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Pier Oddone by David Zierler
Virtual Interview 5 May 2020

DAVID ZIERLER: This is David Zierler, oral historian for the American Institute of Physics. It is May 5th, 2020. It's my great pleasure to be here with Pier Oddone. Pier, thank you so much for being with me today.

PIER ODDONE: My pleasure, David.

ZIERLER: OK. So, to start, please tell me your most recent title and institutional affiliation.

ODDONE: Well, my most recent title is farmer.

ZIERLER: [laugh]

ODDONE: I grow grapes in Sonoma County, and I've been doing this for quite a while. When I retired from Fermilab in 2013, it was my last job in science, we came back to this home we have in Dry Creek Valley, near Healdsburg. And I don't know that you'd call it a title, but farmer is a good enough title.

ZIERLER: [laugh]

ODDONE: Before that, I was director of Fermilab from 2005 to 2013, and, prior to that, I had worked 33 years at the Lawrence Berkeley Lab.

ZIERLER: Right.

ODDONE: Fermilab, for me, was a great adventure, as it was for my wife, Barbara. We really enjoyed the Lab and Chicago. It was an interesting and difficult job because the transition to a new program after the Tevatron had to happen during that time.

ZIERLER: Right, right. I spoke earlier this morning with Ray Orbach, and the Tevatron—

ODDONE: Yes.

ZIERLER: —issue came up in great detail, actually. So, I'll be happy to get your perspective on that, as well.

ODDONE: Yes.

ZIERLER: OK. So, to start, let's go right back to the beginning. Tell me about your birthplace and your family background and your early childhood in Peru.

ODDONE: OK. I was born in 1944 in Arequipa, Peru, which is a southern town, and I grew up there for about four years until my father was transferred to Lima. He was managing the bank branch in Arequipa of the bank that he had started working for when he went to Peru in 1929. I'll tell you a little bit about the background of my parents in a minute. We moved to Lima in '49, and I attended a school in Lima called Colegio Pestalozzi . I was there from kindergarten through high school. It was not a very large school, and it was a school that had half Swiss teachers and half Peruvian teachers. It had Swiss teachers because this was the school for the Swiss colony. Now, we talk about colonies in Lima, which are the groups of people who moved there from abroad, first, second generations, from Switzerland in this case. There are similar colonies for Americans who had gone there for business, English and German colonies, and so on. And so, in Lima, there were several schools that were primarily for those groups, but they were not exclusive; parents could mix and match and many Peruvians attended those schools as well. My parents were Italian, but they did put me in the Swiss school, not the Italian school, which was Colegio Raimondi, probably because Colegio Pestalozzi was considered a somewhat better school. And that Swiss school also took care of the German kids, because during the war the German school had been shut down, and it was reopened only later. Anyway, as I say, I went through that school. It had typically two classes per grade of about 20 or 30 kids, so it was a small school. It still exists today. In 2009, I was invited back to the school and I gave a talk about physics and what I had done in life. And I appreciated anew how orderly the Swiss can be. When I went to give the talk, coming back after 50 years, actually, for the first time since I had left the school, they presented me with all the grades I had had in high school. It was a whole sheaf of papers. And they claimed that with those high grades I would have received a gold pin – a program they had implemented since then. Very graciously they gave me a nice gold lapel pin with the Peruvian and Swiss flags.

ZIERLER: [laugh]

ODDONE: But what really impressed me was not so much the pin but the fact that they could go in and dig out—

ZIERLER: Dig out the grades. [laugh]

ODDONE: —50 years ago and get all my grades. We had many classes. The school education in Peru was somewhat limited—this was a good school given the constraints, that's the way I would put it. What do I mean by constraints? For example, in high school, the government prescribed the curriculum because it was trying to maintain quality but also make sure that all the private schools and those associated with other countries were teaching the right things, Peruvian geography, Peruvian history, all of these different subjects. So typically, in any one year, we would carry 10, 12 different classes. And in high school, the government would send teachers from the public schools to examine that we had been taught what was prescribed by exams that took just about all of the last month of school So there was very little creativity or expansion. If a kid could do more, it was very hard for the school to provide that.

ZIERLER: Yeah.

ODDONE: And, in fact, the principal said at one point—"Why don't you send Pier to Switzerland?"—I was probably 11 or 12—"Because he can do a lot more there than we can do here." But, of course, that was not a good idea to send me away by myself at that young age, and my parents rejected the idea.

I said my parents were from Italy, so the story there is that my dad grew up in a small village, Santa Maria di Moncalvo, between Torino and Milano, and here the genes come through when I'm growing grapes. The Oddones have been farming there and making wine and growing grapes for at least 400 years. I once went to the local archives and found that the Oddones that lived in this particular compound in 1604 were asking the bishop to build a chapel in the little town. That town has about 20 families. It's very tiny. But they were asking to build a chapel there in 1604, so they were already established well enough. And the farm has been continuously in the family. It's a small farm, very modest. The young people don't want to farm the land anymore, they all want to be lawyers and businessmen. There's only one person left that farms the land, whereas before, everybody, all the uncles and cousins, farmed the land together.

My mother grew up in Torino where she met my father. The story goes that they happened to be living in the same apartment building at one point, and she was falling down the stairs and fell into my father's arms. And that was the beginning of a love affair that took place for probably two or three years in Europe. My dad was a posthumous child, his father had died before he was born . All his brothers went to war , the first world war, and he was left to take care of the farm with his mother. When the brothers came back in 1917 or '18, they said, "OK, you worked the farm enough. We'll send you to school. You're the smart one anyway." And so, he got a degree that in Italy is called "ragioniere", which is a kind of a business administration degree. He went to work for the Banca Commerciale Italiana , which still exists today. And he did very well. They sent him to many countries in Europe and at one point—he could speak five languages—met my mother in the circumstances that I described. They had this romance going on in various cities in Europe. My mother also had a "ragioniere" degree and was working for an import-export company. Then the person who was running the subsidiary of Banca Commerciale Italiana in Peru recruited my father. He wanted to have "young blood", what would amount for us to hiring postdocs today, young people to help build the business. He proposed to my dad that he go to Peru and, in those days, it was 1929, it was a big adventure to go to Peru. And for him, the possibility of a career in Italy was somewhat limited. In those days, if you went to work for Banca Commerciale Italiana, you had to be of the right family to move up the ranks. So he decided to go to Peru, and the proposition to my mother was that he would go and try it out for a year, and if it all worked out, she would join him there and they would get married there. My mother's father agreed to all of that. That's what happened. I was told that they wrote each other—no email in those days—every day, and they would get packets of mail that came by ship about 30 days later. My dad found that Peru was a good place, so he invited my mother to come over, and she did. He had the priest at the foot of the dock when they tied up in the harbor, and they got married right there and then. No living in sin in those days. Right away, they went to Arequipa, which was to be his principal site. He had to leave after a few days for Cusco, and then a revolution started there, and he got stuck there. They were separated for three months right after they got married. By the time my dad came back to Arequipa, my mother was fluent in

Spanish and had all these friends. They made their life in Arequipa for many years. And for them it was not only a hospitable country, but also saved them from the Second World War.

ZIERLER: Yeah.

ODDONE: Because my mother was half Jewish. Her brothers had to go underground in the resistance. And it would've been a big problem for my parents, of course, if they had been in Europe. They had gone back in 1939 on vacation, and they barely escaped.

My dad had an arrangement with the bank where he would work straight for several years and then take six months off to go to Italy. So, when we were growing up, apart from getting educated in Colegio Pestalozzi, they took my younger brother and me out of school twice for six months, in 1952 and 1958, and took us to Italy. That's where I learned Italian and met my cousins, aunts, and uncles.

My dad was funny. Even though as a young man he had traveled in Peru in airplanes all over the place, he was terrified of airplanes because he was the sole provider in the family, and he was worried about what would happen if he went down. And so, he—probably from the time that I was born in '44 to probably something like '84--- never traveled on an airplane, always ships. If you wanted to go to Italy from Peru, it was 26 days on a ship to get from Callao to Genoa. For us kids it was great. But you didn't go to Italy for two weeks or three weeks, you went for six months, one month going, one month coming back—

ZIERLER: Right.

ODDONE: —and a few months in Italy. That was my childhood. It's really interesting today, you know, that I had a little 8-mm movie camera and I have films of those trips. It's amazing how empty the world was then compared to today -- you go to Venice or to the tourist places today and they are jammed. For us, getting out of school was scary because then, when we came back to school, we still had to pass the exams. But it was a tremendous education to just go out and see the world in a quite different way than you would in Lima. It was terrific for us kids to be able to have that experience.

ZIERLER: Pier, what languages did you speak growing up?

ODDONE: Well, my native language is Spanish, so in school and with my parents we always spoke in Spanish. They would speak to us in Italian or Spanish, so we understood both languages. And we learned to speak Italian because of those trips to Italy, because there we had to interact with cousins and there was no choice.

ZIERLER: Right.

ODDONE: But we already had the language in our heads—my parents would speak to us in Italian, we answered in Spanish.

ZIERLER: Right.

ODDONE: And so that's the way we picked up those two languages. In school they tried to teach us French and German. I had a smattering of those languages, but—

ZIERLER: But even the Swiss teachers would teach in Spanish?

ODDONE: Yes, yes. So, as I said, it was about half young teachers who came from Switzerland. I think there was something like—not quite like the Peace Corps but they got certain benefits if they went abroad to teach. And that was very good. We had also good

Peruvian teachers. In those days, the director was Swiss. Today, they have two directors, a Peruvian director and a Swiss director, and it's completely bilingual. They force you to learn German.

ZIERLER: Mm-hmm.

ODDONE: Now it's obligatory to teach part of the curriculum in German. In the days I was there, they taught us German but did not insist that we become fluent in it. And I got interested in physics early on from reading all the things that were happening elsewhere, certainly not in Peru.

ZIERLER: Right.

ODDONE: In the '50s, there was this hope that all of these discoveries during the war, like nuclear energy, radar, and all these things, would lead to quite an expansion on the things that we could build—nuclear rockets, go to the moon, all of these kinds of things. And to me, that was exciting. And so, I—

ZIERLER: As a high school student, you mean?

ODDONE: Yeah, in high school. I started reading magazines and journals and learning as much physics as the high school could provide. It was relatively limited. And when it came time to finish high school—I finished high school at age 16—my parents thought, OK, if you're going to do physics—and there was a debate between my mother and my father. My mother was the more intellectual, saying, "If you are going to do physics you have to get out of here now." My dad, more conservative, would say, "Well, why don't you study engineering here? There's a very good engineering school. And then, if you still want to do physics, why don't you go abroad after university?" My mother won, and—

ZIERLER: What about you? Where were you in this debate? What did you want to do?

ODDONE: Oh, I wanted to study physics wherever. [laugh] And certainly the US was a lot closer—I knew I had to get out, but whether it was Europe or the US, the US is just a lot easier for somebody living in Peru.

ZIERLER: Sure.

ODDONE: There is a lot more connection, a lot more traffic, it's closer. The system is better understood. And also, the universities, frankly, from my reading about these places, were really the best places in the world if you wanted to study physics. My parents had figured out that I needed some help beyond what the school could provide, so I had a tutor, mostly English and math, for the last year of high school. And he was very good, and that helped me prepare for the all the SATs and all those exams, which I aced, and I got into every place I applied. Then I went talking to people who had been to these various schools. And I must have been a very strange kid, because I talked to the person who had gotten his degree at Stanford and was running a big plastics factory in Peru. All he could tell me was, how wonderful the weather was and how close the girls' dormitory was to the boys' dormitory, and what a blast the whole thing was. I went to the guy who knew about MIT and he said, "It's a horrible place."

ODDONE: [laugh]

ZIERLER: "It's the hardest place on earth. My cousin couldn't handle it and committed suicide. Don't go there." And, of course, I chose to go to MIT.

ZIERLER: Right.

ODDONE: So, as I say, I was probably a pretty strange kid.

ZIERLER: Not just a strange kid, but, I mean, if you're coming from not a particularly strong physics background in high school, the fact that you have the option among these universities in the United States, you must've been an exceptional student to be accepted, as well.

ODDONE: Yeah. I think certainly, in terms of academics, I was pretty good. I had good grades and my SATs, and all that stuff was fine, but I didn't have the preparation that kids in the US would have had, the advanced classes in high school. So when I came to MIT, my first roommate, Bruce Sunstein, who has been very successful in life—and I'll say a little bit more about that later—came from a district in Philadelphia, Bala Cynwyd, that is fairly prominent and has great schools. When I was talking to him -- we were sharing the same room and studying together -- I found he knew everything that we were going to get the first year. And I realized, my God, I'm in trouble. These people already know all of that. This is really going to be tough. And I probably never have worked as hard in my life as that freshman year. I was just like a machine, go to class, come back, read everything, to such an extent that, at the end of the year, after we had lived together for a year—we're still very good friends, we see each other often—at the end of the year, he abandoned physics. He had wanted to study physics, and he said, well, "if people were going to work that hard in physics it's not for me". He scared me the first time around so much that at the end, by working so hard, I scared him off physics. So, my working like a machine was an interesting reaction to finding oneself in an environment that was vastly different.

ZIERLER: Yeah.

ODDONE: The first year was very tough, but I was able to manage it. Probably the humanities was the hardest course, because we had not really done very many essays in my high school. I could write in English, but with a tendency to write like you wrote in Spanish, in a very florid and convoluted way. For the first paper I had to turn in, I busted my ass and worked very hard on the language and all that. And I hand in this thing—and the teacher was great. I still remember him, and he became a good friend by the end. But he said, "This is bullshit." [laugh]

ZIERLER: [laugh]

ODDONE: And he gave me a terrible grade and he told me all these things that were wrong. But I was able to fix it. At the end of the course he gave me a good grade. He understood what I had to go through, that the way you wrote in Spanish at that time was quite elaborate and not this more direct language, this most direct expression that you use in English. You know, it taught me a lesson that was, again, very useful.

So, let's see. What have I left out from Peru? Well, my parents were both from Italy, my dad worked in the bank and he became not the senior manager but a senior manager in that bank and did very well. He had a very strong reputation for honesty and integrity. He never took Peruvian citizenship. Then, when he retired in 1966 at around—he must've been, what, 60-something—yeah, he was born in 1902, so he was 64—he was not particularly happy being retired. And what then started happening to him is that the government used him as a technical fixer of entities that were in trouble. So, his more interesting career, in many ways, started after he retired from this Italian-owned bank, at rather advanced age, if you consider 64 advanced age. The first thing that he had to do was go fix PetroPeru, the petroleum company of Peru—that couldn't meet payroll. He fixed that. Then he was asked to run the national bank, Banco de la Nación, which is like the

Federal Reserve. And the last job that he had was when the government took over a bank that was very prominent in Lima but had been gutted by the family that owned it, the Banco Popular del Peru, that was run by the Prado family. So, again, the government was in trouble, the thing was going under. They got my father to come in to fix it, which he did. He worked there as General Manager until age 85.

ZIERLER: Wow.

ODDONE: And at 85, he was still running this big institution. The time to quit came when we had our daughter, Sanna, born in 1983. My mother had grown up with two brothers, had only boys as children, my brother Claudio and me, and then our son, Gian, born four years earlier as her only grandson. So, when the first girl arrived in the family it was time to retire and come to the US. And they actually, at that age—and I admired them for it—picked up stakes, sold everything they had in Peru, their apartment and all of that, left their friends and in 1986 moved to Berkeley where we lived at the time. My mother was a stay-at-home mom, but she did a lot of charitable work—these colonies in Peru that I spoke of had charitable organizations to help, in this case, the Italians that had fallen into bad times, had lost a parent, whatever. She also worked very much with the Institute for the Blind, both in Arequipa and in Lima. She taught herself Braille so that she could translate books into Braille for the blind. She did lots of good things in terms of helping people. That was her contribution to being in Peru, this country that was very hospitable to them. So, I think that probably gives you a sense of what growing up in Peru was like for me.

ZIERLER: Yeah. Now, at MIT, what professors did you become close with?

ODDONE: Probably the closest was a Professor named Hale Bratt. He was part of Rossi's group studying cosmic rays. I had gone to Bolivia with him after my first year, where MIT, Universidad de San Andrés in La Paz and University of Tokyo, had a cosmic ray station on Mount Chacaltaya. He was installing an extensive air-shower array. He offered me this summer job to go there. It was really an interesting summer because the lab sits at over 17,000 feet, the highest cosmic ray laboratory in the world. It had been built by somebody who became a physicist after being the ping-pong champion of Bolivia and who was from Austria and needed to have some place to ski. Now, close to the equator, of course, if you want to ski, you have to go high. And he took a truck and used the truck engine to build the rope lift up there so that he could go out and ski. He had cut a road up the mountains, and that laboratory was born because that road and facility was already there. He eventually became a physicist, but the initial thrust was to put a road up for the ski lift up there . And so, I went to Mt. Chacaltaya. I could never get used to sleeping up there because it was just too high. So I would take the Jeep for an hour and a half back to La Paz, which is only at 12,000 or 13,000 feet, and I felt like a million dollars with all that oxygen, went partying to all hours and then the next morning would go up there, in the jeep, taking a nap on the way, to cable up the experiment. And because Hale Bratt didn't trust the ability of later technicians to check the equipment—you could have had magnetic tape, but it was hard to check that the experiment was working with magnetic tape — all the data from the air showers was actually written on a bunch of punched paper tape.

ZIERLER: Right.

ODDONE: And my first job when I got back to MIT was to hardwire a specialized machine that was used to punch IBM cards, the cards that were the standard input for the computers of the era. For this, you would take the reels of punched paper tape and make a hardwire translation inside the machine, and the output would be IBM punched cards, because that was the input to the computer. This was about as stupid a job as you can imagine.

ZIERLER: [laugh]

ODDONE: You had to translate from punched paper tape to punched cards. Anyway, afterwards I stayed with Rossi's group and did my thesis. My thesis was—you know, the undergraduate thesis is not a big deal — was basically to design a probe that would fly up in space, either in a balloon or satellite, to distinguish gamma rays from neutrons from the sun.

ZIERLER: Mm-hmm.

ODDONE: That never got built, but the thesis was basically that design.

So, I did my studies at MIT. For a while, in the beginning, before I met my wife, I was keeping the possibility of going to Peru alive in case I had to go back, and so I took a lot of economics courses and subjects that would be useful if I went back.

ZIERLER: But that was a worst-case scenario; I mean, your intent at this point is to pursue a career in physics in the United States?

ODDONE: Oh, right. Exactly. And the issue was completely settled as I went on. What I thought of physics in Peru was different from what I found it really was like.

ZIERLER: Yeah.

ODDONE: It was even more exciting. And then I met my wife in '63, and so that settled the issue. She was a Wellesley kid, and we got married as soon as we graduated in 1965.

ZIERLER: Going back to Peru was not going to happen.

ODDONE: She was a philosophy major, so I thought, what's she going to do in philosophy in Peru?

ZIERLER: [laugh]

ODDONE: She would've done great, I'm sure. But, anyway, that made it much easier to decide to stay.

ZIERLER: Now, when you were getting towards graduating from MIT, did you know at that point that you wanted to pursue a graduate degree in physics straightaway?

ODDONE: Yes, yes. That was pretty clear to me, and so I applied to various schools. And I was trying the other day to think, why did I decide to go to Princeton? It somehow felt right. I don't think it was something very deep, but there were great people there.

ZIERLER: Yeah.

ODDONE: And it was relatively small, and they guaranteed you got out in four years. So, it seemed like the right place for me.

ZIERLER: Pier, how settled were you in terms of the kinds of physics you wanted to study going into graduate school?

ODDONE: Not completely settled.

ZIERLER: Even between experimental and theoretical, you still had not made that decision?

ODDONE: I think, between experimental and theoretical, I was still toying with the idea that possibly I would go into theory, but gradually experiments won out. I think the fact that my thesis, although it never got built, was to build an instrument, taught me that I really liked experimental physics, so—

ZIERLER: Did you have a better natural ability with experimentation than theory?

ODDONE: I don't think so. I think they were both relatively limited at that stage. I hadn't grown up building complicated radios and taking the engine of my car apart and all of the things that often you'll hear people who do experimental physics say they have done while growing up. And on the theoretical side, I wasn't the kind of genius type that could easily pick up group theory and make contributions very early. I would say it was balanced. I was pretty good at both. And then, at Princeton, one of the things that you had to do right away, which was an interesting process at the time, was to choose a project and get involved in research, even though you had not finished the coursework. And that typically would turn out to be your thesis later on, but not necessarily. I went to Princeton when, of course, it was the excitement of Fitch and Cronin in '64 with the discovery of CP violation and all of that. I went and talked to them, but by the time I talked to them it was too late. Students that were smarter than I was in terms of choosing who they would work with had already filled in the slots, probably even ahead of getting to Princeton. And so I ended up doing an experiment with a couple of young professors studying deuteron scattering and trying to understand how a composite object like a deuteron, that is very weakly bound, colliding with another deuteron, could actually give you elastic events at large momentum transfers. You have a very sharp peak because things break apart very easily, but all of the sudden, you get into this very large momentum transfer regime that you wouldn't naively expect given that the deuteron is so lightly bound. The idea was to try to understand this composite scattering. And so, I joined this group with Maurice Bazin and Al Goshaw and did the experiment at the Princeton Penn Accelerator, the PPA. Then the other horrible thing at Princeton—and since then they've gotten rid of it—was the general exam, which was a terrifying exam. You probably have heard about it if you talk to people at Princeton.

ZIERLER: Sure.

ODDONE: Because for a week or so you were attacked by difficult questions on every subject.

ZIERLER: [laugh]

ODDONE: There are various things that have been said about the exam. One of them is that it's the hardest exam that everybody, in the end, passes. Well, it's not quite true. You know, there were people who failed. It was a really tough exam. For the first couple of years you really hit the various classes, things that you might not be interested in, but everything in physics so that you could actually pass this thing. And then, after you passed, you did your thesis. When the time came for my thesis, I had done the bubble chamber experiment on deuteron-deuteron scattering and it was interesting, but it was almost complete. I could see that I was not going to learn very much more from it. So, I joined a group that was doing an experiment at the Princeton Penn Accelerator using purely electronic methods. In that experiment I worked for about a year, and then realized with another student ahead of me, that I would never actually get to do my thesis. There weren't enough events and several mistakes had been made in the calculation of the rates you could achieve. So, at that point, I jumped ship back to deuteron-deuteron scattering but expanded—with Maurice Bazin and Al Goshaw. We expanded that work to higher energies and went and did the additional experiment at Brookhaven. We added a whole energy series and we could actually do a very nice job in understanding deuteron-deuteron scattering given all the approximations that you have to make, understanding it as the collision of its various constituents.

ZIERLER: Now, Pier, I want to ask at this point, between your work at Princeton Plasma Physics Lab and then at Brookhaven, were these experiences—looking back, did you feel like this was your natural environment and that you would spend a career in the national laboratory context?

ODDONE: Not at that time. I think it was clear that I would use accelerators, but whether I would end up at a university or a national lab as a place to work, I didn't know at that time.

ZIERLER: Mm-hmm.

ODDONE: And I think probably—things happen in different ways. I may have gone to a university where I could've stayed, but I think at the end, working at a national lab for me has been just fabulous.

ZIERLER: Yeah.

ODDONE: And I even had a faculty appointment at Chicago while I directed Fermilab. — Often people want a joint lab/university appointment and positions like that. But, frankly, in the national labs, you can build the big experiments, you can have big ideas, and there is a certain sense of teamwork that is very important. For example, when I came—and we can talk later about the B-factory—when I came up with the idea of the B-factory, I could go and get help from people around me, who knew a lot more about accelerators and who could actually help figure things out, and were ready to do so because in the spirit of teamwork we helped each other a lot. That happens in universities also, but I think the pressure of tenure, not that there aren't pressures at the national lab, tends to make the academic environment a little bit more cut-throat, I would say, than the teamwork that you get at national labs.

ZIERLER: Yeah. And, by definition, at national labs, everybody is sort of working towards a singular mission. That's not the case in an academic department.

ODDONE: Anyway, I finished at Princeton in the four years. Basically, their technique was to just cut your financial support after four years. And it was very different from other places. At Berkeley, if you came to graduate school, you could find students that had been there six, seven years getting their PhD. This was anathema for Princeton.

ZIERLER: Yeah.

ODDONE: They wanted to get you out in the world. And you may not have been as well prepared as you might have been if you had taken more time, but they thought that would basically fix itself. You really needed to get out in the world and do things. So, Barbara typed my thesis. While I was in Princeton, she was first in the philosophy department as an administrative assistant, and then, after a year of that, she got a job at something she is fabulous at: teaching kids. So, these were 10-year-old children in a little private school in Princeton, called the Chapin School. For the rest of the time that we were there, she taught school and I had my fellowship, and we lived very modestly in a place for graduate students that has since been torn down. This was called The Project or something like that. It had been built, a bunch of shacks, during the war in what I believe was the Polo field. They were supposed to be taken down after the war. And here we were 25, 30 years after the war and the barracks were still there for grad students.

And they lived for another 20 years until they finally took them down. But I do remember that the rent for our little place was $50 a month.

ZIERLER: [laugh]

ODDONE: That was great. And because they were little shacks in the middle of a field, you could put up a fence, so we had a dog. It was actually a pretty sweet time we had in The Project.

ZIERLER: Life was good. Now, Pier, you mentioned that you mostly worked with younger professors, but I wonder, did you avail yourself the opportunity to interact at all with some of the more senior—

ODDONE: Sure.

ZIERLER: —more famous professors at Princeton?

ODDONE: I took classes from Cronin and Treiman, and there were great people there. But the thesis itself was done with the young folks.

ZIERLER: Yeah.

ODDONE: The young folks were interesting. Al Goshaw has had a very distinguished career in physics. He is mostly known for his work in CDF. He was a distinguished professor at Duke and recently retired. Maurice Bazin had written a book on general relativity; he was a very rigorously trained Frenchman. And the amazing thing to me with Maurice was that after he became a professor at Rutgers, the next thing I know—and we had never had a political conversation—but the next thing I know is that he drops his position at Rutgers and goes to Chile to help Allende build the new country.

ZIERLER: Wow.

ODDONE: And, as I said, we had never had a political conversation, and I didn't know that he was this flaming Red.

ZIERLER: [laugh]

ODDONE: It was a big surprise for me. And then I lost track of him. I know that when Allende fell—he survived the whole thing and went back to France. But I completely lost track of him. These were fun people to work with. But, to me, to have worked closely with somebody for two or three years on physics and never have gotten into a political conversation was a real surprise, because I would've thought that I would know who he was—I guess I must've been paying attention only to physics, You got a great physics education at Princeton. Wheeler was there, Treiman and many others. You went to seminars and even heard Oppenheimer. For me it would've been better, probably, if I had worked with Fitch or Cronin or somebody like that, but I think you progress, and you learn more and more as you go along. My career might've taken different paths, but at the end it all worked out very well for me.

ZIERLER: And when did you defend your dissertation?

ODDONE: I defended it right at the end—before I left, which was 1969. And I had a couple of papers left to write, which I wrote when I was at Caltech. Because, as I said, Princeton would cut off your support after four years.

ZIERLER: Right.

ODDONE: We even lost our house and we were housed with somebody else who was kind to us. Barbara typed the dissertation, and we handed it in and did the exam, and off we were. But I had work remaining when I was at Caltech, because I still had to finish the papers. And so, there were a couple of papers and Physical Review letters that I was the principal author for and had to finish them.

ZIERLER: Is that when Caltech was a two-year postdoc?

ODDONE: It was a two-year postdoc, right. It clearly positioned me for my next job. Caltech was getting into the bubble chamber business as everybody else was getting out of the bubble chamber business.

ZIERLER: Uh-huh.

ODDONE: And they were doing it with a twist. They were trying to take a shortcut by going to a semiautomatic scanning system. It was called the Polly system that Argonne had invented. But the real new twist was in triggered bubble chambers. As you know, in a bubble chamber you have to expand before the particles get there in order to get the bubbles to grow, and then you take the picture. And so you're limited by the number of pictures that you can take, because the rate at which you can study an interaction is limited by the fact that you take 20, 30 particles in per frame, but you're committed to expanding without knowing what will happen. Most of the pictures are empty; if you're looking for something rare, you don't find it. So the twist at Caltech, in an experiment that Charlie Peck led, was to hang a spectrometer behind the bubble chamber and look at the spectrometer to calculate the missing mass of what's left behind, and that way you can decide whether something interesting happened or not. And if nothing happened, you don't take the picture, so you don't have this enormous burden of millions of pictures. You could use a rapid cycling chamber, like the SLAC chamber that was, in principle, expanded at 40 cycles per second, and do many, many expansions, but you'd come out with a relatively manageable set of pictures because you only take the pictures that have what you want. That was an interesting experiment because it combined for me both the electronic techniques and the bubble chamber technique—that's probably why they gave me the job at Caltech. It combined for me the electronic techniques of having a spectrometer and a fast trigger with what I had done before, which was the bubble chamber work. So, at Caltech I worked partly trying to get the scanning system up and partly getting the experiment to work. That was a terrific education, with Alvin Tollestrup, the head of the Caltech group. He liked to tease me afterwards because he was my boss at Caltech, but then at Fermilab I was his boss.

ZIERLER: [laugh]

ODDONE: Because he had moved to Fermilab to build the Tevatron. But it, again, was a terrific group of people at Caltech with Jerry Pine, Alvin Tollestrup, Charlie Peck, Ricardo Gomez and others.

ZIERLER: Were you doing teaching at Caltech also as part of your postdoc?

ODDONE: Excuse me?

ZIERLER: Were you doing any teaching as part of your postdoc at Caltech?

ODDONE: Yes. Basically, teaching assistant. I even forget the courses; I did a couple of them. You had to have, as a postdoc, a rounded experience, so you had some duties as a TA. And in Princeton I had also run the lab for this "physics for poets" class where we taught physics to pre-med students. So, I have had relatively limited teaching experience because I only did that a little bit at Princeton and a little bit at Caltech. But after I came to Berkeley, I was pretty much full time at the lab.

ZIERLER: Now, how did that connection come about? Did you seek them out or did they recruit you?

ODDONE: They recruited me. When they told me at Caltech that my term was over after two years, I looked around. But at the same time that I was starting to look around, we were doing this bubble chamber experiment at SLAC, BC-25, with the rapid cycling chamber that was a collaboration between Caltech and the Berkeley group.

ZIERLER: Yeah.

ODDONE: And so, the Berkeley group, when they realized I was looking for a job, they said, "Come and work for us."

ZIERLER: Yeah.

ODDONE: So, I was hired at Berkeley, again as a postdoc, in what was called the Birge-Powell group and worked with Bob Ely and George Gidal. Being in Berkeley was also great for Barbara since her family lived in Berkeley. In Pasadena she had gone to school and gotten a master's degree in special education and taught for a year in a barrio school. When we got to the Bay Area, she went to law school at UC Davis and later practiced labor law in San Francisco for many years.

ZIERLER: Now, with regard to the Berkeley group at SLAC, was Berkeley and SLAC—was it more cooperative or competitive, would you say?

ODDONE: That's a long story. I think at the point that we were doing that experiment it was completely cooperative, because there was no facility for doing that type of physics at the Bevatron, and there were no ambitions to build such a thing. Later, when it came to building the positron-electron project, PEP, it was a mix of competition and collaboration because Berkeley had ambitions, at one point, to build such a machine through the Berkeley hills. It was not the right place to build it, but you could. So, there was a certain competition before things settled down and the PEP project was built at SLAC as a collaboration of three labs: Berkeley, Livermore and SLAC. And then, subsequent to that history, there are several incidents of very strong competition because there had been ideas people would say originated in Berkeley, that Berkeley wanted to implement, that ended up at SLAC. And there was also a competition when it came to the building of the Time Projection Chamber, the upgraded Mark II and the rest of the experimental program at the first PEP machine. So, there is a history that goes both ways. There's competition under some cases, but I would say overwhelmingly it's collaboration, simply because you end up doing physics where the machines are, and once the dust settles, you get together and you work together.

ZIERLER: Right.

ODDONE: But in terms of gaining your machine or acquiring your machine, then there can be competition.

ZIERLER: Now, to the extent that you might've been on a track to eventually obtain a faculty position, at what point did you determine you were a lifer in the national labs?

ODDONE: I guess there was no definitive point. —I was so focused on physics and doing certain projects that I wasn't particularly anxious to get a faculty position—

ZIERLER: Right.

ODDONE: I was having a lot of fun at the lab and appreciated having resources. At the Berkeley lab, it was difficult to stay if you came in as a postdoc, to get a permanent job. And the experiments that I was doing, BC25 at SLAC and then a streamer chamber experiment at the Bevatron, the last particle physics experiment there before the Bevatron turned to nuclear physics, they were good experiments, but they weren't going to break fantastic new ground. I probably came to the attention of some of the senior people in Berkeley—and it just goes to show how sometimes life works in strange ways—when the streamer chamber, that my work as a postdoc was supposed to be on, was completely destroyed. And this was a facility that had been built over several years. It was a streamer chamber built at Berkeley by a world expert in streamer chambers, Ladage. It was very fancy, inside a big M5 magnet, with a big open bore for the camera system. We were doing a baryon exchange experiment, where you triggered the streamer chamber with an external counter that told you if a pion went in and a proton went out, so you knew you had a baryon exchange reaction. And a collaborator of mine, who will remain nameless, was working with a movable counter in front of the target, the hydrogen target, to position the beam. And that counter was inside the bore of the magnet and so it's phototube was shielded by probably a thousand kilograms of steel, some big, very thick iron shielding that was moved back and tied down after tuning the beam and before turning the magnet on. And my dear collaborator, a senior member of the group, forgot to tie it down as you had to do before you turned the magnet on. He turned the magnet on, and this thing, that was probably 16 inches across and a couple of feet long and several inches thick flew into the magnet like a cannon ball, straight into the streamer chamber and basically broke it into smithereens. In fact, we vacuum-cleaned it out of the place. I had gotten hired to, a) finish the BC25 bubble chamber experiment at SLAC, but then, b) help with this experiment that, again, had pictures as data but a lot of electronics. And I saw it go up in smoke, so to speak, into these little pieces.

ZIERLER: Did anyone get hurt?

ODDONE: No. We were in the counting house and there was just a big "boom" as this thing flew into the magnet and destroyed the chamber. So, I figured my career was pretty much finished with that. But I thought very hard about what to do, and I figured out that I could actually rebuild that chamber in a much simpler way. The main trick was to be able to rebuild it in place, not to do what had been done, which was to take the magnet apart, build the chamber and rebuild the magnet around it. The chamber was all made out of Lucite. There were a million little threaded holes and would take a long time to rebuild. So, I went to the head of the accelerator division at the time, Herman Grunder, who had spent years building it and making it work. He had brought Ladage from Germany to do it. Herman is now a good friend of mine and I could tease him afterwards, but this was challenging at the time. I went to him and said, "Look, I can do this. Let me try it my way." And he said, "No way. I spent millions of dollars building the facility and we're going to build it just like it was. It was working fine." And I was accustomed to Swiss people, because I went to the Swiss school in Lima, and they're very orderly and very definitive, and, by God—he was a good manager, no question about it. He knew what would work, but it would work for the lab and would not work for me, because that thing would take forever to get put together again. And so, I went to Bob Birge, who then became my mentor. He has an interesting history because his father, Raymond Birge, brought Ernest Lawrence to Berkeley. Bob Birge was the head of the Birge-Powell group at the lab, the group I was in. His son, Norman, is also a physicist. There are three generations of Birge physicists! But anyway, I went to Bob and I said, "Look, I can do this. Just let me do it. I can do it in this particular way: instead of making it out of Lucite with threaded holes and all that I'm going to make it out of closed cell foam. I'll cut the pieces of foam, I'll crawl inside the magnet, I'll glue the thing in place, and it's going to be better than before because the foam is even lower mass than Lucite." He believed me, gave me $5000 for materials and a great technician, Walt Johansen, to work with me. And in two-and-a-half months or so, three months, we had this thing fixed and running. And so, people at the lab—especially because my idea had been declined initially, I think people say, hey, this guy can do things.

ZIERLER: Was it functionally identical as it was before or was it doing different things as a result of being rebuilt?

ODDONE: No. It was doing the same thing but better, because it was less thick for particles to go through. The streaming chamber itself is very simple. The complexity is in the drive. You have to give it a pulse of 400,000 volts and all that, the insulators have to work without breaking and all that kind of thing. But, because the material of this chamber was foam, then the particles have an easier time coming in and out, because they don't have to go through a piece of plastic. In fact, what happened was that the Bevatron then changed to a nuclear physics program after my experiment. When the replacement chamber that Herman Grunder had ordered was completed, built according to the original specs, it went straight to the warehouse, because the foam chamber worked so much better. So, it was an interesting thing that you can have a disaster and sometimes getting yourself out of a disaster brings you a certain notoriety, if you can figure a way out that works. The experiment worked and, at that point, we were getting into '74, and there is the November Revolution, right? And the Trilling-Goldhaber group is down the hallway from the Birge-Powell group and they're having all the fun, right? Because of the discovery of the J/psi, great excitement and all of that. And here we were doing still fairly classic baryon exchange reactions at the Bevatron and Pomeron exchange at SLAC. At that point it became obvious, this science is filling in details but is not going to give you a big breakthrough. People, of course, didn't believe SPEAR was going to give you a big breakthrough at the beginning, either. I mean, electron-positron scattering was expected by many to be boring .

ZIERLER: And, Pier, are you defining success as a big breakthrough? I mean, what's the threshold here for what success means?

ODDONE: I think success is when you break new ground. When you actually figure something out that opens up a new vista. We're always, in particle physics, comparing things with theories or models that we know. They're always imperfect. And so, a breakthrough to me is when you open up a new window that allows you better understanding. And understanding can come from baryon exchange reactions , but then they come in very slow and incremental ways, whereas something like happened with SPEAR, all of a sudden opens a big domain. And so, at that point is when I got interested in the new project to follow SPEAR, the Positron-Electron Project, PEP. And after the November Revolution, why, there would be the top quark, there would be the bottom quark to discover and study. That would be a tremendously rich program. So, I jumped into it with both feet and I ran the workshop in 1975, the PEP workshop aimed at defining the experimental program. And that was a multiweek workshop with lots of people from all over the country. And I really dove into PEP and what PEP could do. So at the point when finally the PEP machine was approved to be built at SLAC—SLAC, by that point, because after the first battle, whether to build it at Berkeley or at SLAC, it was clear it should be build it at SLAC as a collaborative project — there were positions in building PEP that should be filled by both Berkeley and SLAC people in collaboration. The director of the project, John Rees, a terrific person, was from SLAC. And the person that would be in charge of the experimental program was to be a person from Berkeley. And there I got a break, because I think the Berkeley folks that SLAC would've expected to get the position, the really very distinguished physicists at that time, say a George Trilling or Bill Wenzel, or somebody like that, none of them actually wanted to do that job. It involved commuting back and forth to SLAC and it wasn't clear what authority you would have when Pief Panofsky was running SLAC and Rees was building the machine. So, at that point—evidently I had done a very good job running the workshop, was on top of the physics potential, had come up with the parameters for the experimental areas and all that— Berkeley appointed me to that position because there was nobody else who wanted it.

ZIERLER: [laugh]

ODDONE: And so, I think it must have been a bit of a shock at SLAC that here was a—I was still a postdoc!

ZIERLER: Right.

ODDONE: And I'm put in the position to administer this program, which not only had to set the parameters for the experimental areas, six areas, one underground and not very big, but the other five had to meet the requirements of the experiments. And then there was a pot of money for all the experiments, and I had to negotiate who would get what and all of that kind of stuff. And I think it was a bit of a shock that Berkeley would actually put somebody that young in a position like that. That is when I grew a beard to look older and tougher.

ZIERLER: And who was it that promoted you? Who championed your promotion?

ODDONE: I think ultimately Andy Sessler who was director of the lab at the time and was later followed by Dave Shirley. I wasn't in the discussions where they discussed my appointment. But I know that Karl Strauch, who had run CEA, the Cambridge Electron Accelerator, was involved in the early phases of planning. And I think he had seen me work during the workshop and he told them, "He can do it, and you should appoint him." And they took a gamble. I mean, they could always fire me if it didn't work. But I think that push by somebody from the external community that would say, yeah, this is going to work, plus the fact that they didn't have anybody inside asking for that job, ended up positioning me in that particular place. And it was a fantastic experience because John Rees, a great accelerator physicist, was not particularly interested in the details of the experimental program. He was interested in the success of the experimental program, but he wasn't really very close to it. Pief Panofsky was the one who was close to it. So, I basically worked with Pief on all these things.

ZIERLER: Yeah.

ODDONE: And you could not have a better person to learn from.

ZIERLER: So, Pier, I want to ask on that right there, because that's the inevitable question, working with Pief and then thinking toward the future, when you assumed the directorship at Fermi, did you draw on some of the things that you learned from Pief in terms of how to direct the lab the right way?

ODDONE: I definitely did. Pief is in that set of people that you look back on as being influential on the way that you choose to run things. I always felt Pief had a fantastic way to deal with people. You would go into his office and you never came out unhappy, but he would beat you up if you didn't do something that was smart.

ZIERLER: Yeah.

ODDONE: I had to write a lot of papers on policy for experiments and about how I proposed to run the group and what I would do. I remember the first one I wrote because I had this big concept of the group I would build—there were five experimental areas and five detectors, and it looked to me like a big deal. And so, I write this paper about how I am going to run things, how I am going to build this group with this person and this person and that person and all that. And I write it all very sensibly, and I go to Pief, and his first comment to me about the paper I just had sweated over and handed to him, "It made me gag."

ZIERLER: [laugh]

ODDONE: "It made me gag." So, then he explained why he was gagging. He thought it was way too elaborate, that I could do it in a much simpler way. I had proposed to build this big group. It was my first big management job. Instead Pief thought I could lean on the resources of SLAC and do all the various things that I had to do—rather than building this group, I would work with the other groups and so on and so forth. Lesson one, right?

ZIERLER: And that was part of his style, always looking for the simplest way of doing something?

ODDONE: Yes, absolutely. That's the good part of Pief. And then there is another part that I felt, by the time I got to Fermilab, would not work in the environment that we had to contend with.

ZIERLER: It's a different place?

ODDONE: It's not just a different place. I think things had evolved. I think the bureaucracy, the level of control that the Department of Energy wants to have over projects, the management of projects, the very detailed procedure of projects, various critical review stages and all of that, I think that would have been troublesome for Pief's way. I think Pief would have wanted clearly defined independence of roles. After all, we get hired, or Stanford, or whoever gets hired, to manage something, let us manage it. And I think the limitations of Pief's approach showed a bit in the SSC, where he was an advisor to the director, Roy Schwitters. The SSC never really was able to quite handle the bureaucracy and it got beat up all the time. I think—I would say as influential as Pief was, so were my 15 years as deputy director of the lab working with Chuck Shank, where I saw just about everything. It was a long enough period, in a very diversified laboratory. I also saw parts of the Department of Energy other than particle physics. And so that period, before I went to Fermilab, was as formative as working with Pief during the PEP times.

ZIERLER: Yeah. So then, what was your next big promotion at Berkeley Lab?

ODDONE: Well, I forget when, but after working at PEP, I also joined an experiment, a project which was the time projection chamber or TPC

ZIERLER: Right.

ODDONE: —that David Nygren had invented. I played fairly important roles there, eventually became the head of that project. So, I was dual hatted, working for everybody on the experimental program, but then in the science research part I was working with the time projection chamber. And eventually, after both roles, because of the work on the time projection chamber and my work at PEP, I was promoted to senior scientist at the lab. The TPC project was really not only a great project but it also transformed the way the Berkeley Lab Physics Division worked. The Division, when I arrived there, was still made up of individually led groups that had quite distinct projects. Group A, the Alvarez group, did certain experiments, the Trilling-

Goldhaber group did other experiments and the Birge-Powell group, Kerth-Wetzel group, the Segre-Chamberlain group yet others. And the TPC was such a seductive project that it brought many of the groups together. And it—

ZIERLER: What was it about it? What made it so seductive?

ODDONE: Well, the seduction of that project was that you could, in fact, without any hardware in the middle of the detector, have incredibly high-quality tracking with particle identification at the same time. And it was an ideal four-pi detector for a collider like PEP.

ZIERLER: Mm-hmm.

ODDONE: TPCs now have been used in lots of places. TRISTAN followed with a TPC, DELPHI, I think, had a TPC at the LEP collider at CERN and ALICE at the LHC. It has limitations for super-high rates like the LHC, but for the e+e- colliders of that time, this was a fantastic idea because of the superb quality of the tracking. When you have tracking with drift chambers you have to resolve ambiguities. At that time, you never had 100% efficiency. And you didn't necessarily have particle identification. The fact that you have these continuous tracks in the TPC allowed you to capture all the information, capture all the ionization and identify the particles emanating from the collision. It was really a seductive concept. That coupled with the great hopes for PEP, because when we started PEP and the energy range was set, the hope was that PEP would be good for B physics, discover the top quark. The hope was that these would be rich areas for the machine to study, and that you would need a detector as competent as the time projection chamber. Now, there were, again, lots of problems in developing the TPC. We got all those Berkeley groups together and they had lots of young people, David Nygren, Jay Marx, me, a lot of young people wet behind their ears. And we got into a lot of trouble. I think there was no experience at Berkeley in building such a complicated detector. And, in particular, we were fairly naïve that we could actually innovate in multiple aspects, not just a time projection chamber which, itself, was very challenging, but we would build the thinnest high-field superconducting magnet ever, we would surround this with a digital calorimeter running in Geiger-mode, never done before. We had trouble on all of them. First of all, the thin superconducting magnet, the first time turning it on during construction and testing, burned up. I was then in charge of building a conventional magnet so we could get the experiment started. Then, after a year of running, the calorimeters, the Geiger-mode calorimeters that had ethyl bromide as part of the gas mixture, went bad. Literally, two of the modules went up in smoke. They really didn't burn, but they stopped working, and when people opened them up this white cloud came out. I was at Stanford that day, and I had just been appointed the spokesperson for the collaboration. I hear there is a fire at the TPC experimental area. It wasn't really a fire, but I see Burt Richter driving on his motorcycle to the experimental area. I'm behind him in my car. The fireman says, "Shouldn't we evacuate Sharon Heights?" Well, that wasn't necessary. It turned out it was a fairly harmless compound as far as health effects were concerned, but not for the modules. What we hadn’t realized is that it is possible to have an autocatalytic reaction inside a vessel that has aluminum strips and ethyl bromide as part of the operating gas and end up with a horrible compound, I think it's aluminum sesquibromide that goes up as a cloud of smoke. But what the autocatalytic reaction did had not happened in four of the six calorimeters modules. Once the reaction got started, it basically ate the insides of these calorimeters. We had to rebuild them, which we did, and we had to change the gas mixture for them. Eventually, they worked like a charm. And then, when we first turned on the time projection chamber itself in the first data run, we had neglected something called positive ion feedback. When you multiply the electron avalanche on the end-anodes, you create positive ions. If you let them leak into the drift volume, they take forever to go across the whole volume, and they create field distortions that are not constant. So we had to come up with the concept of the gated TPC, where you gate the field wires just in front of the anode wires in such a way that after the event you capture the ions in the short space between the anode wires and the nearby field wires . So, I spent the summer modifying, with Peter Nemethy, all the TPC chambers. We were like car mechanics, because these chambers were going to operate at 10 atmospheres and to test them, they had to be in a pressure vessel that could take 10 atmospheres. In-and-out of the pressure vessel we had to bolt the pressure vessel with innumerable bolts every time. As I say, it felt like we were working in a car shop, changing tires or something.

ZIERLER: [laugh]

ODDONE: But, again, we had a technician that was absolutely outstanding. Ray Fuzesy was a legend at the lab. And we got all down to work and we got it working. The TPC at the end was a phenomenal technical success, even though it's probably one of the hardest TPCs ever built. Because many of the subsequent TPCs were atmospheric TPCs. We ran this one at 10 atmospheres, which, of course, gave us great particle ID and great results. But it was ambitious to an extent that today you probably wouldn't get past any of the reviews if you were proposing all of these innovations without having prototypes for everything.

But the ultimate disappointment in PEP was that the energy of PEP I (there would later be a PEP II), was too low for the top quark and too high for the bottom quark. People at CESR at Cornell were having a lot of fun with the B physics that they were doing. And eventually the Tevatron came up with the top quark, but there we were stuck between these two rich areas. If you look at e+e- storage rings, they have a luminosity curve that is sharp; it peaks at a designed energy, but if you drop from that energy you lose luminosity rather quickly. So, a machine like CESR that was tuned for an energy to produce Bs, did much better on Bs than PEP could ever do. We were in a region, contrary to what the original expectations for PEP had been, that was relatively physics poor —TRISTAN in Japan had exactly the same problem. We did good things, but no great breakthroughs.

ZIERLER: What do you mean "physics poor," Pier? What does that mean, "physics poor"?

ODDONE: In the number of discoveries. We studied many phenomena, yes, we did jets, yes, we studied fragmentation in detail and all of that, but there were few surprises. The one thing that PEP did that was in a sense a breakthrough was discovering the long lifetime of the B meson, which told us that the mass of the top quark was very heavy. And that was done by the MAC and the Mark II detectors that had better vertex detectors than the time projection chamber. The thing that I would say is that for the key discovery at PEP, all the technical glories of the TPC didn't really help you. That came from another corner, the Mark II and MAC that had better vertex detectors. It's interesting the way that things go sometimes. But, as I say, the initial PEP I, compared to what the expectations had been, yielded a less rich program than either the B spectroscopy at CESR and later the b-quark and top quark physics at the Tevatron. And that disappointment, probably at some deep level, had something to do with my proposal for the Asymmetric B factory, or PEP II. I was commuting back and forth from Berkeley and I commuted typically with Peter Nemethy. We would talk about what we could do to this machine to get into the game with B spectroscopy, or other physics that might be more productive than what we were doing with the initial PEP machine. For me, however, it was a great learning experience, coordinating the experimental program, building a very complicated detector like the time projection chamber, seeing what the physics limitations were and where the physics discoveries were coming from. Those were all valuable lessons.

ZIERLER: So, when did the opportunity for Fermilab start to become a reality?

ODDONE: Well, that was after—the more important thing that we should probably talk before Fermilab is the B factory.

ZIERLER: Oh, right.

ODDONE: So, as I said, I was quite dissatisfied with the fact that PEP hadn't—after all the hopes, hadn't really got us either to Bs or to the top quark. And I kept thinking, how can one do some really great physics? And I came up with this idea of producing B meson in asymmetric collisions, initially with linear colliders. At that point, SLAC was pushing for linear colliders already, and I thought, gee, we can do a model linear collider with collisions, at the Upsilon(4S) with asymmetric collisions. In other words, take a linear collider of a certain energy and take another linear collider of, say, half the energy, and then make these collisions at the Upsilon (4S) and you would boost the particles, a B and an anti-B coming from a well-defined quantum state, the Upsilon (4S). You could tackle CP violation in B decays, something that people didn't think you could do with the Upsilon(4S) in the symmetric machines like CESR. The initial concept of using asymmetry to boost the Bs was in connection with a linear collider. And it had—the beautiful advantage in my mind was that actually it was a relatively short accelerator, it could fit in the Berkeley hills, and after all those years of commuting back and forth to SLAC, the idea of rolling out of bed and being right there, working on my experiment was really, really attractive. So, I started giving talks about it until the linear collider idea was shot down. It was shot down after I had given a memo to Andy Sessler asking him to investigate whether there was anything wrong with this concept. And he and John Wurtele in Berkeley worked on it and actually discovered a piece that I had completely ignored because I thought this was such low energy machine that beamstrahlung was not going to be a problem. You know what beamstrahlung is? The emission of radiation as a charged bunch collides with the opposite bunch. And it turned out that, even at these low energies, by the time you cranked up the luminosity high enough, the beamstrahlung was so big that if you tuned the collider to be at the Upsilon(4S) resonance, the energy spread at the peak was much bigger than the Upsilon (4S) resonance. So, you would lose, essentially, all your luminosity because most collisions were not on resonance. It wasn't practical to build the linear collider. At that point I said, OK, what can we do with PEP? And I worked with Ewan Paterson at SLAC. I'm not an accelerator physicist. I had taken courses taught by Matt Sands and others, but I don't consider myself an accelerator physicist. I needed to have somebody to help me figure out whether some ideas to use PEP were right. What I did is actually to try to use scaling, without violating the beam-beam limit or anything else, to figure out what you could do if you built a lower energy storage ring and collided it with the big ring that was there. It’s a long story, but the initial calculation just by scaling, showed that this was actually a pretty good idea, and that you could boost the luminosities by quite a bit. Al Garren at Berkeley and other accelerator physicists at Berkeley, and Frank Porter at Caltech, grabbed hold of the idea and started making designs of this machine. Through a series of many contributions by a lot of people, it ended up being the design of the asymmetric B factory that eventually got built, where the low energy storage ring, for technical reasons, has the same radius as the high-energy storage ring and sits in the same tunnel. It just means it's a sparser machine, but it all fits in the tunnel. And the idea of the B factory really took off, in part because it allowed labs like SLAC and KEK—I said a couple of labs, but there were other labs that wanted to build it as well, including Cornell—to take the infrastructure that they already had there and modify it and build a machine that was at the cutting edge, doing a fundamental physics experiment on the violation of CP symmetry in the B-meson system that people didn't think could be done before. It was a good deal—I don't know what I want to call it—30 cents on the dollar or a quarter on the dollar. Because you had the injector, you had the tunnel, you had the major ring, the high-energy ring, which is the expensive ring. You had to build a low-energy ring and a detector. And so that idea just really took off, and a lot of good people came to want to do the physics, among them David Hitlin, Jonathan Dorfan and many others. I think it took a while to convince Burt Richter, the director of SLAC, to go in that direction.

ZIERLER: Mm-hmm.

ODDONE: He was still pushing linear colliders. I think the SSC was still going strong—

ZIERLER: And so, with Burt, was it an either/or proposition in terms of this or linear colliders? Why not both?

ODDONE: Well, he was very lukewarm initially about the B factory, called it "beautic physics". I don't think he appreciated the full power of it at the beginning.

ZIERLER: Boutique physics, meaning that what it would yield would be necessarily limited in terms of—

ODDONE: Well, boutique—Burt has a great idea. You know, a linear collider is going to do this fantastic stuff and you're studying B physics; that's just "beautic" physics.

ZIERLER: [laugh]

ODDONE: It's what you do in your parlor. But I think, ultimately, Burt had a big problem. He wanted to go with linear colliders, but the SSC was going on. The SLAC linear collider, SLC, the one that they did build, that started in '89, wasn't working very well. And the SSC was taking all the money. And there was a meeting. I think it might've been the HEPAP planning subpanel led by Mike Witherell. I forget which one of the HEPAP subpanels that visited SLAC. You should talk to Jonathan Dorfan because he knows this story. The way he tells it, if I remember correctly, is that there was talk at that time—had to have been about 1992 or so—there was talk at that time within the HEPAP subpanel that maybe SLAC's life was over, 'cause the SLC wasn't working very well, SPEAR was done and turned into a light source, so what was SLAC going to do? The field doesn't have any money to build the linear collider since we're building the SSC and so on and so forth. So, Jonathan got wind of this and he talked to Burt, and I think that's when Burt went in and said, "Look we can build the B factory. I can rearrange the budget of the lab to do this." And it was a sufficiently attractive project, and small enough in the scale of the SSC, that I think that is what finally broke the dam at SLAC to get it going. But Jonathan would be a much better narrator of that particular situation than I am, because I was not involved in the discussions of the subpanel—I was involved in the selling of the B factory, not making the decision whether to build it or not.

ZIERLER: Right.

ODDONE: And then there are several interesting things about the B Factory. Of course, Japan decided to do one also, and they had a remarkably similar situation. What I found really extraordinary is that KEKB, the Japanese machine, and the Asymmetric B Factory at SLAC were neck and neck the whole way through to the discovery of CP violation in the B system.

ZIERLER: Right.

ODDONE: These are complicated machines. There were lots of things to do that could go wrong. It's so easy to fall out of sequence with some component so that you would be six months behind. But it didn't happen. It was neck and neck the whole five years of building the machine, the detectors, all the way to the discovery paper. So, at the end, they have been very, very productive machines. The Asymmetric B Factory got killed probably prematurely with the budget crisis in 2008. The Japanese went ahead and have built SuperKEKB, the successor to KEKB, which is starting to work now to get even 40 times more luminosity than the Asymmetric B Factory. We'll see how far they get. It's not clear. And, of course, there was very productive B physics with CDF at the Tevatron and now with LHCb at CERN.

So then, before getting to Fermilab, I was deputy director under Chuck Shank for 15 years at the Lawrence Berkeley Lab. And, as I mentioned before, in terms of preparation for directorship jobs, that was great, because, for one thing, the lab is so broad that I was able to appreciate the opportunities in science in a much, much broader way than if I had just stuck to particle physics. And I think that, when I was sought out to be director of Fermilab, probably that breadth played a role—

ZIERLER: Sure.

ODDONE: —in that I had now a very broad view of what was possible in science, and I had been able to develop several projects at the lab.

ZIERLER: Now, the broad view, not just the scientifically broad view, but an administratively broad view, as well, right?

ODDONE: Right, right. And, in fact, it's funny how those things go. Partly—you probably saw in my CV—I've been, for the last few years after I retired, advising the National Cancer Institute.

ZIERLER: Yeah.

ODDONE: You say, what the hell is that—

ZIERLER: Yeah.

ODDONE: —[laugh] for a particle physicist? Well, it goes back a while. In the lab, we had researchers in biology—a very prominent one in cancer research was Joe Gray. He has an institute now in Oregon, a very big institute on cancer research. He also has a physics background. He is a person who wanted to see a different approach to how NIH tackles projects because he had seen what you do in physics. He originally worked at Livermore and saw how Livermore could do weapons programs; he saw at Berkeley the physics projects we tackled. He invited me to the Cancer Research Association meeting to give a talk. And he told me, "Just talk to them about how you do projects." Well, what I did then is I studied how biologists do projects and how physicists do projects. And how much comes from the culture of how they do things. This was several years ago, maybe 10 years ago. And I went to this meeting and I gave this talk, and I think it was a bit shocking to the people there hearing the scale of things we were doing in physics with actually a lot less money than what the NIH has.

ZIERLER: Yeah.

ODDONE: And, yet, the projects are worldwide, they're organized. And I tried to dig into the cultural reasons for this, because I had biologists at Berkeley that I could talk with. And that was a very successful talk, I think, because it got people really discussing broadly how biologists and physicists work. And then, after that, Harold Varmus invited me to join a workshop on cancer research and later I joined the FNLAC, the Frederick National Advisory Committee. And I remember at the end of a workshop Varmus telling me, yeah, we should work this way but politically we can't. And that's part of the problem of the NCI and the NIH, where there are all these constituencies—

ZIERLER: I should mention, Pier, tangentially, I just finished a major project interviewing most of the physicists at NIH. And one of the big takeaways there—like with coronavirus, too, physicists have a role to play in these issues, absolutely they do.

ODDONE: Yeah. Anyway, and then I got invited to be part of this committee because they sought a broad perspective, I was able to see how different scientists operate. And there are tribes. There are different ways of doing things.

ZIERLER: Yeah.

ODDONE: It was eye opening for me to help with the NCI because I could talk about how you should do projects and all that in some ideal world, but they live in a very complex world where there are very high-end performers, institutes and private companies, almost every professor worth his salt has got a company going. There's only one national lab, the Fredrick National Lab, but there are all these institutes, like the Broad Institute, or other institutes that are very powerful. And there is not the same sense of what the national lab system can bring to the table.

ZIERLER: Yeah.

ODDONE: I think Harold Varmus tried to change the Frederick National Lab into a lab that would be more like Berkeley as opposed to doing programs that are tightly specified by the NIH in an intramural program. Anyway, it was a very interesting experience, and I was happy to help.

So back to Fermilab —I had been interviewed for the Berkeley lab directorship in 1990 when they selected Chuck Shank, I got interviewed in 2004 again before Steve Chu came to be lab as director. And then I had a new director and I had been 15 years the deputy director. I could've been happy staying, Steve Chu wanted to keep me. He said he'd come and work in the vineyard for me if I would work for him. But—

ZIERLER: [laugh]

ODDONE: you know, he's a funny guy. Anyway, URA, the contractor for Fermilab, asked me whether I would be interested in Fermilab, and even though I had not worked at Fermilab — I had made proposals to Fermilab and I had worked as a postdoc in Caltech with Barry Barish in the neutrino program briefly—it looked like a pretty intriguing job for me because pretty much everything would be up in the air. Would the country go for a linear collider?

ZIERLER: Right.

ODDONE: What would happen to the Tevatron? Certainly, it would have to transition to a new program.

ZIERLER: So that was obvious even before you started with the Tevatron that it would have to transition?

ODDONE: Yes, yes.

ZIERLER: This wasn't something that occurred to you when you got there?

ODDONE: Right. No, absolutely. I mean, I knew I was getting into—in Spanish we have a saying to describe somebody who gets into too big a job or something too complicated, that is "you get into an 11-yard shirt."

ZIERLER: [laugh] OK.

ODDONE: "Una camisa de once varas." You're trying to get out—and I knew—

ZIERLER: And did you see that, Pier? I mean, was your trajectory at Berkeley—did a directorship at a different lab, was that sort of the next logical stage or did you view this as, like, a quantum leap in terms of responsibility, in terms of complexity?

ODDONE: I wouldn't say it was a leap, because there were periods when Chuck Shank would go sailing in the transpacific race from San Francisco to Hawaii and I was left in charge of the lab for a couple of months.

ZIERLER: Yeah.

ODDONE: And, we had a good partnership where we really ran the lab together. And so, I really felt that I could run a lab in terms—

ZIERLER: What about in terms of size and scope and complexity and all of that, coming from Berkeley to Fermi?

ODDONE: I think Berkeley is more complex in that there are many more programs going on. Each is, as I mentioned, a different culture. I think the big difference is that the buck stops at the director. And there is a different edge when you're director versus when you're deputy director.

ZIERLER: Yeah.

ODDONE: When I am the deputy director, I can give my best advice, I can maintain a cool head, but in the end, if you have to fire a senior manager or something like that, it is the director's responsibility.

ZIERLER: Right, right.

ODDONE: For many things in Washington, sending the deputy is not good enough, you've got to be there. So being director has a much sharper edge than being a deputy director. I could be very calm as deputy director because I didn't have the ultimate responsibility. I could be the rational one—being a director, it's a different burden. The other thing that was different from Berkeley—it's not in complexity, it's really in the sharpness of the decisions that you have to make. Berkeley is very well balanced. Tomorrow they cut energy funding; oh, but we're doing biology, we're doing particle physics, we're doing nuclear physics. You don't bet the lab on any particular thing.

ZIERLER: Right.

ODDONE: So the edge at Fermilab—and what was really different— was that you're going to bet the lab in some direction or you're going to have to make major decisions in many areas that are much, much sharper, both because it's particle physics, a single-program lab, but also because you're the director. So, it was the transition from deputy director with a very broad purview and not the ultimate responsibility to one where you must make the ultimate decisions. That's where the real sharpness—

ZIERLER: So would that mean, then, that if you recognized even before accepting that the Tevatron was going to be in a period of transition or even worse, would that have raised existential questions post-Tevatron about what Fermilab would've even done? And are you thinking about that even before—was that part of your—

ODDONE: Absolutely, absolutely, yeah. That was part of the interest in this job, that it was—quite uncertain where things would end up. There were great opportunities, potentially. Ray Orbach wanted to have the linear collider come hell or high water.

ZIERLER: Right.

ODDONE: That would be fantastically exciting. On the other hand, had Fermilab gone and only done that, it probably would not have survived as a laboratory, because we're still here talking about the linear collider almost 20 years later.

ZIERLER: Right.

ODDONE: And Ray was adamant, when I started at Fermilab, that, by God, he did not want anything but the linear collider. He says, "I don't want plan B." And what he told me then—I didn't like what had happened to the first messiah, but—he said, "You have to be messianic about the ILC." That was the order from Ray.

ZIERLER: Was that his word, "You have to be messianic," you remember that?

ODDONE: Orbach said to me, "You have to be messianic about the ILC."

ZIERLER: Wow.

ODDONE: He wanted the ILC. And Ray made it a difficult for me because I felt, going to Fermilab, that we had to have a program. When you go for a machine of the ILC scale, you're trapped because if you don't push hard enough you don't get it. On the other hand, if you push hard and you put all your resources on that and you don't get it, then you're probably dead.

ZIERLER: Yeah.

ODDONE: And there was interesting physics to be done for sure in other domains. So, it was clear that one had to think about where the lab would go and deal with all these contingencies. The other thing I had to deal with, which was not really very visible on the outside world but it was terribly important for the lab, was that the University Research Association, URA, was not really considered a good contractor at the time by the Department of Energy and the contract was to be re-competed. They were running the lab. They are the ones who hired me. I worked for URA. But when I got there, I knew also we had to change—and I had told them from the beginning that we had to look at that. And I had talked to Don Randel, who was the President of the University of Chicago, to try to figure out how we reconstituted things into a much better setup for Fermilab. What we did with the University of Chicago and with URA, was that I convinced them to form a new entity called the Fermi Research Alliance, FRA, to be the new contractor for the lab, which was half University of Chicago and half URA. Now, remember, URA with its 90 universities came about because people wanted to have a true national lab responsible to all the universities. That was the model that led to the creation of Fermilab, and not the building of the 200 BeV machine proposed by Berkeley, where many perceived it would've been the sandbox of the Berkeley faculty. So, initially, when Fermilab was created, there was a very strong sense that it had to be a laboratory for everybody, and we didn't want to lose that. At the same time, by the time I went to Fermilab, I knew the DOE was really upset with this organization where you have 90 parents, which means no parent is really responsible, and the organization is run out of Washington out of a telephone booth, by two or three people delegated by the 90 universities. DOE just simply did not like that. When things go wrong, and they had gone wrong in the initial running of the Tevatron, DOE likes to beat up those responsible, but who should they beat up? You can't beat up 90 university presidents. But when one university president takes primary responsibility, you can actually beat them up, shake them up and have them take responsibility for what's going on. With FRA there would be an institution of weight as the principal partner, in this case the University of Chicago, and the President of the University of Chicago would be the chairman of the board of FRA. And, at the same time, FRA would bring through URA all the national input of the universities pretty much on an equal basis. This arrangement was important for the lab, because the lab, up to that point, had not had as broad a support of the state of Illinois as it acquired later. The relations with the political entities and the business community were OK, but they were not particularly supportive. Whereas now that the University of Chicago came in, we were more closely tied to Illinois, and we could more easily reach politicians in Illinois with the University's help, as well as the business community, and all of that really strengthened the lab. Besides that, the university then promised to take a certain number of managers into their high-end training programs in business administration and leadership. They also provided scholarships for students of lab employees who were admitted to the University. There were many things that the University could do for us. I think FRA has been a very important element for Fermilab. It also initiated—since we shared the University of Chicago with Argonne National Laboratory, ANL,—a lot of Argonne-Fermilab programs. I think that was very important—beyond changing direction for the lab, it was important to change the contractor. There have been tensions between URA and University of Chicago in managing FRA. They have different styles and so on. But, overall, I think that it's been a great thing for the lab.

ZIERLER: How so specifically, in terms of the kinds of projects that have been done?

ODDONE: Well, I'll give you an example of what happened to us in the budget for 2008. Ray Orbach thought he was doing well with Congress—but it was a very bad budget year, especially for particle physics and the lab. We got a big budget cut a quarter of the way through the fiscal year, NOvA, the neutrino experiment, was canceled, and we got a huge 50-million-dollar cut. I had to put people on furlough and the situation really looked very bleak. I think it was a concatenation of mistakes by the DOE and Congress. I have my theories about it. This is somewhat speculative, but I think Ray got distracted in the discussions with Congress, they forgot about a whole bunch of things, didn't add up everything they were doing, and at the end there was an unusually large cut for Fermilab that folks seemed surprised about. So, I went immediately—

ZIERLER: You mean relative to other labs? Fermilab seemed like it was out of proportion?

ODDONE: Well, the whole field, actually, had a cut, but Fermilab had a very large cut.

ZIERLER: Uh-huh.

ODDONE: That bad budget also led to the early killing of the B factory. And I think what had happened is that Ray had been distracted—there were some cuts proposed that were never added up. Dennis Hastert, the Speaker of the House and the representative from Fermilab's district, was the go-to-person in the Congress for us, and he didn't pay attention. People were distracted. I don't think they intended to have this happen, because at the end it was an embarrassment. But I had to put employees on furlough. We had to restructure the lab to make it through. Well, I went public very early on, and explained publicly what a disaster this was. The DOE was very unhappy with me because it embarrassed the DOE as well as some congressional folks.

ZIERLER: And when you say "public," what was your platform? You gave interviews?

ODDONE: Interviews, newspapers, and so on and so forth. I knew that I was being successful when the chief of staff of Senator Durbin talked to our lobbyist in Washington and told her “You have to get your director under control!”—so, after that cut, I think the politicians were embarrassed in Illinois. They might not have been if we weren't so strongly tied to the state. Senator Durbin, who became a good friend and strong supporter, the first thing he had said was, "Well, we have high-end physics or hospitals. We had to make a decision." Which was bullshit, and he was called on it. We had school kids writing, teachers writing from the local communities, the mayors involved as well as the business community in Chicago putting pressure. Anyway, it was a campaign. DOE doesn't like when labs do that, but I had a problem I had to solve.

ZIERLER: And you saw it as an existential problem, like the future of the lab was in doubt?

ODDONE: I don't think I saw it as an existential problem. I saw it as the dumbest thing that could've happened.

ZIERLER: Yeah. [laugh]

ODDONE: And it was ridiculous.

ZIERLER: Did you mince words? I mean, did you say as much?

ODDONE: Oh, yeah. I was very explicit, like I said. But DOE doesn't like to get in the face of Congress. Anyway, we created sufficient embarrassment for everyone.

ZIERLER: [laugh]

ODDONE: And Durbin ended up helping, and it was a bipartisan effort. We got most of the money back at the end of the year in a round- about way.

ZIERLER: Oh, wow.

ODDONE: It didn't change the DOE particle physics budget but, in the supplemental bill for the war in Iraq, there were $60 million for DOE and we got $30 million to restore the program.

ZIERLER: Uh-huh.

ODDONE: We had done all the work to restore the funding. But DOE pocketed the other $30M, which I thought was too high a fee since the cut had been $50 million. Anyway, we had a celebration meeting at the lab with Senator Durbin, all the local representatives, DOE managers and the staff of the lab. To get their help, I should say, would have been much more difficult if we were not so tied to the state. The state also helped us when we had to shut down CDF and the Tevatron. They put up money for repurposing the CDF Assembly building and it allowed us to build the Illinois Accelerator Research Center or IARC. So just having a place as prominent as the University of Chicago as a parent helped us within the state for many, many reasons. And that didn't really happen before. Leon Lederman, a previous Fermilab director and Nobel Prize winner, was very good, and he got lots of things from the state because he was certainly a fantastic personality. He was able to charm everyone without having those formal ties. But for a mere mortal not having those ties would be very different.

ZIERLER: So, what opportunity did that present you with to have these 30 million dollars restored?

ODDONE: Well, for one thing we got NOvA going again. It had been killed and rose from the dead. And that's the big experiment that Fermilab is now running on neutrinos. I was able to stop the furlough before the $30M arrived because a very prominent member of the Chicago business community made a very significant gift, anonymously, to the University for that purpose.

ZIERLER: Yeah.

ODDONE: Of course, you can't bring money to the lab directly without DOE approval, so it was money given to the University to spend at the lab. It allowed me to get people back to work that had been furloughed. But I think ultimately the outcome was that the political people in Illinois had a much higher awareness of the lab after this. And, in reality, yes, budgets were tight, but we never had problems like that again, making sure that people were paying attention, that no dumb things happened in Washington. As I said, it didn't endear me with the DOE, because they liked to have control over everything, and I was a director a little bit out of control. But, in the end, it worked out very well for the lab.

ZIERLER: Yeah.

ODDONE: Following the crisis, the whole national program had to be redefined. A very talented DOE manager, Dennis Kovar, was put in charge of the DOE Office of High Energy Physics. The Particle Physics Project Prioritization Panel (P5) of HEPAP was created to make recommendations for the program. Ray Orbach now wanted plan B. With the very talented management team we had at Fermilab, starting with my Deputy Director Young-Kee Kim, we worked with P5, the community and DOE to define the program. There's still a lot of work to be done, but the programs at Fermilab are complementary to LHC and the strongest in the world in the lepton sector. Experiments with neutrinos, the Nova and DUNE experiments, and with muons, the g-2 experiment and the mu2e experiment, are underway or being built. At the same time, Fermilab is a major collaborator at the Large Hadron Collider and carries out R&D on future colliders. I think the lab is, in fact, in good shape. If you look at the budget, it's quite good, better than I had. Because, ironically, even though the Trump administration proposes cuts to science budgets every year, the Congress ignores the requests and puts more money into all of science. So, all the labs have never done better.

ZIERLER: Yeah.

ODDONE: There is a really good program at Fermilab, and the lab is doing well, as far as I can tell.

ZIERLER: So, Pier, in assessing your legacy at Fermilab, do you tend to separate out your scientific achievements from the bureaucratic achievements, or is it all sort of one big project that you were working toward?

ODDONE: Well, the science comes first. To have a science program that you can defend politically you must have community support. Then you are able to sell it in Washington. So, it's really—the science and the management issues are part and parcel of the whole enterprise. If you have too weak a program, you're not going to get the support of your particle physics community. If you don't have the support of your community, your job in Washington is much more difficult. So those two things have to be together. For example, when the Tevatron was shut down by the DOE, many labs would have fought politically to keep the facility open. You can exert political pressure, and if you can convince the politicians to keep something open, you may run it for a couple more year or something like that. But you would not run it for long without community support. We proposed to the DOE to run the Tevatron for one more year in 2010 because we didn't know the LHC would start quickly. It had had problems, and if the LHC didn't work well in the beginning, we still had an opportunity to make some advances. But we had already a lot of luminosity, and so adding luminosity was a slow process. And, in the end, we decided not to fight the closure. So, the DOE gave us a time, 2011, for when to shut it down and we did shut it down. It's a case in which the science frankly wasn’t there to continue running it.

ZIERLER: Right.

ODDONE: There would've been a reason to run it if the LHC was delayed, but there wasn't any reason to run it with the LHC coming on at the proposed luminosity. And, in fact, that turned out to be the case. The LHC came out so fast that it very quickly overwhelmed the data that we had.

ZIERLER: So, just to be clear, is that an assertion that will always remain true? In other words, is it possible that there's some advance somewhere else in time that would possibly justify resurrecting the Tevatron, or it's forever obsolete in your mind?

ODDONE: I think it's forever obsolete. I think it might even have been taken apart already. I think—everything that happens at the Tevatron you could make happen at the LHC with more luminosity, so—

ZIERLER: Yeah.

ODDONE: —and more energy, which gives you essentially a finer, smaller scale at which to probe nature. Some facilities are turned off prematurely. For example, the B factory shutdown was different because when the B factory was shut down due to the budget crisis of 2008, they demonstrated—in the last run after the decision had been made to shut it down, that they could run the facility at resonances other than the Upsilon(4S) where most of the running had been. In the Tevatron, you go to the highest energy and that's where you run. At the B factory you had a choice, you could run at different resonances. In the last run they did a lot of work outside of the Upsilon(4S) which turned out to be really interesting. Probably it could have gone on for a few more years doing productive physics, but that got chopped off at that time. The Tevatron shutdown, it was done and over the moment we pulled the plug. Of course, pieces of it, like the injector, are still used for the neutrino program. Accelerators that before were used to make particles for the Tevatron storage ring, now are used for the neutrino program. So not everything is shut down. A lot of pieces were reconverted to the ongoing program.

ZIERLER: Mm-hmm. So, Pier, I think for my last question, the five years after your retirement as director of Fermilab, where you were intensively involved in all of these advisory positions—you bring so much experience in terms of at a very broad level between Berkeley and Fermi and understanding of, for large-scale projects, what works and what doesn't, both politically and scientifically, right? So, there's a lot of experience that you have there. So I wonder, between your work with LIGO and the National Science Foundation and FNLAC and all of the rest, if there are sort of general ideas and motivations that inform all of your advisory work, in terms of the kinds of projects that should be supported, how they should be presented, and to whom?

ODDONE: Right. Well, they put these old horses in these committees to get wisdom, and I'm relieved that I don't have to deliver the projects like I used to.

ZIERLER: Right, right.

ODDONE: It can be a much more relaxed attitude. And with the people who want this advice and want my help, it is always the importance of the science first—science and being able to open these new windows.

ZIERLER: Right.

ODDONE: So, if I am advising a committee, I go in that direction of what is important to do. And then you worry about, OK, what other steps you have to take to sell it to the community or to the politicians.

ZIERLER: Yeah.

ODDONE: We have a fairly-evolved system now, at least in the Office of Science of the Department of Energy, where they have these advisory committees, they run workshops, they put priorities together for a whole field, and within that framework then come discussions and decisions of what gets funded. It is a fairly well-established system. Other agencies don't have exactly that same approach.

But I think, in general, there is one thing that I always puzzle about when I look at how science gets done in other countries, like Germany. In Germany and many places, you have a more coherent approach to science. Budgets get prepared years in advance, and you have a certain stability of funding that allows you to tackle projects with some confidence that you are going to finish them. By comparison, what we have in the States appears to be more chaotic. You can get funding from the NSF, you can get funding from the DOE, the Congress looks at the budget every year and messes with funding. They know nothing about science, but they feel they can add or subtract, whichever they want.

ZIERLER: [laugh]

ODDONE: The OMB, same thing.

ZIERLER: Right, right.

ODDONE: Whereas, in other places, this is controlled by people who are following the science in a more continuous way. And, as a director, of course, you're totally frustrated when you get cuts in the middle of the year. Chaotic this, chaotic that, different places to go get money and so on and so forth.

But there are some advantages to our system, because when there is chaos, there is more flexibility in what actually the system would allow you to tackle, and changes in direction are easier. That does not apply to the huge projects; for SSCs and LHCs you need long-term commitment and stability. Frankly, it makes it very risky to build big, long-term, multi-year projects in the States. But, on the other hand, we have this rich environment of different funding agencies that allows us to move in directions that perhaps are not the favorite direction right now but that might lead to something really exciting down the line. It's like, when I started in Berkeley and could get money to rebuild the streamer chamber from a source different from the expected one. If you have one single ministry of science and you go ask for something a little bit unusual, you probably don't get it. You have fewer degrees of freedom to actually try things.

ZIERLER: Yeah.

ODDONE: So, there are some aspects of the system that we have that give us that ability to go to different places to get funded, and different styles of how things get done.

ZIERLER: Yeah.

ODDONE: LIGO is a fantastic success, but headed and built in the Department of Energy, with its rigorous management approach, it might've been killed in phase one, because it didn't meet the criteria or the milestones or the key parameters that it was meant to have. A little bit of slop sometimes helps—

ZIERLER: Do you think DOE appreciates that? Would they ever admit that kind of a perspective?

ODDONE: Well, no. They would point out, like everybody points out something valuable, that LIGO would not have been built at all if somebody that had not come in from particle physics, with the discipline that DOE imposes on particle physics projects, like Barry Barish, to build it. They would say the reason that LIGO exists is because we trained a project manager, like Barry Barish. So, DOE would find ways to be proud of LIGO. But I wonder, after the first phase not working as well as it was promised, whether it would have had a much harder time than it actually did in terms of getting funding. The NSF was patient; it took 20 years to discover gravitational waves. Conservatism also affects the NIH—some people in the NIH community sometimes say you have to have done the research before you get the funding.

ZIERLER: Right.

ODDONE: typically said of R01 grants

ZIERLER: Right.

ODDONE: And that's not really true, but it points to the fact that sometimes institutions or funding agencies get too conservative and want foolproof outcomes rather than a little bit sloppier system that allows for a lot of innovation.

ZIERLER: So, 2018, was that your—did you hang up your hat for the advisory work or is there more advising in your future?

ODDONE: No. I am still on the advisory board for the Lawrence Berkeley Lab—

ZIERLER: Uh-huh.

ODDONE: —and for the National Ignition Facility at LLNL. I'm also the chair of the Berkeley Laboratory Foundation. It's a small foundation that tries to raise philanthropic funding to enhance the impact that the lab and DOE can have on society but cannot directly fund, because it falls at the interfaces of different disciplines. But I decided last year to reduce some of the advisory committees I am on, including for LIGO. I am busy with the vineyard and various projects at home and grandchildren, and I make wine—

ZIERLER: Life is good.

ODDONE: for family and friends, and so that keeps me entertained. And sometimes having to fly to Washington for a day or two, as I get older, is not as attractive anymore.

ZIERLER: Sure. Totally understandable. Well, Pier, it's been an absolute delight speaking with you today. I really appreciate your time and your perspective. It's going to be a great addition to our collection, so I really appreciate it.

ODDONE: Thank you, David.

[End]