BAC
Alexander Chao — by David Zierler
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DAVID ZIERLER: OK, this is David Zierler, Oral Historian for the American Institute of Physics. It is October 8, 2020. I'm so happy to be here with Dr. Alexander Wu Chao. Alex, thank you so much for joining me today.

DR. ALEXANDER CHAO: Thank you very much for the invitation. It's a pleasure. Thank you.

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

CHAO: I'm presently retired. Before I retired, I was Professor of Physics at Stanford University, SLAC National Accelerator Laboratory.

ZIERLER: So you were on the faculty at SLAC?

CHAO: I'm on the faculty of Stanford University at SLAC.

ZIERLER: And how much teaching did you do at SLAC, Alex?

CHAO: I am affiliated to SLAC, not Stanford campus. Teaching is not obligatory but voluntary. So for the few years before retirement, for example, I taught a quarter every other year.

ZIERLER: And what kind of classes did you teach?

CHAO: I'm an accelerator physicist. So for example, a few topics that I taught more would be introductory accelerator physics for fresh physicists who want to become accelerator physicists. Or one course that I like is called special topics. That's for more advanced students. It's specialized into more research-oriented topics. Another topic which I specialized in the old days is called collective effects. In an accelerator, you want the beam to be as intense as possible. So you push the intensity up until it hits instabilities. By definition, you always hit instability because you're pushing the intensity up until you hit it. So you have to understand those limitations. And collective effect is the third topic I usually teach.

ZIERLER: Alex, let's go back all the way to the beginning. I want to hear about your upbringing and your childhood. Let's start with your parents. Tell me about them. Where are they from?

CHAO: Yeah, you warned me about this question. I was born in Taiwan in 1949.

ZIERLER: That's a very interesting time to be born in Taiwan.

CHAO: Exactly. 1949 was right after the civil war – well, it was still during the civil war. Chiang Kai-Shek went to Taiwan. So did my parents. They went to Taiwan as newlyweds, and I was born in an East Coast town called Hualien. As I said, it was right after a civil war. I think we caught a time when the war sort of slowed down. So by the time when I was born, life was peaceful for a change. But living was difficult. Both my parents worked. They worked at the Bureau of Telecommunications in Taiwan.

To make ends meet, they both had to work full time. I have an older sister and a younger brother. We were all born in Taiwan. So we grew up in that kind of environment. Difficult life in general.

At the time, it was such that we three kids all had to manage somehow basically without parents around much. This is especially difficult for me because I was a very slow kid. But as it turned out, the harsh time did not really affect me much. I was falling so much behind that I didn't even sense it was a difficult time. But I was growing up in that environment.

I didn't go to preschool or nursery school. I didn't go to any schools until one day, my parents sent me to an elementary school at the age of 5. I was one year early. Suddenly, the next day, I had to go to an elementary school. That was my parents' decision. There was nothing to do or learn at home anyway.

So I went to school not knowing what a school meant. This elementary school at the time was a primitive environment. To go to that school, I had to walk, I would say, 10 or 15 minutes to a train station. I was very small, 5 years old and physically very small. I then had to take a train ride for maybe half an hour, get to a place, get off, climb up a small hill, then arrive at the school. The whole trip was maybe an hour. On the first day to school --- I was just 5 years old, I managed to get on the train but then did not know when to get off. So I somehow arrived at the end station of the train. So apparently, I didn't know what to do. I got off the train, sat on a bench in the train station, and sat there. Until a friend of my parents -- it was so lucky -- happened to walk by and said, "Hey, isn't this so-and-so's kid?" And found out I was lost on my way to school. I didn't know where I was, and according to him, my rescuer, later --- I don't remember the details myself --- I was leisurely playing with myself on the bench, not worried, just sitting there for hours until he rescued me.

Then I sort of began my school days. I guess that story tells a little bit of my early life. I didn't know what was going on. I had no idea what a school was. I didn't know what it meant to go to school. But I went to school dutifully until maybe when I was in the 3rd grade, when I was 7. My family moved to Taipei. My parents' work changed to the Taipei office, and we all moved there. So then I went to this new environment --- am I answering too long?

ZIERLER: No, no. Was that a big change for you, Alex, to move to Taipei?

CHAO: Yeah, yeah. So we went to Taipei. I have stories to tell.

ZIERLER: Please.

CHAO: So we went to Taipei. I went to a new school. To me, it's just a school, right? So I went to school. A new school, a much bigger school in Taipei. And I went to school not knowing what's going on, until one day, the teacher asked a portion of the students to get in line. She said, "These people, line up." I found out that included me, so I lined up. Then she led the team, after maybe 15, 20 minutes' walk, to a different school. And I realized I was told to start a new school. And later I realized it was a big deal, and all students and their parents had to be well-informed. All the students were supposed to know that, and they were supposed to tell their parents, and parents were supposed to agree, and so on. I did none of this.

So I started to go to the new school. Still didn't tell my parents, not realizing they should be informed of it. So every day, I walked to the new school, as usual not knowing what was going on. Until one day, my parents sensed something's not right. They then found out from me–"You have changed school?" So my parents came to the school to check out. This was already several months later.

What they found was that the school I was assigned to was smaller and much more primitive. Most students came from local farmers and didn't have shoes to wear – although I did. So anyway, I was put in that school. And that school had two classes in my grade. One class had about 40 students, and that's the class for students who planned to continue the education to the middle school and high school etc. and to proceed academically. There's a second huge class, which had about 90 students. Those students were going to terminate their schooling after elementary school. And I was among them. I was put there in the second class with 90 students, not going to continue education after elementary school. Apparently, the teachers must have asked me and found out from me that I was not going to go to the middle school and so on. And I had no idea that destination was where I would be heading.

I was physically very small. I was the smallest and the shortest among the 90 students. But they put me at the very, very end, the far end corner of the classroom. I don't know why. Maybe they asked me and that was my choice.

So my parents found that I was in that class, and I was put to sit at the far corner of the completely packed classroom of bigger kids. They then talked to the principal, and managed to change me to the other class with 40 students so that I could have a chance to continue my education. So that was the situation. And, of course, academically, I was at the bottom of the class until graduation. Should I continue?

ZIERLER: Please do.

CHAO: So I graduated. There was an entrance exam to enter the middle school at the end of sixth grade. I was 11. But anyway, there was an entrance exam. Uh, I was hopeless, right? I mean, I was the bottom student from a bottom school. But then for some reason, I did well enough in the exam to be accepted to the very bottom middle school --- another bottom school. I somehow managed to score enough to enter that school. There are some stories there, too, but anyway, so I managed to hold on to almost the last seat of this entrance exam and I became a middle school student. Then, as you guessed, I was about at the bottom of the class again.

Now, we had a very strict teacher. He gave out the final grades of the students on the last day of each semester. He would announce the students to come up to the platform to receive the report cards one by one, starting with the number 1 student down in the order of the grades while all students watched. The teacher would hand-deliver the cards to the best students. By the time he reached the last few students, he threw the report cards on the floor for them to pick up themselves. So I was among the ones who picked my card from the floor each semester. Funny thing is that I didn't feel bad about it. I didn't even feel what was wrong with it.

Then came the second semester of the second year, when – I cannot reconstruct it, but somehow at some instant --- I suddenly realized, "You're supposed to study!" That was such a new revelation. "Really? You're supposed to study. You go home, you're supposed to study." Of course, teachers told me to study before. Never entered my mind. Until suddenly I realized, "Oh, you're supposed to actually study." OK. So that semester, I started to study. Still didn't know what's going on. I just studied.

Apparently my score went straight up because I studied after school. And I didn't realize that my studies had made any difference until the last day of school. The teacher announced, "Number one, Alex Chao." I was number one of the class that day. I was astonished. I said to myself, "Hm? Some mistake happened." Anyway, so I hesitatingly went along, just totally puzzled. I went home, my parents were not as puzzled as I was, "Something is wrong. There has been a bookkeeping problem. There's something wrong and must be corrected." So my mother went to the school to correct the record. "Something's wrong." What she found was it was true. I was the number one of the class. Not only that, I was actually number one of the 23 classes of the whole grade. I was the number one of the school.

ZIERLER: Not so slow after all, Alex.

CHAO: No, no, that's still slow. I still didn't know what happened. So I realized you had to study. And apparently I must have studied harder than everybody else. So that happened.

So my story is that I was a slow kid. My growing up did not come so much from the school I attended. In fact I don't learn from teachers in classrooms, for my whole life, I guess. I have to manage, somehow, myself. Now that I have the revelation of learning by reading, I just have to do it that way. I have to find a way to study things in my own slow way. Slow, but I guess steady. Anyway, so that's how the first phase of my life went.

Then there's another entrance exam for high school. I did OK there. I went into a good high school. But the situation remained. I can't listen in the class. I can't learn from teachers. I don't hear them. So I began cutting classes and developed into a situation that I don't go to the school. I just do my own things and study things on my own. The topics I liked, I don't know why, I liked geography, I liked history, I liked algebra.

ZIERLER: What about physics?

CHAO: Physics is later. Physics was taught in the last year, the 12th grade, of high school. So I didn't have time to develop a love for it at that time. I just was good at it. But I didn't develop a love. In the 11th grade, I went into algebra. I loved algebra that year. And also geography. I liked geography. Geography was not something important for students because the entrance exam to colleges didn't include geography. So if you study geography, you're wasting your time. But I liked it.

And algebra. So I began to study algebra my own way. I didn't study textbooks. I learned from bits and pieces here and there. And then I had a notebook, which I'd collect problems which I made for myself. I invented problems to work on. That notebook, not realizing how precious it meant for me, I have since lost it. I have a collection of, from hindsight, very interesting algebra problems, but I lost it.

And physics, I liked it and scored well, but I was not ready at the time. The love of physics came actually much later. I was good at it in the sense of I can solve homework or exam problems, and score well in the college entrance exam. But in terms of love, not as much as algebra at that time. Algebra is a much smaller subject, about right for me to manage by myself. Well, gee, if you have more time–we may not have time later on, but I can talk about how physics is so lovely later on. But that, the beauty of physics and the love of it, chronologically, occurred later.

ZIERLER: Now, when you got to college, is that when you started to think about physics?

CHAO: Yeah, kind of coming to that. So then I went to college. Again, remember, I'm now a good student. I was not the best, top student though. Exams were not what I was good at. In fact, maybe I back up one step on that point. As I mentioned, I spent my time on geography, which was considered a waste of time because it was not a subject of the college entrance exam. On the other hand, there was one subject called politics that was in the entrance exam. That's what you're supposed to study instead of geography. But I refused to study it. I didn't read a single word of it. Then I got a deserving bad score on the subject in the exam. But still, my total score was good enough that I was accepted to a university called Tsinghua University in Taiwan.

So I went to that university. It's a pretty good university. At the time, it was actually as good as any other university in Taiwan. Anyway, I guess I still had the problem of listening or going to classes, so I cut classes. And I didn't read textbooks. I read my own selection of books and so on. I learned in my own way. First two years, I didn't do very well because it was not all physics and math. It was miscellaneous. And I didn't like most of them. So I didn't do very well. But then in the 3rd and 4th years, it was all math and physics. That's when I learned to like physics. I would still mention something later, which made another jump from liking and loving to beginning to appreciate it. I really think physics is so beautiful. It really can catch your breath. The love of physics goes much deeper and involves a continuing appreciation that grows with time. I am very very lucky that there is a field called physics and I could have the opportunity to be a physicist for living and for life. It’s like a miracle.

ZIERLER: And, Alex, are you thinking about physics generally? Or are you thinking specifically about particle physics and accelerator physics, that was so capturing to you?

CHAO: I think I meant to imply the whole of physics. But I have to admit, my appreciation, if at all, is biased because I have had a certain path in my learning. I learned through my path. So it's influenced, so I can't really say which area of physics I was referring to. I'm a theorist, so maybe more of theoretical physics and theoretical accelerator physics is what I'm referring to.

ZIERLER: What kind of physics curriculum did you have as an undergraduate? Did you have theory, experimentation? Was it a broad curriculum that you took?

CHAO: Undergrad curriculum, yes, was broad. I mentioned the first two years were all kinds of courses, including politics, the one that I hated. There was a course on mechanical drawing that you had to take, it's a required course. I'm not good at drawing, so I flunked that course. But fortunately, it's OK. I past it taking a makeup exam. There's another course, chemistry. I don't like chemistry. There was also a class called chemistry experiment. I flunked that, also. You see, that's a required course that you have to pass. So I did all those experiments --- you know, tubes of liquids of various colors. Anyway, chemistry experiment, I flunked. I didn't know I flunked it because I didn't check my semester report cards. Until the very end, the 4th year, before graduation. A university administrator came to me and said, "Oh, Alex, you cannot graduate." I said, "Huh? How come?" He said, "You flunked your chemistry experiment in the first year." And he said, "You're coming to fourth year now. Last chance for you to make it up. And if you don't make it up, you can't graduate." So in the fourth year, I took chemistry experiment another time, pouring colored liquids from tube to tube. And this time, the teacher gave me 60 points. So I passed, and graduated.

All physics courses, I liked. I did very well at physics courses. I didn't go to classes. What happened was, before the final exam, I found out when the final exam would be. Before final exam, I would borrow the notebooks from my friends, my buddies. The way it’s done, they would very kindly lend their notebooks to me for one night and one night only because they had to study too. So each course, I studied one night from the borrowed notebooks. And for some reason, I could understand what the teacher wanted to say from reading the notebooks. Then I almost always scored very well. So my later years–this is a little boring.

ZIERLER: Not at all, no.

CHAO: So at the end, I graduated. In fact, in the second two years, my grades were so good, so much better than the first two years, my average was actually among the top of the class. So I did well overall. Graduation was 1970. I graduated from Tsinghua University. After graduation, you have to serve in the military for one year. So I then followed one year of military service.

ZIERLER: What did you do in the military, Alex?

CHAO: Because of the physics major--they actually pay attention to what your college major is -- I was sent to the artillery company. Artillery. The shells, bombs, guns. Big guns. So I served in the artillery. Very fortunately for me, I was assigned to Kinmen. You know Kinmen?

ZIERLER: Yes.

CHAO: Kinmen's a small island facing right next to the Chinese mainland. In those years, it was still in a civil war. So I stayed on the island of Kinmen, at the war front, for my military service. After knowing that, my mother cried, "No, that's so dangerous, too risky." I said, "No, no. No problem." And I was right.

So I went to Kinmen. Indeed, it's a little bit dangerous because we still, at the time, shelled each other. Not with real bombs, but bombs with propaganda pamphlets inside. It still can kill people though. It still explodes. So I went to Kinmen. It turns out that's my best of luck. I went to Kinmen, stayed there, and that was the most leisurely and carefree period of time in my entire life. It's such a blessing. You do have to be careful with the shelling and all that. We stayed in a bunker on a hilltop. And you walk all the way down the hill to an open-field restroom or to take a shower. There's no baths. You take cold water out of the ground from a deep well using a bucket attached to a long rope and shower that way. That required quite some skill to do.

Anyway, that was the life in Kinmen. The job was to report all the enemy's ships going by, because there were all kinds of ships going by. And you have to report at what time there's a ship and so on. You have to report all that. That's my responsibility. And there were a total of seven people assigned to the bunker. But there were enough people to cover the job, so I didn't really have to do the job so dutifully all the time. Nobody's around to supervise me and we always help each other out covering the job. And it was a remote bunker. So every day, I'd bring a few books and walk down the hill, and go to the woods on a beach. The woods was man-made for war. And what happens is, they plant mines in case the enemy invades. So they mined the beach. I would walk down to the beach. Not to the beach itself -- I'd be killed --- but the edge of it. So I would go there, bringing two books. The breezy wind combined with ocean waves, it's beautiful. Beautiful. Nobody else around, only me there within sight for long periods of time.

That was a beautiful time. During that time, as I said, I'd sneak out from my assigned duty. And one day, an army general came for inspection, unannounced. He could not find officer Chao. At a war front, that could be a serious problem. If he took it literally and seriously, I could be– anything could happen. Anyway, that was not good. The rule says you could be punished by trial, but fortunately it didn't happen. But what could happen was that you receive a bad mark on your permanent record, and one potential consequence of that is that you would not be eligible to go abroad to study. That would be the end to my would-be career as a physicist.

Funny thing is that I didn’t feel the seriousness of the matter at the time. Fortunately, my company commander really was a nice person and kind to me. He realized the potential problem and managed to give me an honor recognition on my record. OK, so I had a bad record, right? But he gave me a good record to cancel it. The recognition declared me to have made significant contributions. So he gave me an honor recognition to cancel that bad mark. That actually allowed me the possibility to continue on. I'm forever grateful to the company commander. I still remember him, his name is Zhu Wei, but I have lost contact with him.

ZIERLER: Alex, when did you start to think that you would come to the United States to pursue graduate work in physics?

CHAO: To go abroad to study further was an assumed thing for a physics student, me included. Those days in Taiwan, if you want to do physics or if that's what you decided your career would be, like I was, then you don't stay in Taiwan. You go study abroad to learn more, and then go from there. That was the standard assumed path for a physics student, and I took it. I didn't really think that much about it. The only thing I thought about was I decided to do physics for career. The only brief moment of second thought was from mother’s tears at the airport when I boarded the airplane for the US.

So I went to Stony Brook. It was 1971. There's a reason behind choosing Stony Brook. There's a famous Chinese physicist named Chen-Ning Yang. In Taiwan, he's very famous. He's a Nobel Prize winner. So it went sort of like: "Go to Stony Brook. Chen-Ning Yang is there, so it must be a good place.” So I went there, not thinking I could one day actually become his student.

So I went to Stony Brook. By this time, I began to know more what I wanted. I wanted to study physics, I knew I was kind of good at it. So I stayed on. Up to the first year or so, I didn't think more. But after the first year, you're supposed to arrange a thesis advisor. It happened that I did very well in those exams --- it's called the qualifying exam and I studied again in my own way preparing for it. Apparently the professors talked about a student performing so well in that exam. So when I went to see Yang for the first time, he knew about me. And he accepted me right away. Professor Yang didn't take too many students. Over his career, he had only about a dozen students. Anyway, he agreed and I became his student.

Yang played a critical role in my career. He now lives in China. He's 98 this coming year. Anyway, so he accepted me, and we started to work together. He's very generous to students. Altogether, we wrote three papers. He would suggest a problem for me to work on, and I then talked to him as I made progress. He is different from other professors on many accounts. First, he wanted me to work on a wide range of problems. He would assign me to other professors to work on problems under their supervision. In the process, he assigned me to three other different professors. So under his supervision, I was simultaneously working on several problems. And one of the professors was Courant. His first name was Ernest. He just passed away this year.

ZIERLER: Yes.

CHAO: Oh, you know about him?

ZIERLER: Yes.

CHAO: Yeah. He influenced me as well. So Professor Yang, in addition to assigning me the work he wanted me to work on, he assigned me to Courant to work on accelerator physics. So I took his course called accelerator physics and enjoyed it. At the time I thought that was it, not realizing that Professor Yang has been directing me in a certain direction.

ZIERLER: And, Alex, of course, at this time between SLAC and Brookhaven, many, many exciting things were happening in accelerator physics.

CHAO: Yes, yes, indeed. Brookhaven, at that time, was the frontier laboratory of the proton type accelerators. SLAC was the electron type accelerators. So they're two different types. They're very different types of accelerators and different types of laboratories. But each was at its frontier. Anyway, at that time, I was at Stony Brook.

ZIERLER: Alex, did you interact with Sam Ting at all during those years?

CHAO: We only casually met a couple of times. There was a project proposed called Isabelle at Brookhaven at the time. I think we might have casual conversations over the Isabelle project. But we didn't work together. Only later, we became more acquainted. But at the time, he was experimentalist and I was on the accelerator side.

I studied high energy elementary particle physics with Professor Yang. But I was advised to learn more. So I did a little bit of high energy phenomenology with professor Chris Quigg, nuclear physics with professor Fred Goldhaber, and then, as I said, accelerator physics with professor Ernest Courant at Brookhaven. In fact, on the third year of my thesis, Professor Yang assigned me to Ernest Courant to work, and got paid, at Brookhaven. So I was half time at Stony Brook, and for that year, half time at Brookhaven working on accelerator physics although that's not my thesis.

Then came up finishing the thesis. The thesis was on high energy physics. It was a combination of my published papers on elementary particle and nuclear physics. It was a combination of those papers, except the accelerator physics paper. So that was my thesis. And I graduated.

Right before that, an important thing happened to my career. Professor Yang decided to talk to me again. You see, he's very careful since day one–and me being a slow person, I didn't realize he had a plan for me. He matched the way I work with his vision of what's going to develop in the field of physics. He came to a conclusion of a best direction for me to pursue. That was why he sent me to take a course in Courant's class. That was why he sent me to work with Courant in Brookhaven. And then it's also why he made the special effort talking to me in his office before my graduation.

Right before I finished my thesis, he summoned me to his office. He gave me the advice to pursue accelerator physics as my career. The advice was, “Join a field that is expanding. Don’t join a field that is shrinking.” At the beginning, I hesitated mostly because I didn’t have his vision. So I hesitated, not willing to give up on what I learned in the other fields. He then asked me to think about it.

But I continued to hesitate. You see, accelerator physics at the time was not a recognized academic field. Right now, you can talk about accelerator physics. Accelerator physics is a phrase. "What do you study?" "Accelerator physics." "OK." But at that time, if somebody asked, "What do you study?" and if I replied, "Accelerator physics." He would say, "Huh?" Accelerator physics was not a phrase. And my professor advised me to spend the rest of my career in a field which is not even recognized?

ZIERLER: So this was a risk. You saw this as a risk to some degree.

CHAO: Yes. I didn't say it very well. Yeah. It's a risk. It's a big risk. I liked it, accelerator physics. I loved it, in fact. I wrote several non-journal technical notes on accelerator physics at that time at Brookhaven. Accelerator physics was so much fun. And I treated it as fun.

There was no such field. No professional journals. You only publish laboratory notes. There's just no recognition. But Professor Yang has a point. He says, "That's a field that's developing. You don't want to join a field which is shrinking. You want to join a field which is expanding, and you are just at the right time to be on the expanding side of this field of accelerator physics. Go there. You will do well."

So I sort of hesitated for I would recollect maybe two weeks or so. For two weeks, I didn't go to talk to him.

Let me add something about Professor Yang. Professor Yang is very generous to students. For a long string of days, I'd go to his office, spend his entire morning working on things which a student was supposed to work on, not appreciating the fact that he was an extremely busy man. But he worked with me. Once there was a calculation to be done. A beautiful calculation actually, but was a bit tedious. In all cases I know today, this is a situation when the student is supposed to work out. So I went away and calculated it successfully. He's not good at computer programming. I was. I wrote a computer code and calculated, and then from the calculation, I came up with a neat, very neat, analytic formula from the numerical results. I was very proud of it. I present to him that. And to my surprise, he did the calculation himself. He took out pages of notes. And then we compared results.

There were a few terms slightly different in the long final formula. Something was wrong with either his or my results. All implications say the student's supposed to go back and check, right? But I didn’t. You see, my results were distilled from a computer. It cannot be wrong, okay? Cannot be wrong. I checked every numerical number when I came up with the formula. So it could not be wrong. A few days later, he came to me. He said he went through calculation again and he found his mistake. I tell you, a professor, a Nobel Prize-winning professor, is usually not like this. But he is. And that's the professor I had.

So for two weeks, I didn't go to see him, which was unusual for us. But one day, unfortunately, I bumped into him in the hallway. He said, "Come. I'll talk to you."

So I went to his office again. Immediately, he asked, "What's your decision?" I began to mumble. For the first time, he raised his voice before I even said much. But he didn't have much more new to say. He just repeated his argument and advice in our discussions. "Go think again." Actually, the way I was going to explain to him was, "I like the field. No problem. I'll do it. But I'm not sure the field is mature enough to enter." But that I didn't tell him or had a chance to tell him. So I made the final decision at that time to follow his advice.

So my job applications were to accelerator physics. The place I went to was SLAC. But this changes the topic. Are we done with college? With graduate school?

ZIERLER: Well, did you specifically want to go to SLAC? Was SLAC most exciting for you?

CHAO: First of all, like I said, it’s a frontier in electron accelerator physics, the real frontier. No other labs came close at the time.

ZIERLER: Not even Brookhaven?

CHAO: Brookhaven was not an electron lab. It was a proton lab. And SLAC was electron lab. I could stay at Brookhaven, but I decided to change to something new. Also, being very simplistic, the California sunshine was nice attraction. So I decided to come to California.

That was how I decided to come to SLAC. The person who hired me to SLAC was named Burton Richter, another Noble Prize guy. He discovered the psi particle, same thing as the J particle discovered by Sam Ting at Brookhaven. It's the same particle. I didn't have anything to do with either particle, but I was at Brookhaven when they discovered the J particle, and came to SLAC, and they discovered the psi particle. I overlapped both particles, the most exciting time, and contributed nothing to their discoveries.

So I went to SLAC. Richter was my boss. He was the head of a high energy physics group at SLAC. He hired me as a high energy experimentalist. Those days, there were no job openings for accelerator physicists. So he hired me as a high energy experimentalist but assigned me to work on accelerator theory.

ZIERLER: And what month was this, Alex? Given that this was 1974, it's important to know exactly when you got there.

CHAO: My job at SLAC started September 1, 1974.

ZIERLER: Wow. That's a very exciting time to start at SLAC.

CHAO: Absolutely, 1974 was the year of discovery of the J or the psi particle. There was one morning I came to work, it was a day in November, but I don't remember the exact date. I went to work early in the morning, and then there was Vera Lüth, a high energy experimentalist, tired after working overnight apparently, came up to me. "Alex, Alex." I said, "Huh?" She said, "They found a resonance. They found a resonance." It was several seconds until I realized what she meant. You see, resonance in accelerator physics has a meaning. If you hit a resonance, it means the beam becomes unstable. The accelerator beam is lost. So at first I thought, “OK, they found a resonance, so we'll be careful next time." But she was so excited and she was so happy. I realized that was not what she meant. Then she explained, "Resonance. Particle." So I finally realized that this was a very exciting earth shaking time, occurring once in a lifetime. But anyway, we are talking about my beginning day at SLAC. September 1 was my starting day.

Then I worked, since that day in Burton Richter’s high energy experimental group. Since that time at SLAC, I have worked on a series of SLAC accelerator projects. I grew from a fresh new, young physicist to a full-grown staff member. In total, it took about ten years. It was ten years of the most wonderful research time. Ten years. Again, without realizing how precious an opportunity it had been, thanks to the visionary bosses, Panofsky and Richter. Only much later in my life did I begin to realize that was so precious. Not every boss is like them. And I had the luck that my first ten years, when I really grew professionally, was given such a precious opportunity. I had all the freedom and time to work on any accelerator physics problems that I wanted to.

ZIERLER: And so, Alex, what were those? Given the freedom that you had, what were the problems that were most interesting to you? What did you work on during those ten years?

CHAO: Miscellaneous. My job was accelerator theorist. So I was an accelerator theorist in a high energy experimental group. As Professor Yang told me, this was a wide open field. Basically, almost in every topic, there were interesting problems to work on, and you could make contributions. So basically, I worked on several problems that came to my attention without limitation. Richter allowed me all the freedom. At the beginning, I worked on an interesting problem of a charge going through a perfectly conducting plate from one side to the other side. Amazingly, I could solve it exactly and found an analytic solution. See, I was a student just coming out of school. I was not aware of the urgent problems facing the design or operation of a real accelerator. So the first problem I solved started out as an academic problem. It was very elegantly solved. And then as I slowly engaged as a member of the group, I also contributed to the real projects in design and operation.

So I grew in various directions on several topics. Beam polarization was one of them. I did quite a bit of work on the dynamics of a beam's spin and its polarization. I think that's probably the second main problem I worked on after I came to SLAC. At the time, I was very productive. I was young and fearless, I guess. Another hard topic was beam-beam interaction --- a severe limitation on the intensity of the beams when they are made to collide in a collider storage ring. When the two beams collide, they are focused into very small sizes. Such tightly focused beams carry very strong fields. So the two beams influence each other very strongly. We call that the beam-beam interaction. So beam-beam interaction was another topic that I spent my efforts on. Still another topic is what I mentioned earlier, I wrote a book on it. Did I tell you I wrote a book?

ZIERLER: Papers, but no book yet.

CHAO: I mentioned collective effects earlier. Oh, yes, you asked me what courses I taught. Collective effects is the topic. So this was another topic that I worked on, one big topic. So I spent a lot of time on it. Later, that material was collected into a book. So I wrote a book on collective effects.

There are many other problems. Nonlinear dynamics, chaos in accelerator physics, chaotic motion. That's a big deal as well in accelerator physics. Question is what does nonlinear dynamics and chaos do to the particle motion? I did some of those nonlinear dynamics. So in those years, I was working on several problems at one time.

But then I was very young. In those days, the community had a culture, a bad culture by today's standard. Today, it's all different, but those days, the culture was that young people don't go to conferences. Conferences are attended by established people. You have to be established in order to go to conferences.

ZIERLER: Is that to say, Alex, that young people want to go to conferences, but they're not welcome?

CHAO: No, you have to get funding to go.

ZIERLER: Yeah.

CHAO: And the lab has a limited number of slots. So young people didn't get to go as a result. And I was not only young, I was the youngest. The nearest age to me in the group would be perhaps 12 years older. So I was not going. And yet, I was working on so many problems. So in one of the big conferences that year, the attendants returned and mentioned, "Alex, it's embarrassing. There were four talks in this session. Three of them mentioned your works as the latest advances. And yet, you're not allowed to go." So that was the situation although it was so natural that there were no hard feelings. Fortunately that situation changed later on. Nowadays, it's the opposite. Now, when there's a slot, give it to the young people. The older established people should not go. Give the opportunity to the young people. That is absolutely the right thing to do, but the result is that I, again, don't get to go. So I didn't get to go 45 years ago, and I don't get to go now. (laugh)

How are we doing in time? Should we change topic?

ZIERLER: No, no, no, it's good. Alex, can you talk about your time as group leader of the beam dynamics group?

CHAO: Oh yeah, so you know about this. So I was the youngest member of the group. But it seemed people were willing to give me benefit of the doubt. Richter and Matt Allen decided to appoint me as the leader of the Beam Dynamics group. This group at SLAC was at the time arguably the best accelerator physics group in the world with all its distinguished members and expertise. But there was a lab reorganization by Richter, they would like to have a new group leader for that beam dynamics group, and they wanted me to do it.

I accepted the job. It was a great honor to lead such a distinguished group, and I was young and fearless. I did a reasonable job as the group leader, I think. I took the job and even enjoyed it. And so I became the group leader. In the meantime, SLAC had new projects going on. And I actually not only took the beam dynamics group leader, but soon I was also assigned the head of the task force for SLAC's new project called Stanford Linear Collider. Both these assignments were rather critical. So I was very important those days.

So during the latter part of the 10 years' time at SLAC, without knowing it, I had been taking on a path which, in retrospect, might not have lasted for me. --- Let me explain. The path for me, I realized many years later from the SSC experience, is just do research. But I didn't know much better then. So I went that way. And, even apparently doing well on the job, I terminated my precious ten years of research, ten years of freedom, at SLAC.

Then, 1983, there was a project in the United States called Superconducting Supercollider. It's not a construction project yet. At the time, it was a design project. They formed the Central Design Group in Berkeley. And Maury Tigner, very good friend later, asked me to join the Central Design Group.

ZIERLER: And what was Maury's role in the design group? What was he doing?

CHAO: He was the director. He was the Director of Central Design Group. And he wanted a few people to head a few divisions for him. So he asked me to head the Accelerator Physics Division. I agreed. First of all, Maury is a great person. I liked the idea of working with him. And I could not decline the offer. At the time, SSC was such a prominent, leading project, and not only in the US. It was the project to be in, not to mention to take responsibility for its accelerator physics design. And so I took the job. That was quite a challenging task.

ZIERLER: Because this is new technology. These are going to be energies never before done.

CHAO: Right. Because of the higher energy, because of the large size, and because of the high cost, you cannot just use your familiar technology and go and build it, reproduce it, just make it bigger. That does not work. You have to think of clever ways to prevent skyrocketing costs. So we had to invent technologies. The magnet technology was new. And that’s a big deal. So I decided to join. That was 1983.

ZIERLER: And, Alex, do you take a leave from SLAC? Or do you resign from SLAC?

CHAO: I took a leave at the beginning for one year. And I believe I asked for one or two one-year extensions and they were approved. But at the end of the second or the third year, I had to resign from SLAC. In any case, after I left in 1983, I didn't return to SLAC. I returned to SLAC only after 1993 when SSC was closed. During that time, I resigned from SLAC. During the absence from SLAC, I went to Berkeley for five years, working on the SSC design. And then–oh, there's a lot of stories afterwards. So I went to Berkeley. It was a very demanding job. I worked very hard. Just to mention one thing, for a long period of time, I worked with feeling no time for breakfast and lunch. I had one meal a day, a big meal, for dinner. Not everybody worked like me. But as usual I always fall behind. It's the only way I know how to do my job. It's really not the right thing to do. So for a long time about a few months I worked that way, one meal a day. Just as I felt OK to continue that way, I felt ill. Not that I had a fever. Just tired. Just ill. I took aspirin for a while, until my wife said, "I know what's going on. You have to eat lunch." I said, "A-ha. That must be it!" So from that day on, I ate lunch, and the problem went away. In any case, I did work hard those days. That went on for five years in Berkeley.

ZIERLER: Alex, what were some of the biggest challenges in the design process?

CHAO: Wow, this is kind of a key technical question. I mentioned magnet. The magnet technology, superconducting magnets, that alone was not what was new. What's new was we had to have a severely cost-saving magnet. That cost-saving magnet was not easy. You want to cut the cost down to a minimum–so the magnet has to be small. The magnet aperture has to be as small as we can tolerate. We called that the aperture problem. Now, when your aperture gets smaller, the beam has less room to maneuver. And you want to guarantee the beam is stable in that smaller room. So that's the main challenge. A lot of the accelerator physics effort was to address that issue. It's an accelerator physics issue. But it's driven by cost.

That issue later became an even bigger one because that Central Design Group design was later changed to a more conservative design of a larger aperture. It's more comfortable. Needless to say, larger aperture is more comfortable for the beam. But the cost went up. Later on, it became another issue when the project was transferred to Texas and became the SSC Laboratory in 1988.

The SSC Laboratory changed the design, maybe for a good reason, I cannot say. From hindsight, if we built a smaller aperture SSC, would it make SSC so risky that it doesn't work or it takes a long time to commission? But the SSC Laboratory decided to build a larger-aperture SSC, and the end result was the project has a cost overrun and was eventually canceled. That is a question never to be answered. I myself can't complain that this later decision by the SSC Laboratory was a wrong decision. I can only point out it's a decision which made the design relaxed, but pushed up the cost. Now, was that a wise thing to do or not? Nobody knows.

So there was this Central Design Group for five years, producing the conceptual design. Based on the design, the SSC Laboratory at Texas was approved by the US government. The job of the CDG at Berkeley was completed. A new management, a new team in Texas was to take over. Now, there's the question of how the Central Design Group and its expertise was to be merged into a full-blown SSC Laboratory from Berkeley to Texas. That issue involved a negotiation between the new SSC Lab management and the Central Design Group management, and of course the DOE. The issue was very complicated, both technical and political. Again, I was falling behind, not following the complications. I was aware there were discussions behind closed doors among the high-level people. But I was not involved. I took things for granted. The end result was, by the time the transfer was to be made, I was the only senior person from Central Design Group going to Texas.

ZIERLER: Alex, did you see that this was a problem, that this was an indicator of bigger issues with the SSC?

CHAO: Yes. Yes. But without understanding all the potential issues, I joined. I would not say who is right, who is wrong. At the end, among the senior members of the Central Design Group, I was the only one who moved family to Texas. I did not have a second thought at that time.

ZIERLER: But you moved because you thought that the project was viable, that it was going to happen.

CHAO: Yeah. I designed it. It should work. So I went. Then slowly, after maybe one year -- to me, maybe that's fast -- I began to realize it's a different team. And that new team would have a new design that they could call their own. They were not going to build the SSC using someone else's design, and they had a new guideline, new design philosophy. For example, one philosophy was, "I want to make sure this SSC works risk-free, and it is Ok if the cost goes up." The new team in the SSC Laboratory in Texas adopted that as the new guideline. And needless to say, the CDG Berkeley design had to be redone. I defended the CDG design initially. Later realized that was counterproductive. I'd be standing in the way of what should be happening for the new SSC. So I asked to step down from the leader of accelerator physics group. I was moved to a senior position without administrative responsibilities.

After that, I found a one-year gap free of responsibilities. That was when I wrote the book on collective effects. That's when I found the time to write it. So I ended up enjoying the gap time after all.

In the meantime, the SSC design and construction went ahead. I tried to support the new design, being careful of not to be misinterpreted to read, "my old design was wrong." No, no, I'd refuse to say that. But I could support the new SSC Lab design as a self-consistent design by itself. I would defend its own self-consistency. So I still defended it. Including on HEPAP, a DoE's High Energy Advisory Panel, on which I served. When the question was raised, "What do you think of the new design?", I defended it. So at least I supported the SSC Laboratory to the extent that that question was addressed.

ZIERLER: Alex, who at HEPAP was most important for you to try to convince as you defended the work?

CHAO: It was a whole committee. And they represented the community. I wouldn't say there were a few specific members who needed to be convinced either way, at least not at that time. All the lab directors would be present. Everything you say on the panel represents formal comments. These are very formal meetings. So I had to make my comments in a way that the audience would accept. After that period of time, the SSC Lab's new design was approved, and SSC Lab continued with its new design.

But soon there were some initial hints of problems emerging and the problems seemed to get worse with time. I think it began to become quite noticeable about a year after I stepped down from the Accelerator Physics group leader. Then there were a few rapid changes of project managers and so on because of the uncertainties. It was a complicated situation. Then John Rees, my previous boss at SLAC on the PEP project, came to take on the job as the project manager.

Incidentally, I spent four years in SSC Laboratory, five years in Central Design Group. That's a total of nine years. From age 34 to 43. That was my nine years of being at the SSC. I think that's the record holder of people who worked for SSC. I'm the record holder. Nine years.

John Rees joined, I think, in 1992. So he joined and became project manager. He and I of course knew each other. He asked me to become chair of something called the Parameters Committee. He formed this committee and asked me to chair it. Now, by its design, the Parameters Committee was the committee that controlled the overall design by controlling all its parameters. All the parameters of the accelerator had to be approved by the committee including the initiation for changes. So it was a rather powerful committee. John Rees knew me. He didn't ask me to lead a division of something, he asked me to take on the more general advisory role to chair this committee. I agreed to the assignment, I wanted to be of help to John any way.

So I took on that job. But after a short time, I realized there was really not much more one could do anymore. The design was frozen, and the Parameters Committee could not really change anything very significant any more --- you cannot change the magnet aperture anymore, right? Magnets are in production. So what the committee could do was quite limited. I regret to say that I did not help John as much as he asked and I agreed to do. But I did do my best at the time.

ZIERLER: You saw that you had opportunity to help John, and you didn't? In retrospect, you think that you had opportunity to help John, and you were not able to?

CHAO: That's what I said. Even though I tried, hands are tied by the circumstances and the late timing. The changes the Committee could propose were quite limited–you have to be reasonable. You cannot go in there and change the energy of the booster ring, for example. OK, you may propose to change the location of this magnet relative to the next. I still tried my best to do what was possible. John never mentioned to me to rescue the SSC political situation. But I assume he wanted me to support SSC on the technical side to help relieve some of the overwhelming pressure at the risk of being canceled. I did help him chairing the committee, but I think I did not help him reducing the cancellation risk.

ZIERLER: At what point did you realize that the whole project was doomed?

CHAO: Early. I must say, even before or about the time John came to the SSC lab. But John and I never really discussed this. It was doomed–I would say about the same time when John came to the SSC–it's doomed. The cost kept on increasing.

ZIERLER: Alex, is the cost increasing, or were the initial cost estimates too low?

CHAO: I think it's both. The very initial cost was by the CDG. In fact, I was with Maury Tigner at the time when he wrote down that $2.97 billion -- whatever the exact number was. I was with him when he wrote that down, and for a few seconds, there was a debate of whether the right number was $3.01 or $2.97. I said, "No, no, no, it has to be below three." So it was $2.97 for a while until it inched up to $3.01 anyway. But that cost was very carefully phrased. From hindsight, it was a mistake. At the time, it was carefully stated with qualifying phrases. If you pick up the design document, it was spelled out clearly. But nobody remembers those words. It was said clearly, that estimate was in 1986 dollars, no inflation adjustments, no detectors, no land acquisition costs. It was the cost of the bare bone technical components. For the small aperture design, it's not that wrong. If including the other expenses and in present-day dollars, I think it's then equivalent to perhaps $7 billion? I don't know. It's still on the low side, but it's not all that off. Because it also doesn't include all kinds of bells and whistles added on later. So I would blame the CDG's initial cost on just assuming people would read it that way. But that's not what people read. People just kept recalling the $3 billion number while stripping all the qualifying phrases, not to mention some of those misreading could have been intentional.

As to the later changes, as I said earlier, the aperture change had its own reasons --I even defended for it. You have to ask yourself how much you want to pay more for less risk. Will you pay 20% more cost for 20% less risk? That's the question. There's no right or wrong.

But the design change was not the whole story. Later, I think the SSC Lab made some mistakes in cost overruns, this time not because they wanted to take a different design philosophy, but I must say, they didn't control the cost as well as the CDG, leading to continual overruns. They did not make the same level of effort as the CDG. I don't want to make accusations of any persons. It's just the way that happened.

I think perhaps overall we made two mistakes with the SSC. The first mistake was to approve it hastily when we were not sure we had the will to carry it out to the end. The second mistake was to cancel it hastily when met with difficulties.

CDG was more of a single-minded group with a single purpose. In comparison, SSC Lab had to be occupied by many more complex issues, and at the end we managed ourselves into a sad final outcome. Anyway, a slow person like me finally realized the one lesson I learned, namely I really should return to research. So I returned to SLAC 1993.

ZIERLER: Alex, I want to ask you at this point, given that you hold the record for the longest employee for SSC, did you feel like this was a waste? Did you feel upset that all of your efforts did not go into the project? Or did the work still advance the science? Was it still useful for accelerator physics?

CHAO: Oh, wow. Good questions. Waste of time, no. A person has to go through various life paths. If I had stayed 100% doing research and publishing papers, I think I'd regret at the end. Having gone through this, I learned quite a bit. I went through a different path of life, and I returned to the original path. That's fine. You had a second question. What was it again?

ZIERLER: That even though the SSC was never built, all of your efforts were still useful generally for accelerator physics.

CHAO: For accelerator physics, yes. Yes. I think SSC, especially at the Central Design Group stage, contributed quite a bit to advance accelerator physics. That's something that I'm actually happy about for what I have contributed. Now, what did I contribute? Well, I didn't contribute much on the technical front. Over that period of time, working very hard, the biggest accelerator physics issue was the size of the magnet aperture. Now, the piece of accelerator physics that entered the study was nonlinear dynamics. I mentioned chaos, chaotic motion, and resonance, and so on. These belong to nonlinear dynamics. During that period of time, it happened that on one hand, because of the SSC, nonlinear dynamics became the prime, single most urgent and important accelerator physics of the community, right? On the other hand, academically, it happened that two new approaches on nonlinear dynamics were proposed. One was called the Lie Algebra by Alex Dragt. Another was called the Truncated Power Series, proposed by Martin Berz. The two approaches were proposed about the same time, in the Early

1980s. Each developed in their own corner, and publishing papers. But these two things were exactly what we needed for the SSC study. Those developments were new at the time. So I decided this was a coincidence of events. We must bring the Lie Algebra and the Truncated Power Series Algebra techniques to focus on the SSC, and make them grow and evolve in this fertile ground together. Together with the Central Design Group staff, we grew them together. In fact, these two techniques would be limited for accelerator applications if each is left on its own. You really have to combine the two, and when doing so, they together become very powerful. Lie algebra is an analytical tool. TPSA is a pure computational manipulation technique. Only if you combine the two do they become powerful. Why these two techniques happened at the same time, I really don't know. But they happened at the same time, and there happened to be a fertile soil of the SSC Central Design Group right here in Berkeley to develop them.

So I brought together that team, altogether I would say a dozen people, maybe 8 core people and several additional part-time enthusiasts. A core group put together for, I would say, a total integrated time of more than a year or so. The team continued this work after the members left CDG, but the seed was sown at the CDG. So I contributed in forming that effort with in mind to provide a fertilizing ground for this revolution of nonlinear dynamics in accelerator physics. I did not make too many technical contributions during this exciting time myself, but I made it happen, and I'm really happy about this accomplishment.

Up to today, nonlinear dynamics in accelerator physics, you can sort of divide it into two periods of time. One of them is before the 1980s. There was the classical Hamiltonian dynamics. It consists of one whole set of material. It's very sophisticated. But there was a revolution going on in the 1980s. When the younger students learn nonlinear dynamics today, they basically learn only the second half. The subject has changed. I think that's one area where SSC contributed to accelerator physics. And I consider this a significant contribution.

ZIERLER: Did SLAC hold your job for you? Or when you came back, it was a new position?

CHAO: I returned to SLAC for a new position. I resigned from SLAC after spending a few years at the CDG, and they canceled my job. When I returned at the end of 1993, I had to find a new job. It happened that I was very lucky. What happened was that Richter at the time had become the lab director. He had the vision of developing a Stanford faculty at SLAC. In this faculty, there would be three faculty groups, a High Energy Physics group, an Astrophysics group, and an Accelerator physics group. And there were faculty slots for accelerator physics opening up.

So I send in my application, and with Richter supporting me. Also another important person was Bob Siemann. I don't know if you know him. He was more senior. So he was the first member of this accelerator faculty. Then they had a total of, I think, up to ten slots. So I sent a letter to Bob and expressed my interest. He was very positive. He supported me very strongly. So did Richter. So they arranged me a slot. Without interview. That's what they called a targeted opportunity. So they gave me a targeted opportunity slot. I still needed the university board to approve. But that also came in a few months. So yes, I lost my job at SLAC, but I was lucky that there was a faculty position made available to me, and it was even consistent with what I wanted to do, i.e. to concentrate on accelerator physics research. I had decided not to join any big project oriented work at that point for a good reason on my part.

ZIERLER: Alex, this is a long time away from 1974, and it's after the SSC. So what do you think Burt Richter's big goals with this new endeavor were? What were the big questions in physics, and what was SLAC's role going to be in answering those big questions?

CHAO: SLAC went through a change, you must have noticed. SLAC's founding director was Panofsky, as you know. Professor Panofsky, during my earliest tenure, was the director, although my connection was mostly with Richter. Panofsky was a great director. He and Richter, the best directors there could be. However, the two of them were different. Panofsky was a purist. He wanted academic freedom, he wanted pure high energy physics. He really was an old-fashioned purist scientist. He led the lab in that direction.

Richter was different. He was less of a purist. He loved physics, of course, and he loved high energy physics, he was a high energy experimentalist. But he saw the world differently from Panofsky. I think I, from a certain distance, saw the two of them debated to the extent of Panofsky seemed to want to keep SLAC as a pure high energy physics lab, while Richter basically said, "The world is changing. We have to pay attention to what's in the future." That was when he wanted to include synchrotron radiation research to SLAC. As time evolved, Panofsky passed away, and under Richter, synchrotron radiation became a formal part of SLAC, and had grown even more since then.

ZIERLER: Alex, when you saw Burt Richter and Pief Panofsky debate, was your sense that they debated as equals, or Burt Richter deferred to Pief as the senior person in their partnership?

CHAO: That's a good question. I must say I can only reply by my impression. I think Richter was established enough and at that time generally considered the next director-to-be. I think they were considered equals, however Panofsky was the boss. The final decision was leaning toward Panofsky. It was only when Richter became the director himself that synchrotron radiation research was formally incorporated into SLAC. Before that, it was only sort of a symbiotic relationship. It was a different group with a different funding channel. But Richter was right. And as SLAC evolved even more later on, radiation source has become actually the mission statement of the laboratory. Panofsky would never have allowed this to happen. But as time evolved, the mission of the lab now includes light sources.

ZIERLER: This is way beyond what Panofsky envisioned in 1963, of course.

CHAO: Right. Right. Yeah. I knew Panofsky sufficiently to believe -- and this is a pure guess -- he's not stubborn, and he would admit he’s behind time.

ZIERLER: But also, Alex, after SSC, we have to recognize that high energy physics is going to change fundamentally, and that SLAC will need to reinvent itself.

CHAO: That's right. And SLAC did. I think, to a very large degree, SLAC did. SLAC still has a high energy physics branch, but basically, now, it has been quiet. So back to accelerator physics, I'm now the conservative guy, probably falling behind time. I see a mistake happening. I see, over and over, in terms of accelerator physics, an over-emphasis on radiation accelerators than on high energy accelerators or on accelerators in general. Everything has to be for radiation purpose, otherwise you don't get any support, not even hiring a post-doc. I think this is narrow-minded.

ZIERLER: And what is the influence here? Is this mostly the DOE?

CHAO: I think it's both DOE and the lab. DOE wants SLAC to become a radiation lab. DOE already has a high energy lab called Fermilab. But the same consideration has also been adopted by SLAC. From a shortsighted view, it's to the benefit of SLAC to develop quickly to get to the frontier of radiation accelerators so that we maintain world leadership. SLAC succeeds in doing that. It's still the world's leading radiation accelerator lab. However, being now a conservative, I feel uncomfortable because accelerator physics covers quite a wide range. Radiation accelerator is just a part of the accelerator physics -- and not even a big part -- to the extent that a few of the established top non-radiation accelerator physicists were laid off. That really was not what I anticipated just several years ago. This is carried out to this extent because funding is limited, and regardless of what significant contributions a person had made before, the question raised is, “What has he done for our radiation projects? And why is the radiation division paying him?" The next thing that happens is funding is cut off from him. After some shuffling, he will have to be laid off. This has happened to a few top notch non-radiation accelerator physicists and come close to happen to a few others. I think this is very shortsighted. At the end, it's because that division's money is earmarked to do radiation business, and cannot be used to pay for research doing non-radiation. "No, sorry. I can't pay you. DOE will not allow. And neither do I want to." Basically. I think nothing is wrong with it in a short run. It's even seems to be the right thing to do given how our environment has been evolving. But what if we need that discarded expertise later on? It's not easy to breed talents with that much accumulation of knowledge, and they are let go for so little short sighted gain.

ZIERLER: Not only that, but graduate students won't study this if they don't think there are jobs.

CHAO: That's right. Absolutely. No graduate students are now coming into the field unless it is related to radiation sources. That non-radiation field very quickly dries up. All that knowledge, accumulated over decades, dries up just like that—as I mentioned, radiation physics represents only a small portion of accelerator physics field.

In this regard, I return to a quick comment on the SSC. I assume we are approaching the end of our conversation.

The SSC shut down was a sad story. What do I mean by sad story? See, there were, at the time, about maybe 2,000 employees, plus families. I moved my family also, but I was lucky because I found a job at SLAC to return to. Most of them do not have a job to return to. A few thousand people, not only themselves, but also their families. So it was a large number of people suddenly stranded there. Now, it's especially sad because many of them were very specialized. They're top notch people on the various fields in technology, or physics and engineering, or technician. They were recruited to the SSC, and they were all top notch people. A person who's specialized in connecting superconducting wires from this end to that end without heat leak, for example. This is a very specialized and narrow expertise. Now, how do they find another job which requires a top notch superconducting wire connector? Those people, many of them with family, are now stranded in Texas. Maybe they do small business selling souvenirs at airports. So the cost to society was enormous. How much do you need to spend to bring up one engineer? All those investments were lost.

On top of that, there's also the monetary loss on the SSC itself. The cost was $2 billion spent. And then after its cancelation, there was $1 billion to wrap up. So we spent $3 billion. Ironically,

$3 billion was the very first cost estimate made by the Central Design group in 1986. But anyway, that nightmare is in the past.

ZIERLER: It's important. So, Alex, in your last years at SLAC before you retired, what were the major advances in accelerator physics? What were the successes in Burt Richter's vision?

CHAO: That's a good question. At SLAC, as we both mentioned, we are changing gear. We are changing to radiation source. The lab is doing very well with this change. Free electron laser is the main achievement. SLAC is still holding that title of the world's most frontier free electron laser laboratory. Radiation takes over the emphasis as the lab mission.

Speaking for myself, after the SSC, I no longer work on big projects. You asked what I have learned from the SSC. This was one thing I learned. Not because there's anything wrong with a big project. Not because they are wrong. It's because I found myself more useful doing research and I enjoy research more than management. So I didn't join the free electron laser, which is a big project. And I'm interested in the research of a small accelerator for radiation. A student named Daniel Ratner and I made a proposal in 2010. And this is what I'm spending most of my research time on now.

It's something called Steady State Micro Bunching, SSMB. This particular proposal suggests a small electron storage ring for radiation purposes. If it works, it could radiate very high power radiation for many applications. Right now, the project is being studied by a few groups. These are not at SLAC. At this moment, its storage ring is considered to be around 400 to 600 MeV. It's a small ring. If manipulated by laser properly, correctly, and successfully, it can produce very high power radiation. So that's the idea. It's being studied. I don't actively participate because I'm retired, but I participate on the side. They have meetings, they invite me to sit in the meetings, and so on. Teleconference, not in person. So that's the main effort I've been working on. Also, I'm writing two more books.

ZIERLER: You're a busy man, Alex.

CHAO: The books are a separate matter. I'm writing two books. One is now being printed. And hopefully, the second one can be finished soon. As to the SSMB, I hope it succeeds at some time soon too.

ZIERLER: What is your vision? What is your goal for SSMB?

CHAO: I hope it works. I think the accelerator physics part and the principle of this radiation source should work. I don't doubt it there. To put it into a real facility, however, you have to worry about all kinds of practical matters. If there's enough laser power, if the laser and accelerator are steady enough to avoid noises, etc. My wishful thinking is after a period of theoretical study, I hope to build at least one full size prototype to test the principle, and I hope they work. If they work, they can be a sources of strong, clean radiation, very clean, very high-powered radiation, for example, to do lithography. There's a need of extreme ultraviolet EUV radiation for lithography.

There is at present a commercial EUV source. The power source of that device is not accelerator-based but plasma-based. Compared with that radiation source, SSMB would be cleaner and more powerful. At least that is the dream. If everything works, and if the prototype works, say four years from now, I don't know, then my dream really is a possibility of building a real, dedicated SSMB production ring and apply it to industry. Would be nice to see chips being made using it.

ZIERLER: And there's new physics to be discovered here.

CHAO: Absolutely. Absolutely. Yes. Yes. Yes. This is the exciting possibility. SSMB itself is great accelerator physics. Its radiation of various wavelengths can substantially extend research capabilities in other physics.

Chance is not huge, success is not assured. But personally, I think it's not small either. I think there's a good chance with effort.

ZIERLER: Alex, for my last question, I want to ask you, looking toward the future, and specifically because of your work in both Taiwan and China, and thinking about Asia's contributions to the future, what do you see as the future, both for accelerator physics and high energy physics generally? Where do we go from here?

CHAO: I think that's another very large question. Maybe I'll comment on the easier one first about the future of accelerator physics. On the accelerator side, I think China is doing very well. Why do I say that? They began with not much. If you compare today the integrated strength capacity of Chinese accelerators with that of the United States, it's not close and falls behind at a distance. But why do I say they're doing very well? It's the young people. They put in the effort. We touched upon this comment on young people a little earlier, but in China, students are more driven. They want to contribute, they want to grow. They're disadvantaged because they don't have that much established environment with enough senior people to guide them, not as many as in the United States. They often have to sort of trial and error on their own. So they're disadvantaged in that sense. But every year, they produce 100 accelerator PhDs. And how many do we produce in the US? Three? Five?

ZIERLER: The future in accelerator physics is in China.

CHAO: 20 years from now, I bet it's all their voice. You watch. So in terms of accelerators, I think China will do very well. Taiwan is small, so Taiwan will have a presence but weak in comparison. I don't think they have the drive that China has. They have a project, they work on their project with good results, perhaps more like the United States in terms of the future.

In terms of high energy physics, this is a bigger topic, and it's a harder question. United States is not to be compared because United States is not going there after canceling the SSC. In terms of high energy physics, we still have Fermilab. But that's taking a non-traditional approach, an approach of selecting some important topic on the side. "Let's attack this problem, get this neutrino problem solved. That is an important problem." It's not the traditional mainstream approach of high energy physics. The mainstream approach requires a big accelerator, which United States is not going to participate in. So Europe is considering one, Japan is considering one, China is considering one. Taiwan seems to consider participating, for example, in space equipment. So again, it's more similar to US not taking on the mainstream.

On the big accelerator front, personally, I feel the chance is small in any one of the presently proposed big projects. Now, as an accelerator physicist, a natural response perhaps would be to advocate or support such a big accelerator project, "A big accelerator is good for the accelerator field." On the other hand, I feel that's the wrong thing to do. I feel the very large accelerator for that purpose is the wrong thing to do. First of all, the purpose of such a large accelerator is not clear, and secondly, technology, we don't have it yet. So I'm saying it's the wrong thing to do for two reasons. The purpose is not clear. That is not for me to say, it's for somebody else to say although I have my opinion. But in terms of the technology, I think I have enough knowledge to say with some credibility that the technology is not there. I can go into some details here. But I mean today. If you do another 20 years of R&D, it may be a different story.

ZIERLER: But that requires the support to do the R&D for the technology to get there.

CHAO: Exactly.

ZIERLER: And that, itself, is not a foregone conclusion.

CHAO: Exactly. So you have to commit a very large resource to do the R&D so that you establish feasibility in order to decide whether to go to the next step. So it's really a second-order thing. So in terms of the very big collider for this purpose, me, personally, although against my profession, it would be wise not to do it. I did say that not too openly, but I did say that to friends and colleagues. And my friends know. They know that's my view.

ZIERLER: And now because you've shared it with me, so many other people will know it as well. Alex, I want to thank you so much for spending all of this time with me and sharing your insights, and I know everyone at SLAC is going to be very excited to have this transcript as well. So thank you very much for doing this. I really appreciate it.

CHAO: Thank you very much, David.

[End]