An Interview with Harvey Lynch
By David Zierler
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ZIERLER: This is David Zierler, oral historian for the American Institute of Physics. It is September 24, 2020. I am so happy to be here with Dr. Harvey Lynch. Harvey, thank you so much for joining me today.

LYNCH: Thank you for the invitation.

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

LYNCH: I am retired from what was originally called the Stanford Linear Accelerator Center (aka SLAC), and now the SLAC National Accelerator Laboratory. When I retired, I was an Assistant Director in what was originally called the Research Division and later called Particle and Particle Astrophysics division (aka PPA). After I left, it became the Fundamental Physics division. Yes, the names have been fluid.

ZIERLER: When did you retire?

LYNCH: I retired in January of 2012.

ZIERLER: Now absent the pandemic, would you still be going into the laboratory on a weekly or daily basis?

LYNCH: Normally I would go in about once a week. As it turns out, I was connected with a middle school just down the road from SLAC and helping out as a volunteer in the dark room. They have a dark room photography class, and so I would go to the class and then I'd go up the road. It's just a mile up the road to SLAC, and so I'd go in and see people and say hi and haunt them.

ZIERLER: In what ways have you been connected to physics generally, if not SLAC specifically, since you've been retired?

LYNCH: Oh, very, very little. I was a full-time bureaucrat for seven years before I retired, and before that I was involved with making things happen, but I haven't been involved directly in physics for a long time.

ZIERLER: Well, Harvey, let's take it all the way back to the beginning. Let's start with your parents. Tell me a little bit about them and where they are from.

LYNCH: My father was from Nebraska. My mother was from Colorado. They met in Colorado, and two and a half years after I was born, they came to California in 1942.

ZIERLER: What kind of families did your parents come from?

LYNCH: They were both from farming families. My father graduated from high school at the beginning of the Great Depression, not a good time. He worked on a farm for the whole summer for a ridiculously small amount of money for the whole summer, and said, “This is not going to work.” He and a buddy drove 500 miles, from where he lived in Nebraska, to Colorado because a friend there said, “There’s work here.” The buddy had a Model T and they drove the 500 miles. That’s where he worked, met my mother, and things went from there.

ZIERLER: What was your father’s profession?

LYNCH: Mixed. He was originally a farmer. Then he worked in various things: managing a hotel, then dry cleaner. In the end, when he moved to California, he worked at Lockheed building P-38s fighter planes. Then after the war he set up his own dry-cleaning shop and that’s what he did until he retired at the age of 65.

ZIERLER: Did he have any technical background for his work at Lockheed?

LYNCH: No, but he was very interested in such things. In fact, when I was a kid there were things called “Heathkits” from which one could build electronic things, like a VTVM (vacuum tube voltmeter), or audio equipment like amplifiers or FM tuners. I had several of those things and so he learned along with me. After he retired, he took up amateur radio and got a First-Class license.

ZIERLER: Did your mom work out of the house at all?

LYNCH: She worked with him in the cleaning shop.

ZIERLER: Harvey, where were you born? Where did you grow up?

LYNCH: I was born in Nebraska and spent six months of my life there. My mother couldn't stand it. Nebraska is very, very flat land. You can look 360 degrees and see absolutely nothing higher than a tree. My mother grew up in the San Luis Valley of Colorado with mountains rising to 13,000 or 14,000 feet on three sides, and the flatness really got to her. So they moved back to Colorado and then moved out to California from there. She had told me that story, but I only came to really appreciate it when, many years later, I moved to Texas for the SSC project.

ZIERLER: Harvey, when did you start to get interested in science?

LYNCH: In the second grade, i.e. seven years old.

ZIERLER: In what ways? How did you express that interest?

LYNCH: Oh, I don't remember. I just remember there was a science class in the second grade, whatever that was, and I said to myself, “Gee, this is neat stuff. I want to know more about that.” Some years later, we had gone back to visit relatives in Colorado and Nebraska, and a relative in Nebraska gave me a physics textbook, probably from a high school. That gave me an idea for a thought-experiment lightning detector: An AM radio could detect the lightning itself and a microphone could detect the thunder. The time difference would give the distance to the lightning. I never tried to build it. I got a lot of fun out of the thought, and it grew from there.

ZIERLER: Were you a stand-out student in math and science in high school?

LYNCH: Yeah, I suppose so.

ZIERLER: When you were thinking about colleges, were you thinking specifically about physics programs?

LYNCH: No. At that point, not specifically physics. I knew I wanted to do science of some sort, but the specific was undefined when I went to MIT. I looked around and I was first interested in chemistry. I looked at chemistry journals in the library and said, “This is deadly dull.” I had an interest in acoustics in my high school days, and I talked to people who said, “No, nobody here does acoustics anymore.” So I said, “Okay. That’s that.” Then I just went down the physics course line, and when it became my senior year, you had to do a senior thesis. I connected with somebody in the particle physics part of the department and went from there.

ZIERLER: Harvey, to go back to your decision, there are some pretty solid schools on the West Coast. Why did you want to go all the way to MIT for college?

LYNCH: Oh, I don't know. [Sighs]Certainly maybe just I wanted to get out on my own. I didn't want to live at home and commute to college. So my parents and I sat down with books on colleges, this, that, and the other. I don't think *US News and World Reports* was then doing their reviews of colleges, which are really just popularity contests anyway. We just read through books and we got some good information and we got some bad information. I got an offer from MIT with a full tuition scholarship for the first year, so I said, “Let’s go for it.” Today, it is my turn to pay back, offering support for scholarships to the Institute – with interest.

ZIERLER: Who was that professor that you were mentioning before at MIT in particle physics?

LYNCH: It was David Frisch. Interesting character. Let's jump forward and then come back again just to set the scene.

ZIERLER: Sure.

LYNCH: When it was time to graduate, I was looking around. Where am I going to go for grad school? He tried very hard to get me to stay at MIT, but I decided to go to Stanford. It turned out to be the right decision. But now we get to the meat of why he was an interesting character. For my thesis, he originally wanted to work on the experiment at Brookhaven. Brookhaven National Laboratory was just beginning the operation of the 30 GeV AGS machine. He wanted to do something there, but that didn't pan out, so I had to do something different. After I graduated he just went off into never-never land and decided to do something very different. He worked with the Boston Zoo on reversible vasectomies for lions.

ZIERLER: [Chuckles] Wow!

LYNCH: It turns out the lions in captivity are very prolific, and it's a problem. They have a population problem. Dave wanted to implant a little valve where you could actually turn things on and off. I don't know how successful it was. But going to Stanford was the right answer.

ZIERLER: [Laughs] So to go back to MIT about that particle physicist, what was the kind of work you were doing with him?

LYNCH: Originally, we were going to do an experiment at Brookhaven on pion-proton scattering. The experiment was not approved and so we had to do something else. He simply invented a project where I was to look at cosmic rays with a Cherenkov radiation detector with directionality. That was, in today's terms, an exceedingly primitive device. It was a polished cylinder, probably stainless steel because it was under high pressure of Freon. The project was to measure light yield and particle energy (well, really velocity) spectrum as a function of gas pressure. We took pictures at the end. There was a time-lapse 35 mm movie camera connected at the end to photograph the Cherenkov light ring. Then I scanned through the pictures on a microfilm viewing station at the library to see what was on the film.

ZIERLER: Harvey, what years were you at MIT?

LYNCH: I went there in '57 and graduated in '61.

ZIERLER: I'm curious if Sputnik was a big role for you, if that played a role in your opportunities or decisions.

LYNCH: It didn't play a direct role for me. It was certainly a very, very big deal because the then-president of MIT, James R. Killian, went to Washington as an advisor at their request during my freshman year. It made a big impression on everyone. The Russians had the capability of building missiles that could put something into orbit, and that means they can send a missile. The technology is largely the same. There are some technical differences, (a) precision guidance for targeting and (b) reentry ability. You have to have the impulse to get high enough that it goes into orbit. If it goes into orbit, it can go anywhere on the Earth, so it was Panicsville for the country.

ZIERLER: When you graduated, what did you want to do next? What were the opportunities available to you?

LYNCH: When I graduated, Dave Frisch wanted me to stay at MIT, and I looked around and talked with people in the Physics department; one suggested Stanford. In the end I went to Stanford and worked with David Ritson, and I did an experiment there on inelastic electron-proton scattering. In a way, that was a logical extension of the elastic electron-proton scattering experiments that Bob Hofstadter had done, resulting is a Nobel Prize in '61. At the time we had no idea that inelastic electron scattering at higher energies would be very important.

ZIERLER: So leaving MIT, you know you wanted to do particle physics. That was set for you.

LYNCH: Yes. The only question was where and precisely what it would be.

ZIERLER: Where else besides Stanford did you apply? Clearly at that point you had gotten over not wanting to be in California anymore.

LYNCH: I honestly don't remember where else I applied. I certainly did apply to MIT and Stanford. I might not have applied to anywhere else, but I don't recall. A funny thing happened for the decision. I delayed and delayed making the decision until it was April, I think. Have you lived on the East Coast with snow? Have you been in Boston?

ZIERLER: Yes, I have.

LYNCH: So you know snow in Boston is miserable.

ZIERLER: Absolutely.

LYNCH: Okay. So the day I decided, "Today, I have to make the decision," I got up and it snowed. Snow in Boston is pretty for the first few hours and then it turns black from auto exhaust and clogs the storm drains so that one wades in ice mush to cross a street. I said, "If ever there's a sign, 'Get out of Boston,' that's it." So I went to Stanford. [Laughter]

ZIERLER: And did you know who you wanted to work with, who your graduate advisor would be before you got there, or that relationship developed later on?

LYNCH: That only developed after the first year at Stanford. However, David Ritson had been an assistant professor at MIT while I was there and he left the same time I did and went to Stanford. These were independent events, but I had been told that "David Ritson is going there. He's a good guy." It turns out he is a good guy, but it was a little while later when we actually connected. It was only in my second year in graduate school that I connected with Ritson.

ZIERLER: What was his research at the time that you met him? What was he working on?

LYNCH: This was in particle physics. He had originally started in cosmic ray physics, and then I don't know at what point he changed to other things in HEP (high energy physics). At the time he was at Stanford, he was working on a linear accelerator there called the Mark III. It was a 1 GeV electron linear accelerator. He was also interested in the storage rings, and he and Burt Richter worked together at Stanford thinking about, "How do we build a storage ring?" which eventually became SPEAR at SLAC. That was several years later, though. Before entertaining SPEAR, Burt conceived and built an electron-electron storage ring, but I don't recall if Dave was involved in that.

ZIERLER: Was SLAC underway by the time you got to Stanford, or it was still a few years out?

LYNCH: It was just beginning. I remember I was at Stanford, and there was a Physics department get-together at the beginning of the academic year for all the grad students in 1961, and that's where I first met Pief (W.K.H. Panofsky). He probably was not then the director because there was a time when Ed Ginzton was the director placeholder, but then there were possible conflict of interest problems and so Pief was the first official director.

ZIERLER: Harvey, did you get to witness the rift that was developing between the SLAC faculty and the physics faculty?

LYNCH: Yes, but it didn't sink in. As a graduate student I did not see it. It was only after I had gone away for a post-doc at CERN for two years and then came back to work for SLAC that it was then clear.

ZIERLER: So as a graduate student you really weren't involved with SLAC that much. You were located in the physics department.

LYNCH: It did not exist. They were still digging holes in the ground when I was a graduate student.

ZIERLER: Right, right. How did you go about developing your dissertation topic? Obviously this is a very exciting time in particle physics. What were some of the most compelling things that were available to you to work on?

LYNCH: Dave Ritson and I sat down, and we talked and he said, “How would you like to do this? It’s an interesting topic,” and I said, “Sure.” We didn't actually explore lots of other things.

ZIERLER: What was that topic?

LYNCH: That was inelastic scattering of electrons from protons. Bob Hofstadter got his Nobel Prize for elastic electron-proton scattering, and this is inelastic scattering. So you're looking at the nuclear structure, if you will, in a different way. People at CEA (Cambridge Electron Accelerator at Harvard) had done some work on that topic, but it began a year before I started. So we in fact ended up with results in the same timeframe, although they were about a year ahead of me in terms of publication. When someone from Harvard, a member of the experiment, visited Stanford, I pointed out an error in their radiative corrections, but he didn’t believe me.

ZIERLER: Just to zoom out a little bit, what were some of the broader questions in particle physics at that time that you were thinking about?

LYNCH: Well, quarks. What are those things? Just a sort of fake? Computational artifice that helps you out? There were other competing ideas such as the eight-fold way or specific symmetries, e.g. SU3. These were being talked about, but we didn't really have evidence that quarks themselves *had* to exist. That wasn’t until the deep inelastic scattering experiments at SLAC in 1967. Even then, it was still, “Well, what the heck is that?” Then in 1974 when charm was discovered, the ψ and D particles, then the quark was just hitting you in the face. They’ve got to be there.

ZIERLER: Right.

LYNCH: Finally people were saying, “Okay. We believe.”

ZIERLER: [Laughs] Besides Ritson, who else was on your thesis committee?

LYNCH: The only other one that I remember was Felix Bloch, and he was there just as a member of the physics department, not a specialist. He was not connected with particle physics itself, but he said something to me very profound during the examination for the final thesis defense. He asked a question and I first started off to say something and then said, "Oh, nuts. That's wrong." I had to back up and he stopped me and said, "Look. The Babylonians were good mathematicians. You think hard before you carve it in stone."

ZIERLER: [Laughs] That's great!

LYNCH: Very wise statement! [Laughs]

ZIERLER: That stuck with you.

LYNCH: I've always remembered it.

ZIERLER: Harvey, what did you want to do after you defended? What post-docs were available and most interesting to you?

LYNCH: The only thing I pursued was to go to CERN. That's the only thing I tried.

ZIERLER: Why? Why was CERN so interesting to you?

LYNCH: I wanted to do something different. CERN was just coming online with new things, doing good stuff. I had an NSF fellowship and I was able to go there for two years and do whatever I wanted. I didn't report to anybody financially. Technically yes, but I was not answerable to anybody because I had my independent money. After talking with several different groups, I worked with Carlo Rubbia.

ZIERLER: Oh, wow. What was Rubbia working on at the time?

LYNCH: CP violation had become a frontier field and one parameter describing it is a complex variable η_{+-} describing the K_S – K_L meson mixing. There had been earlier estimates that depended upon regeneration of K_S from K_L for measuring the interference between K_S and K_L in decays, and there some questions of whether enough was understood of the regeneration process to separate out the needed information. This was the first experiment measuring the phase of η_{+-} without regeneration but rather directly from production of K^0's to observe them in real time as opposed to regeneration.

ZIERLER: What was the outcome of the experiment?

LYNCH: Oh, we measured the phase of η_{+-} and we got a good answer, eventually. There was a problem initially. There were two separate analyses going on and they gave different answers. Carlo & Co. did one, and I did another with early participation of Harold Ticho who was on leave from UCLA. Carlo & Co.'s answer agreed with the expectation, but mine did not. Somehow, I don't recall now exactly how, I found an error in their fitting program, so their answer was wrong, but it still did not agree with me. I told Carlo, and he did not believe me, and their result was presented at a conference. To his credit, after the conference he and his people went back and looked at the data and found a background that influenced our analyses differently, because of different event selection. Once that was fixed, we agreed on the answer and published the result. Of course, later experiments got much more data and got a better answer, but we collectively eventually got the expected answer.

ZIERLER: Did you work closely with Rubbia? Did you get to know him as a person?

LYNCH: Oh, yes. Carlo Rubbia is a very bright fellow, and colorful. Sidebar. There was a book written just before I arrived at CERN called *Die Große Maschine* (*The Big Machine*). It was about CERN and the PS (the new proton synchrotron). The author was Robert Jungk, who also wrote *Brighter than a Thousand Suns.* In the book he wrote little tidbits Robert Jungk about colorful people. There's a quote in the book, translating to "There's a brilliant young Italian physicist who, if he had lived in the 18th century, would have been a pirate." That was Carlo.

ZIERLER: That's great! [Chuckles]

LYNCH: Very bright, but everything he did was hand-to-hand combat. One of my colleagues was Klaus Tittel. Klaus, like others would often work in the evening, and Carlo would come in and want to talk---and to fight. One evening, Carlo came bouncing into Klaus' office and said something outrageous. He just wanted to talk, and he can't talk without fighting. Klaus was trying to get some work done but he couldn't, and finally he got a plan. When Carlo came in, "Blah, blah, blah, blah, blah, blah, blah," Klaus said, "You're absolutely right, Carlo. That's really great." Then Carlo said something like, "What?! How can you say such a stupid thing? You know it's obviously nonsense!" [Laughter] It just deflated him! And Klaus got some peace. Carlo and I had a fight very early on; I won and he respected me forever after. If he didn't respect you, you were in trouble. He and I got along fine.

ZIERLER: What was the fight over? What happened?

LYNCH: Oh, it was a statistical question. It was not anything very profound. It had to do with correlated statistics. He got the wrong answer and he finally admitted, "Yeah, you're right." So okay. We got along.

ZIERLER: And you held your ground.

LYNCH: Oh, yeah. [Chuckles]

ZIERLER: Harvey, were you able to take some time and explore Europe while you were there?

LYNCH: Very little. When I first arrived, before I went to CERN, I drove across US to New York, put my Alfa Romeo on the SS *United States*, and went Le Havre. From there I drove around a little bit in France and Switzerland, and then started at CERN. Then while I was there, I made one or two little trips, but not more than a week.

ZIERLER: Did you get involved in any other projects beyond your work with Carlo at CERN?

LYNCH: No. In two years, you can't do much more. I was there for the beginning of the experiment. Jack Steinberger was also on the experiment, but I did not interact much with him.

ZIERLER: After CERN, what was your next opportunity? Did you know you wanted to go to SLAC at that point?

LYNCH: No. I got a call from Burt Richter, saying, "We'd really like you to come to SLAC." I'd known him when I was a student. In my second year of graduate school, I was a TA for him for the lab of whatever course it was, so he knew me. When it came time for me to leave CERN after two years, he called me in Geneva and said, "Harvey, we'd really like you to come," and I finally said, "Okay." It was that simple.

ZIERLER: Did Burt specifically have in mind what he wanted you to be working on when you got to SLAC?

LYNCH: No. No. That was open. He was at that time involved in developing an experiment which nobody remembers. He and David Leith were working together on what was called the π-ρ experiment ($\pi^- p \rightarrow \rho^0 n$) looking at vector dominance in hadron interactions. In parallel with that, Burt and Dave Ritson and others were working on the storage ring project, and it was approved in '69 or '70. So then I said, "Okay, I'd like to work on that," and that's what I did.

ZIERLER: What year did you get to SLAC? What was your first year there?

LYNCH: I arrived in August of 1968, so SLAC was just running, and results were starting to come out. The results of the deep inelastic electron scattering were still in the making. So I remember Feynman coming to SLAC to talk about his parton model to describe the deep inelastic electron scattering.

ZIERLER: You remember that talk.

LYNCH: Yes.

ZIERLER: What was your work at SLAC? What was the first project you were involved with?

LYNCH: At SLAC, the first thing was the π-ρ experiment. Fatin Bulos and I worked on the construction of planar spark chambers with magnetostrictive readout. That's an ancient technology which is no longer used. There were planar spark chambers on both sides a "picture frame" magnet. We looked for two tracks made by two pions coming out of the target, so you measure the momentum in the spectrometer. From the momenta and angles one could reconstruct the ρ produced. It's a very simple experiment.

ZIERLER: What is your initial title at SLAC? Are you a research associate, a post-doc?

LYNCH: Hmm, I think I was called a research associate. Then after a few years I became an assistant professor.

ZIERLER: What was your next project? What did you work on next?

LYNCH: The next thing was SPEAR, and that was for the rest of the time at SLAC.

ZIERLER: Right, right. So did you watch? Were you involved in the development of SPEAR from the beginning?

LYNCH: Oh, absolutely. Yes, I was part of that group.

ZIERLER: Who else was in that group?

LYNCH: Oh, about three dozen people in all. In Burt's group—let's see—there was Rudy Larsen, who was part of the conceptual design of the detector itself. Adam Boyarski was in charge of software. Marty Breidenbach was also a big wheel in software. Vera Lüth came somewhat later. Roy Schwitters and myself. Roy and I worked on the cylindrical spark chamber and I designed the trigger logic. We turned out to be co-spokesmen for phase two of the SPEAR experiment. Then there were also people from LBNL, George Trilling, Gerson Goldhaber, Willi Chinowski, John Kadyk, and Gerry Abrams. There were also some grad students from Stanford and UC Berkeley. There were also some visitors from France and later Italy.

ZIERLER: Since you were right there from the creation, what were some of the major research questions that compelled SPEAR to come into existence?

LYNCH: A the time e^+e^- physics was essentially a new field that offered a new way of exploring the world of "elementary" particles with a well-defined initial state, instead of the complicated situation of hadron-hadron collisions. The simplest question one could ask was the total cross section for e^+e^- goes to hadrons as a function of energy, which was both profound and terra incognita over a wide range of collision energies. We didn't talk about it so much at that time, but it turns out that that cross section is an important piece of information needed for understanding the anomalous moments of the electron and muon because of what is called vacuum polarization. That intermediate state has an important contribution from photon goes to hadrons back to photon. That makes an important contribution to g-2. That wasn't a selling point, but it turned out to be important for that.

ZIERLER: What were some of the major technological or technical challenges in getting SPEAR up and running?

LYNCH: Let me begin with the detector. Namely, this was the first pseudo 4π steradians detector that anybody had made. Yes, "4π" is the HEP jargon, but it is only suggestive. In reality we could cover about 70% of that. That was already a challenge. We chose a cylindrical geometry because we wanted to have symmetric detection in all directions around the beam line. That turned out to be very important when measurements were made while using transversely polarized beams. The only technologies that were available at the time for tracking were wire chambers of various sorts. There were proportional wire chambers, but you had to have an amplifier at the end of every wire. That was prohibitively expensive. Spark chambers were known, but nobody had ever made a cylindrical, wire spark chamber. Dave Fryberger and I pioneered the idea of a magnetostrictive readout in a magnetic field. People had made magnetostrictive readouts before, but that was in the absence of an impressed magnetic field. The readout was called a "wand" consisting of a magnetized, ferromagnetic wire stretched over the length of the backing. Current from a wire of the spark chamber passing transversely generated a longitudinal, acoustic wave in the wire. At the end, a tiny pickup coil detected the longitudinal wave because the compression/expansion of the wire changed the magnetic field. By measuring the time of arrival, one could deduce which wire was hit by the spark, and thus the spatial coordinate of the hit, in one dimension. The location of the chamber wire itself provided the other two dimensions. It is a clever multiplexing scheme that we inherited. The idea of Dave and me was that in an external magnetic field if you put the wand at a slight angle from the normal, you get a bias along the direction of the wire without saturating it, which was needed to make magnetostrictive readout work. We took the idea with some apparatus, went to LBNL, and used beam from the Bevatron in a magnet to test the idea, and it worked. So that turned out to be the technology that we used for the readout. The next question was the circular shape needed. Normally, one wants to minimize physical contact of the wire with surroundings, or the acoustic wave is affected. Using small Teflon tubing for the wire, we learned that the interference was small enough that the readout worked as desired.

ZIERLER: Was your sense that SPEAR was the most ambitious project that SLAC had taken up to that point?

LYNCH: Oh, heavens no. SPEAR was a small project. The whole project was $6 million. Building the machine, the detector—everything was $6 million. The big things were the End Station A spectrometers where the inelastic electron scattering experiments were done. That's where the real money went.

ZIERLER: And when you say you were with SPEAR the whole time, right, what were the challenges there?

LYNCH: Burt, Dave Ritson and Ewan Paterson were initially the people who were involved in the details of the machine. Later machine physicists John Rees and Martin Lee joined the effort. The technology of storage rings was a developing field of its own, and maintaining a stored beam was a major challenge. There were many new instabilities of the beams that were discovered and eventually understood. People at Frascati did ground-breaking work, first with the small, proof of principle, machine called Ada. This was followed by Adone (meaning "big Ada"), a machine built as a physics tool. We learned from Frascati, but there was a lot to be learned. There was a lot of exchange of information between Frascati and SLAC.

ZIERLER: In terms of the challenges of the day-to-day operation of SPEAR, can you describe that a little? What exactly were those challenges?

LYNCH: The operation is in two parts. There's the operation of the machine itself. We were all in one big room. In terms of size, I could be sitting in the control room for the experiment, and the other end of my living room plus dining room is where the machine operators were. There were problems with backgrounds, and the experiment people would talk directly with the machine people and say, "Hey, we've got backgrounds here. Can you tune to fix the backgrounds?" We actually had some apparatus set up to measure background rates so they could tune on that. That turned out to be very, very valuable, this sort of feedback. In terms of the machine itself, there were real problems that needed to be understood in terms of just machine dynamics. There were instabilities which were only being discovered and solved in that timeframe. I'm not up to speed on that, but I know they existed. There were instabilities of particle oscillations about the equilibrium orbit. If there is any perturbation in the field that keeps banging the electrons the same way on every turn, it will throw it out of the machine, so you have to bridge these gaps. There are magic places that you can work and specific places you cannot work, so it took a while for people to understand that and then get the measurement tools to say, “Okay, we know exactly what’s going on. This is how you have to set the machine to do that,” and then in the end it all worked. But it was a tour de force of imagination, technique, and mathematics to find out what will actually make this difference. Your cell phone seems garden variety now, but in those days such a cell phone would be way beyond anything we had in terms of technology and compute power! [Laughing]

ZIERLER: Harvey, what exactly was your role in the day-to-day operations?

LYNCH: Of the experiment? I had designed the trigger logic that was programmable, in a primitive fashion and I played a role in the design of the spark chamber readout. So, initially my job was just keeping my toys working. Roy Schwitters and I worked on the HV pulsing system used for the spark chamber, and so we kept that and the spark chambers alive and well. There were two phases of the SPEAR experiment. The first concentrated on the measurement of the total cross section of $e^+e^- \rightarrow$ hadrons. The second was intended to extend measurements to higher energies made possible by a major upgrade to the ring. As it turned out, however, the discoveries of charm and the τ lepton dominated the results. Roy and I were co-spokesmen of phase two, and I was in charge of day-to-day operations where I would write out the run plan and make sure that everything was working. I was actually on part of every single shift of every day just to see what was going on to make sure things were working, all the equipment was working, people were doing what they were supposed to be doing, and that sort of thing. I didn't have a specific responsibility apart from “Let’s keep it all working, folks.”

ZIERLER: At what point were you promoted from the post-doc position to the faculty position?

LYNCH: 1973, I think.

ZIERLER: How did that change your responsibilities at all?

LYNCH: It made no change.

ZIERLER: It didn't. So with faculty there weren't opportunities or requirements to teach.

LYNCH: Oh…no. Certainly not for an assistant professor. That was not in the cards at all. In fact, at that time there was still a lot of tension between SLAC and Stanford. Roy had students, but I did not. I’m not sure if anybody at SLAC taught at Stanford at that time. They might have just begun. There were Stanford people who worked at SLAC, but teaching by SLAC, that was super touchy. I’ve forgotten when that started.

ZIERLER: Yeah.

LYNCH: When I was working at SLAC, it was just beginning. By the time I left SLAC in '76, the super professors could teach on campus, but that was about all.

ZIERLER: I'm curious to what extent Panofsky was involved in SPEAR, how interested he was in SPEAR.

LYNCH: He was very interested, but he did not take part in it directly. He was the guy who made it possible, but he had no connection with actually making, designing, building, managing anything in the experiment. That was Burt's to manage.

ZIERLER: And by making it possible, you mean he secured the institutional support that allowed it to get funded.

LYNCH: Yes. In fact, there's a wonderful quote in Pief's autobiography, *Panofsky on Physics, Politics, and Peace: Pief Remembers*. SPEAR was never an approved project. It was all done on internal funds. At one point he went to Washington testify and said, "I would like to report the discovery of an unauthorized particle on an unauthorized colliding beam facility." He's talking about the ψ (psi), and he got away with it! Doing this today is forbidden by the strict stove-piping of the budget lines.

He was on very good terms with the then Atomic Energy Commission, and that was something which was just so wonderful. It was not adversarial. Today the relationship between DOE and the labs is close to being adversarial. That's a bit of an exaggeration, but it's really close to that sometimes. Pief has told me this story more than once, and I think that you can find it in his autobiography. He once got a call from AEC and they said something like, "Hello, Dr. Panofsky. We'd like for you to do blah, blah, blah, blah, blah, blah, blah." Pief said, "That's crazy! You can't be serious." "Oh, okay. Goodbye," and that was the end of the conversation. There was respect for the lab management. That kind of interaction would not happen today.

ZIERLER: [Laughs] Harvey, what was the decision behind you being named as the co-spokesperson for SPEAR?

LYNCH: I don't know.

ZIERLER: Who asked you to do it? Was it Burt?

LYNCH: Yes. It was probably related to being the assistant professor, but nobody said it in those terms.

ZIERLER: What were your responsibilities in that role? Would you be doing press conferences? Would you be doing travel?

LYNCH: Oh, heavens no. My responsibility in that role first was in the preparation of the proposal for phase 2 running and then making the experiment run day-to-day. Press conferences didn't exist then the way we see them now. I can't remember there being a physics press conference in that timeframe on anything related to HEP.

ZIERLER: Right. But this would certainly change with the November Revolution.

LYNCH: That did change things, and Burt was central to such press conferences.

ZIERLER: Can you talk about, as a witness to the November Revolution, what that was like for you to see that?

LYNCH: How many weeks do you have to talk? [Laughter] The whole thing began as something exciting and grew enormously from there, becoming what would be known as the November Revolution. Initially I was a skeptic. You know the background, how people found some anomalies in the total cross sections of $e^+e^- \rightarrow$ hadrons as a function of energy. Some specific runs had higher yields than others, and that's what initially attracted attention. Roy Schwitters was one of the prime advocates for that, to go back and see what happened at about 3.1 GeV. I was a skeptic and it turns out I was wrong.

ZIERLER: On what basis were you a skeptic?

LYNCH: Oh, just general principles. I mean, something was going crazy. I don't know what the heck it was, but what makes the yield jump around like that? That's the reason you want to go back and re-measure. I wrote up the run plan for what we were going to do. I said, essentially, "Start at 3.0 GeV to make sure all the apparatus works, and that the yield makes sense. That will be the baseline." Backspace, we need some context.

We actually had to measure the relative cross section online. There was no technology to do that, except we had a one-event display that would show on a CRT (cathode ray tube). A person on shift would manually scan the events as they came up. With a trigger the display would show tracks, and from that events were broken down into three exclusive categories: (1) "Bhabha", really Bhabha scattering (e^+e^- elastic scattering) or μ-pair production, but the Bhabha scattering is dominant, (2) multi-hadrons, and (3) junk. A tally was made in real time by that person, with pencil and paper. For each setting of the machine energy the ratio of multi-hadron events to "Bhabha" events was plotted as a proxy for the true multi-hadron production cross section.

The run plan was to first establish a solid baseline at 3.0 GeV to make sure everything works. This took quite a while, on the scale of what would unfold, to have a reliable base. Then move the energy up in large steps, and then do a binary search inside the large steps to see where things happen. It turns out it didn't take very long to find something. I was on shift and ended my shift writing that the yield around 3.1 GeV was a little (50%? I don't remember) higher than the baseline and said, "Well, if this holds up, maybe we're on to something." It was only later that night that people went back and did the binary search and found this peak that was 100 times higher. Michael Riordan's book, *The Hunting of the Quark*, has a nice narrative on the subject.

ZIERLER: In terms of putting this all together that would lead to the drama around what we historically know as the revolution, did this happen quickly or did it happen over a period of time? Was it a sort of--

LYNCH: Very, very, very quickly. Within 24 hours…maybe it was 36 hours…we were busy… There was a subgroup busy writing a paper because this was just so astonishing. We had to write it up for *Phys Rev Letters*. I don't remember the exact timeframe, but the scale was a day.

ZIERLER: What were the immediate repercussions of this discovery? What was understood now that made this so fundamental and so exciting?

LYNCH: Well, a peak in the cross section—that was totally unexpected. Now in retrospect, people had predicted such things—charm, for example—but nobody had specifically worked it out and said, "Yeah, look for that." There was, however, the idea of a "scalar" particle, but nobody had an idea of the mass. So if there was a scalar, it would have spin zero, and that was a fundamental question. If you find a peak or something, what is the spin? Is it spin zero for a scalar or something else? In fact, we got help from the theorists on how to think of those in terms of just measuring cross sections. The key parameters are the partial decay widths for decay to electron pairs, muon pairs, hadrons, and everything. The bare cross section can be characterized by a Breit-Wigner shape, but the observed production rate is a convolution of the Breit-Wigner shape, the intrinsic energy distribution of the beam energy, and radiative corrections that smear the peak to higher machine energies because of energy loss to photons. This could only be calculated by numerical integration.

Measuring the spin, parity, and charge conjugation was something which I dug up based on an earlier experiment that had been done at Orsay to measure ρ - ω interference. For the ψ case the idea is to look for interference the two amplitudes for $e^+e^- \rightarrow \psi \rightarrow \mu^+\mu^-$ and $e^+e^- \rightarrow \gamma \rightarrow \mu^+\mu^-$, i.e. between this thing and just the ordinary, single, virtual photon production from e^+e^- annihilation. If you go below resonance, there's a negative phase and you'll find a lower cross section than normal due to interference. If you see that interference, then you know that this particle has the same quantum numbers of the photon, i.e. J^{PC} (spin, parity, charge conjugation) = 1^{--} particle, and that's what we did. Adam Boyarski, Vera Lüth, and I did this analysis. The result to me was the clinching argument that this had nothing to do with this scalar boson. Rather, it was something that has the same quantum numbers as the photon. After the existence, that was the first great thing that came out of it. Once the fact that this was there, what the heck is it? It has the quantum numbers of the photon, and then what?

There is some irony in this measurement. A precision QED test of $e^+e^- \rightarrow \gamma \rightarrow \mu^+\mu^-$ could in principle have discovered the ψ because the cross section below the resonance is a little lower than the QED value. Following that further could have found the cross section a little higher above the peak. The two measurements would have been reproducible indicating something odd in between.

ZIERLER: How did that change SLAC?

LYNCH: Put us on the map. The inelastic electron scattering had put us on the map, but it didn't generate the excitement that this did because the deeper reason was more subtle. For the ψ it was, "Oh my god. Where did this come from?" Then very soon thereafter we found the charm mesons and then the quark model became a reality. You've got to believe quarks now. You've just got to. You can't hide it anymore. Those two together, just back to back. The inelastic electron scattering was '67, '68, and then we were there in '74 and that made us king of the Earth for a while. In a way, we had an embarrassment of riches. We published many papers on the charm systems. One time we submitted a paper to *Phys Rev Letters* and a referee wanted to reject it, "because they are publishing too much". The editor overrode the referee.

ZIERLER: Was your sense that the buzz around an eventual Nobel Prize was sort of obvious?

LYNCH: Oh, yeah. Oh, yeah. Well, Burt and Sam Ting got the Nobel Prize in '76. I was at DESY at the time.

ZIERLER: What was the opportunity that compelled you to go to DESY? Were you looking for something after SLAC?

LYNCH: No, I was told to look for another job.

ZIERLER: What happened there?

LYNCH: Well, the simple answer was Roy and I were both assistant professors. Only one was going to get promoted and he did.

ZIERLER: Oh, I see. So it was a tenure-line position.

LYNCH: Yes. That's right.

ZIERLER: But only one of you actually would get tenure.

LYNCH: Correct.

ZIERLER: So what was the work at DESY that brought you there? What was the project?

LYNCH: Well, I had known Bjørn Wiik from much earlier days at Stanford, and they were doing some work on their storage ring, DORIS. It was similar to SPEAR, but a different design, and a totally different experiment, DASP. They were working on that experiment and then down the road people were starting to think about what became PETRA at DESY and PEP at SLAC. So I got involved in PETRA at DESY. We built the experiment called TASSO.

ZIERLER: When you got to DESY, you now have CERN and SLAC as places to compare. What were your impressions of DESY?

LYNCH: Very positive. That brings me to something which was "interesting." At the time I was still at SLAC ('75, '76), people at SLAC were so excited about the SPEAR result. "Let's build a bigger ring," so that was what became known as PEP. Well, it takes people to do that, and when SLAC was originally built, there were some senior people. Then they brought in younger people because you need more bodies, and people were brought on at that time in what was called a "beam-plus-three" contract where they'd work on whatever it was, and then once there was beam time, you got to work as an RA or post-doc or whatever you want to call it for three years. Then after that it was, well, whatever. Somebody proposed the same model for PEP.

In '75, '76, the market had changed drastically from '65, and jobs at that time for fresh post-docs were scarce. So I felt that was exploitive and I said, "Look. If you guys are serious, you senior people need to be involved as opposed to hiring slaves to come in and then wish them luck finding a job elsewhere." I wrote a strong memo to Pief and the SLAC faculty. Sid came to me and said, "Harvey, you shot us dead right between the eyes and you're right."

DESY, on the other hand, in parallel, got excited about the idea of a big ring and they wanted to build PETRA. At DESY, if you worked at DESY, you worked on PETRA. No questions. It was very clear from the beginning. Everybody's involved. Well … there was one exception for someone who was doing work at CERN. There was none of this elitism. That I found very impressive.

ZIERLER: How did that play out on a day-to-day?

LYNCH: Well, that gets into personalities, but it worked. Some things worked better than others, but it did work. Now the other side that did not work was that the relationship between the people running the experiment and the people running the machine. At SLAC it was very, very close. At DESY, the people running the machine didn't want to know anything about the guys who were taking data. "Ja, the beam's noisy? Okay, we'll do something, but diagnostic? No, forget it. We're not interested." That I didn't like and I tried and didn't get anywhere to improve it.

ZIERLER: Was DESY as international as CERN?

LYNCH: Not at that time. Initially there were people from other countries there, but not as much as CERN. Later, however with PETRA, the collaborations were broadly international.

ZIERLER: Was English the lingua franca?

LYNCH: Yes. Among physicists English was the standard language. If you wanted to talk with the techs, it was German, but among the physicists it's English.

ZIERLER: Were you able to pick up German at all?

LYNCH: I had learned German when I was in college. I had two years of German in college from an interesting guy who was originally from Vienna. Here's a fun story for you. He spoke 12 or 13 languages…

ZIERLER: Whoa!

LYNCH: …one of which was Rumanian. He was in Vienna at the Anschluss when the Nazis took over, and they censored the mail. He had a girlfriend, and she also spoke Rumanian. There's an old-style handwriting. I've forgotten what it's called, but it's not the cursive style that you and I know. It's a lot of saw-tooth strokes, and he had learned that. It was common at that time for old people to use it. So he and his girlfriend exchanged mail with the old script in Rumanian and the censors gave up. They just sent the letters through.

ZIERLER: That's great.

LYNCH: But anyway, I don't know how we got onto that. Oh, the language. When I was at CERN, I interacted with German technicians and I spoke German with them, and also German students. Then at DESY, naturally there were lots of German people, so I picked it up. Now I'm comfortable in German.

ZIERLER: When did you get involved with the TASSO detector?

LYNCH: Oh, from the very beginning. In the design phase at the very beginning of TASSO.

ZIERLER: What were some of the major research questions surrounding TASSO?

LYNCH: There were no specific questions apart from, "This is a higher energy machine than SPEAR; there must be lots of great things out there. Let's see what there is." There wasn't anything like, "Yeah, we've got to go find the Higgs." There was an issue which was resolved in that timeframe. That's jets. Jets were something that people talked about. SPEAR found evidence for jets, but it was really kind of fuzzy. The machine energy was just too low for a clean case. The energy at PETRA was much higher so you could actually see three-jet "Mercedes stars" – quark – quark – gluon jets -- once in a while being generated. We saw those at TASSO.

ZIERLER: Was the TASSO detector in direct competition with similar projects elsewhere in terms of what it was looking for?

LYNCH: Oh, yes. Everybody's. That was the problem. PETRA had three or four experiments running, and PEP was going to have three for four experiments running all looking for the same physics. PETRA was ahead of PEP in time and construction, and so we had beam before they did and we had physics results before they did. Early on after we had some data, I came to SLAC to give a colloquium and warned people. There were people at SLAC whose attitude was "The Germans can't do anything right. We'll beat them. Yes, we're behind now, but we'll beat them." I said, "Be careful. The Germans do know what they're doing. It's working. There's going to be serious competition," and there was.

ZIERLER: And you were proven correct.

LYNCH: Yes.

ZIERLER: How fast did it take for that to be proven true?

LYNCH: Oh, within the year. We had results, interesting results the total hadronic cross section and jets before PEP did much of anything, so it was clear that the Germans could do something right.

ZIERLER: Harvey, what were some of the challenges in designing TASSO?

LYNCH: It was a question of scale. We were building something much bigger than the SPEAR detector. It was basically a question of scale. We had to build a big liquid argon calorimeter, which that was a big deal. That was certainly the largest liquid argon calorimeter that had been constructed at that time.

ZIERLER: What was your sense of the budgetary environment at DESY compared to SLAC? How well funded was TASSO?

LYNCH: I don't know of any problems with funding for TASSO. There had been a mild recession, and the government was looking for some public works. DESY "just happened" to have the PETRA proposal in a desk drawer, and it was welcomed. I don't know of funding problems at SLAC, either. But one thing which was very interesting to me. In this country PEP was funded entirely by the US government. From 1974 to 1977 the agency was called ERDA. Then it became DOE. Still, it was all US federal government money. Hamburg with a population of a bit under 2 million is administratively the equivalent of a state in the US. It's a self-standing member of the federal republic of Germany. They put in 10% of the budget to DESY. That was unheard of here at that time.

ZIERLER: That is.

LYNCH: Except later, when SSC came along, the state of Texas put in $2 billion for SSC. I don't expect that to happen again.

ZIERLER: Yeah. Yeah. Harvey, did you have a time limit at DESY? Could you have stayed longer if you wanted to?

LYNCH: I could have stayed longer; the job was permanent. Eventually I did want to return to the US, and it was becoming increasingly clear that the longer I stayed at DESY, the harder it was going to be to return to the US. I finally had to do something, so I took a visiting professorship at UC Santa Barbara (UCSB) as a bridge because if you're in DESY, nobody in the US even sees you except at a conference. Santa Barbara was a bridge step, and eventually I came back to SLAC.

ZIERLER: Was this really your first opportunity to teach when you were at Santa Barbara? Did you take on teaching responsibilities?

LYNCH: Yes, I did. I taught two courses. One was a course in particle physics, and that was a wild ride. It was a one-semester course, UCSB's only HEP course, and I had to go, figuratively starting from the Dirac equation and ending with grand unification in one semester. You might imagine that's a bit of a wild ride.

ZIERLER: Yeah, I bet! [Laughs]

LYNCH: Very highly compressed things. It was very difficult to try to get my head around everything to present it to, oh, maybe 15 or 20 students where they had to begin… We began learning Feynman diagrams. Literally, Feynman diagrams were new to them, and then I'd have to go on and talk about other things. It was a lot of fun, but the first year was very strenuous. The second year I'd already done most of the work. I enjoyed it.

The other was a really fun thing. The course was called Astronomy in the course catalog. In fact it was a physics course in disguise intended for non-technical people. Within the department, this was called "physics for music majors." It had nothing to do with deprecating music majors; it was just that the technical / math demands were low. The students had to be able to do arithmetic and they had to learn to understand scientific notation like 1×10^6, but that's about all you were required to do. Normally it was taught by three professors, and they broke the course up into three parts. While I was there one of the three professors was out for medical reasons. They asked for volunteers to fill in the one part and so I did. It was fun because the third part that I taught was cosmology, and we used *The First Three Minutes* as the textbook. You know the book *The First Three Minutes.*

ZIERLER: Weinberg.

LYNCH: Yes. That was the textbook for my part of the course. It was an enormous amount of fun.

ZIERLER: Yeah.

LYNCH: I can still see some "kids" – 20-year-olds – sitting in the front row, their mouths open. "My god! That's wild and wicked stuff!" It was so much fun, so much fun.

ZIERLER: [Laughs] Harvey, did you enjoy the teaching life? Did you ever think about a career in a more standard physics department as a faculty member?

LYNCH: I would have liked to do that, yes. In the beginning I had an offer from UCSB before I went to SLAC, but I chose SLAC and then at SLAC it was out of the question, basically. So even after I came back to SLAC, teaching was not encouraged for non-faculty people.

ZIERLER: Right. But your intention was ultimately to get back to SLAC. Santa Barbara was sort of a bridge for you.

LYNCH: No, I did not intend to go back to SLAC; it's just that's where I ended up.

ZIERLER: So it wasn't as if there was a job waiting for you at a certain point.

LYNCH: No.

ZIERLER: So what were the terms of you coming back? How did that come together for you?

LYNCH: Oh, just discussions. I went to talk with people and SLAC finally said, "Okay, we'll make you an offer." The job was called a staff position.

ZIERLER: So this wasn't a tenure-line position. You didn't have to worry about not getting tenure at some certain point in the future.

LYNCH: No. It's a non-faculty position. You're there as long as they have money, or until you do something they don't like and fire you.

ZIERLER: What was your first job there? What did you do when you got back?

LYNCH: Let me think. When I was at UCSB, I had been involved with the UCSB group on a PEP experiment, and so I kept that involvement for a while and then at SLAC itself, it bifurcated. First, half of my time was spent with Dave Ritson's group with his experiment called MAC on PEP. I was primarily involved in the measurement of the total cross section for $e^+e^- \rightarrow$ hadrons as a function of energy. Radiative corrections were an important part of that work, and that was my specialty. The other half was to work on the positron source for SLC. I designed the magnetic "flux concentrator" to collect positrons produced in a tungsten target. And in the tradition of the *Car Talk* radio program, there was a third half that was spent with the group of people who then invented SLD a detector for the SLC machine. We scoped out the proposal for SLD.

ZIERLER: In what ways… You know, after your years in the wilderness, in what ways had SLAC changed since you were gone?

LYNCH: You mean to DESY and back in that timeframe, or for SLAC till now?

ZIERLER: No, no. From when you were at DESY and then Santa Barbara.

LYNCH: Oh. Very little change from 1976 to 1982. Changes came later. In 1984 Pief stepped down as director and Burt took over as the next director and that was a big change by itself.

ZIERLER: In what ways? In what ways was that more of a break than a continuation?

LYNCH: How do I say this succinctly? With Pief, the laboratory was a big family. Anybody could go talk to Pief any time you wanted. Everybody loved Pief. Burt was more remote. People did not connect with him, and empire building that I had never seen before began. People started building empires that would never have happened during Pief's time, and that has kept going ever since then. It's pandemic now, I think, with the way people do things. I'm not blaming Burt for that; it's just that was what society was doing. The days of the family gatherings were gone.

ZIERLER: When you say empire building, what does that mean specifically?

LYNCH: The group structure morphed into something different, going beyond the research groups. In today's parlance one might even use the word "tribe" instead of group to convey the flavor. A group people designing things, building things, whatever, would build their own little empires for their own power, really. That's too bald, but you just sensed that, "Okay, look. I'm in charge of X. I want to make X look as important as possible," as opposed to "What is out there that needs to be done and how do we all work together?" That's the change.

ZIERLER: What do you think accounted for that change? What was going on more broadly?

LYNCH: The world. It's just that society as a whole was moving in that direction. You've seen it in the business world. A prime example is Hewlett Packard. Historically, Hewlett Packard was a family, a very big family. Anybody could go talk to Dave Packard or Bill Hewlett. Not anymore, apart from the fact they're gone. It was a family, and loyalty meant something, and loyalty is a two-way street. You were loyal to the company; the company was loyal to you. That makes a very strong bond where when things get tough, everybody tightened their belt and worked hard and got past it. That's gone now. Recently I was talking with a colleague, L. David Montague, formerly with Lockheed. By chance he mentioned that the sense of mentoring and nourishing people began do die out at Lockheed in this timeframe. Loyalty… I don't know if you're a Trump fan, but loyalty to Trump is a one-way street.

ZIERLER: Of course.

LYNCH: And that's what businesses are like today. It's pandemic. Almost all businesses work on the "You work for us at our pleasure, and if things are tough, well, get lost."

ZIERLER: Harvey, where is the DOE in all of this in terms of assessing the changing culture at SLAC at this point?

LYNCH: They play a crucial role in that, although I don't know the extent to which they're free agents. They are the interface between us and the congressional types who know almost exactly nothing. The congressional types are often uninformed, and to a certain extent I have sympathy with them. They can't know very much about science, but many of them don't even want to know any more. So DOE has marching orders to do this, that, and the other, and all they can say is, “Yes, sir. Right away, sir. Three bags full.” They can't do what Pief said, not literally, but in effect, “That’s a dumb idea. Where did you come up with that?” That doesn't happen anymore.

ZIERLER: [Laughs] Now Harvey, when you got back, was your work on SLC and SLD concurrent or sequential?

LYNCH: The three halves of my life, PEP physics, SLC design work, and SLD proposal work were concurrent.

ZIERLER: Tell me about the SLD design. What were some of the challenges and opportunities there?

LYNCH: Scale again was the first one. Cost was another one. Everything was much bigger than anything we had ever built before, much bigger, and that meant changes in the way of thinking, particularly in making the distinction between desires and needs within the constraints. This became the first really international project of SLAC, so we had people from various countries taking part in it. Managing that was a little bit complicated, but we succeeded.

Communication, there are always problems, and one of the oddities is that Berkeley people were collaborators, and we had more communication problems with them than people in the UK. We were too close and too far at the same time physically. It’s a pain in the neck to drive from SLAC to Berkeley or vice versa, so you pick up a phone. But if you go there, you don't go prepared. If you go to Italy, you do your homework. You get everything prepared and you go and you have useful discussion. But if you get in the car and drive to Berkeley, “Oh, rats. I forgot to do… Oh, I didn't think of that.” Communication did not work as well locally as they did globally, I think. Other people may have different viewpoints, but my viewpoint was communications were a significant problem. They’re always a problem, but they were more serious on the small scale than the large scale.

ZIERLER: As an international effort with the SLD, who were some of the main collaborators, and what did SLAC offer that essentially global cooperation?

LYNCH: We had collaborators from labs and numerous universities in the US, Canada, Japan, Italy, and the UK. I don’t really remember how much money, if any, we sent to other labs or universities. Generally they had their own money, but there were probably a few exceptions. We did bring things together, and provided the infrastructure at SLAC and engineering help there as needed. Normally, however, they had to have their own engineers for their own toys. In terms of who did what, things were broken down on the subsystem level, and people took charge of them, like the vertex detector (UK), and the muon detector (Italy). Canada and Japan were attached to other systems, such as the drift chamber.

ZIERLER: Harvey, how did you get involved in CISAC, the Center for International Security and Arms Control at Stanford?

LYNCH: The background to that goes back to Sid Drell. I had known Sid since I was a graduate student, and he had been involved in defense-related things, for many years. He was one of the cofounders of CISAC, and one of the things that they wanted to do was to have science fellows. These would be mid-career people who would go there to use their scientific background to apply to important defense-related issues. I was one of the first people who was a science fellow. Sid asked me, "Would you like to be a science fellow?" I knew about CISAC and I said, "Sure!" I took a six month leave of absence from SLAC, and CISAC paid the equivalent salary. That was enormously profitable for me and I hope for other people.

ZIERLER: What did you get out of that experience?

LYNCH: It broadened my knowledge in defense matters in general as well as policy issues. In particular I learned a great deal about high-powered lasers and the propagation of beams in the atmosphere. Also, I met many new people that I would not have met otherwise, e.g. William Perry and Condi Rice.

ZIERLER: As a scientist, what did you think about just conceptually the idea of defensive or offensive laser beams in space? Was this science fiction? Was this realistic?

LYNCH: Well, for the Strategic Defense Initiative (SDI), science fiction is too strong a word, but let's say…a major technical challenge. Not impossible, but a big challenge. There was nothing that's factually wrong with it. It's just that, oh my god, actually doing it is something else again. There were two kinds of lasers that were talked about. One is an infrared laser, and the other was an x-ray laser. The infrared laser was "conventional" although enormous compared to anything that has ever been built. The other was an x-ray laser pumped by the detonation of a nuclear weapon; there was hardly even a proof of principle, never mind anything close to practical.

The infrared laser used hydrogen fluoride was the lasing component, and it was either hydrogen or deuterium, depending on what you needed to do. An HF laser was more conventional, with a wavelength of about 2.8 μm, and could only be used in space. If it was desired to penetrate the atmosphere, a different wavelength was needed, and that was possible with a DF laser, having a wavelength of 3.8 μm. In either case hydrogen fluoride is an exceedingly aggressive chemical. That plus water will attack nearly anything. The idea was to deploy a swarm of such multi-MW lasers in low earth orbit, each of which could generate tens of megawatts and project it onto a mirror that's like 30 meters in diameter to focus onto the target. The damage mechanism is heating.

The x-ray laser only works in space, because the x-rays would be strongly absorbed in the atmosphere. The damage mechanism is the physical impulse of the exceedingly high power, for a very short time, that can punch a hole in a missile. I did not pursue this option at all.

For the chemical laser, the challenges are (a) just to make the laser work reliably, and (b) all the logistics to go with it to identify the target, direct the beam onto the target and overall battle management. The laser is in orbit, and it is not possible to simply go to lay hands on it and fix any problem. The reliability demand is very high.

Battle management was huge challenge. People talked about the software being a big deal. It was a big deal at that time. People talked of a million lines of code that must work the first time that it is needed. Today, a million lines of code is no longer so daunting. You've probably that much or more in your cell phone. But in the 1980s, that was a really big deal. Reliability, however, is another issue. Software is notoriously unreliable, because of unforeseen events or circumstances. A old, standing joke is that, "If houses were built to the same standards as software, the arrival of the first woodpecker would be the end of civilization."

So technically there was nothing impossible; it's just a huge challenge.

ZIERLER: Harvey, what were the central conclusions of the report, and was your sense that this was something that officials in the Reagan administration were going to read?

LYNCH: The SDI was sold as a defensive system. My project was to look at the possibility that it could also be used as an offensive weapon. Some people had suggested applications using space-based IR lasers directed to the ground. For example there was a paper that was postulating that you could start firestorms with this sort of thing. That raised the mental image of the firebombing of Hamburg when 50,000 people died in one night of raids of July 1943. The scare stories like that were out there. So I studied the possibility, and no, it's not even close to being possible to do that with any credible system. I also looked at using such lasers for attacking ground or airborne targets such as aircraft. That is not very effective.

Whether anybody read my report or not I don't know, but it did give me exposure in the defense world so that I got invited to other things such as one called the Black Sea Experiment. That was a joint experiment involving the Natural Resources Defense Council in this country and the Soviet Academy of Sciences to actually look to see if you could detect a nuclear weapon onboard a ship. At the time, people were concerned about limiting nuclear weapons, and treaties had been worked out dealing with missiles from land, aircraft, or submarines. These are large investments and relatively easy to count. The possibility of cruise missiles being launched from ships could upset the counting, and people looked for some kind of inspection scheme. The Black Sea Experiment was geared to that idea. I don't know the background, however, of who proposed what for this experiment.

Can we find a proof of principle for an inspection regime? The Soviet navy said, "Sure. We'll loan you the services of the flagship of the Black Sea fleet, *Slava*." We went to Yalta, the home base there, went on the ship and made measurements. It was only because I had been to CISAC that I was able to meet these people to do that. That was, I think, a really important piece of work to demonstrate that yes, you can detect nuclear weapons onboard ships.

ZIERLER: Were you working directly with your Soviet counterparts, the scientists on the Russian side?

LYNCH: No, we did not. We only worked together for group discussions during the week we were in the Soviet Union. We didn't interact beyond that. The idea was to try different technologies during one day. We had one day on the ship, and the Soviets gave us free reign. "Go anywhere you want on the deck of the ship except the helicopter pad." We couldn't go on the helicopter pad for obvious reasons: If they land a helicopter, they don't want people in the way, but we could go anywhere else we wanted. The US Navy totally freaked out when they heard about the idea. They didn't want us to go because, "If you go, the Russians will want reciprocity, and there's no way we're going to let the Russians on our ships." But anyway, the Russians, the Soviets, were very accommodating to us.

So we made measurements. There was one bomb on the ship that we were told, "To save time, look here." We brought a germanium detector, a super high resolution gamma ray detector, and that worked very well. We also had a sodium iodide detector which was not nearly as useful.

The Russians had some other things. They had an *enormous* gamma ray detector of sodium iodide on a ship that sailed past us, and they could see gamma rays from the bomb. So they could detect it, but it was not very clear. The most beautiful experiment of all was a Russian, Soviet design. They had a neutron detector in a helicopter and they flew directly over the ship and they could say, "It's here," just by flying over it. At the joint meeting later in Moscow, just to demonstrate that they could do it, they gave us a briefing, saying, "And by the way, we have monitored this American nuclear-powered aircraft carrier at this latitude, longitude, date, time," and the weapon was there. They could distinguish it from the reactor that powered the ship. Later, I briefed the JASONs on that; they didn't know about it.

ZIERLER: Wow! Did you have a clearance for this work?

LYNCH: There was nothing secret about it. Nothing was classified. The Russians just told us that. We saw the helicopter flying directly overhead, and there's a video. I've got a video of it someplace where you can see the helicopter go by. Then we met again in Moscow to present our results.

ZIERLER: You enjoyed this experience.

LYNCH: It was great. Yalta, as you may or may not know, is a big vacation place in Ukraine, and we were there in the beginning of July. In fact, we arrived in Yalta the Fourth of July. There was a problem because there were ten of us from the US, and there must have been about 40 or 50 Soviet scientists and about 200 journalists, all these in Yalta at one time. Where do you put them? Hotels were totally booked because of vacations, so the Soviet Navy just said, "No problem." They parked a hospital ship at the pier there and that was our hotel. The evening of the Fourth of July, they gave us a Fourth of July party with Soviet champagne, caviar, the works. They were wonderful hosts.

ZIERLER: That's great. Harvey, did you--

LYNCH: The interactions were really very good.

ZIERLER: Did you ever think that you would pursue further work in this regard, or was this just sort of a fun leave of absence and it was time to head back to SLAC?

LYNCH: No, it was only a leave of absence. There wasn't a long-term job for anything like that. I had no clearance. I didn't get a clearance until I worked with the National Academy missile study in 2010.

ZIERLER: Yeah. So what did you do when you got back to SLAC? Was the SSC job already in the works at that point?

LYNCH: Not yet. SLD was still in construction at that time, and the machine, SLC, was coming on line, but slowly. The MK-II detector was on the beamline. Also, I maintained contact with CISAC and seminars that took place there. Then in 1991 I got a call from SSC, saying, "Would you be interested in coming to SSC?"

ZIERLER: How did that come about for you? Did somebody recruit you for that?

LYNCH: Yes, I got a phone call from Willi Chinowsky, who was at LBNL, saying, "Hi, Harvey." I had known Willi back from the SPEAR days. "I'm working with SSC people looking for how we're going to build this up, and would you be interested in being a deputy associate director at SSC?" "Oh, tell me more about it," and it went from there. It turned out that Willi was looking around, asking people, "Do you have any suggestions?" and Pief was the one who suggested me to Willi, who in turn called me and it went from there.

ZIERLER: I would have assumed that it would have been like a Fred Gilman or a Roy Schwitters who would have recruited you to SSC.

LYNCH: I'm just telling you what I heard.

ZIERLER: What was the job that he was recruiting you for?

LYNCH: Fred Gilman was the Associate Director of Physics Research Division (PRD), and obviously he needed help. Both Vera Lüth and I were officially called Deputy Associate Director. My portfolio was the detector interface to the Associate Director, so I had to look after what the experiments being designed were doing and communicate with Fred and then with whomever else that was involved.

ZIERLER: So what were some of the main tasks of that work?

LYNCH: Interesting you should ask. We had $500 million available to us from DOE for detectors for a little more than twice that amount of detectors, and the plan was that DOE would come up with half the money and foreign countries would come up with the other half of the money. Unfortunately, the way that Congress set things up, the foreigners did not feel very welcome. Congress' attitude was, "Deposit your money at the door and we'll let you come in," as opposed to being real partners, and that was a major difficulty.

The first problem that I had was almost when I walked in the door, Fred said, "Well hi, Harvey. Welcome. You have a problem." I had had $500 million but I lost $26 million before I even walked in the door because of a sleight of hand in the budgeting. During the budget making exercise people were trying to keep the cost down. The official cost of SSC was $8.249 billion, and somebody got the bright idea that they could reduce the apparent cost by saying, "Well, look. There are two collider halls, two huge underground halls for two big experiments, and well, if the experiments weren't there, there would be no need for heating and air conditioning," so that goes on the detector budget. That was $26 million!

ZIERLER: [Laughs] Maybe this should have been an early warning sign that something was very wrong overall with SSC.

LYNCH: That was an internal bit of skullduggery. There were other problems which we can talk about later if you want. The other part of my job was I had to go to meetings with PB/MK (Parsons Brinckerhoff / Morrison Knudsen. They provided the AECM [architect, engineering, construction management] function for SSC). They had weekly meetings of what's going on, and there had been indications that they tended to look for ways to maximize their fee. My job was to make sure that they didn't pull the wool over our eyes in terms of what they were doing and padding the budget. It was an eye-opener for me to see how some of these guys worked. I'll give you an example.

ZIERLER: Yeah, please do.

LYNCH: Do you know what a Montblanc fountain pen looks like?

ZIERLER: Yeah, with the white star emblem on top.

LYNCH: The men – everyone I saw were men – always wore suits and ties. I had never before seen engineers wearing suits and ties at work. Many made a point of wearing their expensive Montblanc fountain pen – not ball point – prominently displayed in their breast pocket. Then there was one fellow who always had his jacket sleeve stuck awkwardly on his wrist so that everyone could see his Rolex watch.

ZIERLER: Yeah. Yeah. [Laughs]

LYNCH: That set the tone.

ZIERLER: Very different world than SLAC.

LYNCH: Very different world than SLAC, yes.

ZIERLER: Harvey, at what point did you start to see the writing on the wall that SSC overall was doomed?

LYNCH: [Sighs]

ZIERLER: Was that a slow process or was that a dramatic process?

LYNCH: I arrived in Dallas in December of ’91, but I didn't officially begin until January 1, 1992. In the latter part of 1992, there were some rumblings in Congress about cancelling the project, but by the end of the fiscal year that effort had been headed off. Still, there was a scare. When I went there, I rented a place to live. Initially I didn't want to buy because I didn't know where I wanted to live. So I decided to rent for a year and get the lay of the land in the Dallas area and then buy something after that.

ZIERLER: Did you have family with you, Harvey, at that point?

LYNCH: No. I’m all alone. At the end of the year I said, “I don't think I’m going to buy yet. I’m going to hold off.” It turns out I had to move for different reasons. I moved from Dallas to Cedar Hill, but that’s irrelevant for this conversation. And then in fall of 1993 the roof fell in, so I was very, very lucky—very, very lucky.

ZIERLER: Yeah, that you didn't buy. [Laughs]

LYNCH: The announcement of the cancellation of the project caused a sudden, large financial problem. By prior agreement, Texas made its annual financial contribution near the end of the fiscal year. In turn the spending profile of the lab was predicated on that timing, with DOE money up-front to fit. However, when the cancellation was announced, Texas understandably decided not to make that year-end contribution. We then had to stop spending immediately. One of my jobs was to call the various universities or labs that had contracts with SSC and tell them to stop spending money NOW, not next week. Some of those were painful conversations with serious financial consequences for those universities or labs.

Early in '93, after Clinton was elected, there were unfavorable news postings almost every day. There was an active campaign against SSC, and I have a binder of clippings about 10 cm thick for just that year. It's the disaster of the day, this, that, and the other. You know about the scandal of the potted plants?

ZIERLER: No, that I've never heard.

LYNCH: There was a big splash in the news about the extravagant things that these crazy people at SSC were doing. "They've got potted plants in offices." The office complex in Dallas they were talking about was literally a giant warehouse, most offices were cubicles, and there were a few enclosed offices on Mahogany Row. I had an office on Mahogany Row with the senior management. It was really a very austere place, and so we rented potted plants and scattered them throughout the building just to make something that looks alive. I had a potted plant in my office, and so I had one of the offending plants. A lady would come once a month from the rental agency to trim the leaves and make sure everything was right, and somebody would water it more frequently than that.

Here is another example of how weird things sometimes became. Some group obtained information on billing by PB/MK, and a claim of "waste, fraud, and abuse" was leveled for expenditures for coffee for about 400 employees. The amount was about $12,000 per month, and it was allowable under the rules. That amount works out to about $1.35 per employee per working day. It was all about "appearances."

ZIERLER: [Laughs]

LYNCH: But that was part of an organized, negative campaign.

ZIERLER: Harvey, was your sense that most of the problems were budgetary and bureaucratic in nature? I mean, what about the science itself? Was the science underlying the SSC all legitimate and valid as far as you were concerned?

LYNCH: Oh, the goals were certainly valid, as much as could be. The Higgs was the thing that was the first on the list of what we'd be exploring in a whole new energy range. The mindset was that there must be a lot more out there. That was the same motivation applied for the LHC. There's nothing wrong with that at the time.

We did have organizational problems within the laboratory. We had organizational problems within DOE. We had budget problems, some of which were self-inflicted, some of which were inflicted upon us by DOE and/or Congressional types.

The self-inflicted part was empire building, but this only came to light to me when things were starting to unwind. There was a Magnet Division and there was a Machine Division, and so the machine people would say, "We need magnets with these properties." The person who ran the Magnet Division had just left a key role in the submarine-launched ballistic missile program, the Trident D5. He had left that to go to SSC, and he ran the whole Magnet Division like a classified defense project. He expected the machine people to say, "These are the specifications for the magnets," and throw the specs over the transom to the machine people, who would design the magnets. The magnet people were not allowed to talk with the machine people to say, "Look, if you could relax this tolerance a little bit, it would make it a lot cheaper."

ZIERLER: Yeah.

LYNCH: That was forbidden, and it was a *huge* problem. I only learned about that when things were unraveling, and I don't know how much of that, if any, the DOE people or the congressional people knew. There were other multiple communication problems of empire-to-empire problems that I knew about, but I had no idea the scale of it.

ZIERLER: Did you have a job waiting for you if you needed it?

LYNCH: I was technically on leave of absence for two years. Fred was also on a leave of absence (two-years), but he didn't have a job to go back to because he stayed beyond his two years. I did have a job to go back to, because of the timing. When things began unraveling I said, "I'd like to stay a little bit longer to help with the shutdown of SSC. Can I stay an extra few months?" and they said, "Sure, no problem." So I stayed an extra couple months and returned to SLAC in March 1994. Otherwise I would have been without a job. Many people were without a job when SSC folded, many.

ZIERLER: Yeah.

LYNCH: There were people who left good faculty jobs, and they were just out of luck. They had nothing.

ZIERLER: Yeah. You were lucky.

LYNCH: I was very lucky, very lucky.

ZIERLER: What did you go back to? What was waiting for you when you got back to SLAC?

LYNCH: When I came back to SLAC, BaBar was beginning to do something.

ZIERLER: Right. Right.

LYNCH: In December 1993 there was the inaugural meeting for the BaBar experiment proposal. I came to SLAC for that meeting.

ZIERLER: The question with the BaBar detector—who was the driving force behind that?

LYNCH: David Hitlin, who was at Caltech, was the prime mover for BaBar, and there were meetings at SLAC about BaBar. The inaugural meeting was the first workshop to begin a proposal. This was the first big international operation for SLAC. SLD had international work, but BaBar was a much broader international collaboration from that. We had many people from the US as well as Canada, China, France, Germany, Italy, Norway, Russia, and UK. Later some more countries joined. Communications worked then. We had better communications than in the SLD time. People were getting used to remote control where you can actually use a telephone or email—email then was available and common. In the early days in SPEAR, email didn't exist and in the SLD era there was no universal protocol.

ZIERLER: Right.

LYNCH: Email makes a big difference. Email is a good thing and a bad thing. You certainly know that you can die of email overdose. But overall it really worked.

ZIERLER: What did you go back to?

LYNCH: After some negotiation after I arrived back, I was promoted to permanent staff. It's an extra degree of isolation depending upon budgetary restrictions, so it was quasi-tenure compared to just staff member.

ZIERLER: But this was more… You were getting more into the administrative side of things at this point.

LYNCH: No. Technically, I was called Integration Physicist for BaBar.

ZIERLER: What was your work in that role?

LYNCH: My job was to make sure that it looked like I was unneeded. By that I mean that I was to find problems before they developed to be big problems and fix them before we had muck on our hands. I was not entirely successful, but I had many successes in being unnoticed.

ZIERLER: What were some of the problems that you were able to uncover?

LYNCH: Oh, dear. There were numerous problems during magnet design and getting all of the subsystems on the same page. There were many problems of system A interacting with system B causing problems. There was a big concern that A does something and they don't tell B about it, and then that screws up B. So I had to know what everybody was doing and say, "Hey, wait a minute. You have to be careful. Don't do that because that screws up them," and so on. I can't think of anything off the top of my head now, but that was the flavor of the problem. There were many such issues. There were, about nine subsystems or something like that, and they all have to get together for one object in the end.

ZIERLER: Harvey, the overall comment you made earlier about empire building at SLAC and just the culture changing—had that continued in the two years since you were gone?

LYNCH: It had continued. It was worse.

ZIERLER: What was your subsequent job at SLAC after this?

LYNCH: I inherited a job when I returned to SLAC. Burt appointed me to be the chairman of the Radiation Safety Committee, and I served that role for 10 years. In my earlier life at SLAC more than 10 years before, I had served as vice chairman. This was a "citizens committee" to keep track of and review work and practices related to operating the accelerators or experiments in the context of radiation. The idea of a "citizens committee" dated back to the Pief era and was essentially unique among the DOE labs. The model was that in addition to the normal subject matter experts, people with vested interests in operations or experiments served to evaluate work and proposals with an eye to the practicalities and do get buy-in from the affected parties. There were some challenging times, but in the end I think that I was successful in that endeavor.

I worked as the integration physicist, and then finally we had built the experiment and it was running. The first results from BaBar were presented at a conference in July 1999. I got involved in some of the BaBar physics, but then in that timeframe people were starting to think about the Next Linear Collider, e^+e^- collider, the NLC, which is now called the ILC. So at that time, ca. 2002, Jonathan Dorfan was the SLAC director and he asked me to help him with that. He chaired a group called NLCSG (Next Linear Collider Steering Group), and I helped with that.

I was not involved in the design. It's just the logistics of meetings and getting people together and talking about things and then going to meetings with him sometimes in various parts of the country.

In 2004 there was an electrical accident in which an electrical engineer was severely burned by an arc-flash, and that event became a turning point for the Lab. We came under very harsh scrutiny by DOE regarding the safety culture and practice of the Lab. Jonathan asked for help in managing a couple different investigative safety committees. They became all-consuming in terms of my time.

Then finally, in the latter part of 2004 I had been interacting with DOE regarding ILC. Robin Staffin, who was the DOE man for HEP, wanted somebody to help him on ILC / NLC-related things, and so he wanted me to go to DOE, taking a leave of absence from SLAC. We had been negotiating that for some time, and we came right down to the wire. I was supposed to call him at, I'll say 11:00, on the morning of December 17th. That was the last working day of the year. I was supposed to call him to seal the deal to go to DOE. Late the afternoon before, I got a call from Persis Drell, who was the Associate Director of what was then called the Research Division, and she needed help. Because of the electrical accident, Jonathan needed administrative help, and he appropriated Steve Williams, who had been Persis's deputy.

Sidebar on Steve: He had been David Leith's right-hand-man when David was the AD of the Research Division before Persis. Steve kept a low profile but got things done. I still remember a little piece of paper that he kept stuck to his computer monitor. On it was written, "It is amazing what you can accomplish, if you don't care who gets the credit."

So, without Steve, Persis was short-handed. She had talked with Sid, her father, and she said, "One of the few times I listened to my father was when he suggested Harvey to help me." [Laughs] In principle, it was supposed to be a part-time job, but it was not long until it became a full-time.

ZIERLER: That's great. That's a great story.

LYNCH: Anyway, she said, "Harvey, I need help," and my response was, "As long as it's neither illegal nor immoral, I'll do it." I said nothing about fattening. Years later she thinks I said "nor fattening", but I carefully avoided that, because was I serious. I went on, saying, "But I have a problem. I'm supposed to call Robin Staffin tomorrow morning about going to DOE, and I'll have to tell him I'm not coming!" She said, "Oh, I understand." We had talked about DOE or NSF for such a leave before, so it was not a surprise to her. I called Robin and said, "Here's the situation." He said, "I understand…," and so I didn't go to DOE. That's when I became a full-time bureaucrat. I became an Assistant Director at SLAC and a paper pusher forever after.

ZIERLER: Harvey, I'm wondering what… You know, with the work that you were thinking about with regard to ILC, what were some of the things that you might have learned during your SSC experience that might have informed how you approached ILC?

LYNCH: Working on a large scale was the most important thing. A lot of things are happening. A lot of people have oars in the water, sometimes even rowing in the same direction. Lots of money is involved, and there can be people with vested interests who were involved. The big picture, grasping the big picture would be the real takeaway from SSC for me because ILC would be a very big project.

ZIERLER: And just to fast forward to present, what are your feelings on the current prospects of ILC?

LYNCH: I'll try to quote George Bush (the younger) at one point. "The odds are slim to none, and Slim just left the room." I think it's unlikely. I hope I'm wrong, but I think it's unlikely. I just don't see the support in this country, and I don't see the support in Europe. Japan cannot do it alone. The host country is expected to put up half the total cost. The only way I can see it happening is for China to say, "We'll do it." China does not have the political problems we do. They can just say, by executive fiat, "We're going to do it," as a matter of national pride or prestige.

ZIERLER: Also, as I understand, the labor is simply just less expensive in China in order to be able to do something like this.

LYNCH: Well, yes. Labor is certainly one of the things. There are lots of things that go together, but they would certainly need technical input to make the thing work, and I'm sure US and Europe and Japan would be happy to take part in the technical design and construction of the device, but not on the really high level. Whether China will do it or not, I don't know. I hope I'm wrong, but my take on it is that it will not happen.

ZIERLER: Harvey, what was your work when you got into the administrative side of things initially?

LYNCH: I'll give you an analogy offered by Bob Bell. Bob Bell was the chief engineer on both BaBar and SLD. One time we had a BaBar collaboration meeting and he was the last talk. He was supposed to summarize the issues that were outstanding in construction of the experiment. He said, "Well, my job is the last guy in the parade. He's the one that has to sweep up after the elephants." My job was to pick up the bits and pieces of things that had not gone well, so my job was to fix them or prevent them from happening again.

However, there was one part of the job that I much enjoyed. Each year DOE and its Italian counterpart INFN sponsored an exchange program for physics students. The idea was that students from one country would go to the other country to spend a couple months during the summer working side-by-side with practicing physicists to see what real research was like.

We had applications from master's level students who were the cream of the crop of Italy, and my job was to review the applications, talk with SLAC groups with whom students could work, and together with INFN people choose the students. Initially there were six students, but later financial pressures reduced the number to four. Once at SLAC, I was their primary contact, and, if needed, trouble shooter. I have several pleasant memories of meeting with the students for lunch or dinner, or in one case a trip to Big Basin redwood park.

ZIERLER: Who did you report to? You reported to Persis?

LYNCH: I reported to Persis Drell. We connected and got along just fine.

ZIERLER: What was Persis's style as a leader at SLAC?

LYNCH: Very outgoing personality. Very approachable. Very high integrity. Very direct. You always knew where she was coming from. There was never any question of, "Well yeah, she says this, but no, no, she really means that." First class human being.

ZIERLER: What do you see from your vantage point as some of her primary achievements, given the challenges that she was facing?

LYNCH: Part of the fall-out from the electrical accident of 2004 was that that Jonathan was eventually forced out as Lab Director in 2007. Persis had been invited join the search committee for a replacement, but in the end, she was drafted for the job. The electrical accident had left SLAC very vulnerable. There were highly placed people who wanted to close the lab. Persis was able to navigate that mine field and save the lab by talking to DOE and other high-ranking people. In parallel with the disfavor of SLAC, there developed a very negative attitude towards HEP from a high level of DOE. That became a branch point for the mission of the Lab turning away from HEP to what was originally called "Photon Science," using the Linac Coherent Light Source (LCLS). The Lab survived.

ZIERLER: That's certainly an achievement. Did you essentially remain in this role until your retirement?

LYNCH: Yes.

ZIERLER: What else did you accomplish during your tenure in this position?

LYNCH: Well, there are two different questions embedded into one, within SLAC itself and outside. Inside, I swept up after the parade, and I suppose that was generally successful. The safety committee work was eventually successful, as was the student exchange program.

The major accomplishments were outside, and that brings us back to the subject of national defense.

ZIERLER: What was the nature of that?

LYNCH: In 2001 I was invited to join a study group assembled by the APS. The topic was boost-phase missile defense. Briefly, that means intercepting an attacking ICBM while the main booster was still burning. The advantage is that in principle, at one stroke one can neutralize a missile possibly containing multiple warheads and multiple decoys. That is a huge simplification of battle management. The disadvantage is that the timeline to effect such an intercept is quite short, and special technologies would be needed to do that, if indeed it is possible. Being invited to join this study group was an offshoot from the earlier involvement with CISAC and my work on high-power lasers in the atmosphere. Initially, my job was to analyze the efficacy of the Airborne Laser (aka ABL), which would be a Boeing 747 outfitted with a multi-MW IR laser with fancy beam direction equipment and adaptive optics to compensate for atmospheric inhomogeneities. During the course of the study, our horizon expanded somewhat, and I took up the subject of the feasibility of space-based stations to launch defensive missiles. The study group had some true professionals in the field of missile defense, and we worked together well. That collaboration would later bear more fruit.

One person in particular was L. David Montague, who had been the President of Lockheed Missiles Division in Sunnyvale. Missiles had been his whole career. For reasons too complicated to describe here, David and I ended up assembling the contributions and preparing the final APS report for publication in July 2004. A PDF version is available on the APS web site. That report became a “handbook” for the subject, and some people in the Missile Defense Agency (MDA) later told us that they used that handbook. The end conclusion of the APS study was a strong statement that boost phase defense with any of the methods we knew at the time was not feasible. This undertaking was a part-time job for me that I did on the side. I learned a lot in the process, and made contacts that would be very useful some years later.

In 2010 the National Academies were charged by Congressional mandate to look at the boost-phase missile defense issue again as well as alternative defensive means, and David Montague was a co-chairperson of that study group. In turn, he invited me to join. I didn’t take any time off; I just got official permission to work on that in parallel with whatever I was doing in SLAC. I got official, legal office permission to spend one week per month with the National Academy. There were monthly meetings of one-week duration in DC and sometimes at other locations. This study occupied somewhat over a year elapsed time. David Montague collected a group for this National Academy study including most of the people from the APS study who did the technical work. In addition he brought in probably the world-leading expert on radar. We had the technical expertise to make a definitive statement, and we put together an unclassified report called *Making Sense of Missile Defense*. That’s available from the National Academy Press. It’s available for free as a PDF. There’s also a classified report which is not available unless you have appropriate clearances. But there's nothing that you need in the classified report that isn't in the unclassified, so you don't need the classified report to understand the conclusions.

I think, these two studies are perhaps the most important things I've ever done. They may have had some impact on attitudes toward missile defense in the country and in budget land.

ZIERLER: Who commissioned the study, or who was the driving force behind getting this study off the ground?

LYNCH: It was congressionally mandated. Congressman Duncan Hunter asked for a study and Congress mandated a National Academy study of the topic. We had to make some interpretations for the charge of what was in it and what was not in it. We added some things which they didn't specifically ask, but which were essential to the whole picture. For example, space-based interceptors, be they missiles or lasers, were not in the charge, but it's part of boost-phase missile defense. Likewise, they did not specifically ask about midcourse interception, but that's an essential part of the whole missile defense topic, so we added that also. We ended up saying boost-phase is a nonstarter. Midcourse is something which you can do, but it's expensive. Also the existing midcourse system called GMD (Ground based Missile Defense) has some serious shortcomings.

ZIERLER: How much of this work required onsite visits?

LYNCH: The most useful meetings were at the National Academy in DC, but some on-site visits were quite useful. One of the more interesting ones was a trip to Fort Greely Alaska to see the missiles in the holes in the ground. They actually opened one of the silos, and we could look down into the hole and see the missile. There's a shaft parallel to it so we could go and see it an arm's length away. It was interesting to see the hardware that's really there.

We also went to Colorado Springs, where MDA has the command center, just to see those people and talk with people on the ground who are actually doing things. We saw a test run of a missile alert. They do these frequently. It went something like this.

"We have radar contact indicating something may be coming out of North Korea. That's all we know now," and that lights up across the whole of MDA and more, but in particular it lights up the people in Colorado Springs. Then we saw it unfold play-by-play…simulated, of course. "We have bearing for it. It may be going to X. … time passes … We have so and so and so, and then, okay, we see it. … No, it's not threatening." We saw that play out in real time from a previously prepared simulation script and how the people responded.

What was really interesting is that the total elapsed time for a missile from launch to impact is 30 or 40 minutes, depending upon the target. They get first notice some minutes after launch. For the exercise there's somebody in DC who plays the role of the Secretary of Defense for the conversation, and the Secretary asks, "Well, how much time do I have to make a decision to permit launch?" The answer was very few minutes. He's got to decide quickly to authorize this launch or not because it takes some time for these missiles to leave Alaska and/or California and go all the way there to make a midcourse intercept at a location where it can do some good. That was a sobering experience. We of the group all had gone through the exercise with numbers as part of our work, but seeing the play on the stage is not the same as reading the book.

ZIERLER: Yeah. Harvey, who might have been some of the players that would have been not so happy to hear about the findings of the report?

LYNCH: There were various people who were advocates of specific systems. There were some people who have a fixation on space-based interceptors. That was not part of our charge, but we added it, and to this day I don't understand why they are so enamored with it. In principle, yes, it looks great because you have all these missiles positioned in outer space. The missiles are in outer space too; shoot them down; end of problem. The trouble is you need an awful lot of these interceptors because they're constantly moving, and most are out of range. One of my jobs was to work out the logistics of how many interceptors you would have to have. You'd make a scenario for what sort of reliability you need and what areas you need to defend, what launch points you have to cover, and then you work out the number of missiles, and it's a large number. It's many, many billions of dollars that are needed, way beyond anything else. The space cadets didn't like that at all.

ZIERLER: Who was the audience for this report? Did you get a sense of there were really important decision makers who were influenced by the findings?

LYNCH: The audience was literally Congress. The charge was to deliver to Congress a report, both a classified and unclassified report. So the audience, the root audience, was the congressional people doing budgets. David gave briefings to both House and Senate Armed Services committees. The other thing was to inform people who were interested in the topic, perhaps MDA, perhaps others, and then the public at large if they're interested in it.

ZIERLER: So what were some of the policy outcomes or decisions as a result of the report?

LYNCH: Nothing very obvious, but in the end, nobody is really pushing boost-phase missile defense today, but there were certain people pushing it even after our report was published and out there. Well, let me be careful. For quite a while nobody was pushing space-based interceptors. There are indications that zombies are coming back to haunt us again, but I don't know what the status of that is. As of a couple of years ago, there were zombies on the horizon, but they've disappeared again. I don't know what the status is today.

ZIERLER: So to what extent was this work simply an academic exercise, and to what extent do you feel like this was work that, in a real way, contributed to the national security of the country?

LYNCH: Oh, this was not an academic exercise at all. We had experts in the field, in missile design, missile development, missile intercept, detection methods be it by optical or radar means, command and control. We had all the experts on that, so this was not at all academic. This was hands-on work. How much impact it had, I can't judge. I can't do the experiment again to say, "How does the world unfold if we didn't have this report?"

ZIERLER: In a post-Cold War framework, sometimes it's a little hard to conceptualize, you know, what's the scenario that would make these hypothetical concerns become a reality. So what were those scenarios that you and the other members of the team were thinking of?

LYNCH: Well, there were two adversaries at the time that were of interest. One was North Korea. The whole MDA philosophy at that point, the whole missile defense that we have in place now, is geared towards North Korea. Our assets are on the West Coast. If Iran gets in the act, we're in trouble. There is some limited capability to defend the east coast, and of course Washington, D.C., against an attack from Iran, but it's limited. We made that very clear in the report, and I think that got people's attention because a lot of people live in Washington, D.C. who are in this business. "What do you mean this doesn't protect us very well?"

ZIERLER: Yeah!

LYNCH: I'm reading between the lines. Nobody has ever said that, but I think that did play a role. The post boost-phase / midcourse intercept is hard but not impossible, and you would like to have some more interceptors on the East Coast. We said so and suggested possible locations. People put in proposals to study that, and they settled on a site in New York that would be suitable. I think nothing has developed from that, but that was one of the outcomes, whether you like it or not, that is an outcome. If you need defense against Iran, then you need something on the east coast.

ZIERLER: Yeah. When you got back to SLAC on a more full-time basis, what were the last things that you did before you retired?

LYNCH: Oh, essentially nothing changed. I didn't leave to do this. I just was gone one week a month, so my job continued unchanged. I had email, so I could still do much my job remotely from Washington DC, Fort Greely, Colorado Springs, Huntsville Alabama, or wherever we were.

ZIERLER: Harvey, I'll save you the difficulty of reflecting on your own contributions if you can think a little bit or share about, you know, at your retirement party some of the tributes that were given to you by your colleagues.

LYNCH: [Sighs] Persis had some very nice words. She said, "One of the few times I listened to my father was him suggesting you for the job." As it happened, Sid was sitting beside me at the party and quietly expressed some surprise at that statement.

I think the most touching was not from my colleagues. Music is important in my life, and while I was at SLAC, one of my other jobs I didn't tell you about was that I was the interface between the lab and the St. Lawrence String Quartet at Stanford. I would get a phone call, or an email more likely, from one of the members of the quartet, and she would say, "We've got a very promising young quartet…" Young people would come to study with them, and so "We've got a very promising group who are at Stanford this week. Would you be interested in a concert?" and I said, "Sure!" So we would get a quartet to come up for a noontime concert. In the beginning, St. Lawrence themselves did it, but then more often it was these visiting people. When I came into the retirement party that day, the four members of the St. Lawrence quartet were in the room. They performed at the opening. The only other time I know of when they came to SLAC for a celebration rather than a concert for the Lab was for Pief's 88th birthday. Pief loved music, and I asked the Quartet if they would be willing to come to the party; they were pleased to come.

ZIERLER: That's so nice.

LYNCH: That was really touching.

ZIERLER: Well, Harvey, I'd like to ask you for my last question something a little forward-looking, and that is given your long tenure at SLAC, what do you see as its best path forward in terms of remaining viable, changing with the times, and continuing to do, even though it's doing, of course, very different things than when you started there? What might remain the same is that it's always at the cutting edge. So what do you see as the cutting edge as you look forward to the future, and what might SLAC's role be in that?

LYNCH: HEP is fading to black for SLAC. To be honest with you, the other programs are so different than anything I have any expertise in that I have nothing to contribute, really. While I was at SLAC it was called Photon Science. That is not a good name because they are just using synchrotron radiation photons to do studies in biology, mineralogy, condensed matter physics or other sorts which use synchrotron radiation as the tool. Mainly they use x-ray diffraction to examine detail. Some time after I left, the program was renamed to Energy Sciences. I know so little about what they are now doing that I have nothing to offer in terms of what they could and should be doing in the future.

One thing I know they have done which is really neat, and that is to be able to "take a movie," a movie picture of chemical reactions taking place. It's called a pump-probe experiment where you have a sample and you shine a very brief flash of light onto a sample. That starts a chemical reaction taking place, and then you blast it with the x-rays from LCLS to look at the diffraction pattern from that sample and reconstruct what it is. You vary the time between the pump laser and the probe laser and you assemble a moving picture in time at the femtosecond level, of how the reaction unfolds. That I think is a really neat piece of work, but beyond that I am not an expert.

ZIERLER: Well, to broaden the question out a little bit, as you look towards global projects in accelerator physics, high energy physics, experimental physics, what do you think are some of the most exciting projects, certainly not on the scale of an SSC or an ILC, but in what way can that field continue to move forward and continue to discover new physics?

LYNCH: Let me respond on several levels.

On the large scale, we have the LHC, but I am unfortunately of the opinion that is the last pyramid we're going to build. Machines like that have become too expensive. We don't offer enough bang for the buck for people who have money to offer that. I don't believe that ILC will be built, although I hope that I am wrong. Our toys have become too expensive, and I don't see ongoing support world-wide for more such projects.

HEP is going away at SLAC itself. It presently has a small contingent in ATLAS at the LHC. There are people taking part in the LUX search for dark matter. This experiment is on a much smaller scale than ATLAS.

Elsewhere, there is activity with LUX as well as the DUNE-LBNF long baseline neutrino facility experiment. These are both interesting things to do and on a scale that is financially supportable. A new, higher precision measurement of g-2 for the muon is under way. It is a nice piece of work, but it does not excite me.

Finally, the really exciting things for the near and more distant future lie in astrophysics, and in particular gravity waves as a means of dealing with the universe writ large.

ZIERLER: But just to be clear, you're not optimistic about the prospects of these things getting built, but if they were, you are optimistic about there being exciting new physics to discover.

LYNCH: Well actually, not really for HEP. The Higgs was discovered in 2012. Few exciting things has come out of LHC since the Higgs. Some important decay modes have been observed to confirm the Higgs nature. There are nice little things. But there's nothing that said, "Wow! That's really exciting new stuff." I don't see that happening. That is the horror story, that you spend $10 billion or whatever the LHC cost, and all you get is the Higgs. If nothing major comes up relatively soon, LHC will be the end of the line. There's some very nice B physics coming out, but you wouldn't spend that kind of money for that kind of B physics.

ZIERLER: Well, that's important to know. Harvey, it's been so fun speaking with you. I really want to thank you for spending this time with me today.

LYNCH: My pleasure, and thank you for the invitation.