ZIERLER: Okay, this is David Zierler, Oral Historian for the American Institute of Physics. It is August 14th, 2020. I am so happy to be here with Dr. Helen Quinn. Helen, thank you so much for joining me today.

QUINN: You're welcome.

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

QUINN: I'm a Professor Emerita of Particle Physics and Astrophysics at Stanford Linear Accelerator Center, now known as SLAC National Accelerator Laboratory.

ZIERLER: When did you go emerita?

QUINN: Beginning of 2010.

ZIERLER: In what ways have you remained connected to SLAC in the past ten years?

QUINN: Actually, rather little. I made the decision, unlike most physicists, that at the moment of retiring I was also making a change of focus. I was, at that time, chair of the Board on Science Education of the National Academies of Sciences, and I had just been asked to chair a committee on the National Academy looking at how we should teach science K-12. That became my full-time occupation, and the results of that work have been my major focus since retirement.

ZIERLER: Were you always interested in physics education issues, or was this really a brand new focus for you upon retirement?

QUINN: I always had an interest in education. In fact, if you go right back to when I started graduate school, I really didn't think I was going to do a PhD. I applied to PhD programs because I knew the best universities didn't accept master students, and I thought in my head I was going to do a masters degree and then go be a high school physics teacher. But I got too interested in physics, and finished the PhD, and stayed in the field. But during my time at SLAC, I was not a faculty member, so I was not supervising graduate students, but I was involved with and eventually managing programs both for undergraduate summer research experiences at SLAC, and for physics and chemistry teachers to have some two or three week workshops on teaching modern ideas about particles in the context of high school physics and chemistry, and later also teacher summer research experiences. Chemistry teachers actually understand quantum mechanics better than physics teachers in general, because, after all, chemistry is quantum mechanics.

ZIERLER: So, this is interesting. SLAC, of course, does not have a strong education component for its faculty, but it sounds like education interests were always in the background for you.

QUINN: Well, I was not faculty. I was staff until after I became a member of the National Academy. So, as a faculty member, I would have been supervising graduate students. SLAC has always had graduate students, and there's an arrangement with campus whereby some SLAC faculty teach some courses on campus. Of course, Stanford has to pay their salaries for that part of the time, so it's a limited number. But I did teach a few undergraduate courses, for example I taught premed physics for a few years on campus.

ZIERLER: Over the past ten years, were you involved at all with AAPT?

QUINN: Not directly. AAPT -- when we first put out the study about what should be taught in k-12 science, there was an AAPT review of the physics part of it. In fact, some adjustments were made based on suggestions made by AAPT about how best to treat certain topics. Teaching science always involves such decisions. For example: When do you start talking about heat the way physicists talk about heat, and when do you talk about heat the way everybody else in the world talks about heat? Those are decisions that need to be made, and consistently through the education. That was one of the topics of the discussion. Where do we start using the term heat the way physicists do? It's really quite important as you're trying to figure out how you teach energy to think about how you teach heat. So, there was a discussion with AAPT about those topics, which was quite interesting.

ZIERLER: Right. So, as removed as you were for much of your career from actually teaching, these were things that were cropping up in your mind over the years.

QUINN: Right, exactly. When I became a member of the National Academy, Carl Wieman, who founded and was the first chair of the Board on Science Education, invited me to join the board on science education. There are relatively few academy members who have even the level of involvement in pre-college education of my teacher workshops. So, I became part of the group at the academy that was thinking about k-12 science education. That became my interest and focus as I turned towards retirement.

ZIERLER: Let's go all the way back to the beginning. Let's go to Australia, and first I want to ask you about your parents. Tell me a little bit about where they are from.

QUINN: Both my parents were Australians by birth. My father's mother came from Scotland. His father grew up in Australia. And my mother actually goes back to the very first arrival of Europeans in Australia. Her parents were one a guard, and one a convict. Her ancestors had come when England first shipped convicts to Australia.

ZIERLER: That's almost too stereotypical.

QUINN: As an American, you ought to know the date was which England started sending convicts to Australia. Can you name the year?

ZIERLER: I can't say that I can off the top of my head.

QUINN: Think about it. When did they stop sending convicts to the United States?

ZIERLER: I don't know that either.

QUINN: 1776, of course.

ZIERLER: Oh, of course, of course. That seemed like an obvious date, but I thought there might have been some arrangement where they kept on sneaking them on.

QUINN: No. Britain's way of dealing with excess population, basically -- the penal system was mostly debtors, petty theft, so poor people, and politicals, so the Irish and the Scots, that got sent to the penal colonies overseas.

ZIERLER: Right. Where did your parents meet?

QUINN: My parents, I guess, met socially. Melbourne, as they were growing up, the people at a certain level of society were a small society. I don't know exactly when they met first, but they were teenagers, or probably around when both of them graduated high school. Neither of them had university educations, but both of them had careers that today would require at least Bachelors degrees, My father was an engineer but the only formal engineering education he had was a technical school course as a draftsman, that is a course in Engineering drawing. Beyond that, he was self-taught. My mother was a dietician, who also had a technical school course in what was called "domestic economy." Her job when they met and married was inspecting the cafeterias at the railway stations for the state of Victoria, making sure they were clean and doing the right things and producing healthy food for the travelers. The same training was the training that a hospital dietician would have, and she worked as a hospital dietitian part of the time, too.

ZIERLER: And you grew up in Melbourne?

QUINN: I grew up in the suburbs of Melbourne, yes.

ZIERLER: Would you say it's as a typical middle class upbringing?

QUINN: Pretty much. My parents both went to private schools, and they sent their kids to private schools. I think for my father, certainly, my father's family struggled to keep their kids in private schools, and my parents struggled to keep us in private schools, but that was a big social line divider. It's amazing: Class has to do with education, but also in Australia, it has to do with accent, and if you went to private schools, you spoke English with a different accent than if you went to public schools. You were taught to say it again and say it correctly if you began to speak 'strayan (i.e. with a "typical" Australian accent).

ZIERLER: Was this something that you appreciated growing up, or only looking back did you understand these distinctions?

QUINN: To some extent, I knew it. For example, there were gender separated private schools. There were boy schools and girls' schools. When I met a boy, I usually knew which school he went to by his accent -not just private vs public but which private school. So, I knew that these accents were taught. I didn't think about it in terms of being a class issue until afterwards, but I certainly knew that accent mattered socially -- and the other funny piece of this, I noticed this after we came to the U.S. and my parents' friends would come visit. It's also gender linked. At a given social level, the men have a more 'strayan accent than the women. The women have to be more proper and speak more English style. It's a very interesting sociology. It's got nothing to do with physics, of course.

ZIERLER: Helen, tell me a little bit about your early education, and perhaps some of your early interest in science.

QUINN: Well, my early education, I went to the first -- well, I was sent to a kindergarten first. Kindergarten was preschool. School started with first grade. I went to a private preschool which I vaguely remember. From there, to a school that was a Presbyterian Ladies College. The private schools were generally church affiliated. It was very Presbyterian in style; very rigid and controlled. That was close to where we lived. Then, we moved, and I moved to a different school around third grade. The school I moved to in third grade was a new branch of an Anglican private school that had existed in the inner suburbs but was moving to a new campus at the outer suburbs. When I started there, it was 50 acres of bushland with three classrooms. So, there was a lot of outdoor education, science was “nature study” with a lot of interest in the woodland, and the plants growing in the woodland. I think that was my beginning of thinking about science and wanting to know and understand why these things grew there, and the other ones grew over there, and getting to know that land. I found it very strange when I came to the U.S. to walk into a forest where I knew nothing.

ZIERLER: Helen, this is as much a sociological as it is a scientific question: growing up, were you ever made to feel as a girl and then a young woman that maybe science was not something that would be appropriate for you to pursue?

QUINN: First of all, I was going to girls' school, so at school, no. And at home, no, too. My father was the one who generated conversations about interesting things, and he would set up dinner as a discussion about some topic that he found interesting. Whatever position one of us would take, he'd take a counter position to get us to argue. I have three brothers, so it was a case of being part of discussions. They were often technical topics because that was what interested him. I was expected to be as much part of the give and take and the argument as anybody else. In fact, I think my father kind of favored me because he liked, or encouraged, the way I thought.

ZIERLER: Favored you relative to whom?

QUINN: My brothers. I'll tell you a story. This is much later, when I was at Stanford, and I was living at home with my parents. My father came home with a business associate who walked into the house and looked around and said, "Oh, you have sons, too."

ZIERLER: An afterthought.

QUINN: It said something to me about who my father had been talking about. Certainly, from the beginning he expected me to be as interested and as capable of understanding science and math and technical drawings as my brothers. He would be sketching designs for things he was thinking about at home all the time, and he would share his designs. I remember, when I was quite young, he was designing his dream yacht to run in the Sydney to Hobart Yacht Race. He never could afford a yacht, and those drawings were part of our conversations.

ZIERLER: Right. Helen, did you go to girls' school throughout high school?

QUINN: Yes.

ZIERLER: What was the decision there, from your family's perspective?

QUINN: It wasn't a decision. The private schools were girls' schools and boys' schools. The only coed schools were the public schools. Public, meaning state schools.

ZIERLER: Was that ever an option your family considered, sending you to public schools?

QUINN: No.

ZIERLER: Why not?

QUINN: As I said before, because they valued education and they wanted us to be well-educated. They thought that the private schools were worth struggling to pay for to give us the education they thought we needed. Probably also it was a class-based decision.

ZIERLER: Is your sense, looking back, that that was a fair assessment of the different education opportunities afforded in public and private schools?

QUINN: Unfortunately, yes. In my first year at university, the students in my peer group, and essentially all the students in the science courses, came from private schools with the exception of one or two selective public high schools, like University High School in Melbourne. But essentially, the chances of your matriculating, passing the state exams at the level that would let you into the university, was much, much higher from a private school. Even then, at my girls' school, the students who -- there were like 60 girls in a year in 7th grade. Some would leave at 10th, some would leave at 11th, some would finish 12th, but finishing 12th, I think there were two of us who finished with sufficient science courses to study science at the university level. There were more at the boys' schools, so that was one of the factors of the expectation, and the level of teaching of the sciences was not nearly as good at the girls' schools as it was at the boys' schools.

ZIERLER: Were there any benefits that you could see, besides these obvious drawbacks in the girls' school? Were there any benefits in terms of developing your scientific talents and interests that were conferred by going to an all girls' school?

QUINN: I can't really tell you, because I don't know how discouraged I would have been to be in a college situation. First of all, many of the teachers, and this includes my math teacher, were people who would have been professionals or academics in a community that gave women better opportunities. So, the women who were teachers at the girls' school, aside from physics, all of my teachers were women. My physics teacher was totally incompetent, and he was retired from one of the boys' schools.

ZIERLER: He, not she. He.

QUINN: He. And my brother happened to be taking physics at the same time -- he is two years older than me, but I had jumped a grade and he was repeating the final year, so we were taking physics at the same time. When he heard who my teacher was, his teacher said, "Your sister is working at a severe disadvantage." From then on, he sent home notes and problem sets from all of his classes for all the girls in my class. There were five of us. On the strength of that, two of us managed to pass the course.

ZIERLER: Did that indicate to you, or maybe your parents, that on some level you had significant aptitude for these topics?

QUINN: I think my parents always thought I had aptitude. My parents encouraged me to go as far as I wanted -- although, that's not completely true. There's two things, right? If you want to encourage a kid, it's aptitude and opportunity. For example, when I started university -- first of all, because I had skipped a grade, I was only 16. In order to enter the university at 16, I had to get a certain number of honors on the exams at the end of high school. I got sufficient number of honors to enter, but my father said, "Look, you really don't want to be a blue stocking."

ZIERLER: I'm not familiar with that term.

QUINN: This is an interesting, very sexist term. What is a blue stocking? It's an old lady in lisle stockings who never got married, and who is very academic, and therefore not an interesting person socially.

ZIERLER: You don't want to be a blue stocking, I get it.

QUINN: Right. So, he discouraged me from doing the summer work that I needed to do to go into the honors math track at Melbourne University in the first year. I went into the regular math course, and then decided for myself that I wanted to move into the honors one and did the work in parallel to move across. That work, by the way, was using a Marchant calculator, and doing some calculating, basically applied math. According to the math faculty It was too boring to teach in the honors classes, but it was something that you needed to know how to do, so you had to do it independently to get into the honors track. Within the first week or so at the University, I'd shifted over to the honors track.

ZIERLER: I'm not familiar enough with the Australian higher educational system to read into your decision to go to Melbourne University. Besides it being close, what were some of the values and opportunities you saw going there?

QUINN: It was the only university in the state at that time. There were no private universities in Australia. I still think there are not. They're all state supported at some level. But there are now several universities in each state. In fact, I think the school where my father took his drafting course, which used to be called Swinburne Technical College, became part of the university level systems.

ZIERLER: When you got there, were you thinking specifically science as a focus, or were you more broadly interested?

QUINN: Actually, when I was accepted -- matriculated, which means accepted to the university. If you pass matriculation you could go to the university, back then at least -- I applied for various fellowships in various science and math. You narrowed your focus through high school there. I had taken, in my last year of high school, physics, chemistry, pure math, applied math, English as a required subject, and English literature as an extra. It was quite clear I was oriented to a physical science degree, but I really didn't know what science. I applied for and got a fellowship, you would call it here, there it was called a cadetship, from the Australian Weather Bureau. They paid the limited amount of fees that the university required, paid a salary to me while I was a student, and required that I work for them for five years after I finish my degree, or I would have to pay back the money they had spent on me.

ZIERLER: Does cadetship suggest a certain military component to this fellowship?

QUINN: No, except it's the Australian government paying for it. I was a cadet meteorologist, simply meaning I was in training to become a meteorologist and being paid by the government to do that.

ZIERLER: Did you have any particular interest in meteorology at this point?

QUINN: No, but my father said, "Look, if you take this, I'll pay the bond to get out of it if you decide to go a different way, but it's cheaper for me to do that than it is to support you for these next three years as you're a university student. So, take it, and if at some point you decide this is not the right direction -- " I know I would have stayed with it if my family had my family stayed in Australia. I would have become a meteorologist, and probably eventually a client scientist.

ZIERLER: I don't want to ask too many questions that assume anything, because obviously if you switch universities, it's a big switch for you going to Stanford. Can you talk a little bit about how that came about?

QUINN: My father was offered a job at the parent company of the company he was working for, actually had founded the branch, in Australia. He decided it was good for him to make that move. It was a family decision we would come to the U.S., but we really thought we were coming and would go back again in three years.

ZIERLER: So, you said the plan was moving to the States was supposed to be a temporary proposition.

QUINN: Well, the company said they'd pay our fares to the U.S., and in three years' time, if any one member of the family wanted to go home, they'd pay our fares home. For the teenagers among us, we were thinking okay, we'll go. We'll spend three years in the U.S., that will be interesting, and then we'll go back again.

ZIERLER: What were your feelings about making this big move?

QUINN: I think my brothers and I all felt what can we lose? It'll be interesting. I don't think any of us thought we were emigrating. We thought we were going to the U.S. for three years.

ZIERLER: But you didn't have a bad experience at Melbourne. You would have been happy to stay on there to complete your undergraduate.

QUINN: I would have stayed at Melbourne University, but I was still living with my parents. The thought that I would stay in Australia without my parents never arose. I was 17 years old.

ZIERLER: Helen, you didn't just transfer to any college. You went to Stanford, so obviously this speaks to your strength as a student.

QUINN: No, it speaks to my ignorance. I got a book and I looked up what universities were close to San Bruno, which was where my father's job was. The book only listed two universities: Berkeley, and Stanford. So, I applied to Berkeley and Stanford. Both of them said, "Okay, you're admitted, but come talk to us because we don't exactly know how to place you." I went to Berkeley first, and Berkeley was very difficult about giving me credit for any course I'd taken unless it totally matched the course they had. I went to Stanford, and they happened to send me to a professor in the physics department whose name was Jerry Pines, and his attitude was, "Look, if you think you've taken it, I'll say you've taken it, and if you think you need to take it, then you take it." So, it turned out the physics was the easiest major for me to complete. Stanford gave me three years of credit. One for my last year of high school in Australia, which was about fair, but I think they did that on the basis of a single letter from a math professor in Melbourne who said I was a good student. Somebody else showed me the letter. It said I was a good student and a nice girl, and if I stayed in Australia, I would complete a bachelor's degree within a year, and I should be put in a similar position here. And Stanford gave me enough credits to do that and then said find yourself a major where you can complete the requirements for the major.

ZIERLER: Anyway, were you thinking about the physics program in particular when you got to Stanford?

QUINN: Well, in Australia, I would have had a science degree. What the meteorologists suggested in coursework, it was more physics than chemistry, but it was a general science degree. My first year at university was chemistry, physics, and math. Pure math, applied math, physics, and chemistry was what I was taking first year. Second year, I think there was some more chemistry, too. It was a physical science program that I was taking and would have graduated in. Then, I would have gone on, probably, for an honors year, which was one extra year, with a more focus toward meteorology. I was working summers at the Weather Bureau. I was working when the first satellite data began to come in. For the first time, you could actually predict weather in Melbourne, which you never could do before because the weather in Melbourne comes from Antarctica. Without the satellites, you knew nothing.

ZIERLER: It's safe to say that if you only knew of Stanford because it was in your book, obviously you didn't know much about the physics department and all of the luminaries on the faculty there.

QUINN: I knew nothing. I simply went to talk to people in the universities, and I walked around Stanford with my notes from my courses in Melbourne, and talked to professors in the math department, the statistics department, the physics department, etc., trying to figure out what the requirements were, and if I could place myself so I could get a degree in not much more than a year. Physics matched best, because of the flexibility that Jerry Pines gave me, and Stanford because of its quarter system -- we arrived in January, so I could audit courses for that winter quarter and figure out where I stood and visit classes. So, I figured that I could take junior year courses in physics and audited that for that winter quarter, and took them the next quarter, I could be at the level of a senior in physics by the next year. That's what I did.

ZIERLER: Culturally, was it an easy transition when you got to Stanford, in terms of being in the United States, being on an American campus?

QUINN: Yes and no. First of all, when you start university at 16, you're pretty socially out of things anyway. At Melbourne University, I lived in my elder brother's shadow. His social community was mine. Actually, the friends I made myself were the other girls taking physics. They were the ones I knew best. So, now, he was at Foothill College, and I was starting at Stanford. So, I was by myself at a university for the first time, still living at home. I had to decide which university I was going to so my parents could decide whether they would buy a house north of San Bruno, or out of San Bruno for me to commute to university. So when I picked Stanford they bought a house in Palo Alto I settled into the advanced track senior level courses in physics, but to complete the degree, I also had to satisfy the distribution requirements. I had to take the courses that the freshmen took, like Western Civ, and some social science course, and some humanities course. They let me count my high school English for one of the humanities requirements. I had a very funny, split personality where I (was taking courses with freshmen and with seniors and graduate students the other half.

ZIERLER: Helen, the next thing I wanted to ask was, once you get settled in the physics department, were there any faculty members that you became close with even as an undergraduate?

QUINN: Not really. There were the people who were teaching my courses, and I interacted with them as a student does in the courses. I had an undergraduate advisor, who was Mason Yearian, an experimental high energy physicist. But I just went to him to have him sign my course cards. I really didn't interact with him much. I interacted with other students in the same classes. In fact, when I first met my husband, who was taking one of the classes, he was a first year graduate student taking one of the classes I was taking as a senior. I certainly got to know other students, but I didn't really think of myself as somebody who would make friends with faculty members. In fact, it seemed very strange to me -- the idea that graduate students called professors by their first names was odd to me, from the formal Australian system. So, I guess they got to know me as a student, but I didn't really know them at all as my professors.

ZIERLER: Coming from Melbourne, did Stanford feel like an elite kind of place to you?

QUINN: Not really, and it wasn't so much back then as the comparison today. Melbourne University is an elite place. There's a selected group of students that get through high school and get into university. I had competed to get to university through the exam system. I didn't see Stanford as being more competitive or different in that way. Certainly, it wasn't any more demanding. Its program was different. In Australia, it's all the exam system. You take a one year course and the only thing that counts is the final exam. That's not true now, but it was then. I came from a system which was, in a sense, tougher than the system at Stanford. Academically, I didn't find it any more demanding than I had found Melbourne University.

ZIERLER: Was there a specific moment you recall when you said, "I'm going to stay on for graduate school, or I want to pursue a career in this area," or did that just sort of happen naturally?

QUINN: It's kind of interesting. I don't know who initiated this conversation. There was an Australian post doc at Stanford. His name was Tony Hearne. I scarcely knew him, but somebody on the faculty sent him to talk to me to tell me to tell me that I should go to graduate school.

ZIERLER: You, specifically?

QUINN: Yeah, he specifically came and told me that the faculty told him he should tell me that I should apply for graduate school.

ZIERLER: What was that about?

QUINN: Well, I guess they thought I'd hear it from an Australian better than from them. Funny, right? It was weird. I knew him, but I knew him slightly. I didn't think of him as a friend, or somebody that I would interact with.

ZIERLER: You must have read this as an endorsement of your abilities.

QUINN: I read this as an endorsement that I should think about going to graduate school, yes. And indeed, I did. I started to apply, and my undergraduate advisor said, "You know, graduate schools are often reluctant to accept women. They get married and one thing or another, and they don't finish. But I don't think we need to worry about that with you." I was thinking – "Did I hear what he just said?"

ZIERLER: Was he calling you a blue stocking?

QUINN: That was my question in my head. If I go to graduate school am I not going to get married, or does he just think I won't get married anyway?

ZIERLER: Were you also thinking how ridiculous because this is a question that would never be asked of a man?

QUINN: No, I just was so stunned by the way he said. But I think he was warning me that I would not be accepted everywhere, and it was true. The only graduate school that accepted me was Stanford, where faculty already knew me. My record was weird. I didn't have a standard record.

ZIERLER: But you did apply elsewhere.

QUINN: For example, I wrote to Princeton asking them if they accept women in their graduate school. They sent me back a letter that was addressed to Mr. Helen Arnold. It said, "We do not normally accept women." I decided I wasn't abnormal, and I didn't apply. But I did apply to Harvard, and I didn't get accepted there.

ZIERLER: Perhaps you were aware that at a place like Princeton, they didn't even have women's bathrooms in the physics building.

QUINN: It actually turns out that the only women they accepted in their graduate school were the wives of their faculty members.

ZIERLER: Right. Helen, by the end of your undergraduate tenure, in terms of your budding identity as a physicist, did you think in terms of experimentation, or theory, or applications, or was everything still wide open for you?

QUINN: No, no. I definitely was heading in the direction of theory. One of the things that year at Stanford did to me, if I didn't know I was good at it before, I knew I was good at it after that year. I had no problem being the student who knew what was going on in any class.

ZIERLER: In 1963-1964, were you aware of what Pief was building initially?

QUINN: Barely. I was not aware of what was going on in experimental high energy physics until I took -- I think Burt Richter actually taught it--a course in particle physics in my first year as a graduate student. Bert was building a storage ring on campus at that time. He was a faculty member on campus. It was the time of the big split with SLAC. The physics department wasn't talking to SLAC. SLAC faculty, even Pief and Sid Drell who had been physics department faculty, were not allowed to teach on campus by the physics department. I took a course from Sid Drell, but it was in the math department.

ZIERLER: We know so much about Burt, with his extraordinary accomplishments in the lab. What was he like as a professor, to take a class from him?

QUINN: I think that general introduction to particle physics is a funny class. It's just sort of what's going on in this field, and what do you know about it? One of the things you could read from Burt was he was excited about possibilities. It was also a funny time where we didn't know anything about particle physics as we know it today.

ZIERLER: You didn't know anything, but I bet there was an appreciation that there were fundamental discoveries on the horizon.

QUINN: To be made, yes. I think so. The one thing you got from the particle physicists at Stanford was something exciting is coming. That's why I became a particle physicist, because there were people around me who were very excited about the opportunities there. In my first year as a graduate student, I wasn't quite sure what direction I would go in, but I think Bert's course in particle physics was one of the things that said this is an interesting area where there's lots to be learned. He was certainly an enthusiastic teacher, and transmitted enthusiasm for the subject, and for the opportunities in the subject. That was part of what pointed me in that direction, and I think it was also just -- by the time I had to find myself an advisor in the second year, I actually -- again, I broke the rules, because as I said, there was a big split. Actually, one of my peers, another graduate student who was a year ahead of me suggested it to me. I was frustrated by the options in the Stanford physics department for a thesis advisor, and he said, "Why don't you go talk to bj (James Bjorken) at SLAC?" I thought that was a good idea, so I went to talk to bj at SLAC, and he said, "Well, yeah, but you have to get the physics department to give you permission to work with me." Then, I talked to Dirk Walecka, who is a nuclear physicist, and Dirk said, "If you want to work with bj, I'll back you up. If the physics department won't give you permission, I'll sign your thesis. You can work with bj, and you could be nominally my student if you have to be."

ZIERLER: So, you were essentially picking sides at this point?

QUINN: I was picking a person. I wasn't picking sides in the battle between the departments. It actually took over a year for the physics department to approve that bj was a qualified person to sign my thesis.

ZIERLER: Seriously? It's bj Bjorken. Why would there ever be a second of doubt? Because he was young?

QUINN: Well, because the whole relationship between the physics department and SLAC, the university had given SLAC the authority to appoint faculty, but the physics department didn't want to be overwhelmed with particle physics students who were going to work with professors at SLAC. So, the idea that the physics department would admit graduate students who would work with SLAC faculty, I was the one putting that idea on the table and saying I want to do this. It was too early for experimental students to want to do it, because SLAC wasn't at the stage where it was taking experimental students. I had broken new ground saying here we are with these wonderful people who could be our advisors.

ZIERLER: So, they saw what you were trying to do, and they read the writing on the wall, essentially?

QUINN: And they thought there might be too many of us. They didn't want to become a service department for SLAC.

ZIERLER: And bj did not have graduate students before.

QUINN: bj had no students. He was a young, new faculty member at SLAC.

ZIERLER: Of all people, what was the connection to him? What was he working on at that time that attracted you?

QUINN: My thesis is current algebra. He had just published his first current algebra paper, and that was interesting to me. That became the area that I worked in. Of course, he was also working -- I saw the SLAC data interpreted in quarks before anybody else because he was thinking about it, and he told me about it. He told the experimentalists how to plot their data, and he showed the data to me, and plotted that and said it's really interesting.

ZIERLER: Does this mean that from 1965 to 1967, you're mostly at SLAC and not the department of physics?

QUINN: I had an office in the attic of the department of physics. The fourth floor was really an attic. That's where graduate student offices were. I would go up to SLAC to talk to bj, and I would go to seminars at SLAC, but there were no graduate student offices at SLAC, until a little later when there were more students. Then, they started figuring out students should live at SLAC, not on campus. I went back and forth on my bicycle between campus and SLAC a lot, and there actually was even a car service that I could call on to go back and forth.

ZIERLER: It must have been so exciting to hang out at SLAC during those early years.

QUINN: It was, and yet, I was not really that aware of what was going on experimentally, except through what bj was working on and interested in.

ZIERLER: Was it chaotic?

QUINN: Particle physics was chaotic. People were talking about Regge theory, people were talking about -- I forget what it was called, the crazy ideas of nothing is fundamental -- bootstrap ideas. People were talking about lots of things. Current algebra was just one of them. That current algebra made sense if there were quarks, and quarks were asymptotically free, that comes later, right? Current algebra was a way of doing asymptotic freedom without saying asymptotic freedom. The algebra of the currents, you could derive from quarks, if quarks behaved as if they were free at high energies. But you threw away the quarks and didn't talk about them, and just used the currents. It seemed to work to describe physics.

ZIERLER: In terms of developing your dissertation topic, did bj essentially hand you a problem to work on that was related to his research, or did you mostly come up with this on your own?

QUINN: A little bit of both. Actually, it derived from a paper he'd written. I read the paper and pointed out, actually, a mistake in the paper. That lead to my thesis work. Interesting, actually, when I first went to Harvard, which was when I was applying for post docs -- eventually, my husband and I took post docs in Germany, but I had just finished my thesis and I was talking to Sidney Coleman. He asked me what my thesis was, and I told him, and he said, "Well, you know that's already been done, don't you?" I said, "Yeah, there was a paper published in Physics Letters by Abers, Norton, Dicus, and there's one other author on that paper, I forget who, from UCLA. They had started form BJ's paper, and they hadn't caught the mistake. So, they had the same mistake in their paper that had been in BJ's paper. So, I could point that out to Sidney. That was an important thing, because from then on, I was a person that Sidney recognized as having done something interesting. Later, when my husband got a job at Tufts, I came back to Harvard and asked for an office because I didn't get a job that year, and that was important to my future career.

ZIERLER: Right. Who was on your committee?

QUINN: BJ, Walecka, and probably Sid Drell. I don't really remember.

ZIERLER: So, this was not just a SLAC committee. You did have representation from the --

QUINN: From the physics department, right.

ZIERLER: Was it important to have Drell, for you?

QUINN: And there's also one other person. Stanford requires an outside chair on your thesis presentation. It was somebody from some other department.

ZIERLER: Was it important to have Drell on your committee?

QUINN: I wasn't aware enough of whether it mattered or not. You could say that I was pretty oblivious. I just went ahead and did what I did. I didn't pay much attention to what I needed to do in order to make it work for me.

ZIERLER: On that theme of not necessarily having a grand plan, what were you thinking after you defended, in terms of post docs or faculty opportunities? Did you know you wanted to stay at SLAC, I guess is the question?

QUINN: I didn't stay at SLAC. I spent seven years elsewhere and then came back to SLAC.

ZIERLER: You were a research associate, though, through January '68.

QUINN: Oh, that was until my husband finished his thesis.

ZIERLER: Oh, I see. So, you hung around.

QUINN: I was married at that point, and we had applied for -- another story, I'll get to the full story -- we both had post docs lined up at DESY. This was just bridging. SLAC was nice enough to pay me a post doc salary for six months as a bridge between my finishing my PhD and my husband finishing his PhD.

ZIERLER: Right. So, how did the DESY opportunity come about? Were you looking to solve the two body problem, essentially? Was it no more than the fact that you both got post docs?

QUINN: No. We decided we would like to take a post doc in Europe, and we applied various places. Yes, it was a two body problem. We were looking for two jobs. We actually both had an offer from Daresbury, a particle physics lab associated with Oxford, and we got a telegram from DESY. The telegram said, "Applications accepted. Letter follows." And foolishly, on the basis of that telegram we turned down the Daresbury offer. Then, the letter came from DESY. It was addressed to Dan. They'd offered him a job. And at the end of the letter, it said, "As long as your wife works for us too, we will increase your combined family salary, too." The difference was less than half of what they'd offered him. They had some weird logic to this. At that time, probably because of weird exchange rates, they paid American post docs more than they paid German post docs. They figured we did not need two American salaries. So, they offered him an American salary, and me the difference between that and two German salaries. The second year I was there, they paid me a full German salary.

ZIERLER: Oh, good. I would have been happier to hear that you were the one who got the full salary, and it was your husband -- that way we could be certain that sexism wasn't part of the equation.

QUINN: It was very clear when I arrived. Basically, Hans Joos sat me down and said, "Look, we know that you're just here as a spouse and you're interested in physics, so you can be part of this. But if you don't want to work, you don't have to work, because we're not paying you very much." I forget exactly how he said it, but it was something along those lines.

ZIERLER: Did you want to smack him for that?

QUINN: That, and other things. I was very frustrated trying to work with him. The next thing he said was, "As a zeroth approximation, everybody here does what they choose. But there's a first order perturbation on that. There are certain things which must be done." And he brought out a list of things which must be done. Fresh PhD that I was, I figured I was being told that I had to do one of those problems. I wasted six months, at least, more probably, trying to work with him on one of those problems. Well, we never saw eye to eye. He was wrong. But it was just very, very frustrating.

ZIERLER: Helen, what did you know about DESY before you got there? What was its reputation coming from SLAC?

QUINN: It was a particle physics lab. It had an accelerator. At that point, Dan's offer was with Sam Ting's group, and they seemed to be doing some interesting physics. It was a good opportunity for him. I think it actually destroyed him as a physicist trying to work for Sam Ting for two years, but that's a whole other story. I was, in a sense, just saying, "Okay, we're going to Europe, and this will be nice. There are enough good people there that I can do something." I was not very savvy about planning a career or thinking about how this will build to the next step. I just said, "This will be an interesting way to spend a couple of years."

ZIERLER: Had you been to Europe before this?

QUINN: I had been on a tourist trip one summer by myself, I think between my junior and senior year at Stanford.

ZIERLER: How international was DESY? Were there people from all over the world there, or was it mostly a German enterprise?

QUINN: It was mostly a German lab with a certain number of American post docs mostly around the Sam Ting group.

ZIERLER: English was the lingua franca, or you had to pick up German for this?

QUINN: The second year I was there, I did physics in German. Obviously, everybody spoke English, but most conversations happened in German, depending on who you were talking to. There was also a much more hierarchical structure than at SLAC, of course, in terms of status. That was another funny Hans Joos story. He was introducing me to people, and he said, "Herr

Professor So-and-so, this is Frau Dr. Quinn." Then he said, "We are very informal here -- Herr So-and-so, this is Frau Quinn."

ZIERLER: The German version of informality.

QUINN: Right, and German distinguishes if the "you" is a familiar or formal. There's not even a plural that you can use with both familiar and formal. If you're in a group of people, some of whom you speak in the formal, and some of whom you speak in the familiar, you have to say, "you and you." There's no plural "you" that can include both of them.

ZIERLER: Right. How was life in Hamburg? Did you have fun?

QUINN: Yes and no. Being in Europe and being able to visit the areas round about, and Hamburg itself is an interesting city. It has wonderful opera. Dan was working long hours in the Sam Ting group, because that's what Sam required, and it was difficult in some ways, but it was an interesting time for us.

ZIERLER: Did you get to travel around a lot?

QUINN: We went to summer school in at Erice, in Sicily and we drove there and back. That was a big tour, and then the next summer we did some driving tour around Austria. Actually, I did, with Dan's brother and Dan's parents came over. But Dan stayed and worked with Sam. Coming back from Erice, Dan flew home to get back to work, and I drove home with his brother and brother's boyfriend.

ZIERLER: What was the work that you were involved with at DESY? Did you see this as a continuation of your theoretical work from Stanford?

QUINN: Unfortunately, no. I should have continued in that direction, but there weren't people working in that area. As I said, I felt I had to try and work with Hans Joos on the problem he suggested. The problem was trying to understand the shape of the rho meson in photo production as opposed to electro production. It's slightly different, and I don't think anybody really fully understands that, even today. Certainly, we're not going to understand it by making a model of a rho as a non-relativistic resonance of two pions, which is what he was trying to do.

ZIERLER: Was it difficult setting up your next opportunity from DESY?

QUINN: As I said, I had a year without a job after DESY. Dan got a faculty offer from Tufts, and I told him if there's any place in the country where there ought to be two jobs, it's Boston, because there are so many universities in Boston. So, accept that, and I'll try to find a job. And I did not find one. So, then I had a year where first of all I decided I'd be a high school teacher after all. I took education courses, and I did student teaching in high school.

ZIERLER: How did you like teaching in a high school?

QUINN: It was my first time in an American high school, so I loved the teaching and I hated the high school. I certainly did not like supervising study hall. I enjoyed working with the kids, but I did not enjoy the social atmosphere of the high school at all. Somewhere towards the end -- that was sort of the first semester of the academic year -- I also was pregnant, and about to have a child. So, towards the end of that year, I happened to run into Joel Premack, who was then a junior fellow at Harvard, who'd been a graduate student a couple of years behind me at Stanford. He said, "Why don't you come to seminars? Why don't you come visit us at the Harvard physics department?" So I did, and that was really the time where the standard model was just beginning to take off. In fact, the ‘t Hooft and Veltman paper on dimensional regulation had just been published. So, I started talking with Joel and Tom about that. The next semester, I was a visitor at Harvard doing particle physics with Joel and Tom. Actually, I think we did the first one loop calculation in the modern weak interaction theory.

ZIERLER: Helen, with these early collaborations, what did you see as your contribution in terms of your expertise and your interests?

QUINN: I think I was a good calculator. I was quick at seeing how to take on a problem and do it. Tom and Joel both were too, so I can't say that's -- just being interested in the same problems and working together on them and having collaborators. I don't see a very clear division. We were all contributing to the work. Unfortunately, at a certain point, I was also the mediator between Tom and Joel who were not seeing eye to eye on certain things.

ZIERLER: In the theoretical realm, or in the interpersonal realm?

QUINN: How you wrote a paper. How you described your work. They each had different ideas about how it should be written up, if I remember rightly. It's a long time ago. I don't remember exactly what it was.

ZIERLER: It's quite a transition to go from just visiting to being on the faculty.

QUINN: Well, I was visiting -- as I said, that was the second semester -- somewhere in the middle of March, Sidney Coleman came and offered me a post doc for the next year. I was actually talking to people at Boston University about a more education oriented physics job, but the theory group at Harvard had gotten money for an additional post doc late enough in the year that they thought the pool was pretty minimal. So, they offered it to me. That's the way I read Sidney's talk. So, they offered me a post doc for the coming year, and I thought about it, and I took it.

ZIERLER: When did you start to work with Georgi and Weinberg?

QUINN: I think it was probably after I'd become a post doc, but certainly I became part of that group once I had an office at Harvard. I was part of the theory group and talking with Coleman at Harvard. There were six of us, right? There was Weinberg, Glashow, Coleman, Appelquist, Georgi as a junior fellow, me, Alvaro de Rújula was there as a post doc, maybe the second year I was a post doc there. Politzer was a graduate student working with Tom Appelquist.

ZIERLER: That's amazing. What an amazing group.

QUINN: It was. You couldn't not be excited about physics at that moment. And another person who came as a postdoc was Enrico Poggio. People turned over.

ZIERLER: What do you remember from those conversations in the group as the most fundamental or exciting questions that were being investigated?

QUINN: Well, we were doing weak interaction physics with quarks with the new weak interaction theory. There was nobody spending a lot of time thinking about strong interactions, although, clearly, those ideas were coming along. So, David Politzer, who was Tom Appelquist's graduate student, started doing the calculation that was the asymptotic freedom calculation. Actually, he would have published it by himself if he hadn't come and talked to me and Tom about his calculation. We asked him if it was gauge invariant, which should have been obvious to us, but it wasn't. We sent him back to think about that, and because of that his paper and that from Wilczek and Gross came out at the same time, rather than him being ahead of them. What the connections are is always hard to tell, because Sidney Coleman went to visit Princeton, too.

ZIERLER: Helen, I wonder if you ever felt like an interloper in a boys' club, or it wasn't like that at all?

QUINN: I think, as I said, the fact that I grew up with three brothers, arguing with the boys, it just never bothered me to be the only woman in the room. There were times it was awkward, not only at this point. My daughter was born in March of '71. She spent the first six months of her life in the Harvard physics department. My son was born when I was an assistant professor in '74, and he spent the first six months of his life in the Harvard physics department. And I nursed them in seminars, sitting in the back of the room properly covered over. I didn't think of myself as being radical. I was just doing what I needed to do to do what I wanted to do.

ZIERLER: Of course, that's only one part of the equation. There's how you think of yourself, and there's also how others regard you.

QUINN: Yeah, so people must have thought I was weird.

ZIERLER: But the point is you didn't pay it much mind.

QUINN: I didn't. I just did what was interesting to me.

ZIERLER: How closely -- the November revolution, the psi, how closely were you paying attention to the incredible things that were happening at SLAC during this time?

QUINN: Well, you've got to remember that Politzer and Appelquist wrote a paper about charmonium that came out before the discovery.. I worked with them on that work early on, just as Politzer worked with us on my other famous paper, which is the hierarchy paper. The coming together of the coupling constants. The conversations on the whole were everybody talking about everything. I had talked with Tom and David about the charmonium work in the early stages of that work, just as David had talked with us about the hierarchy work in the early stages. But because of who was where, when, authorship on the papers is different from who talked to whom. You can never really trace everybody who was involved in developing an idea just by looking authorship, or even acknowledgements. They don't happen, necessarily. Certainly, I was very well aware of the charmonium paper, and the day that the talk at SLAC was given about charmonium, I walked in and Tom Appelquist said, "Did you hear the news?" I gave my classes, which were electromagnetism, I talked about charmonium in class that day.

ZIERLER: Hot off the press.

QUINN: I told them what it was, that it had been seen at SLAC. Obviously, people had lots of ideas, but we at Harvard were totally convinced from the beginning that this was charmonium. Of course, yes, confirmation was needed. Yes, you needed to see open charm eventually.

ZIERLER: In these tremendously exciting, fundamental moments of discovery, I wonder what jumped out at you as new questions that arose as a result of these advancements.

QUINN: You can even go trace it in the papers. The question to which the answer is there's not just charm, there's also the tau -- what's happening with the total cross section (electron-positron to hadrons)? A couple of weeks before, or maybe a month before the discovery, Bert was giving seminars about the rising total cross section at MIT with Tom and me in the audience saying we know it's charmonium, but he's just not looking. It's going to be this, and that’s that. But the problem was the cross section it was already getting bigger than what you would get by including just one additional quark. The total cross section is the sum of the square of the charges of the particles being produced. That's the quarks plus the leptons. So, the fact that there was tau was messing up the total -- because tau decays to hadrons. So, the total hadronic cross section was going up more than it should by just one additional quark. You can even see a paper which I wrote with Mike Barnett, where we were trying to explain the data, because our computational methods were pretty sloppy, and because the data had large uncertainties we thought there could be even two additional leptons there. It's not just a quark, it's a quark and lepton that are contributing here. So, that was one of the puzzles. The whole -- what's going on in that period is all of the pieces of the standard model are getting built and put together. At the same time, Georgi and Glashow are talking about grand unification. They're going the next step and saying that if those are the pieces, they all fit together in this very interesting way. SU 5 has room for all of it.

ZIERLER: I'm curious how closely you were paying attention to string theory during this time.

QUINN: String theory, per se, was not really a topic-- the modern version of string theory wasn't invented. There were forerunners of it, and certainly, I remember Lenny Susskind gave a seminar about strings, but the strings he meant were QCD strings. The fact that when you separate a quark from an antiquark-- this is a physics story I like to tell. e+, e- goes to two charm quarks, charm and anti-charm. As those quarks move apart, there's a QCD string between them. There's a region of QCD fields, which is confined, and which eventually breaks up and gives you multi-particle final states, unless you're below threshold, in which case it can only give you charmonium and pull them back together. The physics of that process is an interesting question for which you have to understand both QCD and weak interaction decay processes (for the decays).

ZIERLER: Every day must have been exciting in this group.

QUINN: It was. Some days were more where you're doing a calculation to understand something you thought about yesterday, but there were a lot of questions. Many more questions than answers.

ZIERLER: Were there de facto leaders, or a hierarchy in the group, or was it really just equal playing field, and everybody shared ideas?

QUINN: Well, the hierarchy was there were full professors and assistant professors.

ZIERLER: I mean culturally, the way people interacted.

QUINN: We worked and talked pretty much as a group of the whole. There were subgroups -- there were people who gravitated to working with Shelly, and the people who gravitated to working with Steve, and others worked with Sidney. Steve and Shelly didn't work together. But those groups were overlapping and intertwining, too. So, at the level of the younger people in the group, we all talked to everybody.

ZIERLER: In the Harvard system, when you're promoted to associate, that's not a tenured position.

QUINN: That's right. That's just a seniority promotion. It's not a serious promotion. It was kind of made clear to me that the likelihood I would be Harvard professor was pretty low. At that time, neither Harvard nor Stanford promoted from within in any particular way. Very few people would get tenure at Harvard from being an assistant professor at Harvard. You got tenure at Harvard from a national search. Tom Appelquist was not senior faculty at Harvard. Howard Georgi, yes, was promoted from within. He was one of the first.

ZIERLER: This was not any comment only our capabilities, it was just not how it was done.

QUINN: To some extent. First of all, there were no women senior faculty, and it wasn't a comment on that. It just wasn't an expectation. Again, when I was told I should start thinking about how I could position myself for jobs elsewhere, basically, I heard the message. I didn't do much because Dan was teaching at Tufts, but when Dan decided he was making a career change, and had a job opportunity in California, I quickly ditched Harvard. I had a Sloan Fellowship, so I could take a leave of absence and come to Stanford.

ZIERLER: What was his career change? What was his opportunity in California?

QUINN: He was, as I said, an experimental particle physicist and working with the group at Tufts. They were doing experiments at SLAC, actually. He got interested in an area called decision analysis, and using his privilege as a faculty spouse, he took some courses in decision analysis at Harvard. Then, there was a decision analysis group at SRI who were, some number of them, ex physicists, who had moved there. He called one of his friends in that group, just after Christmas. He was asking if there was a chance at a summer job to try out this new career. His friend told him, "No, we don't have such a thing as summer jobs, but we're looking for people, so if you wanted a real job, you should apply." He came and told me that, and I said, "You go back upstairs and call him, and tell him you're interested in a real job." So, he got an offer from SRI for a job in decision analysis. As I said, I had a Sloan Fellowship, so I could pay my salary on leave of absence from Harvard from my Sloan Fellowship for that semester. So, I took leave from Harvard and came to Stanford, assuming I was not going back, but with no particular job opportunity here. Technically I went from being an associate professor at Harvard to being effectively a post doc level again at SLAC for a few months, and then moved up to a regular staff position at SLAC.

ZIERLER: How had SLAC changed since you had left?

QUINN: It had gotten bigger. It was more established. In '67, '68, it was a really new lab. I think the other piece, which I was not particularly aware of at the time, but because of the ring, it had become also a synchrotron radiation facility, which of course became the future direction of SLAC. Now all of its physics is X-ray physics. It's not high energy physics lab anymore.

ZIERLER: What research projects did you get involved with when you came to SLAC?

QUINN: Well, I didn't come to SLAC. I came to Stanford as a visitor, and that's when I did the work with Roberto Peccei.

ZIERLER: Did that happen with Roberto when you got there, or this was sort of the plan upon arrival?

QUINN: No, it wasn't a plan at all. My collaboration with Roberto began because we were talking after Gerard ‘t Hooft] gave a seminar on instantons at SLAC. Driving back from that seminar we started talking about instantons, and trying to understand them, and that grew into a collaboration. Our first paper together is one that nobody reads. It's in Nuovo Cimento, about instantons. I gave a talk at the memorial event for Roberto last week, and that is recorded and they're going to post it at UCLA, so you can get into that history there. But that led to our collaboration which is the well-known Peccei-Quinn papers.

ZIERLER: At this point, he was an assistant professor or associate professor?

QUINN: He was an assistant professor at Stanford, and like Harvard, Stanford didn't generally keep their assistant professors, so he went to the Max Planck Institute in Munich, and then was head of the theory group at DESY before he came back to UCLA. But then I moved to SLAC and started collaborating with Marvin Weinstein -- Marvin was thinking about lattice theory, and I was thinking about instantons, and we started talking about how to understand theta parameters, and theta vacuo in lattice theories, and that built into all kinds of other work with Marvin in a completely different direction.

ZIERLER: To stay with Roberto for a second, these fundamental collaborations are always so interesting. When you're asking why QCD preserves CP symmetry, are you both asking the same question, and it makes sense for you to work on this together, or are you asking different questions, and you decide to team up because those different questions are mutually valuable?

QUINN: Actually, Steve Weinberg was then visiting at Stanford, too. Steve was asking that question. Steve was saying we really need to understand why the theta parameter is so small. Roberto and I started talking about could we answer that question? Could we find a theory? Roberto was the one who turned the question into: can you find a theory where it's automatically small? The whole story is in my UCLA talk, but effectively, we came up first with a wrong answer, and again, we talked to Steve. We hadn't even told him our idea, we just told him we were thinking about CP. Steve gave us a lecture on how to think about CP, which made me realize what was wrong with our wrong idea. So, we never told him what we were thinking, and with Steve's input, we had the right direction to take our wrong idea and turn it into a right one.

ZIERLER: How did you do that? How did you turn it into the right idea?

QUINN: I have to go through to logic. The logic is theta is automatically irrelevant if any quark mass is zero, because you can just change the phase of the quark field and make the theta parameter zero. That's fine. No quarks are massless, so that's not the answer. But in the hot early universe, all quarks are massless. So, why can't you just tune theta back then and guarantee that it comes out zero. Our wrong answer is, when you tune theta back then, suppose you're in a hot early universe, and there is some theta parameter, I tune the quark field to make that theta parameter zero. But the problem with that is when I change tune theta, I also tune phases of Higgs-quark couplings. And then, when the quarks do get their mass, that mass is a complex. and you have to tune the quark fields again to make the masses real, and that puts back a non-zero theta.. in general, when the masses are real, the theta is non-zero. So, how do you make it come out that when the vacuum expectation value arises, it's just in such a way that the phases of the quark masses and any existing theta parameter balance off, so that when you tune the quark masses to real, you're also tuning the theta parameter to zero. The answer is the symmetry. You have to add additional symmetry that makes the Higgs potential theta dependent in such a way that naturally the Higgs vacuum expectation value will acquire a phase so that the quark mass phases cancel the theta parameter.

ZIERLER: When did you realize with Roberto that you really had something fundamental and it was time to publicize this to the world?

QUINN: As soon as we had that idea. Once we came up with the version -- in fact, Roberto was off on a trip somewhere. We were very, very close and then I saw the picture with the additional symmetry as a Higgs potential picture. I saw it in my head before I wrote it as a mathematical thing.

ZIERLER: Which means what -- a Higgs potential picture, can you describe what that means?

QUINN: Well, the idea that the Higgs potential has a minimum, which is theta dependent. It's a pseudo symmetry. It's not a symmetry. If it were a symmetry, it would have a minimum which was independent of all parameters. But the symmetry is broken by the QCD vacuum, and that makes it theta dependent. So, it tilts, and there's a minimum and a particular value of theta. That visualization, and what we had to do, was how I got the idea. It was definitely about how does the universe cool and fall into this place? Well, you've got to tip the potential in just that right way.

ZIERLER: What are the theoretical foundations if the starting point is that in the early hot universe, quarks are massless? What are the theoretical foundations that you're building on, going from there?

QUINN: You're building on the knowledge of the standard model. Let's assume we have quarks, Higgs-like scalar fields, and gauge fields. We know the cosmological history that the vacuum expectation value of the Higgs field is not significant or meaningful in the hot early universe. So, thinking about that, says okay, well, then, of course, what happens when you get a vacuum expectation value? That depends on the Higgs couplings to the quark fields. Clearly, we knew those were the pieces we had to look at. Then, if you look at the standard model, because the up quarks and the down quarks get their mass basically from the complex conjugate of the same Higgs field, you've got no freedom to put that additional symmetry there, to do anything that will make theta equals zero automatic. So, you have to add additional Higgs fields in order to get the idea to work, in order to have the space for that additional symmetry. In fact, the simplest version, which is to add one more Higgs doublet, has been ruled out, because it doesn't have enough additional parameters. That's the model that we put in the paper. But we kind of knew that it was likely that was not a phenomenologically viable model. So, our paper doesn't investigate phenomenology, which was a mistake because there's a generic piece of the phenomenology, which is the axion. That's how come we didn't mention the axion or think about it. We so wanted to publish the idea that we didn't want to take the time to investigate the phenomenology before we published-- so, we put it aside.

ZIERLER: So, you considered it, but then you consciously --

QUINN: We considered that there would be additional particles, and there would be a whole phenomenology, and there weren't very many parameters. Our model is probably not viable but let's not worry about that. Let's just give it as a toy model. We didn't see that there was a pseudo Goldstone boson. We weren't thinking clearly. It's funny, now you express it clearly, say it's a symmetry, but at first we just had a model that added additional pieces and made it possible to make the theta parameter come out the way we wanted it to, and we saw that that was not the only model that would have the same property, but we did not see that all such models also had another feature, this is the axion.

ZIERLER: When do axions enter into the equation?

QUINN: So, because you add an additional symmetry, and it is a symmetry which is not exact, but if it were an exact symmetry, the axion would be a Goldstone boson. So, the axion would be massless. Well, that's clearly ruled out. But the additional pseudo Goldstone boson is the axion. That's why the axion is a very light particle.

ZIERLER: This occurs to you? These are comments to your paper? What are the larger ideas that are swirling about for axions to become part of the equation now?

QUINN: We wrote the paper. The theory, if you look at it, has an axion on it. But we didn't look at it. Immediately, we wrote the paper, both Steve and Frank Wilczek saw that there was a pseudo Goldstone boson, a very light particle, indicated by this theory. Steve actually called me up and said, "Did you notice?" I said, "Yes, of course you're right, but I didn't notice." And then Steve said, "Well, in that case I'll publish it myself." Steve was thinking that maybe we were about to write a second paper that told about the axion, but we weren't. I honestly told him that we weren't, and so he published that idea. It's not really an idea. It's an observation about our theory. Frank Wilczek, likewise. The name "axion" actually comes from Frank's paper.

ZIERLER: Right. So, obviously, the term "axion," you're not using this term.

QUINN: No. In fact, when we started talking about it at SLAC, we called it "the Higglet." But I don't think Phys Rev would ever let us use that name.

ZIERLER: What was your next move after your collaboration with Roberto?

QUINN: As I said, I had the job at SLAC and started working mostly with Marvin Weinstein, and Sid Drell. It was lattice theories of quarks, and trying to understand strong interactions, but not doing lattice calculations, but rather trying to understand how you can write a theory that doesn't double the quark spectrum.

ZIERLER: Were they working together, and then you became part of that trio, or did you work with each of them separately?

QUINN: I don't remember. I think I was working -- it's hard to reconstruct. I don't remember. I think possibly Marvin and Sid had written -- you'd have to go back and look at the publications. There may be some papers on lattice theories of quarks, or QED on a lattice, or something like that, that had been written between Sid and Marvin. But I came in with the question of -- yeah, they must have been because I remember going to Marvin and starting to have the discussion about what is the theta parameter if you have a lattice theory? What are instantons, what is theta parameter in your lattice theory? So, that was where the conversation began, but it went in other directions, and I got involved working with Marvin. Most of my career at SLAC, he was my major collaborator.

ZIERLER: Did you see the collaborations with Marvin and Sid as a logical transition from what you were doing with Roberto, or this was new stuff for you?

QUINN: Both and neither, I'd say.

ZIERLER: I mean intellectually, not scientifically, just in terms of where your mind was going at that point.

QUINN: My mind goes to questions, and I talk to people around me about those questions. The question of lattice gauge theory looks interesting. It seems to be offering options for understanding confinement.

ZIERLER: So, you're asking these questions.

QUINN: Right, and then I got interested. Understanding -- and this is another piece of what's happening at that time. Understanding confinement, understanding QCD in a physical sense. Lattice theory was offering options that nobody else had good answers to. First of all, the spectrum of hadrons, and secondly, what does confinement look like? We don't really know the answers to that from anything other than lattice theory today. It's also a matter of who it resonates to talk physics with. You talk physics with people and sometimes it takes off, and sometimes it doesn't. I think collaborating with Marvin came because we enjoyed working together. Part of what makes a good collaboration is a good relationship as well. It's a funny one because Marvin shouts, right? He's loud, and we argue. You might think that for a woman in physics that wouldn't be a good kind of collaboration, but I was used to that because of my upbringing. Being loud and forceful didn't put me off. What interested me was being really interested in the ideas, in trying to understand something at a deeper level. That's what I did with him.

ZIERLER: What do you see as the primary contributions and outcomes of these collaborations?

QUINN: Not much that's got much traction. We played with formulations which make sense in the Hamiltonian approach to physics on a lattice, but don't make sense -- are not easy to transform into path integral approaches. So, that's why it has no traction, because most lattice theory work is path integral calculations. Our work is still interesting as an answer to what does the spectrum look like, but it's a little bit artificial. One of the problems of quarks on a lattice is when you put a quark on a finite lattice, you finish up doubling, basically because the spectrum is more like a sine wave. You can't get a linear spectrum. You get a quark near zero, and a quark near pi. How do you avoid those additional states in the theory? As you make the lattice derivative longer and longer range, you can push the quark closer and closer to pi, and you can actually get it out of the spectrum, or get the first state to very high energy, even though if it were a continuous spectrum, it drops down. So, it was a kind of artificial way to avoid the doubling. But the question is about how do you write a derivative on a lattice? Is it just next nearest neighbor interactions, or is it long range? If you write a long range derivative, then when you try to turn it into four dimensional theory, you can write a long range derivative in Hamiltonians, but what happens to the time component in the derivative? What do you do with time? It really messes up the theory if you try to make long range time derivatives.

ZIERLER: I'm curious if you were thinking about dark matter before it even had that term.

QUINN: Not really. I mean, we weren't thinking about dark matter when we invented axions either. We were thinking about cosmology. We were thinking about the evolution of the universe, but we weren't thinking about dark matter. It's interesting the way physics does this to you. Somebody pointed out -- if you read Frank's talk at the Peccei memorial, he pointed out that Heinrich Hertz, said that electromagnetism, the equations -- once you derive Maxwell's equations -- they know a lot more than you do. When Maxwell wrote his equations, he did not know about a lot of things that are consequences of Maxwell's equations. For example, radio waves and antennas. Right? So, the equation allowed you to invent radio physics, because you had those equations. So, they knew a lot more than Maxwell did. The same is true for QCD, with the standard model. Once you've written the equations of particle physics, they know a lot more about particle physics than you do. It's quite amazing what you can learn by investigating peculiarities of those equations. The mathematics can lead you in new places that you wouldn't have thought about just from what you knew before you started looking at that mathematics. That's where it went with Peccei-Quinn Symmetry. We had a question, we played with the theory, we invited a new kind of thing, the axion, and then it takes you dark matter and these days it takes you to new ideas in solid state physics, which Frank talked about in that talk. It's interesting. Mathematics is richer than our own knowledge of the world sometimes.

ZIERLER: Where are hadrons in all of this?

QUINN: That's why I was so interested in understanding confinement. That's another piece of mathematics, which, certainly, when quarks were invented, they were considered a mathematical device -- why did Gell-Mann not talk about quarks as real? Because he couldn't make the physics of confinement make any sense, among other things. So, understanding confinement is understanding how you can have a theory with fractional charges and never see fractional charges. That's an interesting question. I gave you the physical description earlier. The quarks are moving apart, but the QCD field between them is energy dense enough to make more quarks and anti-quarks and finish up with a bunch of hadrons. Thinking of that as a physical process helps you understand why we do not see the quarks in isolation. But we don't know how to calculate such physical process. For example, this is another paper I wrote at SLAC with a post doc, Subhash Gupta. Imagine a world in which there are no light quarks. What does light mean? It means light compared to the QCD scale. In that world, if you have an e+, e- collider and you produce quark and antiquark going off back to back, you can't decay into pions, because there's no light pions. The final state is one charmonium plus many gluons, or one hadronium and many gluons. But you can't calculate that in perturbation theory. In perturbation theory, that's an extremely suppressed state. If you have a long energy range before you can make even two charmoniums, and there's a lot of space in that area, there's a bunch of charmonium like resonances, and whatever energy the e+, e- collision is at, you make one onium and many gluons, until you pass the threshold to make two oniums. Just thinking about the physics of that imaginary world shows you that perturbation theory only works for a limited set of calculations, and for some of those only because there are light quarks..

ZIERLER: Okay, perfect. Helen, I want to ask a bit of a broader question. Theorists can work essentially anywhere --

QUINN: Today.

ZIERLER: Today, right. I'm curious what, if any, advantages were conferred by just being in the environment at SLAC?

QUINN: Well, first of all, as you said, today theorists can work essentially anywhere. The archive gives you preprints right away, wherever you are. But that was certainly not true in the '70s or '80s. The preprint library at SLAC got everything. The people who came to give seminars at SLAC came from everywhere. As at Harvard, it was a group that was well-funded and in the center of the network of people who were at the forefront of the field. That's a huge advantage. Today, it's less so, but still, just being where those conversations are happening is not replaced by being able to talk to somebody you already know by internet, as a post doc coming into a new place. Being there and being part of conversations with new people is part of gaining new ideas and new perspectives. So, I think the location and the people around you is always important, even in the day of the internet. It's less important today than it was, and once you're established and have those relationships, it probably matters little. But as you're establishing yourself as a scientist, building those relationships and getting to know and hear people who are actively thinking about interesting problems, that's a very important part of becoming part of that community.

ZIERLER: That's such an important point.

QUINN: Over the years I was at SLAC, it was a center of theoretical particle physics.

ZIERLER: Where the influence of visitors was so important as well.

QUINN: Visitors, post docs, lots of people would informally come regularly in the summer to SLAC. There was a strong connection to people from Israel, for example, who would come in the summers. So, you were part of an international community, which was a wonderful thing, in a time before the world was so international. Now it's much easier to have international collaborations and not be meeting face to face, but prior to thirty years ago, that wasn't really the case. Certainly not 40 years ago.

ZIERLER: I'm curious about your work as assistant to the director for education and public outreach. First of all, whose idea was it that there needed to be a director for education and public outreach? Does that go back to Panofsky?

QUINN: No, no. It kind of just grew, and there was a period where the Department of Energy was encouraging labs to have education and outreach programs and funding a small amount. Like, I was running summer workshops for teachers. Those were initially funded without specific funding through SLAC from the Department of Energy, because it cost so little to do it. So, the order of things was I was doing it, and then the Department of Energy started funding such programs explicitly and said, "We're having meetings for education directors from the labs, so who's your education director?" Everybody said, "Well, it's Helen." So, it wasn't that there was such a title or such a position that existed. It was that position became a title because I needed to go to those meetings.

ZIERLER: I'm curious if the public outreach component might have had something to do with the fallout following the SSC, that the national labs needed to manage their reputation, that they needed to be able to publicize what they were doing. Do you think there were any connections there?

QUINN: Possibly, partly. I mean, no, because it existed prior to and during the SSC. SSC had an education director and had an education outreach group. Fermilab was doing it before we were doing -- well, different things, but K-12, precollege piece of it definitely came at Fermilab before it started at SLAC. But my taking on that role at SLAC actually came from connections to the program -- teachers who'd been at Fermilab, who came to SLAC and said, "Now, what's SLAC going to do for us?" So, it wasn't so much the public outreach -- that title was there because I also managed tours. I managed the people who ran the SLAC tour program. SLAC had a tour program before I had that role, but it happened that they lumped that together with other things, and gave me, as a staff member, responsibility for that set of things.

ZIERLER: Helen, I wonder if during this time you got a front row seat about how relations with the Department of Energy were changing with regard to SLAC.

QUINN: Everybody at SLAC had a front row seat.

ZIERLER: Yeah, okay. Fair enough.

QUINN: It was pretty obviously happening.

ZIERLER: In what ways? What was so obvious?

QUINN: Well, there were several pieces to it. One was safety issues, someone was killed in an electrical accident at SLAC, and all of a sudden, the Department of Energy was much more rigorously looking over the shoulder of them. The other was SLAC essentially, and Bert realized this, had to evolve to survive.

ZIERLER: What did evolve mean? What did that mean from your vantage point?

QUINN: Well, one piece of the evolution had started symbiotically, or we used to say parasitically. Synchrotron radiation physics (SSRL) at SPEAR came about because some people realized that if they just put a hole, they could get some pretty intense X-rays. The idea of X-ray light sources at accelerators was not common when SLAC started doing synchrotron radiation physics at SSRL. Since the ring was built for high energy physics, they didn't get to say what energy they ran it, or when they got the beam, it was when particle physics was running, they could take the X-rays if they could put a window in and get them. So, that's why it was parasitic. Actually, it was more symbiotic than parasitic, because it didn't actually hurt anything. Particle physicists weren't using the X-rays. They were a wasted product as far as high energy physicists are concerned. The fact that electrons radiate when they go around a ring is a nuisance when you're trying to make high energy electrons. But you can't make them go around a ring without them radiating, so if the ring is high enough energy, there's going to be radiating X-rays, and be a more intense X-ray source than we can make any other way. So, that was the beginning of one of the directions that the lab has evolved, studying X-rays interactions with matter, which is now the major direction with the free electron laser. The last third of the accelerator is running a laser built in -- it's turned into an even higher intensity X-ray producing facility.

ZIERLER: When you were named chair of the Department of Particle Physics and Astrophysics, was this sort of a quantum leap for you, hierarchically, within SLAC, or was it the next logical move in terms of your contributions and accomplishments over the years?

QUINN: Well, you see, before they could make me that, they had to make me faculty. So, the real leap was when I became a faculty member, in terms of having a role -- before that, I had no role in academic decision making. I had no role in whatever role the faculty has in planning the physics program of the lab. I took on the education roles which were quite separate from that, but I did not have a voice of authority in the lab at all as a staff member. There's a big hierarchy between being staff and being faculty in terms of the power you have within the lab. For most of my career, I was a staff member, not a faculty member. So, those kinds of appointments were not possible. The transition was to become faculty, and it wasn't long after I became a faculty member that I became the department chair, too.

ZIERLER: Is it the same as in an academic department where in many cases, becoming chair is just your turn to do this service? Is it the same kind of thing?

QUINN: I think so, yeah. Pretty much. It's not a power at all. It's a responsibility, and it has a role in -- as much as the faculty speaks as a whole around issues in the lab, you have a role in mediating that voice with the lab management. It's a very weird thing having faculty in a national lab. It's kind of odd, because the national lab -- let's look at Berkeley versus SLAC. At SLAC, because it was built from the start internal to campus, the lab is a contract to the university. The employees at SLAC are Stanford employees, not government employees. Whereas, if you're at Los Alamos, or even at Lawrence Berkeley Lab you're an employee of a federal contractor. Therefore, at SLAC the roles within the lab are like the roles within a university, rather than like the roles within a government lab. The faculty are the people within the lab who control resources. In the theory group, that didn't really matter because you don't need resources to be a theorist, other than your computer. I guess, if I were doing physics that required large-scale computing, I'd have to look for money to buy the computer time, but I didn't need any money to do the science I was doing. But if you were an experimentalist, it made a big difference whether you were a staff member or a faculty member, how much you could control the science that you were doing.

ZIERLER: Right. Now, of course, 2004 turns out to be a very busy year for you, because you are leading the APS at this time as well. How did that come about? Is there a nominating process? Did you express interest in this?

QUINN: There's a nominating and election process, yes. It's a national election.

ZIERLER: Right. And were you specially interested in this kind of opportunity?

QUINN: I wasn't looking for it, no. It just came to me. I don't know who nominated me. Obviously, what they do is they call you and say, "If elected, would you be willing to take the job?"

ZIERLER: How long had you been involved with APS at this point?

QUINN: I had been involved with their education people, along with the education programs I'd done. Other than that, not deeply involved in APS. Again, these things come to you during your career, so I'd been on the executive committee of the Division of Particles and Fields some years earlier, but I wasn't much engaged in that at that point either. So, I don't exactly know why I was put up for election at APS, but they decided I'd be good at it before I ran for it, and I got elected. You make choices in your career. By being at a national lab, you're not in a path that goes towards things like department chair, or dean, or any kind of academic career path beyond faculty. I had taken the staff position because it was a great opportunity when we moved to California. What I realized fairly soon thereafter was that meant SLAC had absolutely no motivation to promote me to faculty. They had a limited number of faculty billets; the number being controlled by Stanford. If they had an open faculty billet, what did they gain by promoting me unless I was threatening to leave? I wasn't threatening to leave, because my husband had a good job here. So, they had me on a string, and when faculty jobs opened up, the first one I was kind of disappointed I didn't get promoted. Then, I thought it through and realized they're never going to promote me. So, I can either decide to make my life as a staff member, or I look for a faculty job elsewhere. I decided there are lots of advantages to being a staff member and having the flexibility that that position gave me, and I would do that. So, I stayed at SLAC and didn't have opportunities for leadership positions, other than the responsibilities I took for the education work.

ZIERLER: Helen, as president of the APS, I'm curious, at that vantage point, what did you learn about the physics community in general in the United States, that you might not have ever appreciated just working within the SLAC community?

QUINN: Certainly, I was aware of the breadth of physics at some level, but I saw that much more looking at all the different divisions and what they worked on and seeing the wider range of physics than you see sitting in one particular location. SLAC is one of the narrower locations, because it had particle physics and a little bit more with X-ray physics -- I was probably more aware of the physics of synchrotron radiation than most of the particle physicists because of the programs I ran for undergraduate students who would come for the summer. I had to be in contact with that part of the lab and organize a little course for the students who were there, when they heard about what was going on in that part of the lab. So, I was probably more aware a little bit broader than just particle physics, but really, I lived in a world of particle physics only. Certainly, as leader of APS, you look at the world of physics much more broadly than that. And the other piece was, of course, the journals. The whole issue, which still is not a resolved issue today, of access to the journals, and income from the journals, and how you play off those one against the other. The notion that access to research should be free doesn't allow for refereeing process or the archival process to be handled the way journals handle it. So, figuring out the model for the modern world of access to research, and the income that supports the long-term access. It's also the income that supports professional societies. There was a lot going on around that at that time that I got involved in. There was a battle. AIP, as you know, was founded as the publishing organization (printing the journals) for physics and geophysics. In fact, multiple societies at the time began to take their journals away from AIP and went to other publication avenues for getting them out and getting them printed, because AIP had become three times the going rate. Originally, it was founded as a cheap way to do it, but it wasn't providing that. So, it was a big fight, actually. It's so interesting from the woman in physics point of view. Millie Dresselhaus was the president of AIP -- I forget exactly the title, the chair, or the board of AIP, and we, APS, made that decision to go to other printers, and I remember a council meeting of the APS where Millie basically called in and said, "You can't do this to us." I said, "Yes, we can. As professional societies, we're collaborators and partners, but business to business, we have to function as a business. We cannot afford to leave our journals with you." It was a very tense discussion. Afterwards, several of the men sitting around the room said they'd never seen two women having a discussion like that before. We had to make a decision that was important for the survival, fiscally, of APS. It was not an easy decision for the survival, fiscally, of AIP, but that was something AIP had to deal with, and did.

ZIERLER: Right. Helen, during your time leading the APS, did you also see any opportunity to enhance diversity and inclusivity in physics, generally, for women, for underrepresented groups?

QUINN: That's why I ran the undergraduate summer program at SLAC. That program was originally started by Ernest Coleman, who is an African-American physicist, as a diversity program. I ran it as such, although it was difficult because the selecting of students could not be overtly based on race or gender particularly once the Department of Energy got more involved in the application and acceptance process. So, what was our criterion? Our criterion was we wanted to select those students for whom the experience at SLAC would make the most difference and give them the most opportunity. We wanted to select students who hadn't had obvious opportunities to become part of the physics community. That meant women and minorities, but also low-income students or students from small colleges where they had no contact with research physics. So, the categories on which we were counting diversity were much broader than just gender or race. We were trying to bring in students for whom the experience at SLAC would be an experience that made a difference in their careers. But it was a diversity program in terms of the diversity of the physics field.

ZIERLER: Was it successful?

QUINN: Yes, and the challenge to keeping it that way was that we had to select the students and then place them with faculty, rather than saying to the physicists, in general, "You look at the applicants and select the students you want for the summer." We put a barrier between the faculty just selecting the students that looked best to them, who would always be the white males from the best universities. We selected the students, and then we said, "If you want a student, you take it from this pool." That made it harder. That made it more work, for us running the program, to do the selection and the placement. But it did mean our program stayed diverse longer. Unfortunately, it's no longer run that way. Because of the way DOE funded the program, it moved to where the applications went through a general pool of DOE applicants for internships at labs, and DOE wouldn't even give us the race information on the students. You could read between the lines, and the applications, but there's not necessarily comprehensive. So, we did our best to select a diverse group of students and give them all good experiences in research experience for nine weeks at SLAC, and home them all together, and make that an intellectual activity too, what was going on in the house.

ZIERLER: Helen, during these years when you're so heavily involved with administrative issues, I wonder how important it was for you to remain involved in the world of theoretical physics. Were you keeping up with the literature? Were you trying to get your own work done, or was this sort of separate?

QUINN: As long as I was working at SLAC, I was writing papers in theoretical physics. Until 2010, I was still actively doing -- I never switched from being a physicist to an administrator. These other things, either chair of APS, or president of APS, or chair of the physics department, those were side things. Those were not my full-time activity. They took time, of course. They took time away from research, but I was hired as a physicist.

ZIERLER: You never let go of that.

QUINN: Well, when I retired, I let go of it totally. But I was going to meetings at the Board on Science Education while I was still employed at SLAC. Actually, part of why I retired when I retired was in order to transition to more education-oriented work, because I knew I couldn't lead the study I was about to lead and continue working at SLAC. So, that was part of the decision that this was the right time for me to retire. The other part was that Stanford was offering a good incentive to retire.

ZIERLER: What were some of the research projects that you were involved with during this time? What were some of the major things you were interested in?

QUINN: Well, that's another part of the story which we haven't really talked about. There was a period where I was working very much part-time because my son was sick for three years. I really almost dropped out. When he recovered, and I got back into doing physics full-time, I realized I had to look for something new to do. That was when they were just beginning to plan the B factory at SLAC. So, I got involved in the design and thinking about what you could do with such a facility and was part of the working on the physics of the B factory.

ZIERLER: What were the options at that existential level? What could you do with the B factory? What were the options besides that which was taken?

QUINN: It's not a question about what physics you could do. That we knew. How could you design -- you know you want to do B physics, and you need an accelerator of this particular type to do that. You know the parameters of the accelerator. Now, how do you design the detector to be able to maximize what you can get out of it? To do that, you need to think about all the different physics questions, or as many of them as you can think about, that you might answer with that facility, and then simulate what detector you need to be able to answer those questions. There was an international working group. My role as a leader of that group came from getting involved and being part of the discussion, but then I became one of the co-leaders, along with an English experimental particle physicist, on the production of something called the B Physics Book, which is a plan for -- here's the facility and the first half of the book was about the experimental facility, and the second half of the book was about the physics of interest and the detector capability needed to explore it. That task of producing the book meant coordinating a bunch of workshops all around the world, where people were interested in B physics to talk about what you can do with this thing, and then get people to write sections and make it come together into a coherent report, which then provides a basis for the experimental program of the new facility. People are still referring to that book and thinking about new things to do in that facility. That was from -- let's see, let me think about the years. Probably 1988 on, I was doing B physics. My theoretical physics program was related to the experimental program of the B factory.

ZIERLER: What were some of the major research questions for you that were relevant for the B factory?

QUINN: Certainly, confirming and understanding CP violation in the standard model, and all the different channels we can think about. How can you untangle -- again, when you're trying to do weak interaction physics, you have to understand how to untangle the weak interaction physics from the strong interaction physics. So, for example, one the channel of interest is B goes to rho pi. There are certain weak interaction parameters that are involved in that, but there are also strong interaction parameters. How do you extract the weak interaction parameters without being confused by the things you don't know about the strong interactions? That's part of the work I was doing. That's an example of paper on isospin analysis on those channels, and how you do that. How you can separate the piece of physics that you're looking for from other pieces which are not so interesting.

ZIERLER: How successful was the B factory in your estimation?

QUINN: Extremely. It wasn't as exciting as we hoped it might be, because we hoped it might see a big hole in the standard model, and it didn't. But that's physics.

ZIERLER: Now, is that to say that it came with the expectation that the standard model had holes in it?

QUINN: Well, we do know that the standard model is not the full story. One of the ways in which it is not the full story is that the CP violation in the standard model is not sufficient to explain the CP violation in the universe. We thought that was so, but we weren't sure. Now we're sure.

ZIERLER: But not because of the B factory, or because of the B factory?

QUINN: Because of the B factory, now we're sure. Now, all the pieces that were speculative about CP violation in B decays have been pretty well pinned down. There are a few little anomalies left that people are still thinking about, but there aren't the kinds of anomalies that -- there was enough ignorance that there was room for big anomalies. We know there must be more CP violation than just that CP violation in the standard model, because we know the universe is really asymmetric, with respect to matter and antimatter.

ZIERLER: So, what exactly did the B factory do to help move this fundamental discovery along?

QUINN: Well, what it did was pin down all of the different channels in which you can measure CP violation in the standard model and get standard model answers for it. It said, yes, the CP violation in the standard model is as Kobayashi and Maskawa suggested when they first wrote the third generation standard model example, and it's nothing else. At least, not at that scale. If there's new physics, it's at such a scale that it gives very small effects in the B factory experiment. What we did in the physics book is we thought about all the possible new physics that was possible compared to everything else known there could be a big effect in B physics but there was not. So, it's ruled out a whole lot of options for extending the standard model. This is the way physics goes, right? We make up extensions to our theory because we know it's not complete. But as experiment proceeds, it rules out ideas that were viable before you had that experimental data. The physics that came out of the B factories, and not just the SLAC one, but the Japanese one as well, just pinned down a lot of parameters that weren't yet known. For example, if there are additional Higgs, (and there probably are – for Peccei-Quinn symmetry, there are at least some )-- the scale at which they start affecting physics gets moved up by looking for their indirect effects in B physics, and not finding them. So, the CP violation in the quark sector of the standard model could have additional pieces. For example, if you write supersymmetric theories, you put a lot more parameters in the theory. You would, in general, get lots more CP violation from those additional parameters. In fact, you'd have to artificially say the supersymmetric particles, CP sector, looks very like the quark sector to make supersymmetric theories viable in any low scale. People talk about minimal supersymmetric standard models. It means they have artificially constrained a bunch of parameters in the supersymmetry theory to make the world more like the standard model, as far as CP violation is concerned. So, it's not easy to extend the theory and not totally mess up the physics you know.

ZIERLER: Yeah. Helen, what might be successors to the B factory that would continue to push forward improving the standard model?

QUINN: Well, of course, all we have at the moment is CERN, but there is a B physics facility at CERN. They're pushing that part of the physics further with that facility, because they're reaching to regions where the B factories couldn't reach. But, being a facility that is a hadronic collider, it's also much messier, and it's harder. But they're doing it, and they're figuring out how to do it as they go along.

ZIERLER: Where would the SSC have fit in, if at all, with your interests in terms of improving the standard model? Would it have been relevant, the kind of stuff that would have been done at SSC?

QUINN: The SSC is more relevant to understanding, for example, the scale of supersymmetry. So, with current -- since we have not seen anything that gives us indication of super partners -- if supersymmetry were really a symmetry, the super partners would be degenerate with the particles we know. So, it's got to be a broken symmetry. And then there's a scale at which the new partners start appearing, which is the scale of that breaking. That scale simply gets pushed up as we don't discover supersymmetry. We've done some of that, a lot of it, with the facility at CERN, but SSC would have been a better facility that went further. We would have gotten quicker to either way. We could have gone further up, so maybe we would have discovered something, but we certainly would have gotten quicker to understanding where supersymmetry plays a role, if it does. So, that's the major push for it, I think. Yes, it would have clarified other things as well, as has LHC.

ZIERLER: Were there other research endeavors you were involved in besides B factory during these years, or did this take up all of your time?

QUINN: No, B physics was my sole physics interest in those years. There was plenty of physics to do with B physics. There's lots of interesting stuff to play with.

ZIERLER: When the decision for you to retire came along, did you think that you would remain involved, or did you specifically say no, I really want to change my focus now and exclusively work on education?

QUINN: I really wanted to change my focus and get involved in the education work. The opportunity to lead the study that asks how we teach science K-12 was a sufficiently interesting and consuming opportunity, and I knew that would define what I did after that.

ZIERLER: Yeah. I wonder if also it was an opportunity -- just in terms of making an impact for the next generation.

QUINN: It certainly is. I imagine that I've affected more people than I ever will with my particle physics.

ZIERLER: That's interesting. It's so hard quantify something like that, but in what way might you? How would you think of that?

QUINN: Well, over 40 states have adopted new science standards based on that study. If you look at the new science standards of Belgium or Holland, you see the influence of that study. I've seen the document translated into Korean. I know people in China read it. It's affecting how people think about science education worldwide. That is not because I had great ideas about science education, but because I could put ideas that the science education research community had together with the ideas of the scientists. The study was half scientists -- all of the National Academy members, and a couple of the Nobel Prize winners -- and half people who were researchers in science education, or science education leaders at the state level. People who knew kids in schools and science education, and people who knew science. Together, we put together a model for how science education should look. It's not new, and it shouldn't be new because we shouldn't say the whole country should do something unless it's pretty well tested. But we managed to formulate it in a way that is having real effect on how people think about teaching science, not just in this country, but everywhere. That's pretty big influence.

ZIERLER: In making this transition in your career, did you ever think about how there might be diminishing returns in particle physics, in terms of absent massive new efforts, like an ILC, for example, that there would just be far fewer between opportunities to make the kind of fundamental contributions that were possible in the 1960s and '70s?

QUINN: That certainly was part of my thinking, in the sense that particle physics was sort of pricing itself out of the market. Not because of the people being dumb, but because it's really, really hard to get the higher energies without spending a lot of money. But it was also because my interests were there. Because of my work as a member of the Board on Science Education, I was more aware what was going on in the field of science education, and I've always had this interest in education and teaching at the K-12 level in one way or another. I thought, here's an opportunity to do something that's important for the world. Whereas, I could write a few more particle physics papers, and it wouldn't have much influence on the world. I just decided I'd do something different. I'm retired. I don't do anything most of the time.

ZIERLER: The question that jumps out to me, just to play devil's advocate, is look at what you've accomplished in your career without anybody giving much concern about how physics was taught.

QUINN: In fact, I say I became a physicist because it was so frustrating to listen to a teacher who didn't know what he was talking about, and I had to figure it out for myself.

ZIERLER: But that's exactly the question. Why would you not look at your own experiences and say, despite the education, look what I was able to do.

QUINN: But it's not despite my education. My education included a father who kept asking questions and taught me to be curious about the world. He showed me that it was interesting to ask questions and find answers to them. I don't care if anybody learns any physics at all. I want them to learn to ask questions, and to know that answers to questions are found in evidence, not in crazy ideas that you have for yourself. And that when you come up with an idea, you can test it. There are ways of going about finding out things that don't involve just guessing. There are a whole lot of things that I learned as a kid that I want every kid to learn. I think that will not only make better scientists; it will make better citizens. It will make people more competent at running their own lives and making decisions about what medical treatments to take when there's a pandemic and somebody tells them that they might drink bleach. That's an extreme case, of course.

ZIERLER: But it is the reality in which we're living right now.

QUINN: And there's a reality about the world that science education needs to convey.

ZIERLER: That's right. Now, more than ever.

QUINN: And unfortunately for most kids, it hasn't been conveyed, which means we've been doing it wrong. So, we have to figure out how to do it right.

ZIERLER: So, looking back over these past ten years, first of all, it's such a different realm that you're operating in in terms of feedback mechanisms for success. In the world of theoretical physics, there's a certain set of circumstances where you recognize whether you're on the right track or not, but in this realm, how do you know if you're making this impact? What are the data points that you have available to you?

QUINN: Well, state adoption of standards based on our study is certainly one of the data points. We have over 40 -- it depends how you count, actually. After we did the study, another group was funded by the same funder to develop a set of standards, which are called "next generation science standards." Many states have adopted those, but other states have adopted their own version, which looks a lot like that version. So, if I count all of those, it's well over 40. That's a big impact. That means people are trying to teach science a different way in over 40 states. Let me give you the short formulation. What we said, the language we developed in order to describe what we wanted students to do is science learning has three dimensions. The first dimension is science and engineering practices, doing what scientists or engineers do -- we put engineering in because if it's not in science, it's nowhere in K-12. What do scientists and engineers do, and how do they come up with what they do? They ask questions, they develop models, they carry out experiments, etc. We've listed eight practices, and these were the first three of them, of scientists and engineers. We said students should do these things. It's not about learning facts, it's about learning how to investigate, and how to question, and how to argue from evidence, and how to use mathematics in support of your arguing from evidence, etc. That's number one. Number two is something we call cross-cutting concepts. Cross-cutting concepts are big ideas that apply across all of science that typically were never taught. We just expected students to get them. Like when we do science, we think in terms of systems, and we make models of those systems, and we talk about boundaries to the system and what's flowing across those boundaries. There's a whole set of ideas around systems thinking that you need to use to do science or engineering, for that matter. Cause and effect. What we're trying to do in science is figure out the mechanisms of cause and effect in systems. If you don't tell students that, they're not looking for that. Or patterns. If you notice patterns in the system, then your model for explaining that system better be able to reproduce those patterns. If the patterns don't work the way your model says, then your model is not describing that system. So, there's a set of ideas which are actually lenses for looking at problems where you know nothing, and asking good questions to figure them out. Learning how to use those lenses to ask good questions when you don't know what you're doing is a very important piece. That's the second dimension. The third dimension is what people usually listed as the science. The facts, the ideas that scientists have used, developed, and tested using these methods. Just learning to recite those ideas without understanding anything about them or how they were acquired is not learning science. So, it's this three dimensional science learning. If you go out, you'll hear this buzzword. If you look at the National Science Teachers Association, everything they do is about three dimensional science learning. That's a term we invented, so I know we've had an impact.

ZIERLER: Helen, of all of the crises that we're facing right now with Coronavirus, certainly one of them is this --

QUINN: Distance learning.

ZIERLER: It's the disconnect between science and the public.

QUINN: And decision making.

ZIERLER: So, I wonder if this moment -- I mean, you've been at this for ten years now, right? Does this moment just tell you how important this work is, or does it tell you that on a depressing day, maybe, that there are just some things that can't be solved in this realm?

QUINN: Well, we've been at it for ten years. That means the kids who are graduating from high school now maybe got two or three years of it, if they are lucky. Because first, you write the study. Then states take a while to say they want to follow those new standards. Then, only after that, do curriculum developers and publishers start designing new curricula and textbooks, and teacher professional development to implement the new way of doing things gets funded (if you are lucky). So it takes many years before the change is really implemented across all levels of school. So, we haven't yet affected the body politic with this work.

ZIERLER: Right. We really need it. I'm very excited.

QUINN: The huge ambition is that maybe someday we will do better at educating everybody to think like scientists think. That's a huge ambition, and I don't expect it to happen in my lifetime. But I think we're moving the needle in the right direction. The fact that we failed miserably up till now is demonstrated amply by the misinterpretation of science by the public. I mean, people are saying things like, "Oh, they keep changing their advice on this disease and what we should do, so we shouldn't listen to them." Well, they keep changing their advice because there's more data, and they're learning more. If you understood a thing about data, you'd know that when there's a new virus, there's very little you know. So, you're making guess, and yes, they're educated guesses, but they're not all right. The whole process of thinking about the problem as scientists look at it is something our public is very poorly prepared to do. Even those who got good grades in their science courses, because they could memorize that a cell consists of a nucleus plus.., and an atom consists of a nucleus plus electrons, etc., (they thought the two nuclei were the same thing). How could they possibly understand anything with that information?

ZIERLER: There's opportunity here. Despite how frustrating and depressing thing are, you see opportunity.

QUINN: Yeah, and it's not easy to make that kind of transition because you don't change all the teachers overnight. The teachers didn't learn science this way. Most of them had rather limited science education, particularly elementary teachers. So, it's a whole process that you expect to take a very long time, but I just think it's worth trying because obviously as we see today, it matters so much to have some broader understanding of these things in the world we live in today.

ZIERLER: Well, Helen, for everyone's sake, I hope that your ongoing work is as successful as possible because it's really not an overstatement to say the world needs it.

QUINN: To a great extent, I've thrown it out there, and other people are doing the ongoing work. I'm just an advisor here and there to people who are doing pieces of it. It's great to see that there are people doing it, and trying to do it, and that it is having impacts on at least some classrooms.

ZIERLER: Helen, now that we've gotten to your present day work and how you've amazingly handed this off to the next generation of people, I want to ask for my last question, of course, you retired ten years ago, but you're a physicist, right? So, I'm curious what your curiosities are. What are the things that, even if you're not involved in a day to day level, continue to capture your imagination, the kinds of discoveries that you think are possible in the future? What are the things looking forward that remain of interest to you as a physicist who has made so many fundamental contributions over the course of your career?

QUINN: Well, I think it's not just physics. All of science has opportunity to learn new things. I guess I'm not alive if I'm not learning something new. In the winter and spring quarters at Stanford, I took the courses you need to take to become a docent at the biological preserve on Stanford campus. So, I got interested in lots of questions in ecology, and how ecologists think about the world. Putting the physicists thinking about the world together with the ecologists thinking about the world, how do you formulate questions in an area where you have so little control over what is going on in the system? That's one of the things that's been keeping me amused in the last little while, because I was taking the courses. I was asking questions and doing a project related to education, using data coming from the field. That's one of the pieces. I'm chair of the board of a small organization that does mostly NSF funded research around use of technology for science education. I get involved in questions around that. And just generally, personally, I'm interested in the world, and I talk about problems and issues with people. My son, who got a late in life PhD in statistics, but his real deep interest is voting systems, and how you vote, meaning things like ranked-choice voting, but other systems beyond that that he thinks are even better than that. And how do you transition from the system we have to the other systems? So, I spend a fair bit of time talking with him about his ideas in that area. Any kind of question can be interesting. It doesn't have to be a physics question. I get engaged with different ones and learn about them. One of the things I did in recent years, I was on the board of a university for education in Ecuador, a new university being built from the ground up, which is really struggling right now with the situation in Ecuador. We're trying to bring a different pedagogical model to teaching in Ecuador, which was fascinating, but to be on a board of a university in Ecuador, I had to do it in Spanish, so I had to really work on my Spanish. Becoming more fluent in Spanish has been one of the things that's taken my time and interest over these years. That's provided opportunities to talk about physics in Mexico, or whatever, with students.

ZIERLER: The learning never stops, and the curiosity never stops.

QUINN: That's right.

ZIERLER: Well, Helen, it's been an absolute delight speaking with you today. It's really special to be able to hear in your own voice all of the contributions that we all know about from afar. So, I really want to thank you for spending this time with me today.

QUINN: You're very welcome.