Charles Prescott

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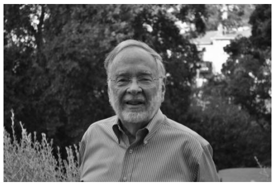

Photo courtesy of Charles Prescott

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▼ ABSTRACT

Interview with Charles Prescott, Professor Emeritus at SLAC. Prescott discusses his activities in physics since retiring in 2006, and he conveys his interest in the muon anomaly results from the g-2 experiment at Fermilab in light of his longstanding work in spin physics. He offers a wide perspective on the creation of the Standard Model and when the field began to search for new physics beyond it, and he recounts his childhood in Oklahoma. Prescott discusses his undergraduate education at Rice and his interests in physics, and he describes the opportunities that led to his graduate admission to Caltech, where Bob Walker advised his thesis research on the eta meson. Prescott conveys the importance of Steve Weinberg's work on particle theory in the late 1960s, and he describes the circumstances that led him to SLAC after a brief appointment at UC Santa Cruz. He describes joining Group A, which was led by Dick Taylor, and how he organized the first parity violation experiment. He discusses the E95 and E122 experiments, and he describes early advances in understanding the nucleon sub-structure. Prescott explains his proposal to add polarized beams to the SLC and a new drift chamber for the SLD, and he discusses the origins of the DELCO collaboration. He describes his tenure as leader of Group A and then as Associate Director of the Research Division, and as chair of the International Spin Physics symposium. Prescott discusses his work on SLAC's Enriched Xenon Observatory, and he prognosticates the poor political and budgetary prospects of future linear accelerators. At the end of the interview, Prescott reflects on receiving the Panofsky Prize, and he segments SLAC into its constituent historical eras as defined by the dominant experiments over the decades.

Transcript

Zierler:

Okay. This is David Zierler, Oral Historian for the American Institute of Physics. It is April 15th, 2021. I'm delighted to be here with Dr. Charles Prescott. Charles, it is great to see you. Thank you for joining me.

Prescott:

Thanks for having me.

Zierler:

To start, would you please tell me your most recent title and institutional affiliation?

Prescott:

Okay. I'm a Professor Emeritus. I'm a member of the SLAC National Accelerator Lab. I think that's the current SLAC title. It's an academic unit, department of Stanford University. So, I'm in the SLAC department.

Zierler:

Charles, when did you go emeritus? What year was that?

Prescott:

2006.

Zierler:

And in what ways since then have you remained connected with SLAC?

Prescott:

So, I took, at that point, a half-time recall to active duty for two years. Then, in 2008, I went full-time retired, but I maintained an office at the laboratory, and I continued to participate in a project which was a double beta decay project called EXO. They were building up an apparatus which eventually went down to a mine in New Mexico, run by the Department of Energy, WIPP, down there. So, for about five years, I continued to go into the laboratory. I'm comfortable in the laboratory environment. I have a very natural experimental background. That's what I've always wanted to do. Until about five years ago. At that point, I gave up my office, and was only going in occasionally. It was getting much harder to do anything in the laboratory, because many of the people I knew were gone. If I go back to the laboratory today, I don't know anyone who was there. They've either retired or have moved on to other things. Even the young people have moved onto other things. So, I'm at home now, and have been mostly at home with my home office I've set up here and do very little professional things anymore. It's mostly being retired. And this year, of course, has been very quiet because of the pandemic.

Zierler:

What issues in physics do you remain interested in, just intellectually or academically?

Prescott:

Well, certainly, particle physics in general. I noticed last week, Fermilab coming out with the results on the anomalous magnetic moment of the muon. Very interesting to me. I know a lot about that because of my background in the spin physics—an area in physics—and not all experimentalists ever pay much attention to the polarization aspect of the muon. It has a magnetic moment, it has a spin, and the fact that it doesn't quite conform with the Standard Model calculations has led to an enormous amount of excitement because there is always a search for what's out there beyond the Standard Model.

So, Fermilab has confirmed what were the Brookhaven results about 13 years ago, and in good agreement. At that time, those results coming out, it triggered an enormous amount of thinking on the theoretical side. Any good theorist would probably have a paper out trying to explain his favorite theory on what the cause of this might be. I think the current results will do the same thing. It'll be a stream of theoretical papers coming out. Unfortunately, the result is just a number which is probably, in my view, accurate and correct. There's been a tremendous amount of thinking about this by a lot of very good people. So, I don't think the experiment is wrong. I think the numbers are right. The theory is, as I said, unfortunately, in the case of the g-2, that number doesn't tell you what is causing it. The theoretical thinking will be trying to figure out what other things to look at out there in particle physics that might be a hint as to what is there beyond the Standard Model that was not included in it.

Zierler:

Charles, of course, this is going to be, by necessity, a purely speculative answer, but if you had to guess, do you think that this muon anomaly is ultimately going to prove new physics?

Prescott:

Yes, I think it will. But I don't think anybody knows what that is, and it may be a long time, or we may never know. It may be thousands and thousands of theories, and papers coming out, and calculating things, but we just don't have the machinery, the accelerators. Or it might be some other breakthrough in our understanding of the basic nature of the universe. There are many other problems. It's not the only one that we cannot explain. Dark matter has been causing a lot of activity, unsuccessfully. We don't know what it is yet. And that maybe will uncover something.

The muon, being a massive particle, it communicates with a part of the natural universe the way the electron does not—I know a lot about the electron. The electron has an anomalous magnetic moment, and it was out in many significant figures in agreement with electromagnetic theory, the quantum theory. So, the electron has an anomalous moment. We don't have any problems with it. We can calculate many digits. I didn't look up how precise it is, but it wouldn't surprise me to see it's 8 or 9 significant figures on its anomalous moment. The muon reaches deeper into the vacuum, so to speak, because it's a massive particle, and it has different components. That's why it's interesting. It's going to create an enormous amount of thinking, and maybe some experiments. I'm excited. It's about the only thing in particle physics that has come out this year that has really, really been interesting. There's always, of course, a lot of speculation, but this is a very real result, and I think they did it right. I think it's correct. We'll just have to see where it goes. I have no idea. I can't speculate what it might be, but I think it's likely to be new physics when it comes out.

Zierler:

A very broad question, given your seniority in the field and your broad perspective. Roughly chronologically, what are the years where you felt you were experiencing the Standard Model being built, and what years in your career was the focus then on moving beyond the Standard Model?

Prescott:

The Standard Model was being built as I was transitioning from graduate student to what turned out to be my second post-doc position. When I went to SLAC, I went there for a three-year appointment as a post-doc, but it was my second three-year appointment. I had already done three years at Caltech. At that point, the deep inelastic scattering was in the books, and the parton model, Feynman's parton model, and the corresponding theoretical version that Bjorken had, which is Bjorken scaling—Feynman explained scaling in his parton model—were also out there at that time. So, this was the beginning of the Standard Model, and I was at that time, transitioning from graduate student to a research physicist with a position at SLAC. So, what year was that? That was 1971, '72, along in there. But the deep inelastic scattering, the beginning of seeing constituents, which were quarks, had already happened at SLAC. I didn't participate in those experiments. I went into a research group, Taylor's. They had lettered groups. SLAC Group A. Taylor was the boss of Group A. He hired me in that group, and that group was continuing to study the deep inelastic electron scattering, doing different angles, different energies. You had to map out the entire cross section, the so-called structure functions were being mapped out. But the scaling feature which turned out to be quark scattering in the proton, those discoveries had been published and were out there before I got there.

Zierler:

And then, the second part of the question is, when did the Standard Model start to be understood as something to move beyond?

Prescott:

Probably about the end of that decade. It was very controversial whether it was quarks or something else. That history is very well-known. The discovery of the charm quark was in '74. I had proposed to look at parity violation in inelastic scattering. Now, that's a different piece of the Standard Model. That had to do with Weinberg's theory which combined weak and electromagnetic forces. He had written a paper. It was the only theory out there which had specifically proposed the underlying theory. He wrote it out as a Lagrangian. You could calculate things in great detail, and he had proposed this. So, when I went there as a post-doc, I had a proposal to look for deep inelastic scattering of polarized electrons. That was a different piece of the Standard Model, and it turned out to be an important result because it was probably the first experiment to measure Weinberg's theory with a precision number on his mixing angle. He had a parameter he needed. He didn't know what it was, and the experiments were looking to measure it—the neutrino scattering were measuring it. This parity violation experiment confirmed a prediction in his theory and threw out other theories that might have been around. His theory was at that time known as Weinberg and Salam. Later, the charm quark got added in. Two years later, the Nobel Prize went to Glashow, Weinberg, and Salam for what is the Standard Model today. So, there was a turning point at one point, and that was the parity violation which showed that the unification aspect was there.

At SPEAR, they discovered the J/psi, and the psi-prime, which turned out to be the charm quark, which was there. All that was happening in the middle of the '70s. I would claim that by the end of the '70s, it was absolutely clear that Weinberg, who had first written this down was correct, and with help then later from Glashow and these other people. QCD got pulled in with the understanding what was wrong with the searches that were looking for quarks and had never found any. It was understood finally by theory, basically, of confinement of the quarks. That was the theory of QCD. That was pulled in, and all this was formed, leaving one thing left to be done of the Standard Model, which was the Higgs. There was a mechanism for giving mass that we knew existed in these particles. Weinberg pulled in the simplest of the theories out there, which was developed by Peter Higgs and colleagues. There were maybe half a dozen theorists who had promoted this mechanism that was an acceptable mechanism in the theoretical structure, and Weinberg pulled it in as a piece of this theory. So, you know the story about that—it took a long time. The Higgs was hidden. A lot of searches went on. I don't know how many years, but then at CERN, they managed, in the deepest darkest corner of proton collisions to find it. It was a very difficult measurement for them and done by these big collaborations that were doing the hadron scattering at CERN—pulled out the signal for the Higgs. Mother Nature didn't make it easy. A lot of other people had looked for it. Back to SLAC, when we were doing our electron scattering experiments, we had theory papers, or speculations to go look for things that might be the Higgs. Nobody was ever able to find it until they finally did at CERN. So, that was the end. I would say the last chapter of the book was the Higgs, on the Standard Model. That has been written, and there are now textbooks out there about the Standard Model.

History has been written by many people about all these developments. But it didn't explain everything. It didn't include gravity, and gravity is an enormous piece of our current excitement about black holes and everything. That's not part of the Standard Model. Einstein wanted to unify it all. He never made any progress in his later life about pulling all these natural forces together in there. So, we don't have gravity mixed in. It's separate, and there are a lot of questions about it. As I said, dark matter remains a puzzle and the thorn in the side of experimenters. We think or thought the best explanation was another particle, and these experiments, many of them have come up empty handed, doing the search for the WIMP, the Weak Interacting Massive Particle that would explain this. It seemed theoretically a very natural fit, and the particle experimenters want to go out and find it. There are underground detectors, and many other thoughts on what it might be, and it's eluded the experimental community completely. It's yet to be figured out.

Zierler:

Well, Charles, let's take it all the way back to the beginning. Let's go back to Oklahoma; tell me about your parents. Where were they from?

Prescott:

Okay. My mom, middle of the country, Iowa. My dad was in Kansas. My grandfather—I was telling this to a friend the other day—my grandfather, same name. My name was from his, Charles Prescott. He lived out around Dodge City, and in his youth, he was a cowboy. Later on, he turned into a merchant, and had a mercantile store in a little dusty town out in West Kansas, near Dodge City, and knew Wyatt Earp and those types, the gunslingers that were out there in those days. He knew them. There was a little bit of cattle rustling that went on. So, my father went to University of Kansas and got an electrical engineering degree, then went to Oklahoma to work for an oil company by the name of Continental Oil, or Conoco. Today, it exists as ConocoPhillips. It's still an oil company. So, geophysics was what he did as his profession. He developed the technique of finding underground structures by setting off dynamite charges and picking up reflected sounds in microphones. He developed instrumentation that did that, and they expanded that into seismology, seismograph technique that is still used today to find oil.

So, there's a lot of oil in my family history. I don't think there's any oil in my blood, or my DNA anymore. I'm out here in California with solar panels and all that sort of stuff. But I do have some oil stain in my background. My father worked in the oil industry and did well. The company, before he retired, was in Venezuela, developing the fields in Venezuela with international collaborators. And in Libya. So, that's the family. I went to Rice—to high school in Houston. That's when the company went to Houston. And then to Rice University, which is in Houston, for a four-year degree in physics. Then, went to Caltech. It took me about five years—graduated in about '66 in physics with my degree, and stayed there for a post-doc, in the synchrotron lab. The head of the lab at the time was Bob Walker, well-known in the particle physics crowd, going way back. I graduated in '66 and stayed around. In 1970, I moved up to UC Santa Cruz as a tenure track assistant professor. But my friend, who I also worked with as a graduate student at Caltech, was Elliott Bloom. He's also on the SLAC faculty today. At that time, Elliott had moved up to Taylor's Group A at SLAC after finishing his PhD. I helped him with his PhD thesis, and he worked on my thesis. We were in close contact.

So, he had gone to Group A, and I had gone to Santa Cruz. Elliott kept telling me to leave Santa Cruz and come up to Group A and join in on the research because the field was growing so fast. There was so much going on. And Santa Cruz was not a pleasant place for me. It really didn't work very well. In 1970, there were the Vietnam protests going on at Berkeley, and there were a lot of sympathetic protests going on, on the UCSC campus. The student body was in turmoil. I was having a very difficult time seeing how I could get any research going when I had other assignments on campus, and a young UCSC campus starting up. The only buildings at that time on the Santa Cruz campus were called natural science buildings. They were getting rocks thrown through the windows, because that was the only physically large building the students could attack. These were all sympathetic protests. It was a bad year to be there. I was there 18 months. I took, in '71, a position in Dick Taylor's Group A, and it was largely with some very specific research projects underway, with Elliott Bloom. It was a rapid cycling bubble chamber experiment that needed the technical background I had. I brought up some computing equipment, and a bunch of stuff, that in those days was pretty much at the front, cutting edge.

Zierler:

Charles, we'll develop this story in more detail, but let's go back at Rice. When you enrolled at Rice, was the intent for you to pursue a degree in physics from the beginning?

Prescott:

I think so. In high school, I had gone into amateur astronomy, which is interesting to me, but when I went to Rice, I knew that I needed something more than astronomy as a career, and I picked physics, and Rice had a good four-year physics course. But at the end of four years, what kind of physics—it was basic physics in general at Rice—what kind of physics, hadn't, in my mind, really settled in. It was when I went to Caltech that I decided I was going to go into what is today called materials science, or condensed matter physics. I think that is the term that's maybe still used on that. I was going to do that in physics. That seemed to be the most interesting thing. So, when I got to Caltech, they didn't have any faculty in that subject. What they had were faculty in high energy physics. That was the second choice for me, to go into high energy physics. I just followed my nose in that direction because of the synchrotron lab. They had a little accelerator there. I started working with Bob Walker as a student at Caltech, and then I found it to be a very comfortable kind of work to do. I was naturally a hobbyist. I had done my own radio control stuff when I was in high school. That was very early in the days—you couldn't buy radio-controlled things in stores. You had to build it yourself.

Zierler:

You were at Rice in the middle of the Sputnik event. In what ways did that affect your research?

Prescott:

Yeah, I suspect so. I was a good student. I loved physics because I was reasonably good at math, loved puzzles and playing with puzzles. I loved any little technology. As I said, radio controls, you had to do it yourself, but I figured it all out. I built some radio control—they were boats, not airplanes, but boats I was doing. Just having fun, and mixing with a crowd of similar, like-minded kids in high school. It was Sputnik era, and there was money for going to school, getting educated. My parents, when I told them I was going to go to graduate school, were like, "How are we going to pay for this? My god." They didn't have to pay a penny. It was all paid for. That's Sputnik, I think. In fact, my whole career has been paid for by my government—you know, tax money into the government. I feel I owe a huge amount to the community and to science.

Zierler:

Did you know by the end of your undergraduate experience if you wanted to pursue theory or experimentation?

Prescott:

Yeah, theory never attracted me. I liked it, I'd read the papers, but I was always a hobbyist as a kid, and doing hobby things. It was clear I was on the experimental side. Never had to think that one out. It was not a problem.

Zierler:

What was some laboratory work as an undergraduate that was formative for you?

Prescott:

I don't think there was much laboratory work there at Rice. It was classroom work.

Zierler:

What kind of advice did you get about graduate programs? Where you should apply, who you should work with.

Prescott:

I wanted to go to Caltech. Somehow, I decided I wanted to go west. I don't know how that came about, but I wasn't going to go to the east coast. I wanted to go west. Well, it was clear, I think, because of my amateur astronomy interest. Of course, Caltech had the Palomar Observatory, and it was the leading thing. So, Caltech had been on my mind for a long time. It was clear I wanted to go to Caltech for something like that. It was not a hard decision for me, and I think it was because of my interest in amateur astronomy that pulled me in that direction.

Zierler:

What were your initial impressions of Caltech when you arrived?

Prescott:

Well, you asked how I got there. So, Caltech had advertised, and I had to go through an application and everything. In there, they suggested you apply for not a PhD, but for a master's degree. So, I sent in my application and everything. At Rice, the professor pulled me aside and said, "You don't do that. You go for a PhD now." So, I had to redo the application, to go there for my PhD. It was just a waste of time to go through a master’s degree if you want a PhD. So, that was very strong, positive advice from my professors at Rice who were wonderful people. You'd get to know them. And as I said, I was a good student, so they got to know me, and they pushed me in that direction. Helped me a lot. When I got to Caltech, I discovered they didn't have research activity in condensed matter. So, I had to pick something else. There was a very strong nuclear physics group at Caltech, probably the strongest in physics. They were the guys who were doing stellar nuclear physics, and all this stuff was spectacular.

Zierler:

Who was there? Who were some of the big names at that point?

Prescott:

You're going to push me for names. I can find them, but at my age, the names come slowly. Some, and it's hard to predict who it was—Fowler—maybe. Sorry for my slow recall on some of them. Names drop out, and later on I'll remember who it is. But for example, Fowler. Robert Christy. I don't know if his name means anything to you. There are others in the nuclear physics. I was over in high energy, and my thesis advisor was Bob Walker. Alvin Tollestrup was there. Those were the two principle people in high energy. Walker was head of the Caltech Synchrotron Lab. It was an electron accelerator in the building where they had ground the primary mirror for the Palomar telescope. The building had been converted to an accelerator lab nearly filling the space. It would go up to 1.5 GeV in those days. But that was big time.

Zierler:

How did Walker get to be your graduate advisor?

Prescott:

As I said, you had the choice of Tollestrup or Walker. You have to go ask them if they would take you on as a student, which I did. There were not many other choices around. He, Walker, and his group of students were busy when I arrived, making that accelerator work. And he had some experimental equipment working, but at that time they were busy making the accelerator work. It had been upgraded and revamped, and it had been off the air for several years. So, when I arrived, there was a backlog of students waiting to do experiments on that machine. I would have to wait, and that affected my career. Walker was my advisor, and he gave me a little project, to work on a magnet design he wanted for a tagged photon beam. You start with photons from the accelerator. You couldn't extract electrons. They only had a high energy X-ray photon beam. You would take that, run it into some target material, and produce electrons and positrons. With a downstream magnet you separate them, pick out the electrons. Now, you had a nice little test beam for high energy electrons. You can tag the electron and detect the photon which has the remaining energy. It's called a tagged photon beam. So, you can measure the energy of the photon. Walker wanted that, so he asked me to start doing the magnet calculations.

I was a first-year student, and that's what I did with Walker for the first year, was to put that together. Then he had it built. I had to learn how to do some optical calculations, but I was good enough I could figure this kind of stuff out. He would help me, and he looked at what I did. I had to do it on a Marchant mechanical hand calculator. I had to do some matrix inversions, andother calculations. I remember the first four-function electronic calculator arriving at the lab in the middle of all this, and I finally finished my calculation with an electronic calculator rather than a mechanical one. But I still had to punch it in by hand. There was no computing available for students in that day. IBM finally came out with a computer which I used when I got my degree. I did it on an IBM central computer, mainframe. IBM mainframes were coming out. But that's what I did for Walker. But with this backlog of students at Caltech, it was clear I was going to have to wait. An experiment could take six months of running because of inefficient running with an unreliable accelerator, that was up and down a lot.

There was an individual, a research associate at that time, who showed up at Caltech. His name was Clem Heusch. You may know about him. He's now deceased, unfortunately. But at that time, he was a young research associate, not a faculty member, a research associate at the laboratory wanting to start up a separate program. So, he asked Elliott Bloom and me to join with him. The three of us started a different research direction using at that point an available but undeveloped beam line. So, we established another beam line on that accelerator with Heusch and Elliott and me, the three of us. We set out to do some experiments. The first experiment, which was to be mine as a PhD thesis, was to study a meson that had been recently discovered, called the eta. It's heavy, but somewhat like the pi-zero meson, with similar properties, and decays into two photons. So, we were looking for the two-photon decay. The purpose was to measure its production rate. The simple cross section measurement was to be my thesis.

Zierler:

Charles, what were some of the major advances in particle theory that may have been relevant for your thesis work?

Prescott:

None. The theory at that time was bootstrap theory in high energy physics. It prevailed–but over in the theory corner at Caltech was Gell-Mann beginning to talk about his theory. He called it the eightfold way. He had invented a set of things he called quarks and was explaining the spectrum of mesons with this. But the main theory at the time was bootstrap theory, and that was headed by people up at Berkeley. Nobody could calculate anything with it or explain the thinking. It seemed to be very fuzzy thinking. The two theorists at Caltech were Gell-Mann and Feynman. They were not onboard with that at all. Gell-Mann went off and wrote a paper at that time. I don't know the exact date of it. His eightfold way paper was rejected by Physical Review Letters. He was angry, and I don't think he ever published again after that, in Physical Review Letters. But that's because the theorists were preoccupied with the strong theory called bootstrap theory. Crazy time. It's not mad to have theorists putting out their crazy ideas, even though they can be wrong. In fact, I should say the opposite, certainly about the Standard Model. All the experiments that are ever done by the Standard Model, in my view, were guided by theory. They were ahead of us, the experimenters. The experimenters like to discover something that can't be explained. The anomalous muon magnetic moment, we love it because it'll create a lot of head scratching by theorists.

Zierler:

Charles, I'll flip the question then. If theory wasn't valuable for your research, looking back in the longer term, how was your research helpful for theory?

Prescott:

Well, my thesis experiment at Caltech was, as I said, measuring a production rate of a meson. That's interesting, it gets in the books, and it gets published. You put it in the big catalog as one of the measurements made out there. I didn't get anything done influentially until I took on the polarized electrons at SLAC. The parity violation was a major turn for me. I don't know if you know the whole history of parity violation. There were people trying to test Weinberg's theory, which unified the weak and electromagnetic forces, and came out in 1967. SLAC first turned on its beam in 1968. So, it was around the same time there was this theory out there which had been a goal of theorists and the experimentalists for a long time. There was this weak interaction, which was responsible for the radioactive decay in nuclei. It was weak because the rates were very low compared to the well-established electromagnetic force.

By that time, quantum electrodynamics had been shown to be correct. Feynman, for example, in his theory, he developed little cartoons called Feynman diagrams. The whole theory was how to put together the amplitude for electromagnetism at the quantum level, and how to do calculations. And indeed, had shown that you can do some precision calculations -- Lamb shift, I think, was one of them. So, electromagnetism was a golden theory. Weak interactions, which was responsible for similar effects—it looked very similar—must have something to do with electromagnetism. It must be similar. It was the driving force to try to figure out what was similar in structure of the weak interactions, and whether it looks like electromagnetism in its structure of the theory. But then it was Weinberg that pulled them together in 1967. He wrote it all out as a technical formula. You see it in his paper, Lagrangian. Indeed, you take that, and you start to calculate things. Nobody knew whether it was right or wrong in the early days. So, you have the discovery of quarks, and all that was going on over in hadron physics, there was this effort to test this unification idea by Weinberg. The way you do it is to look in the processes involving electromagnetism for the signature of the weak interaction, which is parity violation. The known weak interactions at that time all exhibited maximal parity violation. Why does nature violate parity in one force, whereas electromagnetism and strong forces conserve entirely parity? Parity is the symmetry of reflection. If you put up a mirror, your left hand looks like your right hand in the mirror.

That symmetry should be preserved in forces. Electromagnetism doesn't care what parity is involved. That's been shown many times. In the calculations, for example, one assumes that. So, unification according to Weinberg hadn't really been tested, and unification was a strong driving force to do experiments. How do you do experiments? That's hard to say. You look at an electromagnetic process for the signature of the weak interaction, which is the parity violation. How does parity show up as observable in an experiment? Usually with the spin. If you look at spin component of one of the particles, along its direction of motion, it's called helicity, that violates parity. In the reflected world, it flips sign. So, if an interaction has a spin component like that in this force, the amplitude for that process will change if you flip its spin. The original weak experiment was done by Madam Wu—I think it's polarized cobalt that she used. And when she turned over the spin of the nucleus, the rate of detected decay particles in her detectors changed in that process. In fact, the weak force turned out to be maximally violating parity. That was the weak interaction. The signature of the weak interaction is the failure to conserve parity in the force. And usually, it's some spin component that does that. Atoms couple to the nucleus through the electromagnetic force. One way to start looking for parity violation in atoms was in atomic level structure–there were these experiments that were doing this.

After Weinberg's theory came out, immediately people proposed looking in atoms. The way you do that is you shine a laser on the atom, and you look for a polarization effect. The one that they were looking for is a rotation of the linear polarization. As the laser beam goes through the cloud of atoms—they were using, for technical reasons, bismuth. As it goes through a cloud of bismuth atoms, they were looking for a rotation of the plane of linear polarization. This is a laser experiment. It's bench top experiments that you can do in the basement of your physics lab. Lasers were getting to be very good by this time in the 1970s. So, there were multiple groups looking at lasers and atoms. Paris, they were looking at cesium. In Washington and Oxford, they were looking at bismuth. In Berkeley, they were looking at thallium. All these atoms have levels in them that you can scan across by slowly sweeping the laser frequency. You excite a line. As you scan across and excite this line, you should see the linear polarization rotate, which is a parity violation signal. These are very complicated experiments. Unfortunately, theory, because bismuth had lots of electrons in it, the theory was not fully understood by anybody, including the best of the world's theorists on atomic structure. That was one of the problems. This was in 1977 when I was at SLAC. I should back up a minute. Another way, it turns out, to do parity violation, is scattering of polarized electrons from nuclei. You flip the polarization of the electron and see if the scattering rate off the nucleus changes. That's a parity violating effect also. This was obvious to me when I was at Caltech, that polarization in the deep inelastic or in elastic scattering were places to look for the parity violation. That had caught my attention as something that was an important thing to do – to test the Weinberg theory. So, to fill this in a little bit, in 1970, I had gone to Santa Cruz, and was at Santa Cruz, but collaborating with Taylor's Group A.

Zierler:

So, Charles, just to be clear, when you got to Santa Cruz, it was already the plan for you to be involved at SLAC.

Prescott:

Yes, absolutely.

Zierler:

How did that come together? What was the connection for you at SLAC initially?

Prescott:

So, I talked about my friend Elliott, who was in Group A. He had been telling me to leave Santa Cruz and come up and join the group. I was at Santa Cruz during this period and had arranged to collaborate with Taylor's group at SLAC as my research plans for the future. I wanted to set up a research activity as a professor at Santa Cruz. I was an assistant, tenure track professor at Santa Cruz for a year, and during that time, I had arranged to start collaborating with Taylor's group at SLAC in my research activities. The plan was to develop a group and go on and have a research program from Santa Cruz. But my friend Elliott was telling me to come up and join his group, full-time. He had his own designs on an experiment that he wanted me to be full-time involved in. So, a year and a half later, I did that. As I said, there were a lot of problems on campus, and one of them was the Vietnam protests that were going on. 1970 was the peak of the Vietnam War protesting going on, and it was a very difficult time. In the year that I was at Santa Cruz, it was becoming clearer and clearer that I wasn't going to make much progress on research from that position, and I had this attraction, my friend telling me that I should move. And I did. Taylor supported that. Joe Ballam—it helped that I was working on Elliott Bloom's project, which was the bubble chamber that was Joe Ballam's favorite project. Rapid cycling bubble chamber. He was associate director of research, and I got to know him, and Elliott was talking to Taylor. Taylor and Ballam were happy and they gave me an offer again. I had earlier turned them down. I didn't say that, but they had originally made an offer, and I went to Santa Cruz instead. They reopened that offer, and I went up to SLAC.

Zierler:

And it was at SLAC that you organized the first parity violation experiment.

Prescott:

Yeah, so I showed up, and the original idea was to look for parity violation in elastic scattering. I'd done some calculations, and this was while I was still at Santa Cruz. I showed up in Taylor's office when I was arranging to collaborate with him, with this proposal looking for parity violation in elastic scattering. He took it, looked at it, read it. It went right into his circular file, the trash can while I was there, saying something like "This is crap." It was a draft of the document I had written, a calculation. That was my early experience with Taylor, and it was very important that I had talked to him about parity violation, because shortly thereafter, in the fall of 1970, Vernon Hughes from Yale came to SLAC to give a seminar, and he had a device, a source, that produced lots of polarized electrons. His proposal was to accelerate the polarized electrons to high energy, and to scatter the polarized electrons off a polarized proton target. He was interested in the quark model and was interested in showing that the quarks were contributing to the proton spin. The experiment (E80) should show a very large asymmetry in polarized electron—polarized proton scattering. So, that was the seminar in the late summer of '70. I was at that seminar, up at SLAC, and on the way out, I turned to Taylor, and said, "Dick, this is the way you do parity violation, with that polarized beam."

Zierler:

What made you so confident of that, Charles?

Prescott:

Well, I had thought about it. I don't think many people had thought about weak-electromagnetism unification, but I'd been thinking about it, and I understood that it required polarization to do that. So, it wasn't a big stretch for me, when he started talking about polarized electrons. But I hadn't thought about accelerating polarized electrons. My initial idea was to look for the polarization of scattered particles in elastic scattering. But it never occurred to me you could accelerate polarized electrons until Vernon Hughes showed up with that idea. Now, you have to realize that Dick Taylor was doing deep inelastic scattering. He was also the master and captain of the spectrometers in the End Station A that needed that. And he was probably looking for experiments to do, because the unpolarized deep inelastic scattering, which he was still doing, was winding down. So, I hit the right guy at the right time with the right idea. And thank god that he supported it. Parity violation in the inelastic electron scattering would not have happened without Taylor's support. And he did support it.

Zierler:

What was the partnership with Yale, at this point, and SLAC?

Prescott:

Okay, well, I wrote up a proposal, but not the elastic scattering idea. I wrote it in terms of deep inelastic scattering of polarized electrons. It's the kind of thing you can do almost on the back of an envelope if you know the right numbers. It's not a complicated calculation, but of course, by that time, I did it on the computers. I put together both the discussions of what you want to do, and then the counting rates and how much time, and all the simple details of what it would take to do it, and showed it to him, Taylor. He actually had taken a sabbatical by that time, and it was when he came back, a year later, that I put on his desk this document that I put together. I said, "This is what I want to do." He looked at it, and it involves using the Yale source, so Taylor said, "You have to go to Yale and talk to them about it. Present it to them and see if they'll join in on doing combined things." And I did that.

So, that's how it happened. I went to Yale and talked to them and they agreed to collaborate. That was the first time I met Vernon himself, who was at Yale. That's how it started. Later I had to present this to the SLAC program committee to get approval. There was a committee of experts from outside of SLAC. All laboratories have these committees that review proposals. So, I had to go to the committee and present it, but it helps to know SLAC and its way of doing business. Taylor didn't particularly bother worrying about the committee. He went to Panofsky, the director, and said, “We’d better do this experiment. I, Taylor, want you to approve this experiment." So, Taylor got the approval for it. Unfortunately, because the Yale source wasn't very intense, it could produce only about 1% of what the accelerator could deliver.

Zierler:

And this is E95 we're talking about.

Prescott:

It was called E95, and it did not reach the sensitivity needed to test the Weinberg-Salam theory.

Zierler:

Why not? What's the issue?

Prescott:

That's the reason. The source was 1% of what was really needed; what was really needed was the full intensity of the accelerator. It's just a statistical argument. It's a small asymmetry you're looking for. You don't get enough counts. It's the kind of thing you can do on the back of an envelope, almost, if you know the numbers. It said in the proposal that one would not reach the weak interactions. It's not sensitive to it. But it was looking for parity violation-like process in the deep inelastic, because that had never been tested before. There were no experimental tests of that. There were theoretical biases, but there were no experimental tests ruling in or out parity violation in the deep inelastic. That was the argument for E95. We should look here anyway. It was somewhat of a weak argument, because we all wanted to go test the Weinberg-Salam theory of the weak interactions, and we couldn't do it with the Yale source. So, Taylor got it approved anyway. He talked to the director, and said, "I want to do this." It was essential that he was interested in doing this and made it possible. So, I owe my career to him supporting this idea I wanted to pursue.

Zierler:

When, you say, Charles, your career, this also helped you achieve a permanent staff position at SLAC.

Prescott:

Oh, absolutely. Along about that time, I was involved in the bubble chamber experiment with Elliott. That was Joe Ballam's,—the associate director's—rapid cycling bubble chamber—favorite thing. And we did a good job with that experiment. The experiment was to look for quarks. It used a muon beam, scattering in the bubble chamber. You take photographs. Elliott wanted to see quarks coming out of the inelastic scattering. That was the way you were going to see them in the days in which everybody was looking for these quarks. Had we seen quarks coming out of the events in that experiment, Elliott would have won the Nobel Prize. It would have been the experiment. Unfortunately, quarks are confined, as we now know. There's a different theory that explains it all. We didn't see quarks. The experiment ran, and it was beautiful. We put together a lot of apparatus to make it happen. We had to track the scattered muons out of the bubble chamber and through spark chambers. I put together the computer setup, built the spark chambers, put the data on tape, and we associated the electronic digital information with the photographs that Joe Ballam's cameras were getting.

All that was a beautiful experiment, and a lot of fun. It went on for a year and a half, or two, and showed that the muon scattering looked like simple photoproduction. We made no particularly interesting discoveries. It was a great idea, but it's one of those things—in the oil industry, you call it a dry well. So, it was a great experiment, and in that process, of course, I had made connections with Joe Ballam's group, and had done well. So, it was an easy transition for me to go from post-doc to staff, and that's what happened somewhere in that time. I don't remember the exact years. It seems like a continuum of going up the ladder at SLAC and ending up emeritus—I've been there my whole career.

Zierler:

Charles, tell me about the origins of E122.

Prescott:

Well, okay. So, that was the origin, E95. It was clear that we needed a more intense electron beam. At the moment we started working on E95, we began the search for a laser driven source, with Charlie Sinclair, to get that current up to the level where we could test the Weinberg-Salam theory. We understood that was the basic thing that was needed. As it started out, Charlie Sinclair had an idea of a laser driving a helium source. We got Pief interested in polarized electrons, and Pief started coming to all the meetings. He was interested in doing this, and we went through a whole bunch of ideas. Magnetic needles, I remember as one idea. Coated needles using field emission to get high currents, and other things.

Through all this process, in the middle of that, came up with the idea that I personally had heard about. There was a material science lecture on the Stanford campus in which people were studying the internal electron structure in gallium arsenide. They were using the fact that the internal electrons were polarized. So, I remember going to the seminar where that was being talked about and asking the presenter if there was any way to get those polarized electrons out of gallium arsenide. He didn't know anything about it. But I remember going back to SLAC and talking to our colleague, Ed Garwin. Garwin, you probably know his brother Richard Garwin. Ed was head of the physical electronics at the laboratory, a Mr. Wizard. I have infinite respect for him. I learned a lot in working with him, but I remember at that time talking to him about these electrons in gallium arsenide. Well, Ed knew something more, and that was that there were people down in Silicon Valley developing night vision devices. What were they using? Gallium arsenide. And what were they doing? They were coating the gallium arsenide to make the images by focusing electrons on to screens. One of the guys on campus who was involved in that, Prof. Spicer, came and talked about it. It turns out that what you do to get the electrons out is well-known; you have to put a coating on the surface. It has to be clean, first of all. You can't have dirt on the surface. It has to be clean gallium arsenide, and then you coat them with cesium and a little oxygen that helps bind the cesium and lowers the surface work function. When you shine light on it, the light excites internal electrons that come out as free electrons. The photons produce electrons, electrons come out, and if it's a camera, you just focus that onto a screen or something, and you can make yourself a little night vision device. This works in the infrared, so you can do this in the infrared for the night vision. Ed proposed with a colleague in Zurich, Hans-Christoph Siegmann, to test whether those electrons are polarized. In Zurich, they did this. They set up in a lab that Siegmann had ready to go.

So, within the year or two, he had a student—the name's gone, at the moment—involved in it. They set up a small bench experiment in which they would shine polarized light on gallium arsenide and measure the out-coming electrons in a polarimeter. They had to make the polarimeter. You'd accelerate them to some modest voltage. 10 kilovolts, maybe higher. Maybe 50 kilovolts and scatter them off a gold foil. And you can measure the polarization of the electrons in the scattering distribution. Well-known techniques in atomic physics. It's not anything that was hard—you've just got to do it with gallium arsenide. It was now about a year later or so, they showed that for the gallium arsenide that they were playing with, that indeed the extracted electrons had preserved the polarization of the internal electrons, which had been measured by other techniques. So, this was a simple gallium arsenide crystal source. Pure gallium arsenide, you could buy from companies in those days. They'd cut it into little circles the size of a quarter coin. Put it in your apparatus and you could have polarized electrons. You have to first clean it. The vacuum techniques are severe because one can easily contaminate the surface.

Anyway, that was 1974, and the experiment showed that you could get polarized electrons out of the crystal. That was Garwin at SLAC, Siegmann at Zurich. And Dan Pierce is the name of the student who got his degree from that, and he went off to the National Bureau of Standards. I think he's probably retired by now. He was my age, so he has to be retired by now. Anyway, that was a breakthrough. It was clear in '74, this was the path for SLAC to pursue. Now, what you want to do is shine a laser on the crystal. Lasers are powerful, and they have to be at the right wavelength. So, Charlie Sinclair began to develop the tune-able dye laser that we used. Garwin developed the physical structure. All this was setup starting in '74, in Charlie Sinclair's lab space he had at SLAC. The three of us, Garwin, Sinclair, and I, spent a lot of time on this. I was pretty much involved full-time with building this source, starting around '74. I went there in '71, and helped on Elliott's bubble chamber, but in '74, by this time, I was on the staff, and I was full-time working on this laser source with Charlie Sinclair and Ed Garwin. I learned a tremendous amount from Ed Garwin about vacuums. I was the technician of the lab and learning from them.

Charlie Sinclair was the laser guy, and Garwin was on the materials side. He managed the physical electronics group, but what he knew a lot about were surface effects on materials. And the strategy was to build a prototype, test it and then to install it directly on the accelerator. So, we started in '74. We made a tremendous number of mistakes along the way. It took us three years, but it was in 1977 that we had an intense source of polarized electrons. Polarization about 40%. It wasn't 100%, it was about 40%. But it was an intense source. We could get amperes out of this gallium arsenide if you would just crank the laser power up, because lasers were powerful. Gallium arsenide quantum efficiency, if you put in a number of 1%, which is a typical number we were getting, you can calculate how many electrons you get with the lasers. The laser put out short pulses, but the currents were very large, far beyond what was needed for a SLAC accelerator. So, that was it. When that source finally worked, it took us more than three years to get it to work, by that time the experiment had been approved. It was called E122. I had gone through the steps to get it approved, and it was then a matter of getting it scheduled. It was late in '77 that the source worked. We had put the experimental apparatus together and ready to go in the spring of '78, and when it started to run, we began to see the parity violation signal online immediately. Within hours of running with this intense source that we were running with you'd begin to see the parity violation asymmetry in the data.

Zierler:

Charles, in the earlier years at SLAC, what were some of the advances in understanding the nucleon sub-structure?

Prescott:

I think the most important thing was QCD, coming out of that. Remember Bjorken's initial scaling laws that he uncovered? Feynman explained scaling in clear terms, while Bjorken's language, which came from current algebra, was quite unfamiliar to most. Current algebra was quite new in the field and was started by Gell-Mann at Caltech. Bjorken applied it to electron scattering, and I don't know much about the details. I'm not theoretically inclined, but out of that came Bjorken scaling, and Feynman explained it in terms of Feynman's parton model, which was more a model like billiard balls, with kinematics in it. But he could derive the same kind of results with some assumptions. So, they had the similar ideas of scaling. Well, soon after scaling came what was called scale breaking. This was the QCD aspect coming into play. Logarithmic breaking, and it became very evident when the Europeans' measurements of the scattering with much higher energy muons showed the deviations from the scaling law. There were logarithmic deviations. So, as you go up an order of magnitude in energy, you begin to see, the scale change. This became very apparent, and it was explained in a theory. It was quantum chromodynamics. Gluons were responsible for that. So, QCD succeeded in explaining the structure of deep inelastic, that theory really got understood, finally. And it, of course, has been incorporated into the Standard Model as a piece of the strong interactions, which involved gluons and quarks. That's a whole field of its own as well, strong interactions. But it showed up in the electron scattering, and that probably was one of the more important things coming out of the deep inelastic, the discovery of quarks and the quantum theory that's underneath them.

Zierler:

Charles, tell me about your decision in 1980 to propose adding polarized beams to the SLC.

Prescott:

Well, it was obvious that the parity violation was important—the parity violation experiment was in '78, and Nobel Prize in '79 went to, guess who? Weinberg, Glashow, and Salam for what is the Standard Model. We were then—I say we. I was in Group A working with Dick Taylor, and [it was] a very good group. It wasn't just Dick Taylor. It was a very interesting group of people capable in using the SLAC facilities there. Dick Taylor and I were discussing at times over group meetings and things what to do next. We had this tool, which was polarized electrons, in the back of our minds. With the thought of doing something with polarized electrons, we went through a lot of different ideas. We even considered an experiment which was electron-electron scattering. The parity violation experiment E122 was done with electron-deuteron scattering, which had some theoretical advantages over electron-proton scattering. It also had some experimental advantages, the deuteron, of providing higher counting rates. The statistical counting rate was important. But we were casting about for other ideas when we looked at electron-electron scattering, which turned out was very difficult to do. We hadn't really decided on anything. I started talking to Dick Taylor about taking polarized electrons to Burton Richter's SLC. It was e+ e-, the project that Richter was promoting at that time. This was probably late '79, maybe 1980, about the time that he was proposing converting the linear accelerator into a linear collider by accelerating both electrons and positrons down the accelerator, and then splitting them into two arcs that would take them around and bring them into head-on collisions. That was the basic idea. It was a collider, using the linear accelerator. He had been talking about that—Dick Taylor and I had been talking about what to do next with polarized electrons. I started telling Dick Taylor the right thing to do was to go to the collider with polarized electrons.

Zierler:

And Charles, to be clear, nobody before this was talking about using polarization for e+ e- colliders?

Prescott:

Not to my knowledge. I think the first time came to me sitting in some boring seminar trying to figure out what I'm going to be doing with myself later. It certainly came to me from just having been working with polarized electrons and looking for something new to do. I did write the idea up. What happened was, one day, in the office, I was sitting in the office down the hallway from Taylor, the phone rang, and Taylor said, "Come down to my office. I have Burton Richter here in my office. Explain what you've been saying to me to Burton Richter." So, that's how it started. I told Richter that I wanted to put polarized electrons on his collider, which eventually happened. That's the way it evolved.

Zierler:

Was Richter convinced right away?

Prescott:

I don't know. What I know, today, is he had a basic design, but to deal with polarized electrons requires some manipulation of the spin, particularly in his version of the SLC. You first accelerate electrons—you accelerate electrons from the injector at the beginning of the linac. You put them into damping rings in order to collapse the emittance of the beams. Damping rings can do that by radiating off soft photons. The phase space that they occupy just naturally collapses in a properly designed, circular ring. It's the damping process from synchrotron radiation, well understood by accelerator physicists. Well understood at the time. There were these damping rings attached to his proposal, and you could see in his diagrams he'd talked about. To handle polarized electrons, you have to worry about the spin, and what happens to it, and you have to be able to manipulate the spin. In order to do that, he had to move things around. So, later that year, the diagrams coming out had the damping rings oriented in the proper position so you could transmit polarized electrons through the damping rings, and not destroy the polarization. It was a manipulation requirement.

So, you could see at that time that he had accepted the possibility for polarized electrons. He didn't talk to me about it, but the diagrams changed. He had talked to his accelerator guys, and they had put in the possibility of putting in polarization. The beam lines were readjusted so you could put in the proper components to handle the spin. So, yes, he'd accepted it at that time, but Burton's strategy for the collider was to be the first accelerator there (i.e., the Z-pole). We had a competing proposal coming from CERN, and the major development was LEP, Large Electron Positron rings. The proposal, being so large and so big at CERN, would take longer to construct and build, and Burton's strategy was with the linear accelerator existing at SLAC. He would modify the accelerator and get there first. He was going after the Z boson. Everybody was, by this time, convinced that there was a Z boson. The Z boson being heavy and massive was going to reveal many of nature's secrets through its decays. All these exotic particles and everything else that could come out, because it was very heavy, would show us a lot of things about Mother Nature's catalog of particles and forces. So, there were a lot of ideas flying around at the time, and Richter's idea was to get there first.

Zierler:

Charles, tell me about some of the technical challenges that might have been posed with the SLD proposal to build a new detector for the SLC.

Prescott:

Well, of course, simply the scale of these detectors had gone up dramatically. So, I don't think it was so much new things involved—we had knowledge of how to build the detectors. That was conventional stuff. Richter, at SPEAR, had built Mark II, and his strategy was to upgrade Mark II to be the first detector at the SLC. Following that there would be a second detector that became the SLD detector. It was to be a new, modern, larger detector, but the technologies were pretty much the same. There was a lot of instrumentation development that was going on. Liquid argon was being developed as a great way to do calorimetry. It was, of course, incorporated in these detectors. So, you had cryogenics that was coming along. When the SLC got going, I joined with Marty Breidenbach and several others on a proposal that became the SLD. There were about a half dozen of us in-house SLAC people at the beginning. Staff, faculty, a mixture. We were about half a dozen charter members on that proposal for the SLD. I participated in those meetings to get the proposal going, my interest being to have polarized electron data. What happened was Burton was invited to go to a conference and give a talk in Europe. One day the phone rang at my desk, and Burton told me this and said, "Would you go instead?" to me. And I took that opportunity to write up polarized electrons at the SLC as a proposal. It was in 1980, in Switzerland. That was the first time it came out, any publication saying you should polarize the electron beam for the Z boson—not a profound idea. It was just a claim that there's a way to do it, and I wrote that up as a talk at this conference in Lausanne in 1980.

Zierler:

Charles, best case scenario, as you're proposing this, what would the outcome of the experiment look like? What would you be hoping to find if this all came together as you had hoped?

Prescott:

I understood the Z boson would be produced copiously in these machines. That was the expectation. Burton's big mistake he made was advertising how many of these heavy gauge bosons the SLC could make. The calculations of a functioning machine showed millions of these coming out. The boson would decay into a number of things, particularly the quarks. So, because of the large number of them, you could do precision measurements of the nature of the weak interaction, the neutral current. The Z boson is the gauge current of the neutral part of the weak interaction. You have the charged gauge bosons, W+ and W-, which are carriers of the charged weak force, and the Z is the neutral component of the force. And if you're producing this thing, it would decay into all its capable channels, which is all the quarks and other things. So, you're going to learn, at a precision level, the weak interactions. That was clear. Just simply put numbers in.

So, the proposal was to make those precision measurements. Adding the polarization greatly enhances the sensitivity to these parameters that you're measuring. You're measuring couplings. If you want to prove the theory's right, you want to measure all these couplings to the gauge boson, the quark couplings. There are a lot of numbers that come out of the theory, and you want to measure them all, and this was the way to do it. The polarization was a big enhancement on that. It was clear. It didn't take much arguing to convince people of it. I did make those arguments, in terms of workshops that were around at that time. I had put out this one paper in 1980. I think it was probably the only paper about polarizing the beams for the Z. You couldn't do that very well at LEP, which was a problem for LEP. I didn't mention a detail, but it's the longitudinal component that you need, which is easy in the linear collider. And in the storage rings, it's very hard to do. Storage rings like to polarize in the transverse mode, but even that becomes very problematic at high energies. A lot of accelerator physics studies on how to polarize beams in the circular ring exist. LEP was to be a circular storage ring. For a large, high energy accelerator, polarization is very difficult to do in a circular ring. But it is trivial in a linear accelerator. And that was the key point. This was something that the SLC could do that the competition could not. They had, in principle, many other capabilities, but they couldn't look for these spin-dependent asymmetries. That was the argument for including it in the SLC.

Zierler:

And Charles, why was it important to design a new drift chamber for the SLD?

Prescott:

Well, yeah, I've joined the collaboration. I had to earn my keep there. The drift chamber was, for me, very interesting. It was somewhat challenging because of the small inner space available. As I recall, the space available wasn't what was typical for a large solenoidal detector—you wanted to achieve resolution. It's a tracking device. You wanted to achieve good momentum resolution, and you needed to use big magnetic fields and a large space. In the SLD design, this was a compressed space. The reason was the ring imaging system for that detector which had been proposed took up a good portion of the detector volume. I was challenged with—and I had accepted my job as part of the collaboration—the task to worry about the drift chamber. A conventional device. Many had been built. It wasn't so hard, but the challenge was to squeeze it into this small space, and still maintain the resolution.

So, there was a lot of work to be done, both calculations and some beam tests and things, to understand how to work with a particular gas that would allow for better resolution. We chose adding carbon dioxide to the otherwise conventional isobutane-argon mixture. I can't remember exactly what we ended up with as the gas in the drift chamber. But this was fun. I was very good at hardware stuff. I'd had a lot of experience in building instrumentation of the various kinds and took it on as the boss of a group of people whose job was to design and build the drift chamber, and we did. It worked. It had a failure after about five years, which I considered to be infant birthing failure. One of the wires broke and shorted out internally. That brought the whole SLD operation down for a month or two, because they had to go in and figure out how to extract a wire that had shorted out. Sad to say, because the drift chamber almost made it to the end without problems. Five years of working well, and then, suddenly, the drift chamber dies. We repaired it and resumed data taking shortly thereafter. I thought it'd been perfect. Anyway, I'm complaining about failures. I have many other failures in my career.

Zierler:

Charles, tell me about the DELCO collaboration.

Prescott:

Not a lot to say about that. The group—it was a hard time at PEP. Dick Taylor had gone on sabbatical and basically asked the group to collaborate with the Stanford group. This was a group headed by Stan Wojcicki at Stanford, and it was called the DELCO—it was an acronym, one of these buzzwords. Direct Electron Counter is what it stood for, and its claim to fame was it had very good sensitivity to the electrons, which is hard to do. In the collider, you get a lot of garbage around, photons and stuff flying around. It's easy to get confused. A good Cerenkov gas counter could focus light only from tracks coming directly from the events, and that's what it was, a direct electron counter. So, that was what we were doing, running a detector at PEP and processing the data. I had proposed polarized beams for the SLC, and nothing had happened toward that end during the PEP era. I was in the Group A and got to work on some of these other things, these other projects, including this one at PEP. So, DELCO finally took data. DELCO, when it was at SPEAR, was famous for, I think, having confirmed or supported the discovery of tau lepton at SPEAR. Group A was not involved in that earlier. The experiment, DELCO at PEP, went off and did measurements. But frankly, I don't think of it as producing a lot of unique physics. There were a lot of measurements made, of e+ e- scattering, but nothing that, in my mind, struck as profound. I think I was probably focused more on working on the source, doing other things, and focusing on the DELCO was not something I consider a main part of my career. Participating with Taylor's group on the projects the group had undertaken was what I was doing.

Zierler:

Charles, tell me about how your day-to-day changed when you were named group leader of SLAC research Group A.

Prescott:

I'm not even sure I remember that ever happening. I was acting group leader when Dick Taylor became Associate Director. In the early '80s, he moved from Group A, still head of Group A, to the Associate Director's office—Joe Ballam had retired from it in the early '80s—and when he did that, I was asked to be the acting Group A leader, while he remained formally the group leader, and the Associate Director. So, you say, the question was when I got named as group leader of Group A, I think was your question. I don't recall specifically a day. It was a continuous transition. Taylor moved upstairs to Associate Director, and I took over the day-to-day running of the group, doing whatever we were doing in the early days. As I said, my interests were largely with polarized electrons at the time. He lasted about three years, and I ended up replacing him as Associate Director when he stepped down. It was sometime during that period that the title got changed from acting to group leader. But the day it happened is—I certainly don't remember specifically. The job was still the same, even as Associate Director. At some point, I think I began as acting group leader, and I think formally a letter got inserted into the files, and the title got changed to group leader of Group A. I was starting to say, Taylor was pulling away from day to day activities. This business of the SLC, I had been talking to Taylor about going to the SLC with polarized electrons.

You have to realize that Dick Taylor and Burton Richter were part of the original captains of the research groups. My opinion is Taylor was very uncomfortable at the time that Burton Richter had become Director of the laboratory. In the early days they were head to head competitors for time using the beam, equal in their group leadership, and Burton Richter had assumed, at some point, around then, the head of the laboratory. Taylor was just not very comfortable working under Burton Richter in management. That's a line management position, and you do management stuff, and Taylor began to pull away. He got his Nobel in 1990. But he had pulled away, to a large extent before that. He had gone on a second sabbatical, and since we were talking about DELCO, he was not involved in DELCO. He set the group up to work on DELCO, and I think he took a sabbatical and didn't participate. So, Dick Taylor, when I was talking to him about working on the SLC there, he liked the idea, but he didn't like the idea of working under Burton Richter. He was his own man. He had his own career. He had his own Nobel Prize, eventually. Both of them. So, he'd step down from the Associate Director position.

You asked about being group leader—some point during that time, a lot of things were happening at this point. He stepped down and I ended up replacing him as Associate Director. Somewhere along that time, I think the letter in the file got changed to actually being the group leader, but it may have been during the period when I was Associate Director that Burton Richter made the change official.

Zierler:

Yeah, you were dual-hatted for a time, as Associate Director of the research division, and group leader of the research group.

Prescott:

Yeah, I was group leader through all of that. Even after I stepped down as Associate Director, I was still group leader. But at some point, that had been formally changed, and you asked me when, and I don't really know because my day-to-day activities were all the same anyway as acting group leader.

Zierler:

And what were some of your administrative responsibilities as Associate Director of the research division?

Prescott:

I'll look at my list of what happened during the era. I made a list when I was thinking about this. The first story is day 1—well, day 0. I had gone on sabbatical. This was 1985, I went to CERN on sabbatical. SLD, at this point, had gone into construction, and I had been working on the drift chamber. I was generally responsible for that one component. Typical of a collaboration, you have to build something to be part of the collaboration, and I was responsible for the drift chamber. I had been working on it, and by this time I was faculty and had not had a sabbatical leave, and I desperately felt I needed that. It's a career kind of thing. Many of your colleagues were in Europe, and CERN was the obvious place, so I went to CERN. Dick Taylor, who was Associate Director, had stepped down, or had said he was stepping down. Burton Richter was looking for the replacement. He had already asked two other individuals who were not at SLAC. They were well-known people outside of SLAC. He offered them the job of Associate Director and both had turned him down. I was in Geneva, and Burton contacted me and said to meet him in London. My wife, who had had a very pleasant year, certainly knew something was up. So, I told her this was likely to be a job offer of an open position at SLAC. She said, "You're not going to accept this, are you?" I said, "Don't worry, dear. I understand the problem. No way am I going to." So, I went to London, met Burton, and he offered me the job of Associate Director, which I didn't want terribly much. I was worried about my research and the effect it would have. And I told my wife, "Don't worry, it's not going to happen." When I went back to Geneva, and she asked, "Well? Did it happen?" And I said, "Yeah, I took the job." I felt, of course with the SLC was having lots of problems, I had to pitch in and help. We had to completely rebuild the accelerator lab.

Everything on the accelerator had to change. They had to put in new tunnels, a new experimental hall, every klystron on the accelerator had to get swapped out. The klystrons weren't powerful enough. The control system had to be rebuilt. It was a new accelerator. He, Burton, was in the middle of this, trying to finish it off in time to beat LEP, to get it running. You would need a year or two of it running, and the schedule wasn't going well. There were all kinds of problems. He needed help. He needed somebody to worry about the other parts of the lab. He couldn't do it all. He was just too busy. I didn't want it, but I felt duty called, and said yes. I told my wife, and she said, "That's a bad idea." So, anyway, the sabbatical was about over. Taylor has been trying and trying to get out of the job. I went into my first meeting. This is the fall of '86 when my sabbatical ended. First thing they told me was that I had to do a layoff. I've never done a layoff. I don't know what a layoff is, you know? I spent the next six months on this—it was a budget driven decision. They had to cut out and move money around, and I had to downsize the research division, and that was why Taylor had stepped down. He didn't want to do it. Taylor didn't tell me this, and Burton didn't tell me this, because I wouldn't have taken the job. I didn't want to do a layoff. I didn't know how to do a layoff, but that was the job. So, for the next six months I spent a lot of time with HR, the personnel division of the laboratory and their people and the lawyers, going through and making sure that what we did was how you do, properly, a layoff. And it took about 6-9 months before that settled down. Then I began to settle into being Associate Director and paying attention to other matters.

During that time, the deal with the SLD collaboration was I needed to transfer work on the drift chamber to someone else. So, the drift chamber responsibility was moved to Abe Seiden, at Santa Cruz. His group. He took the task as a temporary assignment. He was a collaborator, and he took it on temporarily and did the job very well—I think finished it up. It was pretty well along by that time, but he took it over as his temporary job because somebody needed to watch over that construction. I was basically upstairs, still head of Group A, and still involved in Group A and in the SLD, as the research of the group–and I wanted it to happen. As Associate Director, I had an agreement with Marty Breidenbach. He had a monthly meeting where the entire collaboration would come in, SLD collaborators from all over the country. MIT was involved, and Colorado, and a lot of other groups working at SLAC. It was a big group, and it was typical for collaborations to be getting bigger and bigger. SLD ended up with 350 people, I think, to compare it to CERN's 1,000 or so collaborators on current projects. That's wild to think about that, but you can organize it in some kind of structure. People can do that. They do a good job of it. So, I had this agreement with Marty Breidenbach.

The Associate Director was someone you go to for things. You have to elbow your way around and push to get things done. Marty was very good at shooting upward at people in the management structure above him. He likes to shake things up. He had, of course, a lot of issues he wanted to discuss with his collaboration council, and he didn't particularly wish to have the Associate Director in those same discussions. Who knows what conniving schemes might be put forward, or whatever? At any event, the agreement I had with Marty was I'd wait until 5 o'clock in the day of his planning meetings. He had everything done that he wanted to discuss without me by 5, and then I would come down. Anything leftover that would allow me to fit in with the meeting, we would discuss after 5 p.m., when the workday was over. That's how it worked I would wait until 5 p.m. and I'd go join the SLD council. The council was some technical council that Marty would run, he and Charlie Baltay. There were two co-spokesmen for SLD. And that worked very well during those times. I have to say, with SLD—SLD had the reputation of being well-run. It was. It was Marty Breidenbach and Charlie Baltay, co-leaders, co-spokesmen of the collaboration. They did an excellent job of running it, handling it, because they have to handle the politicking that goes on in the physics community at the national level. You have to go explain yourself to various committees, and he and Charlie always did a very good job. Financially, running it, it was probably the only big detector that didn't have a cost overrun.

Zierler:

Why would you say that's the case? Why was it in budget?

Prescott:

Because of Marty. He watched the money, and he was very good. Simple as that. There was no issue. He was good. Often people build something and it's not technically good, or it doesn't work, and you have to build it again, or your schedule is blown, and you can't get finished in time. The whole thing is how to control the budget. So, good management understands that you've got to watch all the details and keep it under control, and he did. And the competition, in those days, was D-Zero at Fermilab, building their detector, and I don't think they did as well in that. I don't know. I mean, there's always the competition with the other labs over budgets and so on.

Zierler:

Charles, was D-Zero a direct competitor?

Prescott:

No, no. Not in physics particularly. They did it quite differently. They were a competitor, as I say, in the budgets. You have to justify yourself and keep a piece of the pie that's big enough. There's never enough money. It was always tied to reviews and overall budgets. Schedules will slip because of money. As I said, I had a layoff to do. That was a budget problem. You have to do it if you don't have the money. You have to downsize. So, that's management's problem to deal with. Marty was a very good manager of his project. Excellent. Charlie Baltay as well. Charlie Baltay was superb in handing the national scene and getting these things like reviews done. So, SLD was very successful. SLD had one problem, and that was the basic SLC was late in turning on, and LEP, the physics competitor, was at CERN. The four detectors there were getting tons of data coming out and publishing. The one thing that, frankly, saved the SLD project, was the fact that when SLD came online, we were able to turn on polarized beams at the SLC. So, the data coming out of SLD was physics measurements that LEP could not do. But LEP did a lot of the measurements that SLD would have done, had it been early on. So, it was important. That whole evolution. SLD's biggest problem was its competitor in Europe.

Zierler:

Charles, tell me about the decisions that ultimately led to the end of the SLD in 1998.

Prescott:

I don't think it's a hard one.

Zierler:

Did it complete its mission? Was the science settled? Did the SLD do everything that it was hoped to do?

Prescott:

Pretty much. LEP did most of the Z physics before the SLC was working well. The physics you're after with the SLD—initially it was hoped to be a discovery experiment. The heavy Z boson was going to decay into all this exotic stuff. It didn't happen that way. The Standard Model is what it did. What came out at LEP, primarily, were the measurements of the different quark couplings to the Z. That addressed everything but the top quark. The top quark is too heavy, but you can see bottom quark pairs in the Z decays. But you get the measurement of the up and down quarks, the charm, the strange. All of these come out in the decays. And these detectors can tell which quark it is. Quarks aren't seen directly themselves, but their decay products are. So, you can tell that the Z has decayed into a quark pair, a charm pair, etc. All these could be measured. SLD had this silicon vertex detector, which uses a whole bunch of chips—uses the silicon chips in cameras, little pixels—and it made a tracker using a whole array of these little pixel chips. So, you could track very closely into the decay vertex—the SLC had a collision vertex of only microns in size. Very tiny spot, so you could find the origin of the decay. You could measure the track with precision. You could tell from the decay kinematics what the unobserved quark was that the Z decayed to.

So, that was the physics, measuring with precision these couplings to the neutral current, the carrier of the neutral current, the Z boson. CERN had done this with their experiments with very high statistics. SLD eventually did this by this beautiful vertex detector, and with the polarized beams. It eventually ended up with some of the more precise measurements where precision in a theory is important if you're going to test the theory. And of course, there's other things going on. There's always the search for the Higgs, and it was determined with these precision measurements that the Higgs was going to be too heavy for SLC. We couldn't quite reach it. It was close, and we looked for it. But it was too heavy. It took the collider at CERN to finally find it.

Zierler:

Charles, during this time, you also serve as chairperson of the International Spin Physics symposium. Tell me about that.

Prescott:

Yeah, that was a big deal. I'll tell you how it happened. The spin conference is a subset of a community who are interested in polarized features in experiments. Literally, it's a subset. It's probably a pretty small subset. Most of the physicists were hadron physicists, and hadron physics is parity conserving, strong interactions. So, polarization phenomena don't tell you much. The spin physics community was somewhat small and often was somewhat isolated doing their spin thing. I got into it because of my interest in polarization in electron scattering. The biennial conference–an international conference–was organized by Alan Krisch at Michigan. He started it in the '70s, around the time I was doing polarized work at SLAC. The conferences were at Argonne and—the first ones were at Argonne–was structured as an international conference, every other year. It was not every year, but on even-numbered years there would be a spin conference. The series came partly out of nuclear physics. There was a lot of spin interest in the nuclear physics community, and that moved over to the high energy field. So, he formed this international spin conference. He was very much hands on, very tight. It was his baby, Alan Krisch. He loved it.

And there were collaborators—of course, he had this advisory committee. Every international conference, to be proper, had its international organizing committee, and its famous people on the committee to build up respect for the committee. Alan had done a good job of putting together an advisory committee, except for the fact that one of the individuals he had gotten onto the committee was Vernon Hughes from Yale. The same guy that brought polarized electrons to SLAC was on this advisory committee. Vernon Hughes, if you ever got to know him, was something of a character. Strong willed, I would say, independent thinking. He clearly had opinions on most everything he was involved in. He ran a very tight group—his own group at Yale. He was on this advisory committee, and he was becoming very unhappy that Alan Krisch was controlling everything. The decisions, everything. So, Vernon Hughes began to lobby for a change of the head of the committee—this was a chair position—of the international advisory committee at the time. At the beginning, I hadn't gone to any of the spin conferences until they began to schedule talks in electron scattering. Now, Alan Krisch is a proton guy. You have to understand there was a real cultural difference between people who worked at proton accelerators and people who worked on electron accelerators. There was always a cultural difference. Alan was very much a polarized proton guy. So, I got involved, but peripherally in this, after beginning to work with polarized electrons at SLAC. Vernon Hughes, who wanted to move on from the strong control of this committee by Alan Krisch, proposed changing the chair position. It turns out that it got approved by the committee, and they decided that Charlie Prescott would be the new chair of the committee, without asking me about it, whether I wanted it. I was informed that I was now chair of this committee.

Zierler:

Although, Charles, at this point, you don't have a good reputation for saying no to things.

Prescott:

I didn't say no. I probably didn't say thank you either. It was, again, one of these jobs that was a lot of work. I was interested in my research. I didn't like doing this when I was Associate Director in the management. I didn't like that job much because I wanted to do some research. Same thing here. This was a lot of work. Every other year, you're busy for several months getting the conference organized. You had to worry about funding, and lots of organizational issues. So, I ended up working very closely with Alan. I was not that comfortable with attempting to do it new and alone. I got to know Alan very well. He was okay. He was a fun guy. I enjoyed it. Needed his advice on lots of matters. I would have made even more mistakes than I made in my career if I hadn't spent a lot of time on the phone talking to Alan about the next conference, and what needed to get done. So, I actually very much appreciated him. He died recently, and the issue now is getting a proper memorial for him at the next conference. We were struggling with it because of the pandemic. I've been seeing some email coming about that. I'm not involved anymore. I was chair for six years. That was for three conferences, and that's enough hard work. You've got to get up and give a speech at each one, at the beginning, and I did. I probably gained a reputation—I'm not sure if it's good or bad, but a reputation in the international community because I'd get up and give a speech. I remember the one I gave in Protvino, to the Russians. They seemed to appreciate it. It was a chance to see Prescott from the west coast. I assume I had international name recognition by then, but I didn't have contact with any of them.

My one sabbatical leave was for one year in Europe. I didn't have a lot of contacts in Europe that I knew personally. I should say, Dick Taylor was very good about bringing Europeans to Group A. I met some of the Hamburg people, the physicists, because Dick Taylor was very good at providing a sabbatical home for them at times. So, I had those three conferences, and got to travel to them. One of them was in Japan, one was in Russia—where was the third? It was in Amsterdam. Before that, I had been asked twice to give the summary talk. I gave two summary talks, but that was before I was chair.

Zierler:

In what ways was E155x an extension of E155, and in what ways was it its own experiment?

Prescott:

Oh, boy. Let me think. That’s somewhat complicated. To answer directly, one must talk about the structure functions. In deep inelastic electron-proton scattering with unpolarized beams, the electron scatters off the charged constituents–quarks it turned out–inside the proton. Measurements result in two structure functions, W_1 and W_2, which define the proton’s internal composite structure. Likewise, for deep inelastic electron-proton scattering where both the beam and the target are polarized, the asymmetry probes the distribution of spin among the charged constituents of the proton. These measurements result in two additional terms, G_1 and G_2. So E155 and E155x, with polarized electrons scattering from polarized protons, focused on G1 and G2 respectively to understand the proton's composite spin.

The proton's somewhat of a mess. It's a composite object, and to measure all of these relativistically correct terms in the cross section require different kinematics to do them. So, the leading term, or the most important one that signifies quark spin in a proton is G1. Some theoretical work had been done by Bjorken on spin structure. He derived what is called the Bjorken sum rule involving G1 for the proton and neutron. It was closely related to QCD and became a rare test of the QCD theory, ultimately shown to be correct according to the accumulated data, a very important result.

Then there is G_2 that you wanted to measure and is part of the structure of the proton. E155 measured G_1, and E155x was an extension into different kinematics, to go and extract this other term, G_2. So, that's a complicated answer to your question. It's part of the engineering of the proton. I regard all the spin structure measurements of the proton as pretty much engineering work. The ultimate values that we measure, the precision numbers we measure, establish what a proton is. That's the best way to describe a proton. It's a relativistic term, and does it help you understand what's going on inside? As I said, a proton's composite, I regard as something of a mess. That was an argument I had with Alan Krisch, who was a polarized proton guy in his research. He always argued that the proton was a fundamental particle, and I explained to him many times that, no, it's a composite. You have all this stuff in there. Alan wouldn't accept it. He didn't like it. It was kind of a joke between us.

Zierler:

Charles, tell me about your work with Marty Breidenbach developing SLAC's Enriched Xenon Observatory.

Prescott:

Yeah, okay. Well, I worked with Marty, first of all, with the SLD. Marty is consummate leader of a group of engineers and physicists. He's really very good at that. I don't know if you know him. Have you interviewed him?

Zierler:

I have not had the chance yet.

Prescott:

It'd be fun. I tell you. He's a character. Very good. Excellent at running a group. He loved to do the job. It's the kind of thing, right down his—what do you call it? — wheelhouse, or something like that. He still does well. He's younger than me, by about five years, and has just retired, but still active. During the pandemic, he got together with some of his colleagues and doctors of medicine and he built a ventilator machine that Stanford has offered to anyone out there who wants to fabricate them cheaply and sell them. It was for third world use, something to do.

So, you asked about EXO. It was the years of SLD that, of course, I started working with Marty. I was involved in this E155, E155x experiments in Group A. I had pushed for that. That's quite different, fixed target experiments. Quite different from the collider program. But I was collaborating with Marty in SLD, but when the collaboration ended, I needed to be in a collaboration where I didn't have to spend time worrying about all the details. In SLD I had someone to do the worrying and the details and I did the simpler thing, like work on drift chamber. That was just another component of the SLD. I did a lot of instrumentation work as sort of a hobby, almost. The kind of thing you do when you're not doing committee work. That's what I could do working with Marty. It was a number of years of this on the SLD. So, approaching retirement, I was looking for something to do. I hadn't quite given up my research activities. As an emeritus professor, you certainly can continue with research. You can continue research, and continue to have students, academic rules long ago were established about that. But I needed some research activity, and Marty had decided to join this EXO project. He also was looking for his own research activities. It was shut down in about 1999, I think, or so. We're now talking post-2000. 2001, 2002, somewhere in there, that this had started up. Marty had joined with Giorgio Gratta (a Stanford professor), who was spokesman for EXO down in the Physics Department on campus.

Having worked with Marty for so long and looking for something to do in retirement—I joined with that group, and in fact, I was actually formally put into Marty's SLAC group as I retired. The resources, which were office space, the administrative help occasionally, came through his group. I didn't need much. So, I was in his group as part of the EXO collaboration. For EXO I never went to the underground facility at WIPP. He did, but the EXO detector was actually moved there. It was built on campus, the detector, but then it was moved to WIPP, and the early phases of moving it in and getting it installed was a lot of work, in the mine in New Mexico. So, he did go, and so did a lot of people. I never went down there. The thought of going in a mine just didn't quite settle with me. Actually, I have been in a few mines. I'm not comfortable with that. I think I have a little claustrophobia, or something. I'm not comfortable going down in mines. You get into this little tiny elevator, and you go down, and maybe the cable breaks or something. I don't know what it was. I didn't have to go to WIPP. I didn't agree that I would do any of the mechanical work or anything. As a post-doc, I had to do all the mechanical work, pulling the cables, soldering circuits, and everything in the early days. It was all done by young people, but as an old, retired guy, I was happy to sit back and watch the data come in, or whatever. So, I joined with that point of view.

Zierler:

Charles, what was the experiment's success in understanding double beta decay?

Prescott:

It's not been seen. Double beta decay is a sign in the case of neutrinos being a Majorana particle. It is both particle and antiparticle at the same time, and the double beta decays were essentially a sign of that. It was a search for a particular kind of decay and has always been that. EXO's not the only one looking for double beta decay. It uses xenon because it's self-identifying. That is, xenon scintillates when you get decays and particle tracks. So, you can use it both as a source and a detector. Xenon-136 is the particular isotope of interest. It had to be enriched; it had to go through the centrifuges, and to enrich it to 90% xenon-136. Very hard to do. You had to pay a lot of money to get somebody to do that, and DOE did. I think EXO contained 200 kilograms of enriched xenon. I don't know what they did with the rest of the xenon with the 136-isotope removed. They gave it away or something.

Xenon is always useful. I think it comes out of the air. That's where they usually get it. It's an expensive material, and yes, there was a fair bit of money spent on just xenon itself. But the detector is like a cubic meter of this material—200 kilograms of xenon in a can, but it's big. I got involved to help, again, early on, on the instrumentation of this detector. I worked on the photodiodes. Instead of photo multipliers, which are too bulky to put inside the detector, we used photodiodes that were sensitive to the xenon light, the scintillation light that comes out of the xenon. With that you measure the total energy in the decays. They're very rare, decays, but you can see them. You measure the total energy, and you're looking really for the endpoint—I think it's likely to be the endpoint of the spectrum that is distorted by the double beta decay. I'm a little foggy on the details at the moment. I haven't thought about it for quite a while. I worked on EXO in my recall to active duty, when I had taken academic retirement. I think I kept my office at the laboratory for about five years more. I gave up my office at the lab and have been at home—my home is my office now—for the last ten years or so. My brain, on some of these details is foggy—I haven't thought about it in quite a while.

Zierler:

No, that's fine. Charles, given all of your work in collider physics, I wonder what thought you have given to the future of linear accelerators. Where they might be located, what new physics they might uncover, what the funding prospects might be for these things.

Prescott:

I think it's poor. You asked the question, and I have never been optimistic about a future linear collider, following the end of the SLC, which was in 1999. What happened was, the Mark II got pulled aside in 1990, and Burton's strategy with the Mark II to be first ahead of the European competition at CERN didn't succeed. The machine was being rebuilt as an upgrade from an the old SLAC machine. It was so hard to get everything right—almost everything had to get fixed. Components had to be changed; all the klystrons changed. So, the time to do that ate into the time that Mark II was scheduled to take data. They started in about 1989, or so, and ended in 1990 with a few hundred of the Z bosons. LEP had turned on, and was gaining 10,000 a week, or something larger. Burton's race to get there first failed. The SLD was scheduled to come online, and 1991 was the year of swapping detectors, and of further upgrading the accelerator. And it was also the year that Burton decided it's time to bring on the polarized beams. So, that got started around the time when I stepped down from being Associate Director, I had a call from Sid Drell who had been asked to head a task force to get things moving on the SLC. I got a call from him to help get the polarized beam going. So, I spent about a year working on the physical device that eventually was put onto the SLC, the polarized source, and in 1992, SLD came online with polarized beams. It was the combination of polarized beams and a good working detector that kept SLC alive. Many critics of the SLC project were asking for the money to work on the SSC in Texas, and Burton was arguing the case for the linear collider at SLAC. Money was the main issue. They wanted money to help get the supercollider further along. And LEP doing so well and being SLC's competitor in Europe was part of the argument for turning off the SLC. The counter argument was, well, it's polarized, and there's a beautiful new detector, the SLD. So, SLD managed to run, and it ran for six of seven years. 1998 was the last year it ran. That was when the SLC collider achieved its design goal for luminosity.

The SLC didn't get a lot of running at its design goal, because within a few weeks of having achieved the design goal, the project was shut down. SLD had five years of less than design goal running, but got a good set of data, and did very well, on its own terms in the physics. SLC achieved its design goal, but it took nearly ten more years to do it than Burton had planned on. You asked about the future of colliders. Well, it was at this time, in this history of difficulty getting the SLC to work, that the accelerator physicists were proposing what was called the NLC, the Next Linear Collider. And there were workshops going on around at various venues. So, there was a lot of ferment among the accelerator people to build a linear collider of much higher energy. They wanted to search for the Higgs and other comparable things beyond the then current accelerator energies. I was put on some panels at that time. HEPAP panels, or sub-panels of HEPAP, because they were trying to understand the cost. And every year they would do a reanalysis for cost. They would find more things that needed to be included. Around—what year was that? I don't remember what year. It was around 2000 or so, when they were doing these studies for the new collider, that the cost for a collider had crossed over $10 billion, and it was still going up. Every time they'd do it, it would go up another 20-30%.

Zierler:

This is SSC money at this point.

Prescott:

The NLC was certainly in competition, yes. SSC had its own money, maybe not enough. SSC had its problems with politics. It was most successful when Texas politicians were in charge. That was Lyndon Baines Johnson, and I think Jim Wright was head of the House—the chair. They were successful and they got the site located in Texas, until they were put out of office. I'm not surprised about that. And they even got started with tunneling, but then when the elections took them out of office, and put other people in office, that was when the will in Congress failed for the SSC. That's not quite the whole problem. The NLC was struggling with its own high cost. I remember one panel that I was on, and we had three different estimates being made by these different groups, and we're trying to bring some understanding into the cost of the NLC, but the cost by this time had gone well over $10 billion, and was on its way, ever higher up. So, it was in trouble itself, by cost. There was the other issue of what accelerator technology to be used. This was an argument among the community, the experts in the accelerator community, whether the NLC should be a superconducting linac, or should be using warm copper.

Zierler:

What are the options? Why one or the other?

Prescott:

Well, S-band rf, —there were proponents of the S-band—using warm copper cavities like the original SLAC old accelerator, which was S-band. The rf frequency is three gigahertz. S-band is the radar term for that frequency range. The NLC proposal is called X-band. That's 11 gigahertz. Much higher frequency, which means the accelerating copper cell cavities are much smaller to do that. You have all these copper structures, X-band cells, that hadn't been tested yet. S-band was a mature technology. Several S-band accelerators existed at SLAC and in Europe. A lot of experience. X-band was an extrapolation. It saves energy if you go up to higher frequency. So, one almost had to do that. But that was one option of being proposed. There were the people in the superconducting cavities business who had a lot of success, who were proposing a superconductor machine which had its advantages. I wasn’t part of much of those deliberations—you know, my experimental background is not about accelerators. This was really among the accelerator community people to debate this. At all times, of course, cost remained an issue, but another problem with the linear collider was that it provides only one interaction area.

Zierler:

Meaning, less bang for your buck.

Prescott:

Yes, how do you satisfy the experimental community if you only have one experiment in the detector? So, much effort on the experimental side was to modify the design to have more than one interaction region. Of course, that would drive up the cost again. These higher energy detectors are very expensive-- they come in at the $1 billion level. When you get up to that high an energy, the detectors get very big to measure anything. So, it was very expensive. You asked, what's the prospects? Not good, but maybe that can change. The last cost for the project, I think, that's still being considered in Japan, by some people, is north of $20 billion.

Zierler:

And Charles, in terms of justifying the cost, it's all about new physics, right? So, what are some of the more effective justifications or scientific arguments that it's worth this investment? Theoretically, experimentally, what might be learned if some of these collider projects actually get off the ground?

Prescott:

Well, there was one case at one time—maybe still—which I think is the strongest theoretical case, and that is the Higgs factory. You can produce copiously, this Higgs particle which is very important in its contribution to the Standard Model. What would you learn by studying the Higgs? You would learn whether it's in fact the original Higgs, or whether there is one or more than one. It's an absolutely fundamental feature of our universe. It certainly exists and is certainly there. That's solid, interesting experimental and theoretical physics. The Higgs factory, because of the mass of the Higgs, is one of the lower energy versions of these linear colliders. Some theorists want to go way on up, and are hoping for some discovery that will come, but as an experimenter, I don't see the case being made strongly enough, for the cost that you put in.

For the higher energy versions that people talk about, where you run the energy up into the multi-TeV range—the Higgs, I think is a 500 GeV, half a TeV, and it's not that far an extrapolation from the facilities that were done. You could think about doing it. But the case for multi-TeV, I don't think has been made strongly enough. It would be a search-for machine, and there's been no compelling target that is as good as, for example, when the SLC started building a machine to examine the Z gauge boson. No one had ever seen a Z boson when the SLC was first proposed. It was a theoretical idea, but the theoretical case for the SLC was strong enough that there were several proposals around. LEP being built, SLC being built. Those were the two that got built. There were still other proposals that didn't get built for a gauge boson that no one had ever seen. Did we have enough confidence that we would do that? Yes. But the cost to modify an existing linear accelerator was fairly modest on the scale of a national program, whereas the cost of a TeV linear collider today is way beyond the money that's in the program for doing it. And the theoretical arguments are not very strong. I mean, they're speculation.

Zierler:

Charles, what about supersymmetry? Is there any talk about seeing supersymmetry at these energies?

Prescott:

Yes, all the time. That's a good question. I don't know quite the answer to that. I suspect supersymmetry comes into the g-2 anomaly, for a muon. I suspect there are tons of papers that are written about that. I think that's the kind of thing people will look at very closely. Supersymmetry has been around 30 years or so. The expectation that there are theoretical arguments for it, somehow, I can't make. I have enormous respect for theory. As I said, in the Standard Model, theory led the way all the time. I read, to the best I can, the theoretical papers. Certainly, did then, and still do. But supersymmetry has been around and hasn't produced anything yet. Yes, there will be papers written, probably, revised or updated or whatever, now with this new result coming out at Fermilab. Maybe Supersymmetry. Theorists will love it.

Zierler:

And then, of course, after supersymmetry, we can think about string theory, and possibly testing string theory.

Prescott:

Well, string theory, that's not yet viable. To be a theory, you at least have to be able to predict something measurable. You have to be able to make a calculation. Electromagnetism predicts numbers out to, I don't know, 10 figures, or something, in quantum electrodynamics. Higgs, you know, was a prediction that's been found. That's a real theory. String theory, it's an idea. I would prefer to call it a religion. The theorists will talk about it at great length, but I don't know, coming out of that, anything that the experimental community can get their hands on. It's been around for quite a while now. String theory has been around 25 years or something. So, it's amusing to me. Certainly, some theorists can talk at length about string theory and what it can do. And I think there's been some progress on the theoretical side of what calculations are possible. I believe it has a big impact on expanding the techniques in theory that good theorists can use. But on the experimental side, I haven't seen a prediction that's measurable, or approachable yet. Still waiting for that to happen. A problem I have is that I grew up professionally during the era of bootstrap theory, and I know you have to take some theory with a grain of salt, but let the theorists have their fun.

Zierler:

Charles, for the last part of our talk, I'd like to ask some broadly retrospective questions about your career, and then we'll end looking to the future. So, first, one great honor we haven't talked about is your receipt of the Panofsky Prize in 1988. That must have been incredibly meaningful to you.

Prescott:

Well, it was a surprise, but I can tell you what happened. Thanks that to Dick Taylor. He, of course, was a consummate insider at SLAC. He's Canadian by birth. He went to Stanford and got his physics degree at the HEPL linear machine down on the campus, doing nuclear physics. He worked on that, but he actually didn't get this degree there. He left Stanford without finishing and went to Europe. When he came back, he got hired back at SLAC by Pief Panofsky, to help with the laboratory construction, having had a lot of experience at HEPL. At that time Pief said, "But you've got to finish your degree." So early on in his return to SLAC, they got him to finish whatever he had left to do on the work he did with Bob Mosely down on campus, to finish his degree. What was the question again?

Zierler:

Just, the honor you must have felt from getting the award.

Prescott:

Oh, yeah, ...so, (now much later) Dick said we needed to do something for Pief. And he went out and started lobbying with various people to get a physics prize established in Pief's name—I think there had to be some money attached to it, or something. The idea was a prize in Pief's name. And it was a prize with some money attached. He managed to get some money to go with it and had to get people behind this. So, this was the first year following the E122 parity violation results in 1978. The APS set up a committee to go find the winner of the prize. Dick Taylor was not on that committee. The first prize landed on an individual in his own group. That was me. So, I was chosen number one, and Dick was very embarrassed. He told me that he did not intend for this to end up in his group. But he was happy, I think, but he did not intend this to be the outcome. He started that for Pief, and I was number one by circumstances. I was looking at it recently, the long list of winners. Wow, I mean, to be the first on that list is with great pride. As I said, I think there was money, but I don't remember anything about what I did with whatever the prize was. I do remember getting a bottle of cognac from my mother-in-law. She was very happy. She was a professor in economics at Case Western. So, she understood academic stuff.

Zierler:

Charles, if you look at your long affiliation with SLAC as a whole, how might you divide it in terms of eras? Either with leadership, or in terms of the direction of the lab, or in terms of the major projects, what would be the chapters of the SLAC legacy from your perspective?

Prescott:

Well, okay, polarized electrons is the thread that goes through my whole research career. I think E122, the parity violation experiment was the key to my career. I had started to talk about the atomic parity violation. In 1977, they published back to back papers saying parity violation was not there. They had null results looking for parity violation in atoms. This caused great consternation. Weinberg himself, who was at Harvard at the time, was publicly saying those experiments had to be wrong. His theory, he kept arguing, was right. There were two published null results. You can still find them in *Physical Review Letters*. Parity violation, at the time, out there in the community, was considerably debated. It was very important, in some sense, this argument by Weinberg publicly that those experiments which both agreed had null results, were wrong. One at Washington, and one at Oxford, published articles back to back in Physical Review. So, it looked like Weinberg's theory was invalid, and this set the scenario for a parity violation test coming out at SLAC. The experiment E122 at SLAC was so clean, in terms of the cross checks, and the low systematics. The first to be shocked by how well it worked was me.

We had expected E122 to be difficult—the signal, the asymmetries we were measuring were so small. But we were shocked by how the data came in as clean as it did. Absolutely clean. There were no measurable systematic problems with it, and the cross checks, of which there were several, ending up with the g-2 precession—we're talking about the electron g-2 causing the spin to precess in the transport system. So that was an additional asymmetry, an additional spin flip, that we could do, which was guaranteed associated with the electron. At different beam energies, you would see this oscillation in the parity violating asymmetry. We measured at different beam energies the asymmetry, and you could see this g-2 oscillation of electron spin being the source of the asymmetry. The experiment was clean. I'd like to characterize it as a home run at the end of the 9th inning, bases loaded. It cleared the bases and ended the game. There was no question that parity violation was there.

And it turned out, with subsequent measurements we did—we ran for a second run later that year—and showed that other theoretical models were ruled out. Other models had popped up immediately when we saw parity violation in electron-deuteron scattering, while in the atomic parity violation experiments there was a null result. There were models saying we can explain this. The second run that year showed that those models couldn't be right. The atomic parity violation experiments had made a mistake. So, that was the major turn. It got me a promotion from staff physicist to faculty as associate professor, and the rest of the career simply followed. But then I went on – so this was the thread through my career—you asked about the impact on various things. The thread in my research was the polarized electron beam at SLAC. The spin structure measurements became a program that started with Vernon Hughes's son, Emlyn Hughes, who came to SLAC. Dick Taylor hired him while I was upstairs as the Associate Director. Emlyn was a naturally good leader, I think. Emlyn came and said he wanted to start some spin structure measurements. Could he do that? I told him, that's why you came to SLAC. Go do it. Here's what you have to do. I gave him a list of things he had to do, and I could support it as Associate Director. So, he started a project that ended up being four or five different efforts, programs, on spin structure using the existing polarized beams.

And then, of course, the polarized beams for the SLC, which I was involved in, had a big impact. It validated the reason for running SLC during the era that LEP was running well and accumulating data on the Z boson. SLC kept running, and the accelerator people, and thanks to them—they kept improving the luminosity. Nan Phinney had a task force, and Pantaleo Raimondi, and Mark Ross, and Paul Emma, and other names of the accelerator people are the heroes that made SLC finally work. Because of polarized beams, John O'Fallon at DOE was able to make the argument that we were enough different that we should continue to run. SLD eventually got precision data that were the best. CERN did a very fine thing. Toward the end of that era, there were four detectors at CERN. They wanted to put out a book on all the data and everything known about the Z, and they came and invited the SLD to please join with them. They were the competition, but they were kind to incorporate what, in some measurements, were the best measurements and which were from the SLD.

So, the major contribution you're asking about, to the laboratory, was getting polarized electrons involved. Pief understood that at the beginning. Mostly in my time there was Burton Richter as Director. Pief, rather early in my career, had retired. But he understood that polarization was important, and that was a capability of the accelerator. He didn't particularly understand the physics. That was not his thing. I don't even think Burton particularly understood the physics details. He understood it was important, but he left the details to workshops. Actually, I didn't understand it all either at the beginning. You'd go to a workshop with an idea, and people pile in and do various calculations, and write workshop books and things happen.

Zierler:

Charles, in all of your experimental work, you were both a witness to and a participant in so much fundamental discovery, during the real building of the Standard Model. If you survey all of that work, on the flip side of all of the success and all of the discovery, what are some of the thorniest issues? Some of the most unsolvable, intractable problems in experimental physics, that have haunted you over the entire course of your career?

Prescott:

Didn't haunt me. I'm not a theorist but let me tell you the one I think is one of the biggest problems for theorists, the cosmological constant. You listen to these theorists talk, particularly Bj—I listen to him very carefully when he talks.

Zierler:

Bjorken.

Prescott:

I'm sorry, yeah. Bjorken. When he talks, I hope he still can talk—I haven't seen him recently. It's been about a year now, with the pandemic. When he talks, you listen carefully. He doesn't give particularly good talks, but you can figure out what he's been thinking about. The last time I heard him talk, it was something that sounded to me like he's been worrying about the cosmological constant. Einstein, when he did his theory of gravity, thought the universe was in a steady state. His gravitational force would make it collapse. He had to plug in what he ultimately called his greatest mistake, the cosmological constant. The cosmological constant behaved in his theory like a pressure. It's what kept the universe steady. He thought he had to have it. Of course, Hubble comes out with his data showing an expanding universe. It's not steady. It's actually expanding very rapidly, like from a big bang. And that's when Einstein, himself, said it was his biggest mistake. He put this constant in there and he didn't have any justification for it. His theory was right without that. It's not quite the case. There's still a piece out there that looks like a small cosmological constant—it's there, but small. The theories tend to have infinities. They try to calculate quantum theory, what we know of the quantum theory. Can't calculate that.

So, when I recently listened to BJ talk, what I think I heard is that he's still worrying about the cosmological constant, and how you get the theories to do that. Well, there's a lot of ways, probably, you could try to fix it up. String theory probably has tried to figure it out. But they have to let their imagination open up and go on to matters such as extra dimensions which can have a lot of possibilities, and so on. We don't have any guidance yet on what to do, but somewhere out there, has to be a "gotcha, oh my god," simple idea that explains why the cosmological constant isn't infinite, and it's not zero. It's something small. That seems to be what the current cosmologists seem to be saying about it. So, here's a comment about something that's not in the Standard Model. Gravity. Gravity is off and running really well because of LIGO, and its partners in the gravity wave signals. They're beginning to understand what black holes are doing in our universe, and maybe there's something in it to be learned about dark matter. Maybe primordial black holes are what they're looking for, because that might be the source of dark matter. I don't even know what the story is on that today, but it's one of the candidates. Axions, and other things, are still being looked for as candidates. Somebody is going to stumble across something—I think dark matter will get solved. I think it will. String theory, I don't think, will likely give us more insights any time soon.

Zierler:

We'll see. Charles, last question, looking to the future, as a SLAC faculty member, as a SLAC professor, you've had a unique opportunity to work with students. So, I'd like you to reflect on your career and think about some advice you might give the up-and-coming generation of young physicists out there who are, in many ways, picking up where your work left off. What advice might you give them to think about as the most important issues in physics in the future?

Prescott:

Yeah. Wow. I hadn't thought that much about the future. I've thought too much about the past. The field today, right at the moment—of course, it's not physics. I think, if you look at technology in general, I think high energy physics has begun to run out of opportunities. There's still been activities. Dark matter is the perfect example of people still continuing to look at that physics. It's technology. Technology and where it's going. And it's mostly in biology now. Huge opportunities in biology. In physics, gravity is the current thing. They've done some beautiful work on the instrumentation, but there's probably only a few places you can do that, like MIT. Caltech will continue. Stanford has a program, lasers. They really push hard on lasers. There's clearly going to be more interferometers built, a few. But if you're a physics student, and if you can get into an activity like that that's growing, you'll do well. But I remember in the early days of gravity, at Caltech, the students who were studying general relativity couldn't get jobs. It took the discovery of black holes, and the astronomers started getting interested, and the theorists, in general relativity. People who can do these calculations began to be favored by the physics departments, and they started hiring them.

So, in physics, I think it's about getting a good job, and I think there are good jobs. Probably more in technology. That's always been important in physics, the jobs in technology, which are applied. There's more money—I mean, there's never enough money in the pure research, like particle physics, which is purely research. Astronomy is somewhat like that. A lot of astronomers are particle physicists who've moved over. So, there will be positions in astronomy, and in a university. I think the question is will there be enough university positions? There's never enough. So, if you're in physics, and you've committed to physics, and you're really very good, you can probably take a job that doesn't pay very well. But if you're eager to get well-paid, you can do well with physics associated with other fields. Physics, out there in industry, has a huge amount to contribute. Even in biology, the cross disciplinary areas where you can do instrumentation, there's beautiful stuff coming out. Physicists working in other fields, they use their physics background. That's where, if you really want a good job that pays well, you can probably do very well in that direction. You've got to look at yourself very carefully and decide whether you want to do fundamental work, very satisfying but less well-paid, or whether you want to have a well-paid job and a good life. So, I don't know. It's a hard decision for anybody, and usually, you make a decision that never turns out that way anyway. You follow whatever path and opportunities that finally show up. And yes, physics is a good way to do a life. That's for sure.

Zierler:

Charles, it's been a great pleasure spending this time with you. I want to thank you for doing this, and this is going to be a tremendous addition to the SLAC Oral History Project. So, thank you so much.

Prescott:

Thanks for asking me.

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