NBWM **by David Zierler**
Lowell Klaisner **18 May 2020**
Videoconference

DAVID ZIERLER: OK. This is David Zierler, oral historian for the American Institute of Physics. It is May 18th, 2020. It's my great pleasure to be here with Mr. Lowell Klaisner. Lowell, thank you so much for being with me today.

LOWELL KLAISNER: OK.

ZIERLER: OK. So, to start, tell me your current title and institutional affiliation.

KLAISNER: Retired.

ZIERLER: Straight retired?

KLAISNER: Yes.

ZIERLER: What about your most recent title and affiliation?

KLAISNER: Well, I retired in January of 2010 from SLAC, and I was—I'm not sure of the last title I had. I was in the management of the laboratory. Anyway, I retired from SLAC in 2010.

ZIERLER: But in terms of your own company, you have a management consulting company.

KLAISNER: Yes, that was meant to be a way to keep active and keep in the field, but not—I didn't want it to get to any more than about 10% of my time because I was planning on retiring. [laugh] So I have been serving on a group at SLAC that reviews current projects on a monthly basis. And then I've done some work at Berkeley. I was reviewing projects on the ALS-U, the Advanced Light Source Upgrade, and other opportunities as they came along too.

ZIERLER: Well, good. So, let's take it right back to the beginning. Tell me a little bit about your family background and your early childhood.

KLAISNER: OK. I'm a Californian. I was born in San Francisco, December 30th, 1938. And my mother was born in Palo Alto, so I'm really a Californian. And I grew up there in Daly City, which is immediately south of San Francisco.

ZIERLER: What about your father? Where'd your father grow up?

KLAISNER: He was born in Chicago, but grew up in Billings, Montana. And then he moved to California, and met my mother, and that is how we ended up here.

ZIERLER: Did you go to public schools as a kid?

KLAISNER: Yes, I did. And one little anecdote from that, I had one very good teacher in high school in advanced math, Donnel Behm. But the science education I got in high school wasn't [laugh]—it was characterized by the fact that the high school physics teacher asked me to teach the electricity and magnetism section of the high school class.

ZIERLER: [laugh]

KLAISNER: And of course, I was young, and leaped in and did it. [laugh]

ZIERLER: When did you figure out that you had a talent for math and electronics?

KLAISNER: You know that sort of creeps up on you. I'm not sure. I—you know, when I look back, and I see there's always been an aptitude and interest there. My father was a pressman working on printing presses like those used for newspapers. And he was very talented and innovative mechanically.

And I participated in some of this. We rebuilt a car engine and things like that. So, I had that sort of exposure to creativity and mechanical things. I don't know. I guess in high school, math came easy to me, and science for that matter. And, as I said, it creeps up on you.

I don't know particularly—I was pretty sure when I started at Stanford that I wanted to be an engineer. And if you remember that environment, I graduated from high school in '56 that was the Sputnik era. And there was a big push in this country that we'd been beat by the Soviets in this key area, and we needed to upgrade the mathematics and science education. And so there was a push for upgrading the science and mathematics curriculum.

ZIERLER: Now, did you apply to a lot of schools, or was Stanford—that was it for you?

KLAISNER: [laugh] That was it. In retrospect, that was foolish because I could have easily not gotten into Stanford. But it worked out in the end.

ZIERLER: And going in, you know you wanted to do engineering?

KLAISNER: Yes.

ZIERLER: When did you settle on electrical engineering?

KLAISNER: That goes back a way too, because I had a shop in my basement, and a buddy across the street, and we strung wire across, and sent Morse code to each other. And so, I was fiddling around with electricity from early on.

ZIERLER: Now, is electrical engineering, is that a program within the engineering school? What's the arrangement?

KLAISNER: Yes, it was a department in the School of Engineering. And you have got to go back and try to project yourself back to 1956 when I started at Stanford. There were two fields there. There was electrical engineering, and there was radio engineering. And radio was what we would call electronics now. And electrical engineering was power distribution and that kind of stuff, heavy machinery. And so, I was in the radio engineering group. [laugh]

ZIERLER: What was the—I'm not that familiar with an electrical engineering curriculum. What were some of the main kinds of courses that you would take for that program?

KLAISNER: Well [laugh] that's interesting too. There's sort of a continuum in my mind from, you know, maybe civil engineering at one end, and theoretical physics on the other. There's applied physics, which isn't very far from engineering. So, it was sort of a mix. And because of Stanford's background and reputation, there was a fair amount of physics mixed in. But the other thing about that this era was the transition from vacuum tubes to transistors.

The transistor had been invented a few years before but was just beginning to become common. So, we took courses both on how to design with vacuum tubes and how to design with the transistors. A lot of solid-state physics thrown in. The tools hadn't really been developed for analyzing transistors, but the basic solid-state physics behind that…

ZIERLER: And why was the transition to transistors such a big deal?

KLAISNER: Well, yeah [laugh] it turns out, you know, the transistor is much smaller and uses much less power, cheaper to make. It has advantages on almost any front. The only advantage the vacuum tubes had for a while was their ability to handle the high power. But as time has gone on, transistors got handling more and more power too. You know, they had a whole radio that was a cabinet [laugh], a piece of furniture. And then it went eventually to things like Walkmans and things like that that you could just carry around, which you couldn't do with vacuum tubes.

ZIERLER: Was there a senior thesis for the program?

KLAISNER: No, there was not.

ZIERLER: Did you think about entering industry after your bachelor's degree, or did you know you wanted to stay on for the master's?

KLAISNER: I knew I wanted to stay on for the master's. I did interview a couple of places, stuck my toe in the water and fortunately, you know, nothing appealed to me. There was one job offered to me by Bell Labs was to be on a ship laying telephone cable under the Atlantic and overseeing the amplifiers that get put in line with the cable. That might've been an adventure, but I turned it down.

IBM had one for communicating by radio to their other installations. But I liked a challenge, and I liked the school. I liked the people I was with. And I think all my friends were—all my—yeah, all my friends were going on to graduate school too, so it's a group activity.

ZIERLER: Did you think when you were entering graduate school that you might have been on an academic track to go on for the PhD, or was it more a self-contained program?

KLAISNER: It was more a self-contained program. It was pretty rare that—you're making a career choice there because if you go on to the academic, you're really headed to academia. PhDs in electrical engineers end up being professors in general and researchers in academic institutions. Working engineers, out in the field—well, how do I put this? [laugh] The master's was well-recognized, and it fit in nice too, so.

ZIERLER: And what does the master's degree do in terms of experience and knowledge, situated between the PhD and the bachelor's degree?

KLAISNER: Well, it's getting you more skills in basic techniques of engineering and electrical engineering, electronics. I took more solid-state physics classes. There was some circuit design, circuit synthesis classes. By that time, there was coding theory, and things to do with information theory.

ZIERLER: And what did you do? I assume you had a thesis project.

KLAISNER: No, I did not.

ZIERLER: No thesis project?

KLAISNER: No thesis project. [laugh]

ZIERLER: Is there lab work, or it's just courses?

KLAISNER: There's lab work, yes.

ZIERLER: What were some of your options available to you after the master's degree?

KLAISNER: Well, you know, I wasn't really sure where I was headed. And I think fortuitously, I went to the placement office at Stanford. They would—at that era, engineers were in demand. And interviewed with people, and I interviewed with a man from Argonne National Laboratory and ended up going there to work on an accelerator.

I didn't sit down and say, "I want to be a [laugh]—I want to design accelerators." It just happened that I ran into a guy that I identified with. He connected with me, and so off we went.

ZIERLER: Was there anything unique to your training or your interests that made working on accelerators a natural fit, in retrospect?

KLAISNER: [laugh] Well, what I ended up doing there, and what they were looking for is somebody to work on the low-level radio frequency system for the Zero Gradient Synchrotron, which was a synchrotron they were building there at Argonne. And so I had experience. I was a ham radio operator. [laugh] I had exposure to radio activities. I had actually worked at Hansen Labs during that period, and got some exposure to microwaves in particular.

ZIERLER: How did you get connected to Hansen Labs? What was the point of contact there?

KLAISNER: You know, I tried to remember, and I can't. I reported to Dr. Shaw.

ZIERLER: [laugh]

KLAISNER: [laugh] I think it was some of my physicist friends connected me. I—you know, it just sort of happened. And so, I was a technician. I was not doing research. I was doing a technician's job. But it was very interesting. This is a common theme in my career, building the apparatus that the scientists use for doing the research.

I was with a group that was generating millimeter waves using spin resonance and that was fun. And the other thing relevant to SLAC [laugh] is Hansen Labs; the same building was where they were developing the klystron for Project M, and where they were doing research for microwave windows. And one of the limitations of those machines was break down the klystron window, the vacuum window.

And so, they had a resonant ring, and they were looking at —they would put all films on the windows and see what happened. And, you know, it was pretty brute force. The mechanisms were not well-known. In fact, the answer was to split the power, and put it through two windows and then, rather than any sort of fundamental breakthrough in windows.

ZIERLER: So, what year did you get to Argonne?

KLAISNER: '61, summer.

ZIERLER: And what was it like? What was—what did it—what was the scene like there when you arrived? What were your initial impressions?

KLAISNER: Well, it's a beautiful campus. Have you ever been to Argonne?

ZIERLER: I've not, no.

KLAISNER: It's much more of—you'd call it a campus rather than SLAC, and rolling hills, and grass, and brick buildings. And it was a multidiscipline lab, so there was a chemistry building, a physics building, a mathematics building. And they were building this Zero Gradient

Synchrotron when I arrived there, the civil construction was underway, and most of the big components had been decided, designed, and were coming together.

One thing about Argonne is that their central computer, their supercomputer was called George that they had built themselves. And that was the scheme in those days. A number of labs had home-built vacuum tube computers, and, you know, were batch processing jobs. And it was at the time it was an amazing computer. [laugh] Today, it's nowhere near what you have in your pocket.

ZIERLER: Right, right. Now this was—you were working on the Zero Gradient Synchrotron at Argonne, right?

KLAISNER: Right.

ZIERLER: So, let's unpack that a little bit. What is zero gradient? What does that mean?

KLAISNER: Well, where to start? If you look at the synchrotrons that had been built up till then, they were all what's known as weak focusing synchrotrons: the Cosmotron, the Bevatron, CERN had one, the Soviets had one or two. And a new principle had been developed, which was strong focusing synchrotrons, essentially you're continuously focusing the beam back on the centerline.

And it had a lot of advantage—potential advantages: a smaller vacuum chamber, therefore smaller stored energy in the magnet, and on and on and on. And the strategy in the US for high-energy physics was to build two machines simultaneously: one at Argonne, the Zero Gradient Synchrotron; and the other was the AGS, the Alternating Gradient Synchrotron at Brookhaven.

And the idea—I think it was a 28-GeV machine, and it was going to set the high-energy frontier in the US. And the ZGS was going to set the intensity. And so to get the intensity, they have a big vacuum chamber. The inside of the vacuum chamber was a meter by 10 centimeters. And the zero gradient has to do with the fields in the middle of the magnets that had parallel poles, and the field was nominally the same all the way across.

And that had some advantages going into higher fields, you delay the onset of unwanted gradients at high fields. In fact, it had a fairly fancy cross section. It had holes that were essentially pre-saturated ion to even out the fields as you got higher. And I think it ran up to 21.5 k gauss or something like that—2.15 Tesla.

ZIERLER: And what was your job title at Argonne?

KLAISNER: [laugh] That's another funny one too. It was an Assistant Engineer. And the story there that I chuckle about is that, you know, the DOE labs are operated through a contractor. And in those days, the contractor was almost always the university. Argonne was operated by the University of Chicago; SLAC by Stanford, and such. That put the lab sort of at arm's length from the government, and there's a lot of advantages to that—I digress. [laugh]

This idea that—so the engineers were, you know, all Engineers, Associate Engineers, and Assistant Engineers, just like the professors (but no tenure) And the engineers weren't too happy about this because here you have a master's degree engineer called an assistant engineer, when in the rest of the world, you know, that would be a technician or something equivalent. So, I think eventually they changed their way of doing it.

ZIERLER: How big was the team?

KLAISNER: Probably 150 or something like that.

ZIERLER: And what was your specific role in all of this? What was your job?

KLAISNER: Well, there is a master oscillator, which is a very technically challenging device. The frequency out of this oscillator must track the magnetic field with a fair amount of precision and not getting any details. So, I started working on that as a design engineer, and we were making good progress.

Issues developed with the high-power amplifier, which put out of 500 kilowatts radio frequency power that swept over 3:1 frequency range (4MHz to 14Mhz). And they were having trouble with that. And I transitioned to that. [laugh] And then when we got that operating, then the machine as a whole started to operate and I was involved in the commissioning of the machine. And then I had the title of Chief of Operations for a while. And I left that and went into other kinds of research activities around the campus, mainly film measuring machines for bubble chambers and spark chambers. The output of those particle detectors are photographs of tracks, and then you have to take that film and measure the images and get the information out of them,

ZIERLER: Now bubble chambers and spark chambers, what's the difference? What is each one good for?

KLAISNER: The bubble chamber is—well, the one that's at—there was a couple, I guess. But the main one at the ZGS was a 30-inch liquid hydrogen chamber. So, the, you know, active volume was this 30-inch disc of liquid hydrogen. And the way they work is just before the beam is sent through, the pressure's released on the hydrogen. And if you didn't do anything, it would boil.

So then as a charged particle comes through, and nucleates boiling along its path, that's where you get the bubbles. And then you photograph it and put the pressure back on before it develops into real boiling. That's all happening on the 100-milliseconds level, and impressive. But they're very precise.

The bubbles themselves were—and you can measure the center of the bubble to tens of microns. It was a very precise measurement. Spark chambers were less accurate but had better triggering capabilities. The bubble chamber, you activate it, put the particles through, and then photograph whatever happened.

In a spark chamber, you put the spark chamber into the particle beam, and then you would have triggers that would—if something happened, they would create this trigger, and apply high voltage to the chamber causing sparks. And the sparks once you get them, there was a rare gas inside so that sparks again would occur along the particle's track. The output was a photograph of the trail of sparks. They're not as accurate as bubble chambers but they're trigger-able.

ZIERLER: Now, by the end of your tenure at Argonne, I see you had become chief of operations by the time the machine was commissioned. So clearly you had been promoted several times over the course of those seven years.

KLAISNER: I think that's true. [laugh]

ZIERLER: So what does that mean—

KLAISNER: I just sort of moved around to new jobs that fit my strengths.

ZIERLER: What exactly was your responsibility? I mean, I guess, my first question is what does it mean when the machine was commissioned? Is this a dramatic moment where it's turned on? What does that look like?

[unrelated conversation]

ZIERLER: I don't know if you caught my question, but I was asking when the machine is commissioned, what does that look like? Is that a dramatic moment where the machine is turned on for the very first time? What does that mean for the machine to be commissioned?

KLAISNER: Well, first there was many stages. There was a linac that accelerated the protons to 50 MeV, and then they went into the ring. So, the first stage was to get the linac to accelerate the proton pulses to the right energy and characteristics to inject into the main ring

And then there was a long process just to get the beam to circle in the ring. And it was due to timing main magnet settings, setting of magnetic lenses, etc.. And then you ramped the magnetic field, and worked to keep the beam in the ring up to the maximum field. And then you put it on a target and get the desired particles from that target. Much of the commissioning time is taken by diagnosing equipment failures and repairing that equipment.

ZIERLER: Now I'm curious, two questions relating to the physics of the synchrotron. So, the first is, from your vantage point, how well associated were you with the basic research questions that the synchrotron was designed to answer? In other words, did you need to understand what the science was that the physicists were looking to answer in order for you to do your job, or was that more of like a stove-piped kind of issue? You were just doing your narrowly focused thing, and you left those questions up to the physicists, or did you really need to understand those concepts?

KLAISNER: Yes, it's a mixture: I didn't understand details the high-energy physics processes. But there was a lot of issues that came up. The one that popped in my head when you asked me that, the experimental area had a very long particle separator, which had a horizontal magnetic field and a vertical electric field, and it separated particles. So you had to know enough about what was going on down at the end to be able to interpret what's good performance and what was bad performance. And, you know, at that interface between the machine and the experiment that there was a need to know the basic principles of what was going on. Lastly, it was important to me to understand the physics well enough to know how this machine fit into the overall international physics program.

ZIERLER: And in what ways did the physicists communicate with you what it was that they were after? In other words, would they say things like, "We need the beam to do this, and so therefore can you"—you know, how were those things communicated?

KLAISNER: [laugh] Well, there was an organization built up, to formalize those communications because you want timely decisions and actions: the machine runs 24 hours a day. So, there was an inter-shift communications and chief of operations. I would get calls in the middle of the night and some physicist is unhappy with his beam.

It was my experience was that everybody wanted the right outcome. So, operations were not contentious except for beam time allocation There are multiple experiments in the experimental hall, and the beam time is divided amongst them. An experimenter has three shifts of machine allocated to him.. First shift starts, and the machine dies.

He's very interested in getting the machine back running. And that's when I would get called in the middle of the night. That machine had a bunk room under the control room. And for a while there when the machine was not running well, you know, we would be there 24 hours a day. Go down, catch a little nap, and come back up.

ZIERLER: And what year was it commissioned?

KLAISNER: 1963.

ZIERLER: Oh, so really for the majority of your time there, you were—I mean, so your time there was split between building it, and then managing it?

KLAISNER: Yes, after operations became relatively routine I took on other engineering tasks.

ZIERLER: So, the day-to-day, how did the day-to-day change with the commissioning as the before and after?

KLAISNER: During commissioning it was a 24 [hour] a day job and after I went back to a 40+ hour a week job. As you commissioned, you run for a while, you uncover problems. And then somebody has got to fix those problems so you can move on.

When I was at the Booster at Fermilab, commissioning started by 10 pm and went until 6 in the morning. And then you had stay around for at least maybe four hours to communicate the problems and interact with the solutions, and then you get back in at 6 pm to continue commissioning. It's a real campaign.

ZIERLER: And would there be some days that would be more successful than others? How do you measure success in terms of, you know, assessing what the synchrotron is doing on any given day? Are you aiming for consistency? What's the feedback for knowing that the synchrotron is doing what it's supposed to do?

KLAISNER: Well, the parameters that people are interested in, I mean, obviously they are interested in what energy is and intensity. But that's not a really big issue. The energy—the issue is how many protons did you put on the target? And because that is related to how many interactions they're going to get. And so it's that intensity, and can it be measured. The other major issue is up time.

There's two measures of that: one is in the machine, you can measure how much charge is rotating around the machine. And then when you extract it, you won't extract for 100% efficiency, so another parameter is what the efficiency of the extraction is. Experimenter wants to know how many protons he's going to get on his target.

ZIERLER: Was there a strong culture of visiting scholars at Argonne? Would people come in and visit and see what was going on from other institutions, other universities?

KLAISNER: The experiments were typically collaborations that involved multiple universities. There would be a lead scientist who was the spokesperson for the experiment, and he was communicating with the operations group . So from that point of view, you had, people coming from all over the world to do these experiments. People just dropping by and looking, that happened, but that wasn't the biggest activity.

ZIERLER: And then when did you know it was time to move on to Fermilab? How did that come about?

KLAISNER: Well, building an accelerator is a lot more fun than running one, at least for me.

ZIERLER: Why?

KLAISNER: Well, you get to design things. You get to make it work. And there is a real satisfaction of given an engineering problem, coming up with a concept, fabricating the device and it works. That is part of my motivation is a piece of hardware that's doing what it's supposed to do as a result of my activities.

It was a little risky at the time. I was in the first 100 employers at Fermilab. And at that point, they were on the 10th floor of an office building in Oak Brook, Illinois, so the land was still being bought and it wasn't a foregone conclusion that this was all going to converge.

ZIERLER: [laugh]

KLAISNER: And so—

ZIERLER: Now were you recruited? Did you answer an ad? How did that work out?

KLAISNER: I didn't answer an ad. It must've been a personal connection, you know, knowing somebody who knows somebody there. I went to work on the Booster. And Arie von Steenbergen from Brookhaven was at Fermilab, and he was in charge of the Booster. So, he was the one who hired me, but I don't remember the story. It was a good choice. [laugh]

ZIERLER: It must've been an exciting time to really be present at the creation of all of this.

KLAISNER: Oh, absolutely. It's—for one thing, there's money around. [laugh] And so, you know, you're not sweating budget every day. But the technical challenges and, yes, it was a very exciting time. And of course, R. Wilson, the lab director, was a real character. [laugh] If it wasn't interesting he made it interesting. [laugh]

ZIERLER: And were you brought on specifically to work on the Booster synchrotron? Was that—?

KLAISNER: Yes.

ZIERLER: That was known from the beginning?

KLAISNER: Yes, I had the title chief electrical engineer, and there was a chief mechanical engineer.

ZIERLER: Right. And what was your understanding of how this was going to be different than the synchrotron that was in Argonne?

KLAISNER: Well, it was different in lots of ways. One was the Booster's a rapid cycling synchrotron. It runs at 15 hertz, and so it's a resonant magnet that runs at 15 hertz. It had a much smaller aperture. The beam dynamics were quite different because the beam was only in the machine for a 30th of a second. So some of the instabilities in synchrotrons take longer to grow , and so you didn't see them.

ZIERLER: And why call it Booster? What was being boosted?

KLAISNER: [laugh] Protons. It was another sequence of accelerators, and I'll describe what it was when I was there. There was a linac again, very similar to the one at Argonne. I think it was 50 MeV at the time. They've extended it now. It's something higher anyway.

And then the beam would enter the Booster for a 30th of a second [laugh] and then was transferred in the main ring. The main ring was had a mean radius of a kilometer, and so it would take a number of booster shots to fill the main ring. So that's—it boosted the energy from 50 MeV to 8 GeV.

Anyway, it boosted the energy from whatever the linac energy was up to the injection energy of the main ring which is how low you go in magnetic field in those magnets and still have good control because of the residual magnetism. So you can't just inject from the linac into the main ring at [laugh] 50 MeV.

ZIERLER: And I know that it was new, so that's an obvious difference. But in what ways did Fermilab differ from Argonne culturally in terms of the kinds of people who were there, the ways that they interacted with each other, the larger—its larger place within the national laboratory system? What were your impressions of how Fermilab was different?

KLAISNER: Argonne was [laugh]—lots of things come to mind. [laugh] Argonne was operated by the University of Chicago, and so there was that one-to-one relationship. Fermilab was operated by URA, I think it was, the Universities Research Association, which was a group of the universities that had formed this corporation to actually operate the machine.

Culturally, Argonne was a multidiscipline lab, and so you had a chemistry division, a reactor division, a physics division—meaning non-high-energy physics physics—and so on. So it was much more of this sort of college appearance to it, and structure. Whereas Fermilab was dedicated to produce high-energy protons for high-energy physics research. The other cultural difference was Robert Wilson. I don't know if—do you know of him? [laugh]

ZIERLER: Sure, yeah, sure.

KLAISNER: [laugh] Did you ever meet him? [laugh]

ZIERLER: I did not. Never had the pleasure.

KLAISNER: [laugh] I'll give you one story.

ZIERLER: Please.

KLAISNER: This is sort of a week after I arrive—maybe two weeks after I arrive. Anyway, early on, we were on the 10th floor of this building. In the basement of the building was a shop; the rest of it was offices for lawyers and whatever. In the basement, there was a small shop for building models. And so the guys from downstairs were very proud, and they brought up this die table with a wooden model of the end of the Booster magnet. And in those days, you know, part of that was so that you could look at how the lead dress goes, and such, you know, get it all worked out—

ZIERLER: [laugh]

KLAISNER: —which you would do in CAD today, but then it was physically—so sitting there is this beautiful wooden model of the end of the Booster magnet. And [laugh] Wilson came out of his office. It was an open area with offices around. Came out of his office and says, "It's too big." [laugh] That had to do with the size of the magnet, the cross-section of the magnet, had to do with how conservative you were, you know, both mechanically and magnetically.

And [laugh] he just said, "It's too big." And that led to a fight between von Steenberg and Wilson that ended with von Steenberg leaving. And then Roy Billinge took over. And I went through—you know, I was consistent through the whole Booster construction at the—as chief [laugh] electrical engineer. And I think there was five different physicists [laugh] that—because they were the ones that interacted with Wilson.

ZIERLER: Now I wonder to what extent you—from your vantage point these were scientific differences as much as personal differences?

KLAISNER: Oh, personal. I mean, he had his opinion how—I guess they get tied together. But he had his opinion how this thing should be built. One of his rules was all engineers on the Booster had to make a trip to Cornell. He came from Cornell, and they had a synchrotron—electron synchrotron and you had to go to Cornell and get a tour and see how accelerators were [laugh] really built. [laugh]

If you look at the Booster, it has a unique magnet. The vacuum chamber is on the outside of the magnet and so—but on that first story about it being too big, the next person who was there was Roy Billinge. And he said, you know, "Sure, we'll make it [laugh] smaller." And we made it smaller.

And if you look at the design book from Berkeley, it was—the Booster was supposed to be 10 GeV. And so the whole design was based on a 10-GeV booster. As a booster, it never operated over 8 GeV, and because [laugh] the iron saturated [laugh] now that you had a smaller magnet, you know. [laugh] Now whether Wilson had thought that through and knew that 8 GeV would be OK or not, I don't know.

But the other thing about that machine, it was commissioned—let's see. I started there at '68, so it was sort of '70, '71, '72 was when that was turned on. And Booster still running. They've upgraded pieces of it, but the magnets are the same magnets that were there originally.

ZIERLER: That's amazing

KLAISNER: Yeah. [laugh]

ZIERLER: Hey, what was your sense at the time of where this particular machine fit in within the larger research scope of everything else that was going on at Fermilab? Was this—how big a deal was this particular synchrotron, relative to the other things that were going on?

KLAISNER: There was nothing else going on.

ZIERLER: That's it?

KLAISNER: Yep. The only thing was going on was building that machine. It was a greenfield.

ZIERLER: So obviously there was a lot riding on it in terms of—

KLAISNER: Oh, yeah, [laugh] absolutely.

ZIERLER: —all the other things that Fermilab was thinking about doing.

KLAISNER: Yeah. Another Wilson-ism [laugh] was that—well, at that time, Fermilab was single-minded to build this machine, get the 200 GeV, and then they had detectors that would work at that energy. And they were not imagining any other accelerators[?] then.

ZIERLER: And then in 1972, you decide to enter—you enter into an entrepreneurial phase.

KLAISNER: [laugh] Yeah, we were commissioning the Booster and another engineer that I worked with at both Argonne and Fermilab was Jim Stevenson. And he and I—well, as a detour, there was a standard at the time called CAMAC, which was a modular instrumentation that you could put counters and discriminators and that kind of stuff, to build up your experiment out of these standard modules.

It was sponsored by the Bureau of Standards, I think. And we were very active in that, Jim and I and other people there. And we implemented the controls for the Booster using the CAMAC standard. It was originally for scientific experiments, but it had the bus structure that we thought we could use for controls.

And he and I had been through this campaign of operations and we said, "We don't want to do that again." "We'll get it up and running and be proud of it, and then move on." So, we took that CAMAC standard, and applied it to industrial applications.

ZIERLER: Such as what?

KLAISNER: Steel mills, aluminum rolling mills. We had a system for scoring bowling with the computer at the bowling alley. [laugh] But mainly industrial applications and then we did sell into physics labs like Fermilab and SLAC and the rest of them.

In fact, I made a trip to Moscow, and this was in the mid-'70s, which was the height of the Cold War. And the US had decided that, I think correctly, that one of the things they wanted to do was promote East-West trade. And the idea was you would diffuse some of this nuclear standoff by getting the two nations economically interconnected. So, the US Department of Commerce put on a trade show in Moscow, and invited companies that they knew had materials like that. So, I went with an exhibit [laugh]

ZIERLER: What was in your exhibit? What did you decide to show?

KLAISNER: We had a subsidiary in Geneva, Switzerland, and the manager there was putting together the planning. I was just sort of the technical brains of it. It's much different than a US trade show.

There's a booth, and a little area in the corner. And the booth comes with—I started to say a maid, but a woman whose job is to keep things in order. And you could entertain back there, including alcohol and the rest of that. We had some equipment, but not much because the problem of importing and exporting equipment into Russia, it was mainly just literature, and talking about things.

And my scientific host was a high-energy laser lab, and they had been using some of our equipment. So, I went there, and saw their installation, and then gave a talk, you know, a simultaneously translated talk, and then got a tour of their high-energy laser lab. The one overriding thing I took away from that that they were way behind us technically.

For instance, that was a period of integrated circuits, TTL logic and such, logic circuits. And the standard piece has a pitch of the pins at a tenth of an inch—a pin every tenth of an inch—worldwide [laugh] except the Soviet Union. They decided that they're going to invent theirs—they were going to leapfrog and invent their own, and so they picked 2.5 millimeters as opposed to 2.54. No problem. You know, over 14 pins, you don't accumulate much error.

But by the time they got into business, people had 80-pin devices and then they were incompatible. And so what the people were doing, the engineers in the labs were doing is they were getting their integrated circuits from I think Yugoslavia, one of the Eastern Bloc countries. And they were clones of Texas Instruments integrated circuits and they only came with instructions on how to solder them into the board.

If you want to know any of the electrical properties, they had this row of Texas Instruments manuals.   They were behind in lots of areas.

ZIERLER: And then was the Eldec Corporation, was that an outgrowth of KineticSystems, or that was a new venture?

KLAISNER: No, Eldec was an ongoing organization. I went to work there for them. They had a range of products. And at that time, Eldec sought me out. Goddard Space Flight Center was looking at a way of using the CAMAC standard for implementing experiments on the space shuttle.

And so, they had already engaged Eldec in two programs: one is to take standard-looking equipment, and making it space-compatible. The other one was building space-compatible modules that were the same functions. The idea was that if you're an experimenter, you could build up your experiment on the ground using conventional CAMAC modules.

And then when it came time to fly, you would reconfigure that in the flight modules, and which were much more expensive, and get that advantage of cost and schedule. And it never got off the ground. But we actually did build a space compatible CAMAC crate and we participated in that.

ZIERLER: I'm curious if you felt like you were working in an entirely different field that given the fact that this was—had applications for space, or was it the same sort of projects, the same problems you had been working on?

KLAISNER: Well, I think one of the great features of my career [laugh] has been this going into new areas and, you know, learning new things. And so, it was very educational for me to learn the space aspects. There were people on the staff there with years of space experience. It was an ongoing operation. It had a lot of projects. So, I had plenty of support there to help me understand the issues.

ZIERLER: And where was—where were you located? Where was Eldec?

KLAISNER: Just north of Seattle.

ZIERLER: Oh, I see. OK.

KLAISNER: So, I started in California , Chicago, Seattle, California. So it was in Seattle.

ZIERLER: What do you feel like were some of your contributions to, you know, moving the space program forward?

KLAISNER: Well, I sort of skipped a step there. Goddard dropped this whole plan, and they were funding this project, and so that went away. Eldec had another program that they put me in charge of, which was the signal conditioning electronics for the Space Shuttle. And they took signals from thermocouples and strain gauges and things like that, isolated the grounds, amplified it to a standard level, passed them onto a multiplexer.

And so that, you know, you had a sense that you're really participating in the space program, and that was very successful. Our boxes worked and they kept on working. And as far as I know, they were still on the last shuttle. So that was satisfying, and a little crazy making because this was a manned spaceflight, so they're very particular about every little detail.

ZIERLER: Because the safety issue is paramount?

KLAISNER: Yes.

ZIERLER: So, what was your role in terms of ensuring—you were working on signal conditioning equipment. So, what was your role in making sure that, you know, that the safety of the astronauts was second-to-none?

KLAISNER: Well, at the level I was participating, you know, it was the matter of the letter of the law. And, you know, the NASA people were—had all these rules, and if you meet these rules—their attitude was if you do these things, the right outcome will occur. And it included on-site inspection, stuff like that. And so, from that point of view, it was a pain.

I remember going down to Rockwell, and spending—I don't know—I think it was a week arguing about solder the bend radius of components on printed circuit boards. And you know, you just shake your head and wonder. I don't think that had much to do with them. But I don't know if you remember the shuttle program, the first shuttles were dropped off the back of a 747.

ZIERLER: Mm-hmm.

KLAISNER: And then they glided down. That was the first test. And the first time they did that, the shuttle had five IBM computers for flight control, and two were running the whole time, and the third one was monitoring the two and—anyway. And when they released it from the 747, I think three of the five failed in that instant, whether it was firing the explosive bolts or something that it—well, that creates a tremendous fire drill. We had to go back and re-inspect all soldered joints and —but, anyway, it was fun. I got to go down and stand next to a shuttle.

ZIERLER: And how did you know it was time for your next opportunity at Intermec?

KLAISNER: Well, the—I was hired to do this CAMAC job. It went away with the Goddard. So, then I moved onto this signal conditioning job, managing that. And we finished.

ZIERLER: Oh, so the job was essentially completed?

KLAISNER: Yeah, and I didn't have to leave. I could move onto something else. But I didn't see anything around there that was really that interesting. And a friend of mine had gone to the bar code company, and said, "Do you know there's an opportunity over here."

ZIERLER: And what was that opportunity? What were you going to do next?

KLAISNER: I was VP of Engineering at Intermec. And they produced bar code systems, and printing and reading labels, bar codes, and the systems that go along with that; not so much for your grocery checkout but for industrial applications.

ZIERLER: Oh, so this is a significant change in fields?

KLAISNER: Yeah, right , absolutely. It's still electronics but, yeah, and it was fun.

ZIERLER: Although as VP, I bet you were a little farther away from the engineering side of things, and more on the management side of things?

KLAISNER: That's true, although it wasn't really that big a corporation. I think it was $100 million or something like that in sales. So, it was a pretty hands-on job.

ZIERLER: How big was the company?

KLAISNER: I said I thought $100 million. There was a couple hundred people, I think, yeah.

ZIERLER: And then in 1989, back in the National Laboratory at SLAC.

KLAISNER: Yeah, that's an interesting story. Well, it's interesting to me.

ZIERLER: Did you feel like you were always destined to go back to a laboratory environment after Fermilab?

KLAISNER: No. What happened there is Intermec has a hostile takeover. And in the process of the hostile takeover, they fired all the VPs. And so, I was out looking for a job, and this is one of those fortuitous events. The Superconducting Super Collider, the SSC that was being built in Texas?

ZIERLER: Right

KLAISNER: The chief physicist—or I don't know what her title was. But Helen Edwards was in the upper echelons of that project. And I had worked with her commissioning the Booster. So, I had this idea that I'll get a hold of Helen and see if she had a position for me because she knew me and knew my abilities.

So, I called a friend of mine at SLAC, Bob Daniels—we worked together along the way—and said, "Do you know how to get a hold of Helen?" And he said, "Yeah, I don't know." And I told him my story, and he said, "You know, my boss is leaving. You might be interested in that project, that job." So I applied, and I got it. That was head of the electronics department at SLAC.

ZIERLER: And so, what were your impressions when you got to SLAC? First of all, did you feel like you were coming home, to some degree?

KLAISNER: Oh, yeah, sure, yeah. I mean, I was familiar with all the roads and buildings on the university campus. And when I left, there was no SLAC. SLAC was started in '62 or something like that, and I left in '61. So, it was a rolling hills so I wasn't coming back to that. But as far as coming back to the campus and—yes.

ZIERLER: So, what were some of the big things that were going on at SLAC during those days?

KLAISNER: Well, the main activity at SLAC when I arrived in ’89.—was called the SLC, and this used the Linac to generate short pulses of 50 GeV electrons and positrons. And at the high-energy end, they were split, and went around banjo-shaped path that, and the beams passed through each other; the idea being electrons and positrons interacting, and in a detector out there.

And the 50 GeV was just enough to find the Z^0 particle, and so SLAC was trying to discover it and measure its mass. They had built this SLC. At the same time, CERN had built the LEP, the Large Electron-Positron machine and it was a circular machine, where this is not really a circular machine. This is a single pass. So, they were in competition for Z^0. And there was a lot of—

ZIERLER: And what was looking to be achieved? What was the big goal for Z^0?

KLAISNER: Primarily to measure its mass. You know, clearly in the scheme of things, it was predicted s uncovering that and measuring it precisely was what electron machines do well.

Anyway, oh, I know what I was going to say. There was an ulterior motive for building this. It was called the SLC. It’s Stanford or SLAC Linear Collider. And that’s two beams aimed at each other, and going through each other once. And the question is could you get enough luminosity, to get enough interactions to be worth it?

And the reason that that was an interesting engineering and physics question is there was proposals to build two linacs aimed at each other to do high-energy physics experiments. I’m talking about 25-kilometer linacs aimed at each other to do a single pass, and measure—

precision measure particles at that energy. And so, there was a lot of skeptics that one could not focus a beam and aim it well enough to get enough interactions to work.

And they did, and so they proved that principle. That's independent of their search for the mass of the Z^0. I started in '89 and they detected their first Z^0 particle that summer. I took full credit for it.

ZIERLER: [laugh] Now, I'm curious, at this lab you had obviously come in with a lot more experience than at your previous, you know, work at Fermi. So how did that work in terms of incorporating your experience within a new national lab infrastructure? Did you feel like you were working at a higher level, or were you sort of right back to the same kind of engineering stuff that you had been doing previously?

KLAISNER: No, this was a management level, and I knew a lot of the people there, and the history there. I mean, we had never worked together directly. I worked with Bob Daniels before, but I knew a lot of these people, and I sort of fit in.

I mean, I knew what work they were doing. They were doing CAMAC stuff. So, you know, I fit in pretty smoothly, but it was mainly management. I was not doing any design engineering.

ZIERLER: And I'll ask the same kind of question just in terms of—you know, from your vantage point, comparing the different kinds of laboratories just culturally, what was unique about SLAC and the way that people collaborated, and the way that people worked with each other, and the way that people even socialized with each other?

KLAISNER: My first reaction is that they're quite different, but now you want me to tell you why it's different. The people at SLAC didn't socialize much. At Fermilab with Wilson there, there was an overt attempt to make those things happen.

And that wasn't really true at SLAC, certainly at our—my level of the organization. So, it was—you know, it was a professional organization. People treated each other with respect and…

ZIERLER: Did you feel like you were there at a time of transition for SLAC, I mean, during your tenure? What were the big transition points while you were there?

KLAISNER: OK. I thought a little bit about that. One of it was that the original machine was built to do high-energy physics.

ZIERLER: Now, which machine is this? Which one are we talking about?

KLAISNER: 1963, the original Linac.

ZIERLER: OK, the Monster?

KLAISNER: Yeah, the Monster. It was built to do specific high-energy physics things, you know, and big spectrometers, electron beams into targets, looking at the products in the spectrometers, and searching for things like—well, they didn't know quarks existed. But that was where they were generally headed with that. But it was a high-energy physics lab.

There was a machine, a small circular machine called SPEAR which was independent. It got its electrons and positrons from the main Linac, but they did physics experiments with colliding beams. And Burt Richter got a Nobel Prize for his work on SPEAR.

By the time I was there, it was transitioning over to doing photon optics, SPEAR was. And the idea was that—and it's universal —electrons radiate when they go round a circle. And these beams that radiated off the electrons can be very intense X-rays..

And those can be used to do research on proteins, materials and phenomenon other than particle physics. It was running, and it was doing photon science related research. What I'm leading up to is how did SLAC get out of the high-energy physics business?

ZIERLER: Right. That is one of the big questions.

KLAISNER: [laugh] When I arrived the SLC was operating studying the Z_0 particle., and then after that came PEP-II. There was a machine that had been built in the '70s that was called PEP, the proton-electron collider (later becoming the positron-electron collider). PEP-II was built in that same tunnel.

It was asymmetric in that the positrons and the electrons had different energies. It had two rings: one for the positrons going one way and a second ring for the electrons. Both rings had very high current compare to previous machine, so it was called a B Factory since it was intended to produce large quantities of B-mesons, and it did. And that was a high-energy physics experiment.

Then came the Linac Coherent Light Source (LCLS), which used the last third of the Linac to produce short electron pulses, put them through a 100-meter long undulator, and get short X-ray pulses. Short means 30 femtoseconds. And those could be used to study the properties of proteins and materials..

They were intense enough that they could be used for studying matter in unusual conditions, suddenly heated up to vapor, to study the transient effect. There is a whole class of experiments that use the unique properties of the LCLS, but this was not high-energy physics. Studying the structure of proteins is not high-energy physics.

When I started at SLAC in 1989 the SLC was running and starting to collect data and the B Factory was at the conceptual design phase. The Superconducting Super Collider (SSC) was under construction in Texas. Not long after the B Factory project started congress canceled the SSC project. Once the B Factory and the SLC had finished their runs, accelerator-based high-energy physics was essentially over at SLAC and, with the exception of Fermi National Accelerator Laboratory, for the rest of the country. The SSC was meant to be the premier high energy physics machine in the US and a prominent machine in the world. It was well along when this Congress decided to kill it. And we could spend our whole two hours talking about how that happened.

ZIERLER: Oh, I know, right.

KLAISNER: But we won't. But in some sense, the US either blundered or gave away its leadership in high-energy physics at that point. It moved to CERN. CERN built the Large Hadron Collider (LHC), which had a similar function. It continues to operate now . The Higgs boson was detected at an experiment at the LHC. So, the discovery based experiments for high-energy physics moved to CERN.

ZIERLER: Right. But just to interject there, was your sense—it's not like that Congress killed the SSC because they knew this was going to be done at CERN.

KLAISNER: No.

ZIERLER: CERN took this on because the SSC never completed.

KLAISNER: I think, you know, that Congress killed the SSC because it was costing too much, and the DOE/Congress didn't think the management team could get the costs back in control.

ZIERLER: Do you think that the end of the Cold War had anything to do with that also?

KLAISNER: My first reaction is, 'is the Cold War over?'

ZIERLER: Now—right.

KLAISNER: We still have missiles pointing at each other.

ZIERLER: Yeah, fair, fair.

KLAISNER: But, anyway, I don't think that was connected. I think it was the worry about the cost, and a lot of politics in there that I don't really know. But back to your original question, I think CERN would have built the LHC no matter what. And if the SSC had completed, then the world would have two machines, and you could compare the results.

So, I think they would've gone ahead. That was the next natural step for their science program—they already had this tunnel. They had a 26-kilometer circumference tunnel that they had built for the Large Electron Positron (LEP) project that they could put the LHC in, so.

ZIERLER: How did Fermilab change when it was obvious that the SSC wasn't going to be completed? Was there opportunity in that in terms of a role that SLAC might be able to fill now that SSC was not going to be operational?

KLAISNER: Not SLAC, certainly, because the limit on the site and the processes that they have for accelerating electrons, they were sort of at the maximum without making significant discoveries in techniques for accelerating electrons.

ZIERLER: No, but that's what I mean. I mean was there ever consideration, as far as you can tell, that there might be a moment of opportunity to make those big changes to do the kinds of things that might have been done at SSC?

KLAISNER: Ah, no. There is a detail here that SSC and LHC are both proton accelerators. And they could get to very high energies because, protons radiate very little energy when bent around the circle, but electrons radiate lots of energy. And so the proton accelerators have been used for things like discovery of the Higgs.

The electron accelerators have been used to do precision measurements. You can measure the mass to a much higher precision, or the structure but at much lower energies. But they don't directly compete. And to some extent that has been an advantage for SLAC because they carved out a piece of precision physics that they could do.

Now Fermilab, if you want to get a story, you should probably talk to them. But from California looking that way, they have hung on to being a high-energy physics lab, and doing neutrino physics. And the skeptics would say that they're struggling to keep alive.

ZIERLER: To this day, you mean?

KLAISNER: Yes, they are doing some neutrino physics. They have in the works, and it's at some stage of design, a system for producing high-intensity neutrino beam aimed at a cavern in

South Dakota. And that's very expensive, and I'm not in a position to evaluate the physics gain for the amount of money that's been spent on it. But they doggedly stuck to high-energy physics.

ZIERLER: So we touched on a little bit of these—each of these issues during your tenure at SLAC. I just want to ask some specific questions on some of the particular projects. So with the polarized electron gun, when this allowed the SLC to become competitive with CERN, I'm curious, from your perspective, what collaboration or contact there was with CERN with regard to this particular project?

I mean, were people going back and forth? Were there visitors? Was there a culture of there not to be cooperation because the emphasis was more on the competition, and doing it better? What was your feeling with regard to these issues?

KLAISNER: CERN was using LEP, the electron-lepton—anyway.

ZIERLER: The Large Electron Positron Collider.

KLAISNER: Positron [laugh]—Large Electron-Positron. And so that's a circular machine, and so they could get some polarization in the machine like that, but not from a gun. I mean, the Linear Accelerator uniquely needed a gun that produced polarized electrons.

When we collaborated with on the gun, there's—it had a strained lattice gallium arsenide cathode, photocathode. And so we collaborated with some people on the Stanford campus who knew about gallium arsenide. And I had a friend at Hewlett-Packard who was in charge of their gallium arsenide program, and they made their contributions too. There's a side story here.

ZIERLER: Please.

KLAISNER: I took this over because it was dragging on. I was an engineer and there were well-meaning, highly skilled scientists going at this. But they didn't really have an engineer that was going to make it actually happen. The problem was that the photocathode would get poisoned very quickly.

The quantum efficiency for emitting electrons would go down way too quickly. And so the question was why? And, it took a long time to figure that out. But one thing, a one monolayer of gas on the surface was enough to poison the cathode, so it had to be in an extremely high vacuum. And the question is why weren't we getting it?

So, Burt Richter—and this is one of my most awkward times—called me into his office, and said, "What's going on?" And I said, "We don't know the answer yet. We have this set of things we're doing to find the answer, but we don't know it yet." And I saw him sit back and contemplate, "Am I going to get rid of this guy or not on this project?" And so he decided to keep me on it, and he put Sid Drell as a watcher.

ZIERLER: [laugh]

KLAISNER: And I went to see Sid Drell once a week to report on the project again. And that was one of my most wonderful experiences. [laugh]

ZIERLER: Oh, good. [laugh]

KLAISNER: [laugh] Yeah, he was a great guy. I'd interact with him directly. But I would go to his office, it was an hour meeting, and in five minutes I would finish my report on what we'd done the last week. [laugh] And the rest of the time was on arms control. And I learned more about arms control in those sessions. It was very delightful.

We figured out—I mean, there was some field emission going on in the gun, and when the field emission hits the wall, it scrubs the wall, and scrubs the molecules off the wall. And so, it ended up we dropped the voltage. And there was an accelerator physicist, Roger Miller, there that was calculating the trajectories of electrons off the cathode. And I kept pushing to go to lower and lower voltages. We came to a compromise, and that's how we got there.

But, by the way, I think that one of SLAC's biggest strengths is in accelerator physics. I mean, they are known the world over for understanding and calculating parameters of particle beams.

ZIERLER: Right. Now on the B Factory, it's—of course, it's well-known that it exceeded expectations. I mean, that's a massive understatement, right?

KLAISNER: Yes.

ZIERLER: But I'm curious to just sort of fine-tune that a little bit. How much did it exceed expectations? In other words, from your vantage point, what did you understand as the goals that the B Factory were looking to accomplish? And then in retrospect, how much more did it accomplish relative to those initial goals?

KLAISNER: Well, when we were building it—or designing it and building it, we had a set of criteria that everybody agreed on, the primary criterion was to have 1 ampere of electrons colliding with 2 amperes of positrons. From the science point of view the primary criterion you're measuring is the luminosity. How many interactions do you get per second? How many B-mesons do you create per second?

And so that was our goal. We knew what that number was. And all the parameters we built to, and the one amp of electrons, and two amps for positrons and the two energies that we had and all the rest of that, was aimed at that goal. And it was understood all along that we would produce that, and then people would make improvements, and the luminosity would get better.

And I actually wasn't involved in that process, by the way. John Seeman, who is an outstanding accelerator physicist, was mainly responsible for getting the maximum performance from the B-Factory. —We were competing with KEK a laboratory in Japan which was similar to SLAC. . It was a very similar machine to the B Factory at SLAC with two rings, one for positrons and one for electrons that they had built an existing tunnel, you know, it's the same—there was a lot of connections—they're in Japan—a lot of connections between the labs, and they're doing very similar things.

They, incidentally, were ahead in luminosity to the B Factory, partly because their tunnel was a bigger circumference. I don't have the numbers. I wasn't involved in that improvement program. I was involved in getting it working the first time.— The B factory reached its design luminosity in 1 ½ years and 4 times that by the time it shut down

ZIERLER: What personal connection did you feel to the Nobel Prize in 1990?

KLAISNER: I'm thinking which Nobel Prize?

ZIERLER: Right, the one that was connected to the B Factory.

KLAISNER: The B Factory operated from 1999 to 2008. The 1990 Nobel Prize in Physics honored work done at SLAC using fixed targets.

ZIERLER:

KLAISNER:

ZIERLER:

KLAISNER:

ZIERLER: [laugh] Clearly, clearly. What about your work on the telescope, the space telescope, the Large Area Telescope? When did you get started on that?

KLAISNER: By the way, can I tell one more B Factory story?

ZIERLER: Oh, yeah, please, please.

KLAISNER: And this goes back to when it was started. And when I signed on in '89, there was a conceptual design report that was in that stage of trying to pick a site. And they were competing between Cornell and SLAC. Both had a tunnel.

And it turns out that the Cornell machine was funded by NSF; and SLAC by DOE. And they had much different rules. And so, you know, we couldn't use graduate students to build an accelerator. That's just not allowed. In the NSF world, they give grants and then it's up to the university's rules of how they make that play out.

And so when they tried to compare the costs of these two proposals, it was very difficult. I mean, I think Cornell was proposing half the cost of SLAC. But you had to weed through all that, and say, you know, it's in the other environment was—anyway. And there had been committees meeting to work that out.

And then President Clinton came out to San Francisco.

KLAISNER: Clinton came out and gave a speech in San Francisco, which we referred to as a jobs speech, he put it a little better. It was economic development in California to get—to stimulate the economy.

And in that jobs speech, he said, "We're going to build"—the Loma Prieta earthquake had just occurred a couple of years ago. "We're going to replace the Cypress Freeway," which had collapsed, and "and we're going to build a B Factory at Stanford." And none of us who were working on the project knew that was coming.

ZIERLER: Oh, wow.

KLAISNER: Now, I'm told that the lab director and such knew it, you know, a couple of weeks in advance. But it was—and that made it a presidential initiative, which I don't know all the subtleties of that, but it had special status. And it was important to this project that Clinton got re-elected because I think if he didn't get re-elected, and here's a project sitting out there that says "presidential initiative"—

ZIERLER: [laugh]

KLAISNER: —we would've had real trouble. But he did get re-elected. And what happened to that is that that project was funded on its original profile—funding profile. And it was finished on time and on budget. And that's such a gift. It's the only project I've ever worked on that stayed on its original funding profile.

ZIERLER: [laugh]

KLAISNER: There's always, you know, let's move money from this year out to some future year, which isn't equal. So, anyway, it was a presidential initiative. Now what you… you asked me something else. I forgot.

ZIERLER: No, so that's the—your work on the telescope, the Large Area Telescope, had you done anything like that before?

KLAISNER: Well, I'd done space projects, like the Space Shuttle signal conditioning equipment. This work was new to SLAC. In some sense, I hadn't sometimes—you know, my reaction is I'd been involved in pieces of it, silicon detectors and things like that, but not that in all.

It was built at SLAC because of SLAC's expertise in tracking detectors. You know, they built trackers for BaBar and for SLC. And, clearly we had a lot of experience with silicon trackers, and track identification algorithms.

ZIERLER: Did the idea for the telescope originate within SLAC?

KLAISNER: No, the Principal Investigator was Peter Michelson at Stanford. It was a follow-on instrument to a previous gamma ray detector. I was involved in the construction of the instrument. The conceptual design and much of the detailed design was in place.

ZIERLER: I know that NASA and the Department of Energy were involved.

KLAISNER: Yes.

ZIERLER: But I don't know if they were involved from the beginning, or they originated the idea.

KLAISNER: No, it was originated locally, but not necessarily at SLAC. There's a subtlety I'll come back to. It was the University of California at Santa Cruz was active, scientists and the Naval Research Lab and at Goddard Space Flight Center and Principal Investigator was at Stanford. A project like this is put forward by a collaboration of scientists and they work with the potential funding sources to work out a shared responsibility.

By the way, well—so my job was project manager during the construction phase. I was managing the building science instruments and we at SLAC didn't have a lot of expertise, almost none, in space. So we brought some people in, both hired and contractors because we're in an area where there's a lot of space people around. So that was pretty easy to do.

And the DOE, the way that this was funded, you know, it's like the DOE and NASA. When it was originally proposed, which was before I was involved, the argument was that they were going to be equal partners. NASA's going to throw up some money, and DOE's going to put up much less money but they were going to bring along with them these collaborators, foreign collaborators that would contribute in-kind to the project.

ZIERLER: Now, "foreign collaborators" means what? Other countries' space programs? What kind of collaborations?

KLAISNER: Other countries' high-energy physics labs.

ZIERLER: Uh-huh.

KLAISNER: But those—the high-energy physics people—because the DOE was arguing they could bring them along; not space people but people to contribute in-kind.

And that makes a project exciting because you have no control over those funds. There was a problem with every one of those foreign collaborators, you know. Sweden did all right. They supplied the cesium iodide bars from the Ukraine for the Calorimeter.

It was going to go to France, and they were going to build the calorimeter. Before that started, the government cut off the funding. The relationship was that the people we're talking to in the lab were very enthusiastic about the project and wanted to participate. But when the government cut the money off, then they couldn't participate.

So, we moved that construction to a space-related company not far from the Naval Research Lab in Washington, DC. NASA picked up the extra charge and it was in the ten millions dollars range. The Italians did pretty well. They built the tracker and—but they had agreed originally to modify a … a ground station on the equator that they owned to increase its bandwidth and make it appropriate for GLAST. And the money for that was cut, so we had to change the way we got data back to the ground.

Japan supplied the processed silicon. They were going to pay for half of it, and the US was going to pay half of—their government changed, and changed the way they were funding laboratories, and some of the funding went away. They still supplied the silicon, but we had to put some US funds in there to do that. So, a lot of balls in the air and—but…

ZIERLER: What was your sense of what the telescope was designed to do? What kind of images was it looking to capture that had not been captured before?

KLAISNER: Well, it's a gamma-ray telescope, so here's my version of the story. NASA has put up a series of satellites, telescopes, the Hubble Telescope covered the visible range. They had microwave telescope, an optical telescope, an X-ray telescope. And our telescope was the high end of the spectrum , the gamma rays.

So, you make images of sources of gamma rays. And people knew about gamma-ray bursts, for instance, but they had not been able to localize them and, you know, be able to identify the source of the bursts. So, it gave a whole new tool for looking at the universe.

I think it was sort of redefined as because people made the discoveries that there was—there's gravitational lensing that occurs around massive objects. And it turns out that I think about after it launched, people realized that dark matter, dark matter lenses, the gamma rays, and you can map dark matter, for instance. And so there's been some pretty exciting work coming out of that. You realize that, you know, there's no lenses for gamma rays.

ZIERLER: Right. [laugh]

KLAISNER: So, a gamma-ray telescope doesn't look like a telescope at all. It tracks each photon that comes in, and project that back where it came from, and then integrate it to get the image.

ZIERLER: And what was your particular work with regard to the telescope? What were you working on? It was a more hands-on capacity?

KLAISNER: No, I was the project manager, and my job was to keep all these balls in the air, you know, equipment coming from all over everywhere.

ZIERLER: Yeah, and it was particularly complex, given all of the collaboration involved?

KLAISNER: Yeah, absolutely. We were having some communications problems, and things were not working out well, from my point of view, in Italy. They were interacting with the guys in Santa Cruz, and they were not on the same page, you know, all the problems were the other guys' problems. Do you know Persis Drell?

ZIERLER: I talked to her two weeks ago.

KLAISNER: Oh, [laugh] OK. Did she tell this story? [laugh]

ZIERLER: Well, let me hear your side.

KLAISNER: [laugh]

ZIERLER: I'll tell—let's see how it compares.

KLAISNER: Well, at that point, she was the director of research at SLAC. This is before she became lab director. And so I talked to her. I said, "You know, we've got a problem." I reported to her at that point.

And we discussed it for a while. and I'm not sure who put it on the table first, but we—I convinced her to take over managing the tracker in Italy. And she put Steve Williams in charge of her office temporarily, you know, took a year off to deal with the Italians.

And she was practicing shuttle diplomacy. She was over there regularly, which I think is absolutely needed. And she spoke some Italian, so that helped a little, although they speak good English, and turned around that. So that's the kind of thing, you know, that I did.

Recognize when there's—the sooner the better, you recognize when there's an impasse, and come up with creative solutions. I remember talking to her. She was a little concerned that I reported to her, and she was going to report to me.

ZIERLER: [laugh]

KLAISNER: "Oh, don't worry about it. We'll work it out." So, you know, there's that kind of part of the project management.

ZIERLER: Well, I wonder if you could talk a little bit about how the lab changed under different directors, particularly the transition from Burt Richter to Jonathan Dorfan? In what ways did he take the lab in new directions, and in what ways was he looking to keep things going in the way that they had been under his predecessors?

KLAISNER: The interesting transition is from Dorfan to Drell. But

ZIERLER: We'll get to that one next.

KLAISNER: [laugh] I—you know, Dorfan was the leader of the B Factory project prior to becoming director. And I worked directly with him. I was chief engineer, and we had a great relationship.

When he took over—I don't know how to put it—I don't think he made any radical differences. I can't think of any. Things just moved forward. It was on his watch though that—unfortunately, he had one foot in the BaBar and PEP-II and the other in laboratory management. And when it came to lab priorities between PEP-II and LCLS, he would say something different about lab priorities, but then decide in favor of BaBar.

That became a sticking point, particularly because Pat Dehmer, who was in charge of Basic Energy Science at headquarters, and her baby here was the LCLS. And she wanted to see that be the number one priority at the lab because she was putting a lot of money in that, and betting on it. And Dorfan would say, "Yes," and then next decision would go the other way. And so I—it was a point of tension that I—in my point of view, that ended up with Persis being put in that position.

ZIERLER: So now let's talk about that transition.

KLAISNER: Yeah, I—you know, I'm not at the university level of picking lab directors. Actually, I was. There was a search for a new lab director after Dorfan, and I was given the job of being the clerk of the search committee. And this is, you know, the kind of job you keep to yourself.

And anyway, so I was doing that, and as that went on, she was on the committee. Oh, she was running the committee. She was the chairman of the committee. And as it went on, a committee member came to me and said, "You know, the rest of the committee thinks Persis should be the next SLAC director—how do we handle this?"

Well, the committee reported to the provost. And so I had a meeting with the provost, and said, "Here's what's going on." And that led to Persis being appointed to lab director. And that was clearly a decision in favor of basic energy sciences and…

ZIERLER: As opposed to what? What's the other choice?

KLAISNER: High-energy physics.

ZIERLER: I see.

KLAISNER: I mean, Dehmer was clearly a high-energy physicist. Persis is a high energy physicist, but had a broader view on the laboratory's future. And then this question of the transition, that was a clear transition of the laboratory into photon science and…

ZIERLER: How much do you think that that was a function of the changing of lab directors, and how much was it about the state of play in high-energy physics generally?

KLAISNER: I've been asked that before, and, you know, essentially 'what went wrong?' I don't have a lot of vision into this. But it seemed to me that Pat Dehmer particularly in the Office of the Basic Energy Sciences. Pat was very well-organized. She had a plan laid out of a sequence of accelerators to be built to do things.

And so, you know, she—and that carried theday because she was much more organized and purposeful and decisive than the people that had the equivalent roles in high-energy physics. Now, how did that happen? I don't know. It happened over a period of time.

But the early days of high-energy physics, the high-energy physics community was pretty integrated in the US, and had, as a group, put together a proposal, you know, a plan. "Here's what we need to do in high-energy physics." And for a variety of reasons, that sort of fell apart.

And I'm not sure what all the reasons are, and it's influenced by the demise of the SSC project. But, you know, from my point of view, there wasn't the united front from the science community. There was not the strongest leadership at the Office of High Energy Physics.

ZIERLER: Now this is obviously toward the end of your tenure at SLAC.

KLAISNER: Yes, right , that's right.

ZIERLER: By the time you—

KLAISNER: I'm very proud of that telescope, by the way. It was designed for five years with a hope that it would run to 10 years, and I think it's on 13 years right now. It's still sending back data.

ZIERLER: Yeah. So, Lowell, I'm curious, you know, by the time that you had left, Persis had been in charge for two years, right. And so I'm curious how well developed the changing vision and the changing scope of projects that SLAC took on had been achieved by the time you had left. Was it still in its conceptual stage, or had you really experienced that transition in those intervening two years?

KLAISNER: No, I think I had experienced that transition. I mean, I wasn't so much directly involved. There was a couple of incidents there that—certainly Persis was well-received at the Department of Energy and represented the lab well.

And, for instance, they had an initiative, which was the Science Lab Infrastructure, SLI. And it turned out that there had been a concern that science labs had been using all their budget to do science, and let the buildings run down. And the infrastructure needed updating because the labs were, you know, like SLAC, go back to the '60s.

And so that initiative was—and I was involved in the first cycle of that, which was $1 million for building one new building, and retrofitting a number of others. So that was all this new energy. The new energy was not just LCLS. It was also the attracting investment in SLAC in terms of building a new campus and the rest of it. And that's going on. There's been a tremendous building enterprise in the last 10 years. It's quite a different lab today. [laugh]

ZIERLER: Right, right. When you left in 2009, did you think—where did you think SLAC was in terms of its relative strength, you know, compared to 20 years prior? Was it on solid footing?

KLAISNER: Yeah, absolutely.

ZIERLER: And in what ways? How was it on solid footing?

KLAISNER: Well, they had developed certainly in the field of—use their facilities for photon science. SPEAR got that going, and LCLS continued on, and now they're building LCLS-II. By the way, LCLS was great. It turned on in no time at all and it impressed everybody and demonstrated—and I think that's because they have a very strong accelerator physics organization that continues on with these new instruments.

And so they're leaders. They're leaders in photon science and astrophysics. They have a good astrophysics program, which GLAST was part of. But it's much more than that, looking at data. They're building the—they're keen—they're building the camera for LSST a ground-based telescope. And again that's astrophysics. That's not accelerator-based physics.

ZIERLER: Right, right. So, Lowell, I'm curious, in the last decade-plus, how well you've kept up with what's going on at SLAC?

KLAISNER: Pretty well. Main mechanism is they have a project management advisory group, which I'm on, and [??] and the active projects project managers present their projects monthly, so I get lots of insight in what's going on.

ZIERLER: Well, Lowell, I think now that we're right up to the present, I want to ask you one last question that's a very broad question that sort of will ask you to sort of put it all together for me and assess, you know, your experience in all of these different institutes and organizations. And that is, you know, you have so much experience at so many different labs, and in the private sector, right? You're kind of the perfect person to ask.

KLAISNER: [laugh]

ZIERLER: What do the national labs do better than the private sector, and what does the private sector do better than the national labs? And then as a follow-on to that, in what ways can they collaborate to ensure for the most productive and positive scientific future for our country?

KLAISNER: Nice narrow questions for the last two minutes. [laugh]

ZIERLER: [laugh] That's why I save it for the last one. [laugh]

KLAISNER: [laugh] The—I've always thought that when we talked about the sort of stuff I've done, it's been called Big Science. And I think the definition of Big Science is science that requires government funding to be executed.

ZIERLER: Say it—I missed that. Gives up?

KLAISNER: That needs government funding to be executed. You know, it's on a scale that's—and the other thing about Big Science, it's—at least a lot of it is pure science. You know, you're not developing a new product or even a new process. You're finding out about the fundamental nature of the world, universe.

And so the paths from, you know, measuring the mass of the Z^0 to having a better whatever a practical device is very long, and not very practical for commercial—you know, the horizon's way out there, and there's almost no—and so you're sort of betting that the people that—you know, Newton back there made discoveries, and now we're using his discoveries every day routinely.

In fact, I had freshman physics from Panofsky, the first director of SLAC. And somebody asked when does a physics theory become a physics law. And his answer was, "When it becomes common engineering knowledge."

ZIERLER: [laugh]

KLAISNER: [laugh]

ZIERLER: [laugh] When it's real?

KLAISNER: [laugh] Yeah.

ZIERLER: [laugh]

KLAISNER: And to me that—you know, that sort of transition there is—I mean, now, for instance, relativity is common engineering knowledge. It's used in the space program. It's used in lots of places where it's relevant to precision measurements. So cooperation—I was trying to think.

I think there are very few examples of where commerce and, you know—for instance, I talked to—I went to HP Labs where they had their gallium arsenide lab when I was doing those cathodes. And they were developing gallium arsenide crystals for practical products. They wanted to build a better computer. But if you're aware of that, you know, and you're aware of the synergy then—but I think in most cases, it's the science side that exploits the commercial discoveries. That make sense?

ZIERLER: And what do you see as some of the most important contributions that the national labs can make for the future?

KLAISNER: Well, I think if you look at SLAC, SLAC was certainly in the early days purely science. They were just—

ZIERLER: Discovering.

KLAISNER: —looking for quarks. [laugh] That was—is a long ways from your everyday life. But as time has gone on, these accelerators—the LCLS is an example where experiments there—if you can look at the fact that SSRL right now is doing some experiments on the COVID-19 virus, and they can measure the structure and things like that, and learn things about it. So sort of the activities that this lab is—are getting much closer to the practical every day, and hopefully not losing its pure science emphasis, but contributing practical stuff.

ZIERLER: Well, that would be a pretty practical thing to contribute to, that's for sure.

KLAISNER: [laugh]

ZIERLER: [laugh] Well, Lowell, I want to thank you so much for your time today. It's been so nice talking with you.

KLAISNER: Yeah, it's been a pleasure. [laugh]

[End of recording]