's evolved in really positive ways over time. And a lot of that has to do with people starting to forget the history a little bit.

ZIERLER: Yeah. Now, in terms of the ongoing evolution of SLAC that you had perceived, you know, since when you came on in 2002, what was the status of that evolution at the beginning of your tenure, and then throughout your directorship? What continued to evolve? What new things did you want to sort of, you know, create that would take on their own evolution, you know, from your tenure, and forward?

DRELL: So, I actually give Chi-Chang, the current director, enormous credit for the links to campus. Because he's been tireless in building those links, and particularly in the areas of science, of photon science, which are his areas of expertise. I think—I'm just trying to figure out—and here I also give credit to Bill Madia.

When I started as director, people on campus didn't know anything about SLAC. They really didn't understand how SLAC worked. And now we have a Board Of Overseers structure. And the Vice Presidents on campus are part of that, so they've learned how the lab works. And it's much less—both sides are much more integrated now. And in particular, campus understands the strange world of the DOE.

And SLAC stops using DOE as an excuse of why they can't do things the way that Stanford wants them to. So there's just—the level of integration and the depth of the understanding is—I mean, it's just orders of magnitude different. And Bill, in his role as vice president for SLAC, was just phenomenal in how he helped that transition over the decade.

ZIERLER: So it would seem to me that there would only be upsides to increasing this integration between the campus and SLAC.

DRELL: Oh, yeah.

ZIERLER: But as it played out, were there any downsides that sort of emerged? In other words, this seems so self-evident. Why had this not happened so much earlier?

DRELL: Oh, that is such a good question. Downsides in that there were—there are cultural differences that just have to be overcome. And you have to believe that the benefit is going to be

worth the investment to overcome those kinds of cultural differences. But I think there's still enormous potential to go, quite frankly.

ZIERLER: So but you need partners on the campus. So who are those partners?

DRELL: Right. Well, a lot of them are in engineering. There are many more researchers on campus who are engaged up at SLAC. And Chi-Chang is engaging campus faculty in leadership roles at the lab.

Kam Moler, the dean of research, is actually also close to SLAC and DOE. So she's deeply engaged now in ways that just didn't happen before. So, right, it's always the people in the end.

ZIERLER: Right. I've heard that time and again. It's about the—I mean, uniquely about SLAC, it's about the people. So many people have said that. So by 2012, this is five years, when do you know that it's time to move on? How did that play out?

DRELL: I know it's time to move on when first we're through the crisis, right. The project's completed. The future—we've got—LCLS-II is on the horizon. Fermi's launched, and is doing well. People have stopped mourning the B Factory. And so we've kind of gone through this terrible crisis. We've got a safety culture that's working. We're not blowing things up anymore.

ZIERLER: [laugh]

DRELL: And it's clear there's still another big step to go. But I felt that—well, it was really a feeling that I had done what I was needed to do. And now another person would be better at taking the next step. And I'm ready to get out of being a lab director, and go back, and teach courses, and do some physics.

ZIERLER: Now, looking back to 2007, you have taken a series of extremely bold steps, visionary steps for what you felt needed to happen at SLAC. And that had—those had massive implications, right. So by 2012, obviously there's a period where you have to assess did you make the right call at the right time? So I wonder if part of their decision-making in 2012 is that, you know, you felt that you had made those right decisions? That you weren't holding your breath on any of these major decisions you made at the outset?

DRELL: It didn't feel like that at all. I think I started at the outset with a crisis, at the bottom of a well. All directions were up.

ZIERLER: [laugh]

DRELL: The DOE had given us a future with the LCLS. All I needed to do was go out and grab it. So it was not that I come in, I have bold visionary ideas, I execute them. It's not like that at all.

ZIERLER: OK.

DRELL: It's we're at the bottom of a well. All directions are up. I just need to do something, anything, and the lab—we're better off than where it started. The DOE has handed us this gift of a project that is transformative, and all we have to do is execute it.

ZIERLER: Yeah. So by 2012, you've gotten there.

DRELL: We've taken the first step. And, yes, there's still tons more things to do. And it's kind of clear we've got to get more science out, and we've got to get more engagement with campus.

But I was—I didn't—I never did want to be a lab director, you know. It had been painful in many ways. Very satisfying. I have no regrets. But I was done.

ZIERLER: So when looking back at your tenure, do you see any single achievement as your greatest, or any moment that is your proudest that stands out from others?

DRELL: No. I mean, I don't think I have the right to be proud of it, but the moment that was really special was the 10 o'clock phone call: "We have a laser." In that moment, that's when I knew it was going to all be OK. Well, it wasn't really right then because it took me two or three days to really believe that the LCLS had turned on and just worked. But then once I really realized it really was lasing, that was—then everything was going to be OK.

ZIERLER: So when 2012 comes along, do you know that you want to stay at Stanford? Is that part of the plan?

DRELL: So I don't have any idea what I want to do next. But—

ZIERLER: So it's a sequential decision? You're committed to leaving before you have your next opportunity in place?

DRELL: Absolutely, yeah, absolutely. It was not I'm leaving SLAC to go to something else. There was no plan. I sort of figured something would come along. Dean of engineering? That really was nowhere on my radar.

ZIERLER: [laugh] This is a recurring theme I'm hearing, there's no plan. [laugh]

DRELL: Yeah, it's probably a good theme, right.

ZIERLER: [laugh] So how did that come about? How did your next appointment at Stanford come about?

DRELL: John Etchemendy, the provost, called me up and he said, "The search committee for dean of engineering wants to talk to you as a candidate." And I said, "That makes no sense." And then I said, "Is something wrong?"

ZIERLER: [laugh]

DRELL: Because I figured, OK, they bring me in when something's wrong. And he says, "No, no, no, nothing's wrong. Everything's great. Committee would like to talk to you." And it was kind of like—and then he says, "If you're 100% sure you would never ever think of it, you don't need to come talk to us. But if you're not 100% sure, you should just come talk to us." And I said, "What the hell," and I went and talked to them.

ZIERLER: Now this idea that, you know, you just want to get back to science, right, it's kind of a double-edged sword because in one sense, you're leaving essentially a purely executive administrative position, right. But then on the other hand, how confident are you that you're going to be able to get back to teaching and research, leaving SLAC and taking on this new opportunity?

DRELL: I actually didn't have any hesitation about that at all. There's an outstanding group of young staff scientists at LCLS, and they know a lot more of the science than I did. But I knew how to get things done, and so we started—there's a new beam line because of that year. We got the design, and I helped get the funding, and that was really fun.

And then we wrote a *Reviews of Modern Physics* article, or they wrote it. I just sort of shepherded the process, and that—it was—I got a couple graduate students engaged. I didn't have any hesitation that I could make contributions because I had skills that were going to be useful.

ZIERLER: Did you see a greater opportunity for more impact as dean in the engineering school than at SLAC? Were they just different ways of—you couldn't compare the two?

DRELL: Totally not comparable, yeah. It was just fun. Something—I like to learn new things. I've never taken an engineering class in my life.

ZIERLER: And clearly, I mean, when you assumed—when you became dean, this was—the metaphor of being at the bottom of a well, and the only way was up, this was not the situation that you inherited?

DRELL: No. In fact, it was really, really interesting because it was exactly the opposite. It was—Stanford's school of engineering -- it's great. You're on the top of the mountain. And everybody's afraid to change things.

So I came in saying: 'I know the great engineering school 20 years from now looks different from the way that we are today'. And it's like, oh, but just don't break anything. And it's like, no, no, no, no, no, I know the great school [laugh] of engineering looks different in the future and we have to get there which means we have to change. And so we started off on some things that have been really fun to watch unfold.

ZIERLER: Now as a physicist, was it—did you ever experience any difficulty to be accepted as one of their own?

DRELL: So I had this conversation with John Hennessy. I was the first Dean not from the Stanford School of Engineering. I was the first dean who was not an engineer, and I was the first dean who was a woman. And so I said, "That sounds like three strikes." And John says, "No, no, no, no, no, you don't understand. They're engineers." And he was right. It was—I spent the summer at meeting all the faculty in the school….and they were wonderful and welcoming.

ZIERLER: What does that mean, they're engineers? It's quantitative? If you deliver, they don't care about that other stuff?

DRELL: They're just—yeah, they're very sort of results-oriented. Get it done. It's not—if I could convince them that together we could do something, that was all that it took. And so, yeah, I had a blast. In fact, I still feel very much that Stanford engineering is a home, which is kind of funny.

ZIERLER: And then how did the opportunity to become provost, how did that come about?

DRELL: Marc called me into his office one day, and he said, "You want to be provost?" [laugh]

ZIERLER: [laugh]

DRELL: So I said, "Let me think about that. [laugh] I'm having a pretty good time as engineering dean."

ZIERLER: And this is obviously a very different thought process because as opposed to transitioning from deputy director to director, a job that you knew exactly what it entailed, you probably had very little idea of what the provost position entailed.

DRELL: Yeah, that's true and provost at Stanford is an immense job actually. It's chief academic officer, chief budget officer. And, as you might imagine, right now is a really interesting time to be Provost because, you know, this year's budget's not going to close, and next year's budget's not going to close. And what's important at the institution to keep going when every…when the world is shut down?

ZIERLER: Right. And I mentioned this in my email to your assistant, and I want to, you know, focus for the remaining time that we have left. You know, talking to people like Bob Birgeneau and Bob Dynes and people like that, I'm curious—a theme in these discussions with physicists that went on to, you know, major university administrative positions is they never let—even if they stopped practicing as a physicist day in and day out, they never let go of seeing the world and problems at their—on their desk through the lens of a physicist. And I wonder if that resonates with you as provost?

DRELL: Yeah, I think it does. I think it does. That does resonate. I'm trying to think. I mean, there's a couple of simple things. You always take limits, right.

When somebody suggests something as a good idea, you say, "Well, if that's a really good idea, and I take it to the limit, is it really a good idea still?" that kind of stuff. Break problems down into little pieces, and then put them back together again. Those are all very sort of standard physics things to do. What else?

ZIERLER: What do you see as your most important responsibility as provost?

DRELL: Attracting the most outstanding faculty and the most outstanding students, and supporting them to be successful.

ZIERLER: And being at Stanford, that job isn't done for you just by virtue of it being Stanford?

DRELL: Oh, no. Let me give you a very specific example, which goes back to something we touched on earlier, and I'll give an example from engineering. I show up as Dean and we do some strategic planning, and the young faculty tell me diversity is a huge issue. So I start looking at that issue. And I see that our engineering departments at Stanford are less diverse than, say, MIT or Berkeley or Caltech.

And I'm going, hmm, gee, you know, how did they do that, and we didn't? And what I discover is that the faculty haven't internalized that the future is diverse. You know, I just go in my classroom, the future's diverse. So the greatest—

ZIERLER: You mean you see it among the students? That's just what it is?

DRELL: Yeah, oh, my students. Yeah, my classroom is incredibly diverse. And then—and you look around the world. The future is diverse, period. Not a question. And so then you say, well, if I want to recruit the best faculty and the best students 20 years from now, I better reflect some of that diversity.

That means I need to start today or I will not be a great institution 20 years from now. I will say that that was something that had to be pointed out to people. So the danger of being Stanford is you're can be a bit arrogant. And you say, "Well, if I offer somebody a job, they'll come. You know, that young woman, she was good but let's let her go somewhere else for two years first. And then if she's really good, we'll bring her to Stanford because we're Stanford and we can get her."

Oh, yeah, with our cost of living here, very, very difficult, two-body problems, very, very hard to solve. No, no, no, we can't be that arrogant. We need to look for potential. We need to have to take risks and bet on potential. So that's the way in which being Stanford can be actually a hindrance, not a help.

ZIERLER: Right. I want to pivot a little bit because you have a unique vantage point in terms of, you know, your expertise in science but also your long tenure, your career in administration. And, you know, with the coronavirus right now, there are layers of crises. There are multiple crises.

And I wonder if you can comment on the scientific crisis, the crisis in science, and the crisis in the disconnect between what it is that scientists do and how they communicate that with the broader public that is sort of at the heart of so many problems that we're facing right now. While, you know, today states are reopening, and now we're looking at double death rates, you know, for the next—you know, these are clearly both scientific crises for how we got into this problem in the first place, and they're also crises of communication. And so I wonder if you could speak broadly not specifically on this topic and your involvement in it but structurally how the situation came to be where we are now?

DRELL: So, I think over decades to some degree—I mean, it's documented—the public has lost trust in institutions, and they've lost trust in science. So we are not viewed the way we were perhaps in the '60s or the '70s as the source of all solutions.

So what happened? I don't know how to look at it except from the perspective of what are the things that we as scientists could've done better? And I think we—we've gotten into bad habits

of over-promising and under-delivering. Some of that has been—there's a feedback loop with the federal funding mechanisms.

But I do think that that is actually where the reset is needed. So I actually—I feel there's a lot of culpability on scientists for where we are. And that this lack of—you know, it's the—what—in the DOE system, we used to always say, if one lab—something went wrong in one lab, one lab caught a cold, the entire lab system got chemotherapy, right. Somebody somewhere makes a—you know, buys a Mustang on a credit card, on a p-card, and, you know, there's new rules for the whole system.

We live in a world where one scientist acting unethically, one lab over-selling their results, actually damages the credibility of the field. And I find it deeply, deeply disturbing. You know, when did we as scientists go into marketing instead of just sticking to our science? It's a very difficult problem to unwind. But that in my heart is where I'm most disturbed.

ZIERLER: Well, isn't so much of that and yet—you know, I talked to physicists at NIH, for example, and I asked to compare their day-to-day with those of academic—their academic colleagues. And the biggest thing they say is, "We're not writing grant proposals all day." Right. So I wonder if that's part of it in terms of the rhetorical question, "When did we get into marketing?"

DRELL: Yeah, I think that—

ZIERLER: And so what's the solution then?

DRELL: Well, there are many who—well, they're almost all too old now to even say it. But you could go back to the good old days, right, where you trusted people, and you sent—you gave

money to smart people, and they did good things. That's still to some extent the Max Planck system in Germany.

I don't really think in today's demands for accountability that's going to work. The—I actually think where universities and philanthropy, there's a real niche for them is—and particularly at places like Stanford is to provide the resources for the crazy ideas where you can actually afford to have 99 out of 100 fail just because that one that succeeds is so wonderful. And if you think about it, that is what universities can deliver to the world.

We do get superb faculty here. We do get superb students and we can give them the flexibility to explore new ideas without the need to oversell so that they can actually get things far enough along that they then can then sell it with integrity to the granting agencies, if you will.

ZIERLER: Right. Well, I'm sensitive to the time. I know it's limited. We have only a few more minutes left, so I'll limit it to two final questions. The first is what message do you have to undergraduates in particular who are looking at who knows what for their future?

DRELL: You know, I'm a professor. I have students, and they are scared. They are despondent. They feel like the rug has been pulled out from under them, and they're looking for answers.

ZIERLER: Right. What do you have to say to them? What's the message right now?

DRELL: So there's [laugh] two parts. The first part of the message is that it's really, really hard to be in a pandemic that is going to be the defining event for their generation. And they're being told that the best way they can contribute is to sit at home in their parents' basement. OK. So first recognize that there's just a real problem with that. And then I would say that this pandemic is only exacerbating and even more exposing the inequality that exists in our country.

And their generation is the generation that needs to take some responsibility for fixing that. And you don't fix things by telling other people what to do. You fix things by going out and doing it. So I think it's a call to arms for that generation. And, no, it's not going to be as easy as they might've hoped it was going to be. But that's the problem that needs to be solved.

ZIERLER: Optimism is a reasonable thing to feel? That's not just selling them a bill of goods? They can be optimistic?

DRELL: If you don't have optimism, why bother? You have to have optimism. You have to believe you can make a difference.

ZIERLER: OK. So last question. I'll bring it right back to physics. In your long and celebrated career as a physicist, and then in physics and national policy, what are you excited about for the future as a physicist, as a champion of physics, as someone in a position where you can continue to shape the dialog at a national level about where physics should go, and the kinds of things that should be supported?

DRELL: Well, in my heart of hearts, I would just really like, before I die, to know what the dark matter is.

ZIERLER: [laugh]

DRELL: And in my heart of hearts, I'd like to know before I die—I'm not going to know what dark energy is, but at least I'd like to know what the right question to ask is about it. [laugh] And those are just such compelling questions, such compelling questions. How can we sleep at night without knowing those answers?

ZIERLER: Right. Leonard Susskind shared with me that he wants to see the inside of a black hole. You think that's a fair game plan to answer the questions that you just raised?

DRELL: No.

ZIERLER: What do you see?

DRELL: I just—I'm an experimentalist, not a theorist. I just see a lot of experiments that we're going to go out there and do a lot of measurements, and there's a lot of hard work, and it's not glorious, and you don't get to say you're seeing in a black hole. [laugh] You just got to go do the work. But we'll get the answer.

ZIERLER: Well, Dr. Drell, I'm honored to have spent this time with you. I truly appreciate it. I want to say on behalf of the American Institute of Physics all we do every day is champion the work that physicists do, and it's been a great pleasure spending this time with you. And on behalf of everybody at AIP and at SLAC, it's really so nice that you were able to do this, so thank you very much.

DRELL: Well, thank you because this has been like a total distraction for an hour and a half. [laugh]

ZIERLER: Yeah. [laugh] And I'd like to say with of these interviews that I do, if we have not sort of—except for my last question about coronavirus, if we have not thought about that and all the other problems for just a little bit of time, mission accomplished.

DRELL: Right.

ZIERLER: So I'll cut it there, thank you.

[End of recording]