ZIERLER: Okay. This is David Zierler, Oral Historian for the American Institute of Physics. It is January 20th, 2021. I'm delighted to be here with Professor Pierre Sikivie. Pierre, it's great to see you. Thank you so much for joining me.

SIKIVIE: Thank you. Thank you for having me as your interviewee.

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

SIKIVIE: So, my title is Distinguished Professor of Physics, and my institution is University of Florida.

ZIERLER: When did you get to the University of Florida? What year was that?

SIKIVIE: 1981.

ZIERLER: Oh, long time.

SIKIVIE: Long time, yes.

ZIERLER: 40th anniversary, actually.

SIKIVIE: It is, yes. My wife also got a job at the university and she taught Chinese literature and language. I think that makes a very stable setup, because we both have a job, and we don't think of leaving easily.

ZIERLER: Over these 40 years, of course, you've seen not only the incredible growth of Gainesville, but the growth of the university also.

SIKIVIE: Yes, it has changed quite a bit. I think it has become more established. It's become better established than when we first arrived, yes.

ZIERLER: That's right. Pierre, I'd like to ask a very in the moment question before we go back and develop your narrative and family history, and that is, I'd like you to reflect a little bit about what this year has meant for you in the pandemic. As a theorist, I wonder, in what ways has this moment of physical and social distancing allowed you to concentrate on problems or equations that you might not otherwise have been able to? On the flip side of that, on what ways has this isolation not been good because even theorists require collaboration and being able to communicate with your peers and colleagues in a more freeform manner than these past ten months have allowed, in certain respects?

SIKIVIE: Well, in a way I hate to admit it a little bit, but I can't say that the pandemic was bad for me personally. I think this is because I'm really not that sociable by temperament. I like to have a lot of quiet, and I don't like to be interrupted. So, various things changed. One thing that changed is that there was a set of trips to conferences that were canceled. You know, often I'm enthusiastic about signing up for these conferences, but when they come, I think it's difficult to leave, and to take a flight, and go another time zone, and maybe have to prepare a talk or something. So, in a way, I didn't miss that. Each conference that would cancel, I said, "Oh, good." You know? For the teaching, I think that teaching online suits me pretty well. In many ways, I'm just as communicative on Zoom than in the classroom. I prepare a little bit more thoroughly, I think. I don't know what the students think. They may have a very different experience. As far as collaboration is concerned, you know, we collaborate by email and also by Zoom session. We send back and forth various documents. I don't think it has really been a detriment to collaboration. I do think that people are tired of the pandemic, even myself. I think people are unhappy. You go into the streets and people don't look happy. I think it's because of the pandemic, the long time of social distancing, the weariness.

ZIERLER: Pierre, the caricature of the theoretical physicist who likes nothing more than a quiet room and a pen and piece of paper to work out the equations, have you found out over these past ten months, are there long problems that you've had the bandwidth to deal with that perhaps you otherwise would not have?

SIKIVIE: Yes. I guess, perhaps a little bit more bandwidth. I've always been very adamant about having a lot of bandwidth. But I must say, it's not like in the past. I've always wanted to have a lot of time to myself. So, just maybe a little bit more during the pandemic.

ZIERLER: Pierre, let's go all the way back to Belgium. I'd like to hear first about your family origin, starting with your parents. Where are they from?

SIKIVIE: They are from small towns in the province of Limburg, Belgium. So, that's the northeast corner. My father was a physician by training, but when he went to do his residence, he decided he did not like to actually work as a physician, and he became a dentist.

ZIERLER: Less blood?

SIKIVIE: Because when people go to the dentist, it's because they are in pain, and you can help them objectively. I think my dad was not very comfortable with the psychological aspect. Physicians have to deal with various non-existing diseases. Also, I think he was not very comfortable with the atmosphere of hospitals. He said they were depressing to him. So, when he realized that, he just decided he would do an extra year and become a doctor specializing in something called stomatology, which really is dentistry, and related diseases. So, my father really -- my grandfather was a blacksmith -- so my father really worked his way up in society. My mother was a housewife, and her family is sort of small rural gentry. They had not a huge farm, but a sufficiently large farm that they didn't really have to work themselves. My father had two brothers, older brothers, who both became Catholic priests.

ZIERLER: Pierre, of course, in Belgium, geography is wrapped up in questions of culture, identity, language, religion. How would you describe both of your parents' background in all of those categories?

SIKIVIE: Both are Catholic. Both are Flemish speaking -- Dutch speaking, as their first language, but sufficiently educated to be bilingual. They speak both Dutch and French . They come from small villages, and my dad decided to move to a town which is also just a very small town. When I grew up, there were 5,000 people in the town itself. It has grown a little since then, but it was a very small town.

ZIERLER: What were your parents' experiences during World War II?

SIKIVIE: It's very interesting. When I grew up, there was still very much the memory of World War II. So, the two brothers, who were priests, of my dad, were in the army, but because they were priests, they worked for the Red Cross. They were helping people who were injured. They got made prisoners of war, and they went to a German camp. But, I think, first, because they were priests and not really soldiers, and I think also because they spoke Flemish -- the Germans tried to make friends with the Flemish, because Dutch is very close to German -- they were released pretty early, and my dad didn't have to go to the army because his two brothers had been. At the start of the war, he rode his bicycle all the way to Southern France, sort of as a refugee, and maybe also as an adventure. But, you know, the Germans went all the way to the south of France, and then he just rode back, and went back to study. I think it was relatively difficult for them. When I was a kid, I was constantly told not to waste anything. I'm often surprised by how easily people waste things in the U.S., because they never went through this experience of really having to conserve everything. My father went through some bombardments, because the town, Leuven, where he was a student, was a railway connection. So, the Americans bombed that a lot. My dad was usually very pro-American, but he was not happy to be bombarded by the Americans. They're not very precise and bombs fell all over the place.

ZIERLER: Pierre, as a young boy, either in concrete versions of what you remember, or generally in the culture, what were some of the legacies of World War II that really stood out for you in the memory of your early years?

SIKIVIE: Well, there is this sense of having to be careful with resources. Everything. Food. My parents came from a rural background, so they had actually more food available than city dwellers. So, they were better off, but they still had to be very careful. There was a resentment towards Germans. When people would go to a cafe, or restaurant, and Germans would come as tourists, some people would be very upset. Otherwise, I don't think there was all that much. I think people sort of moved on. Also, there was a big boom after World War II. My father, having survived the war, was in a pretty good position to prosper, because he had lots of patients and there was a lot of work to be done.

ZIERLER: Pierre, did the Cold War loom large in your childhood? Did you have a sense of the division of Europe and the Soviets and the Americans and the dangers that entailed? Was that something that was real or remote to you?

SIKIVIE: It was real, but I think I should have been more aware of it than I was. Well, I was just a kid, so I was usually not worrying about world affairs. But, yeah, I think there was a real issue. People were aware of it, yes. And in Belgium, people were very pro-American. I grew up among very pro-American attitudes. So, right, we were very grateful to America for having established this boundary, because without the Americans I think the Russians would have just run over all of Europe. So, yeah, I was aware of it, but I think I was probably not sufficiently aware. Older people, I think, would have been more aware than I was at the time.

ZIERLER: What kind of schools did you go to as a child, a young boy?

SIKIVIE: For my primary school, I went to the Catholic school in our small town called Landen. Sounds like London, but it's Landen. It was a small town. And then I went to a boarding school. Landen is Flemish, so I spoke Flemish. And then for secondary school, I went to a boarding school, which was only ten kilometers away, but across the linguistic barrier. This was because my parents wanted me to know French. They thought I should go to a French boarding school. My brother was there, too. And it was also a Catholic school run by an order of priests. I didn't realize at the time, they're called “P\`eres Croisiers”, which literally means "crusading fathers". I always thought it's just a name, but recently I went to look it up on Google. Actually, this order was started during the Crusades. It goes all the way back. They don't talk about this anymore. And then I went to University of Liège, which is not Catholic. University of Liège, there's no denomination.

ZIERLER: Did the church have a big part of your family background? Was that something that you would do? Were there cultural traditions that were important to your family?

SIKIVIE: So, my father and his brothers -- his two brothers were priests -- certainly identified as Catholic. My father dabbled a little bit in politics, and there was a Catholic party. He was part of the Catholic party, but I don't think, in his heart, he was really that religious. He didn't always go to church. And my mother was even less so, I think. You know, Catholicism pervades Belgian culture. It's all over the place. So, I grew up with it. I'm a little critical of it now. I saw, for example, when I came to the U.S., information in books, and any kind of information is far more readily available. Traditionally, in Belgium, books are not that available because the church wants to control what people read. And if a book is not the church's liking, it's actually banned. There was a so-called index of banned books. I think Belgium, in particular, is somewhat intellectually repressive this way. The university in Brussels is called the Free University of Brussels. It's not called "free" because it doesn't cost that much money. It's called "free" in the sense of free thinking. The university was really started as sort of a counterweight to the influence of the Catholic Church.

ZIERLER: Is it more like the British system or the American system in terms of the need to declare a specialty or a major right from the beginning of your time as an undergraduate?

SIKIVIE: I don't know how much this is true in the UK, but in Belgium, yes, you enter as a physicist, or medicine student, or whatever, law student. And that pretty much determines the whole curriculum. So, the American system is far more flexible.

ZIERLER: For you, when did you start to get interested in physics? Was it even before college?

SIKIVIE: Yeah, I started really thinking I should become a physicist in high school, and mostly this had to do with me having a great facility compared to most of my classmates. I usually thought the problems were very easy. I would help everybody. And I thought if I'm going to be useful in society, this is the obvious way. Of course, that's a miscalculation. For one thing, your classmates in high school are not as good as the ones you're going to meet in college, or in graduate school, now the real competition. Also, it's maybe a little bit idealistic to think that just because you are good at something that that's what you should do to help. I don't know. But that was the starting point. I remember when I was entering the university -- there is this, as you point out, you have to make a decision. When you enter university, what are you going to do? And I was pretty young. I was not yet 17. And I remember the day, because I still had some doubts about it. I thought I might become a medical doctor. That seemed like the safe thing to do in Belgian society. Or I might make this gamble with becoming a physicist. I remember, well, I'll just gamble.

ZIERLER: What was your physics curriculum like? What was the emphasis on in terms of theoretical physics versus experimental physics?

SIKIVIE: Right. So, the curriculum was all fixed. It had basic physics, including labs. It had chemistry, including labs. And it had a lot of emphasis on mathematics, calculus, and complex variables, and so forth. It was pretty good. I mean, I felt, for example, at the end of four years, I knew quantum mechanics. I didn't realize how little I actually understood, probably, but I had four semesters of quantum mechanics. And there were various labs. There were labs in electronics, I remember, and crystallography. There were labs in more advanced experiments, like you have in the U.S. But the curriculum is mostly fixed. There's very little choice about it.

ZIERLER: What did you realize were your talents, or the things that were most interesting and compelling to you as an undergraduate?

SIKIVIE: I did pretty well in theory. I had good grades and so forth. Many undergraduates, they see the example of the famous theorists, and they learn about their work in books, such as Maxwell's equations. There tends to be an emphasis on theory versus experiment. So, I naturally was attracted to theory, as many people are. Not sufficiently realizing, I think, how it is really experiment that revolutionizes physics. The theorists just put the final cap, when they make the final formulation. But the real revolutions come in the lab. So, that's something I didn't understand very well. I seem to have a real talent, not that much in the lab generally, but somehow, for optics, I was very good. Somehow, the optics experiments always worked for me. Those other experiments might work or might not work. I was not very good at chemistry.

ZIERLER: On the social side of things, given when you were at university, did the countercultural movement reach Belgium? Was that a part of your undergraduate experience? The Civil Rights Movement, Women's Rights, the Vietnam War, was that part of your reality at all?

SIKIVIE: Yes, it's interesting. A lot of the things that flowed from this counterculture in the '60s in the U.S., we saw on TV. We saw the Vietnam War, and we saw the protests, the assassination of Martin Luther King, and actually, Bobby Kennedy and his brother. This had its counterpart in Europe, but it was a little different in flavor, I think. It was more like we want to have our voice heard in various matters. There were some demonstrations, and so forth, but in the U.S., it was much sharper because of the Vietnam War, people really getting killed, and so forth. So, it did not have this sharpness to it, but there were protests, and some fights in the streets. I think that the American protests had a very good cause. I think the European protests were more along, let our voices be heard on these various things. So, we were complaining about this and that.

ZIERLER: Did you think about staying in Belgium, or even in Europe, in pursuit of a graduate degree in physics, or what made you think about coming to the U.S.?

SIKIVIE: Yeah, that's a very interesting question, too. I left in 1970, and that's only ... '45 to '70, these were American years. Actually, they continued later, but certainly, America was very influential then. In the '60s, America put someone on the moon. That was on TV -- there was the Vietnam War, but there was all this glorious stuff going on. I think what happened, first of all, is that I became aware of the quality of American physics through books. For example, the

Berkeley Series, I thought was really very good. And of course, Feynman's lectures, and so forth. Kittel's book on solid state physics, I thought was very good. I had this sense that the way physics was taught I really liked a lot. There were also the books by Landau and Lifshitz, which were excellent, but were not that easy to read. I had this sense -- and correctly -- that it was a very open enterprise, science in the U.S., with many institutions, and a lot of back and forth. I had the opportunity to go do my doctorate in Belgium, but I don't think that was really a temptation. If I could go to the U.S., I really wanted to go to the U.S. Also, I was very young, and when you're young, you want to see new places and have new experiences. So, it was very much something I wanted to do. And I was very glad to have the opportunity. I'm grateful to the U.S. for having had this opportunity because it may not have happened. It could have been impossible.

ZIERLER: Pierre, how well defined was your identity as a physicist at the end of your undergraduate degree? In other words, did you know that you wanted to pursue theory, and that ultimately it would be astrophysics and cosmology that you would focus on?

SIKIVIE: No. When I was an undergraduate, I was interested more in condensed matter physics. I wrote a small thesis on that, and I was also mostly interacting with condensed matter physicists. At the end of the four years in Belgium, you are required to write a thesis project. It requires that you have some specialty. It could be astrophysics, or could be something else, but I picked condensed matter physics. Interestingly, when I came to the U.S. -- well, one of the reasons I went to Yale was because Onsager was there. I didn't realize, Onsager was in the chemistry department. And then, I thought I was going to become a condensed matter physicist, because that's what I had done before. I did think I wanted to be a theorist, but I think that's partly because of insufficient appreciation for the role of experiment. I thought I wanted to do condensed matter, but here's a very interesting thing. At Yale, the condensed matter faculty was in engineering, and my dad, who -- I think every parent who sees his son become a physicist is a little worried.

ZIERLER: Especially when your father is a blacksmith

SIKIVIE: That was my grandfather. My father was a dentist.

ZIERLER: Right, so I'm saying your father's father was a blacksmith, so that was the intellectual tradition he was coming from.

SIKIVIE: Yeah, yeah. That's right. So, he thought, okay, physicist -- maybe you should be an engineer. So, I was in a little bit of a struggle with my dad saying maybe I should be an engineer, which is a much safer thing to do than becoming a physicist. So, interestingly, when I went to Yale and condensed matter was in engineering, I thought that going to engineering to do condensed matter was, in a way, like giving up to my dad. I thought, I'm not going to do that, you know? I remember after the qualifying exam, I made my rounds, and I picked Feza G\”ursey as my advisor, because he was recommended to me, and I thought that's what I could do, too.

ZIERLER: What were some of the most exciting topics to work on that you might have had an opportunity to choose at this point?

SIKIVIE: Well, in the '70s, as you know, the elementary particles were completely revolutionized. Up to the '70s, people knew a lot, but it was all muddled. After 1975, it was all clear, basically. So, in the five years of my graduate studies, everything fell into place. The standard model fell into place, and it was improved upon later, but basically it was set down. So, I had the good fortune to come at a time when the standard model was invented, but the bad fortune to be too young to really have an impact. So, while I was a graduate student, this transition happened that people came to formulate the standard model. They came to realize quantum chromodynamics for strong interactions, the electroweak theory of Weinberg and Glashow for the weak interactions. All this came into place. And this was sort of a revolution, right, because people had other ideas before. They used this S-matrix. I don't know how much you're aware of all that happened in particle physics, but they had these Regge poles, and stuff like that, which I never learned, because when I was a student, I could see the standard model was going to be correct, and that's what I should do. So, basically, I just wanted to work on those topics, but I was a little young to really have any influence, yes.

ZIERLER: I'm curious, Pierre, coming from Belgium, and whatever the peculiarities are of the Belgian system, what were some areas, when you got to graduate school, where you feel very well prepared in relation to your cohort, and what were some areas, perhaps, where you might not have been as prepared?

SIKIVIE: I think that, in general, I was well-prepared in the sense of knowing the material. I think what was really new to me was the sort of free-wheeling thinking that Americans have, and the habit of questioning, and so forth. I was shocked -- I remember very well, when I was taking E&M, one of the classmates told the professor that he was wrong about this. He said this quite loudly, and I thought this was really crazy that the student would tell the professor he is wrong. But in the U.S., it's considered normal, and maybe you have to be polite, but that was new to me, this openness in discussion. That was not allowed in Belgium. In Belgium, we would not argue with the professor. This would not even occur to you. I notice this when I teach people, many of the students from abroad also come with this attitude, and you have to teach them to think on their own, and to speak up.

ZIERLER: So, just to understand the broader narrative here, you're exclusively in the world of particle physics at this point. Astrophysics, cosmology, this is not on your radar yet.

SIKIVIE: That's correct, yes. I did not know about any of that stuff. One thing that I did, I always like astrophysics. That's true. The other thing, about astrophysics, is that I loved the book by Weinberg on gravitation and cosmology. I think it's a wonderful book. Often, the topics in particle physics were rather difficult. Topics in quantum field theory are difficult, usually. Weinberg's book on gravitation and cosmology gave me the sense, yes, I can really understand what he's saying from the beginning. Weinberg is really good at explaining everything very thoroughly, and this was very satisfying to me. So, I knew a little bit about cosmology, but I didn't think I was going to work on it. I just like the topic.

ZIERLER: How did you go about developing your dissertation topic?

SIKIVIE: Yes, so, my dissertation is various pieces, but the piece that endured has to do with grand unification, and the exceptional group E6, which is one of the possible unification groups. That is really owing to my advisor. Feza G\”ursey was very fond of things like octonions, and they're related to exceptional groups. Maybe I can take some credit, because at the time, Georgi and Glashow did unification in SU(5), another group, and Pati and Salam had done also some unification in SU(4). I just combined, if you like, the fondness of my advisor for these exceptional groups with this unification. I thought, let's unify with E6, and that is the part of my thesis that sort of survived. Not that it is necessarily a part of reality, but people still give it citations, and so forth. Then there's the more obscure part of my thesis. I thought that I could understand the pattern of quark masses in these gauge theories, but the ideas that I had did not turn out to be reality. They were not really wrong, but you know, you can have ideas and they might be somehow correct, but they're not reality. It's a different thing.

ZIERLER: Pierre, it's a bit of a fuzzy question, because it would ask you to put yourself in the moment, and not remember all the things that happened subsequently, but as a graduate student, given all of these literally revolutionary ideas and discoveries that were happening in particle physics at this point, did you sense the historical import of what was going on? I mean, if we look back at the 20th century, on the whole, the early 1970s stands out for just how much was discovered in so short a period of time. In retrospect, in historical hindsight, that's obvious, but I wonder, in the moment, as a graduate student, if these world-shaking discoveries were felt by you? You were aware of them.

SIKIVIE: Yeah, I think I was aware of it. The atmosphere was very different at the time, from what it is now, certainly. People would go to conferences and really bring back news. This had been observed, or the news came that this had been observed, or someone had this idea. And people would really react to it. At the time, it was truly revolutionary, the early '70s. But I should say that the condition of revolution had really been going on for a while. People discovered many other things before the '70s, like parity violation, CP, time reversal violation. They discovered all these particles. They discovered SU(3) symmetry, flavor symmetry. It was a lively time even before the '70s, but then the early '70s were a particular time where things sort of gelled. But, yes, people knew, I think, it was important, yes. Even the graduate students knew.

ZIERLER: When did you know that you had conducted enough research that you were ready to defend the thesis? What was that process like for you intellectually?

SIKIVIE: Well, I have to say something which is a little uncomfortable. I decided to graduate even though I didn't really have a thesis. I just thought I wanted to do one. Yeah, it was okay. I mean, I had done some stuff.

ZIERLER: Did you do the proverbial staple together a bunch of papers and call it a thesis?

SIKIVIE: No, I had written very little. I had a pretty good reputation that I was a good student, but I had essentially written nothing, and still I wanted to graduate. People thought, well, I found a job, and in fact, I had some papers.

ZIERLER: Did you do this with your advisor's blessing, or was there a problem associated with leaving before you may have otherwise been able to?

SIKIVIE: He did not fight it. It was okay. You know, I think that in the American PhD system, getting a PhD is only like a first step. There's no real great achievement for most PhDs. Some PhDs are different. Feynman's PhD on path integral formulation is an exception, but most PhDs are sort of just like a, you know, how do you say, first piece. That was true for me. In fact, for me, there was no even a first piece, and I already decided to graduate. I just decided I need something else. In a way, Yale was not exactly the center.

ZIERLER: That's right. So much of what was going on was happening at Harvard and at places like SLAC. Yale was actually a bit of -- I remember talking to Tom Appelquist, and one of the ideas with Allan Bromley, to recruit Tom because Yale needed a shot in the arm, essentially, with the particle theory.

SIKIVIE: Oh, so you were at Yale?

ZIERLER: No, no. I'm saying, Tom Appelquist told me this.

SIKIVIE: Oh, but you know very well. Bromley was in nuclear physics. Okay, well, that was not being revolutionized then. There were some very well regarded particle theorists, but they were not at the center of the revolution. I think that was partly my impatience. I felt that I was not quite in the right environment. Tom Appelquist actually came later. I graduated before he arrived. I did some stuff, and it was okay. But probably, I think I felt I had to move on, that I wasn't growing sufficiently well. A lot of stuff was happening, and I wanted to be part of it.

ZIERLER: That has various dimensions to it. One is, professional opportunities that you might have had, and others is, you had done all that you were able to do intellectually at Yale. So, how did those factors play out in your decision making?

SIKIVIE: Yeah. In a way, I think I did the right thing, because by telling people I'm going to graduate no matter what, stuff began to happen that might not have happened if I just sat there.

My advisor, Feza G\”ursey, told me I could see him anytime I wanted to. But I'm a little shy and I didn't want to bother him that much. So, in a way, I was just sitting there and watching stuff happen. By saying I want to graduate, I was saying, I need to get something going. And that actually started to happen. So, it is circular thinking. You can decide to graduate because you've done a nice piece of work, and so you have a thesis ready, or you can decide to graduate and you don't have a thesis, and now you have to come up with something, so you do something that gets you rolling. In my case, it was more like that. It was more that I didn't have much to say when I decided to graduate.

ZIERLER: So, the opportunity at Maryland, your first post doc, were there any reservations in College Park with regard to the fact that you really didn't do a full thesis, or they weren't concerned because they saw what your capabilities were, and where ultimately, they hoped you were heading?

SIKIVIE: No, I think they were comfortable. In fact, by that time, I had written a preprint, and I think it got some attention. This is the stuff that I mentioned that is not reality -- it was sufficiently interesting and did, in fact, fit into the new ideas of gauge theories. So, people thought the quality of that was good, and I think I had a good reputation as a student, so I probably had very good recommendation letters. Yeah, I don't think it was a problem. I didn't get many offers though, I should say.

ZIERLER: Pierre, the distinctions between departments of physics and astronomy are often significant about what they say in terms of the curriculum, in terms of the ideas you were exposed to. I wonder, when going to Maryland, if you started to think more about astrophysics by way of the fact that the astronomy and physics programs are perhaps a bit more integrated than they are at Yale.

SIKIVIE: That could have happened but didn't. I didn't do any astrophysics at Maryland either. I only really got interested and started to learn about cosmology by accident when I was at CERN. So, that's really quite a bit later. In Maryland, at SLAC, I was always doing particle physics. Even at CERN I was doing particle physics. Only at the end of my stay at CERN did I get interested in cosmology.

ZIERLER: So, what was the main thrust of your research during your time at Maryland?

SIKIVIE: Interestingly, the people at Maryland were very understanding in the sense that they let me do whatever I wanted. This is something that doesn't happen so much today anymore. Many post docs are required to work on some projects. But in those days, you got a post doc, and you did whatever you liked. So, now I had this stronger interaction with my advisor working on exceptional theories of grand unification. They actually were getting some attention, so I continued to work with my advisor when I was in Maryland. Then, I also struck up a collaboration with someone at John Hopkins, Kim. We wrote a couple papers. Interestingly, I never wrote a paper with someone at Maryland, but I interacted a lot with Jogesh Pati, who had a lot of exciting ideas about particle physics at the time. But the group was very nice. There was Wally Greenberg there, and there was Joe Sucher. You know, Joe Sucher passed away last year. I thought he was a wonderful guy. But basically, they let me do whatever I liked.

ZIERLER: Pierre, another sort of broad historical question transporting you back into the moment. The big question has been the physics beyond the standard model. Given that this is 1975, was there a lot of hope? Was there momentum that it's not just what Glashow and Weinberg had done in '73 and '74, and that was the standard model and that was the end of it. And yet, now, to fast forward to 2021 when we're still trying to break out of the standard model, was the sense in 1975 that that momentum, the tremendous rate of discovery, that that trajectory, you were still on that path, or not necessarily?

SIKIVIE: I think that you're saying this very accurately. Right. So, I think you've given the answer to the question. It's a rhetorical question. During the '70s, you have at the beginning, a tremendous number of things falling into place, but they might still be a little bit different from what Weinberg had thought. There were variations, but as you know, pretty soon people realized it was the standard model that's correct. And then there was grand unification, right? Actually, my thesis is in part about exceptional groups of grand unification, people were hoping that they would see proton decay and grand unification. So, that was a very lively area of discussion. That's basically what I was working on. Also, at the beginning, it was not entirely clear that the standard model is correct, so you could have various things. I wrote a paper about CP violation, and that also -- again, I said something that was possible but was not actually reality. CP violation is Kobayashi-Maskawa CP violation in the standard model. But I think I wrote an interesting paper. Weinberg also wrote a paper about this, and I think I got his attention with my paper, which is very good to have Weinberg's attention.

ZIERLER: What do you think about what you wrote attracted his attention?

SIKIVIE: I think I know how it happened. I think Ben Lee -- do you know Ben Lee? He was at Fermilab. He noticed my paper. And he told Weinberg. They were very close. Weinberg was also interested in this and had written something similar. I think that's something Weinberg appreciated.

ZIERLER: Pierre, were you thinking at all about gravity or general relativity during these years? Was that a focus in your research at all?

SIKIVIE: No. No, I was not thinking about gravity or cosmology. That came later. I was thinking about grand unification, weak interaction phenomenology, CP violation. And then when I went to SLAC I was thinking about properties of classical non-Abelian theories. Again, that's stuff that is correct but is not particularly relevant.

ZIERLER: Pierre, another personal question, at what point -- I'm sure there was no grand plan, but at what point, maybe it started at Yale or maybe by the time of your second post doc, when did you realize you were going to make a life for yourself in the United States and not return to Europe for your full-time career?

SIKIVIE: Well, that's also -- you have an art for asking questions that bring out a lot of story. This is actually an accident; my wife is maybe the one who is the reason I stayed in the U.S. I had a post doc in Maryland and one at SLAC, after which I thought, well, maybe I'm really more European. Once you have lived in two different countries like that, half of you is one way and the other half is the other way. But I thought maybe I should go back to Europe. So, I took a fellowship at CERN, even though I had a faculty job offer from North Carolina. People thought it was pretty crazy I didn't take a faculty job. I took a fellowship at CERN, likely because I thought I should integrate myself into the European system. It's interesting, on my way there, and during the very last weekend, I met my wife. Nine months later we were married.

ZIERLER: Where did you meet?

SIKIVIE: Well, you should ask her. She likes to tell the story.

ZIERLER: How about you tell it, and she can fact check you?

SIKIVIE: It's an interesting story, very romantic. But the question where we met is actually we had a blind date at JFK Airport.

ZIERLER: How romantic.

SIKIVIE: Yeah, romantic. So, what happened is that she got a PhD from Stanford in Chinese literature, and we had a common friend, an older lady who always wanted to introduce us, but it never happened. Then, she graduated and had a job in New Orleans, actually, as a curator in a museum. I was going to go to CERN, and before leaving, this friend of ours had a little dinner for me. She said, "I've always wanted to introduce you, so I'm going to put you on the phone." And then we talked, and we had this date in JFK, and we liked each other. So, nine months later we were married even though we hardly spent any time together.

ZIERLER: So, this is to suggest that perhaps your interlude in CERN was definitely temporary, and had you not met your wife, you might have gone back to CERN and spent the rest of your career in Europe, potentially.

SIKIVIE: Right. In another part of the wave function, that's what happened.

ZIERLER: Pierre, let's go back to SLAC for a minute. Who were some of the key people you worked with at SLAC, and in what ways was the November Revolution still ongoing, the excitement from 1974 and into 1975? Did you feel that at all by the time you got there two years later?

SIKIVIE: It was a wonderful place. Really, it was like -- well, first to be a post doc in a big lab, you really feel you're at the center of things, because it's a big lab. Post doc is the main person, so that was great. The lab was wonderful. I mean, they did all this wonderful work. So, the November Revolution, that's in '74, is it? So, it was after that, but people were still discovering things, like Perl discovered the tau lepton. Also, the experiments that really established the standard model were done at SLAC. It happened when I was there, and I saw the new ring being built and the detectors being built and inserted. It was very exciting. There were very good people. Did I participate? Well, not initially, because I'm pretty stubborn. I like to do my own thing, and I wanted to do non-Abelian classical theories. Turns out, it has nothing to do much with reality, but I wrote a bunch of papers about it. They were well received. I collaborated with Nathan Weiss, who was a visitor. He went later to UBC, University of British Columbia. Again, near the end of my stay, I did in fact begin to really insert myself by working on technicolor. Do you know what that is? Dynamical symmetry breaking of weak interactions. Lenny Susskind was there, and he was like a font of ideas. That was very exciting. So, that happened for the last half year, or maybe eight months, while I was at SLAC. This is what I carried on to CERN, and that was very much part of what everyone was interested in. So, then, I felt really in the center of it, yes, for the first time, perhaps.

ZIERLER: Pierre, was being in an environment of such experimental, intellectual ferment, was that valuable to you just being in that environment intellectually? Did you feel that? Was that useful to your research?

SIKIVIE: Yeah, I liked the environment of experiment, of the lab. Yeah, I do very much like that. The way the people talk, I like, yes.

ZIERLER: The offer from North Carolina, were you on the job market, or were you getting recruited at this point?

SIKIVIE: They already offered to Jack Ng, who was also a post doc at SLAC. He took a job at North Carolina, and he, I think, had the opportunity to push for another hire. He wanted me to come, and I did get the offer. At that time, I was not ready for this. It seemed like a small town and out of the way, even though it's --

ZIERLER: And this was not a post doc. It was an assistant professor offer.

SIKIVIE: Yeah, yeah, it was. But anyway, I decided to go to CERN.

ZIERLER: What was the decision making? What was so compelling to you about going to CERN at this point?

SIKIVIE: Well, CERN was like SLAC. It's a big lab, and a lot of stuff happening. It's a wonderful place to be, too. If you can have a post doc at one of these places like SLAC, Fermilab, or CERN, it's very nice because of the atmosphere of stuff happening.

ZIERLER: In what ways was the theory group similar to CERN, and in what ways was it different than SLAC?

SIKIVIE: I think that it has a lot of similarities in the overall functioning of the theory group and its relationship to the lab. I think one thing at CERN which is a little different is that the fellows or the post docs come from various parts of Europe. There's a complicated process by which European countries get their students to be fellows at CERN. It's not pure competition, like in the U.S. At SLAC, there's a certain number of post docs available, and the faculty picks the best people -- who they think is the best people. At CERN, I think this is what the faculty, the permanent staff, would like to happen, but it's not realistically what happens. The various national influences and various stakes that, say, Italians, or English, or Belgians have in the lab, imply certain rights to have their people picked. So, I think what happens is that the fellows are very good, but they're not used to this open competition that's present in the American system. So, there is a bit of a danger, and there are many fellows, for them to get lost because they come, say, from -- I don't know. Pick some country. I don't want to pick a particular one, but they come from, say, Norway. Very brilliant student, goes to CERN, is in this big lab with many other fellows, and who does he talk to? At SLAC, this is not so likely to happen, because there's only a few post docs, and everyone talks. You know what I mean? It's just more focused, more concentrated.

ZIERLER: As a Belgian who's spent so much time in the United States, and between Yale and SLAC, you've got a pretty good viewpoint of high level physics in the United States. Culturally, coming back to CERN, did it feel very different in terms of the approach to physics? Was there a uniquely European approach that did not exist at a place like Yale or SLAC?

SIKIVIE: No, I think it was excellent. The people at CERN were very good. I was close to John Ellis. John Ellis was at the center of so many things. He's very prolific for CERN, and he's very good at interpreting data and remembering a lot of stuff.

ZIERLER: Was Mary Kay at CERN at this point?

SIKIVIE: Yes, Mary Kay was there. A lot of very good people were there. Mary Kay was there, Bruno Zumino was there, Altarelli was there. I mean, a whole list of really very good people. John Bell was there. But there are more people, and maybe in that sense, it is less interacting, because there are various groups. So, there's a tendency for people to not forget their nationality.

You know? Like, maybe there's a bunch of Italians talking to one another, and so forth. That's a bit unfortunate. You don't have that in the U.S. That doesn't exist.

ZIERLER: Pierre, did you see your time at CERN, essentially, as a continuation of what you were doing at SLAC, or this was an opportunity to continue to refine these ideas?

SIKIVIE: So, yeah, at the time I felt really at the center of things, because technicolor, this dynamical symmetry breaking, was at the center of everyone's interest. I had collaborated with Lenny Susskind, who was the guy working on this, and John Ellis was very interested in this. A lot of people were very interested. Bruno Zumino. So, I could give talks all the time. I was a person of interest to everybody. So, that was great. Then, as you know, technicolor kind of fell by the wayside, but that happened at the end of my stay.

ZIERLER: Yes, why is that? What happened there?

SIKIVIE: Yes, so, you are a historian of science, but you know a lot of stuff, I noticed. So, do you know about flavor changing neutral currents? Do you know what is meant by that?

ZIERLER: I do.

SIKIVIE: You know what that is. Okay, so you're a particle theorist, too.

ZIERLER: It's just what happens when you speak to so many particle theorists. But, please, I'm asking for your perspective. Why did technicolor go by the wayside at this point, in your view?

SIKIVIE: Its basic failure was its inability to account for the suppression of flavor changing neutral currents. This is one of the ways we can see the standard model, even in the 1980s, was going to be very close to the truth. Because it suppresses flavor changing neutral currents, and it does this in a very precise way. You do just about anything else, beyond the standard model, and you'll fail at this each time. So, dynamical symmetry was something going beyond the standard model. Very hopeful, actually, I thought it was a brilliant idea. And it failed on this count. So, it was very difficult to understand why flavor changing neutral currents were suppressed. Actually, this is something that people don't really want to hear, but if you were to guess the scale where you can have new physics, it's very high above the standard model. You know, you're used to naive, back of the envelope estimates. You think, well, flavor changing neutral currents will require that new physics will not come in before, maybe, 100 TeV, 1000 TeV, unless something very special happens. So, my idea already at the time was we have to do something completely different. I don't want to speak like a great, wise person, but that actually turned out to be correct, and you alluded to it. In the '70s already you might have guessed this. You would have predicted, I think, there's not going to be much else happening for a while. You know, you might have predicted after Newton understood the solar system, not much else is going to happen.

ZIERLER: Pierre, do you see these developments in as much an experimental situation as theoretical, or it was entirely a theoretical advance that allowed for these conclusions at this time?

SIKIVIE: So, that logic that the flavor changing neutral currents really tell you that the standard model is going to be it for a while, that is a theoretical argument but is very simple and solid, I think.

ZIERLER: How much is this technicolor falling by the wayside, to what extent is that so clearly related to your germinating interest in cosmology and astrophysics? Is there a direct connection there? Are you searching for the next big thing?

SIKIVIE: It's an accident, but there is a connection. Accidental connection. It all comes from there, actually, because what happened is that with two others, Subir Chadha, I think who left physics. I think he became a banker. And then Pierre Binétruy, whom you may know. Pierre Binétruy?

ZIERLER: Yes.

SIKIVIE: Yeah, he was a student of Mary Kay. Mary Kay was at the time at Annecy, and Pierre was a student. We, the theorists, were studying technicolor, and were studying the vacuum structure of technicolor. Actually, some other work had already been done on this, and we were doing this more thoroughly, if you like. This is really how I got to cosmology, because what happened is that this vacuum structure has degeneracies, and it has domain walls. I realized that this degeneracy is really there because of axions. So, any axion model would have this degeneracy. The technicolor models happened to have axions that formed these degeneracies, a little by accident. But I realized that these degeneracies are there whenever you have an axion.

ZIERLER: Pierre, just to be clear, sorry to interrupt, but at this point -- are you using the word axion retroactively, or was the word axion already in active use by 1979, 1980?

SIKIVIE: So, the work on -- yeah, it was in active use. The word axion was established, yeah, and it was known to be an important idea. So, this was the first time that I had a sense that I'm actually doing something that people will not be able to ignore. Everything else I had done before, maybe it was good, but you could ignore.

ZIERLER: Do you remember your first time interacting with the word axion? Was it in a lecture, was it a paper, conversation?

SIKIVIE: Yeah, you know, I won the Sakurai Prize a year ago, and I had to give a lecture, and I thought it would be interesting to do sort of the history. So, in this lecture I did -- I don't know, it may be online someplace. The lecture is called Encounters with the Axion, so it tells of various times, not all of them, but the main ones -- and the first one, of course, was at SLAC, because Helen Quinn was at SLAC. She was the one that made me aware -- I wrote a paper on CP violation, and when I arrived at SLAC they asked me to give a seminar, so I gave a talk about CP violation. Helen Quinn was in the room, and she was the one who made me aware of CP violation in the strong interactions. I had not known this. It was news to me. She was the one who told me, Helen Quinn herself. So, during that year is when the axion -- 1977, 1978 -- when the axion phenomenology was established by Weinberg and Wilczek, and the word axion was invented. You know, Weinberg wanted to call it the higglet. Do you know that?

ZIERLER: Yes.

SIKIVIE: It was very cute, but it didn't --

ZIERLER: It didn't stick.

SIKIVIE: Yeah, it didn't stick. So, the other big revolution -- the axion was looked for but was not found. Actually, I had an interaction with Bjorken, which I value. I didn't tell this in my lecture. I wanted to, but in a way, you have to make some kind of decision and not try to put in everything. BJ was a wonderful guy. I had some idea about axions, and I went to talk to BJ about it. BJ liked it, and he found some experimental consequences of this idea. He went to talk to Gail Hanson. Do you know her?

ZIERLER: Yeah.

SIKIVIE: She was an experimentalist, and they went to look at data, and it was ruled out. So, this was an example where being at the lab with BJ and Hanson and so forth was really wonderful. I can tell you what the idea is but it's probably not worth it.

ZIERLER: Please do. I would love to hear this.

SIKIVIE: It's not a good idea really, but I had this idea that if you have axions, then you could have apparent CP violation, because the axion, which is CP odd, would be invisible. So that when you had K-long decay -- what is it? K-long decay to 2pi, that is a CP violating decay, right? Normally it should be K-long to 3pi. K-long decay to 2pi, it only decayed to 2pi because there was an invisible axion. That was my idea. K-long actually is not going to 2pi. It's going to 2pi and an axion. So, I thought; well, that was the idea. And BJ looked at it and rightfully said, "Well, if that's true, then there's a radiative tail, and you should be able to see that in the spectrum." And there was no radiative tail. It was dead. In many ways, it was not such a great idea, because if it were true, then there's no need for the axion. The axion would kill its own motivation, because there is no CP violation anywhere, and you don't need an axion to explain why the strong interactions are CP invariant, because there is no CP violation anywhere. Normally you need the axion to explain why you have CP violation in the weak interactions, but not in the strong interactions. But since, by this process, I would remove CP violation from the weak interactions, the basic motivation would be gone. So, in a way, the idea had its flaws already, but we played around with it.

ZIERLER: Pierre, did you have a sense, at the time, that your intellectual transition was part of a larger trend in the field where particle physicists were increasingly thinking about cosmological and astrophysical questions?

SIKIVIE: Yes, yes. Definitely. There are various pioneers. Weinberg is one of them. Lee and Weinberg invented this mechanism of freeze out for heavy leptons. All the influential people, like Zel'dovich. Zel'dovich is a person who knew particle physics, knew cosmology. More and more you saw people using ideas in cosmology to do particle physics. Preskill pointed out a monopole problem, for example. Then, there was the revolution of inflation. That was very important. So, particle physicists, some knew cosmology, yes.

ZIERLER: When did you start to think about your next move after CERN? Did you know you were coming back with your wife and your new situation? Were you specifically looking for opportunities back in the United States?

SIKIVIE: Yeah, so we decided we were going to go back to the U.S., so I went to look at jobs in the U.S. I could go to University of Florida. They just started a new particle theory group, and it seemed very nice.

ZIERLER: Pierre, to the extent that being a new hire on a faculty, the idea is that you might fill a niche that might not exist already in the faculty. Given that your interests were transitioning so clearly at this point to astrophysics, cosmology, thinking about axions, was your hire at Florida -- was the idea that you would be part of that revolution of particle theorists going into astrophysics, or the expectation or your readiness at that point was still more in your scholarly grounding in particle theory?

SIKIVIE: It was in particle theory. I think cosmology had no part-- because, actually, I didn't know hardly any cosmology at the time. I had not done anything. No, really, my name recognition came from working on Technicolor. Technicolor was very popular. I gave a lot of talks, and so forth. That helps, right? The other thing that helped a lot is that Pierre Ramond -- do you know Pierre Ramond?

ZIERLER: The other Pierre, in Gainesville.

SIKIVIE: Yeah, he knew me because he was assistant professor at Yale when I was a graduate student. Actually, we wrote on this exceptional group unification with Feza G\”ursey. So, you know, yeah, he basically hired me.

ZIERLER: I love to contextualize the term cosmology in history. So, this is right of the time of inflation and things like that. If you were looking for jobs circa 1980, 1981, in the United States, cosmology was not even a category for the faculty to advertise for. It simply was not yet on the radar.

SIKIVIE: Not yet, no. Not in 1980, but a few years later it was. It changed very quickly.

ZIERLER: I must ask then, Pierre, given your understanding of how big the frontier was in cosmology and astrophysics, did you take the job on the basis of your expertise in particle theory, but you knew where your interests were heading? Was that something that you knew that you might have had to hold privately, because that might not have been a good look? What was your thinking along those lines?

SIKIVIE: Certainly, I did not know, and I certainly was not hiding it. But basically, I did not know, because the realization that you have these domain walls in axion models came after already accepting the job. Even though I knew these domain walls somehow were important, actually, I didn't understand very well what was going to be their role. In fact, I only understood this -- I remember very well -- in the fall, my first fall in Florida. I had discovered something that was already known, just showing how little I knew. There was already a paper by the famous Zel'dovich, Kobzarev and Okun, that if you have spontaneous breaking, of CP symmetry in this case, and you have domain walls, then you have a cosmological disaster. Basically, my education in cosmology was to rediscover for myself this conclusion.

ZIERLER: Now, this was a joint appointment initially with the departments of math and physics, or it was a unique job title.

SIKIVIE: You have unearthed all these dirty secrets.

ZIERLER: I do my research.

SIKIVIE: I'm amazed. It was a little strange, actually, because as you know, in many departments, if one group grows very fast, there may be some unease. The particle theory group had Pierre Ramond, Rick Field, Charles Thorn and Thom Curtright. But they also wanted to have me. What happened is that the chair was sympathetic, and the dean was sympathetic, but to hide this fifth hire, they put it in math and physics. I was very green. I didn't know all this stuff. I was hired and then I was a little bit surprised when I arrived. People were surprised. They saw this guy, who is he? Here's this hire in math and physics.

ZIERLER: Were you prepared to teach any math classes if you had to?

SIKIVIE: Maybe some simple classes, but this was sort of a device, I think, to hide my hire. It was written very nicely from my perspective because I could just choose to be in math or in physics or keep a joint position. But as soon as the opportunity arose for me to be entirely in physics, that's what I chose. I don't like to be split, prefer to be narrowly focused, so I thought I just want to be in physics.

ZIERLER: Pierre, to set the stage, you're not sure yourself, professionally, if axions is going to be something you're going to be able to focus on in this sustained manner. When did that change? What was that process? Was that more gradual or sudden?

SIKIVIE: Right now, I think that axions are really the thing for me, but actually, I think for all of physics. I think they are really there. But that's just my personal belief.

ZIERLER: But this is a story that goes all the way back at least to 1982, 1983.

SIKIVIE: Yes. Well, there's two levels, I think, in answering your question. As soon as I saw the power of cosmology in telling us about particle physics, and the back and forth you can have, I was won over. I didn't want to go back to questions in purely particle physics. I already had a strong feeling that the standard model was it, and whatever question I could work on, there were a hundred other people working on it too, and many of these people have already done whatever I can think of. You know what I mean? I felt completely useless. But here was a new thing, you see? So, to me, I was very quickly won over by this new way of thinking. But that doesn't make axions the truth. So, that was a much longer process, yes.

ZIERLER: Were you in touch at this early juncture with people like John Preskill and Frank Wilczek?

SIKIVIE: Yeah, yeah. Right. Yes. I met them occasionally at meetings, yes.

ZIERLER: When did all of this become sort of a mature field? What was that process like?

SIKIVIE: Well, I wrote a paper on domain walls -- I think this is not the most important paper, but it did show that there's a connection with cosmology. The paper that really was important, is more important, let's say, is the one that shows that axions can be the dark matter. You know, there's three different papers next to one another in Physics Letters that are saying basically the same thing. This was some kind of -- everyone knew this was important. So, there is a paper by Preskill, Wilczek, and Wise, and this paper by Larry Abbott and me, and there's a paper by Michael Dine and Willy Fischler. All these people were talking to one another, and you know.

ZIERLER: Pierre, a broad question at this point, historically. What were the big questions in the field, and what was the unique approach that studying axions promised to answer for these questions?

SIKIVIE: I think there is an accident happening here. It's important to bring back the context of inflation. People had invented inflation -- well, first of all, of course, the psychological effect. A lot of people are interested in cosmology. But inflation brought about the problem of reheating. You know, you have inflation, and then you have reheating. When you finish inflation, your inflaton field oscillates, and it reheats. I think people realized with axions there's a similar issue. The axion acquires mass, and then the axion field begins to oscillate. So, in this context, I can see why people got the idea. In fact, a lot of the context, the original calculations, is the realization that in the axion case, there is no thermalization. In the case of inflation, the whole game is to try to show that the energy in the inflaton field becomes ordinary stuff. So, in the case of the axion field, precisely, that's what does not happen. The axion field just keeps oscillating. Also, the original focus was to show that axions -- to try to rule them out. People said you can have this invisible axion that solves the strong CP problem. Actually, I had pointed out that domain walls already tell you that there are some constraints. When people realized you have this oscillating axion field, they saw a way to constrain the axion model further. In fact, it does that, because it makes it impossible for the axion decay constant to be arbitrarily large. So, the original focus, I think, was to rule out axions. Then, when you can't quite rule it out, you realize, well, actually, it could be the dark matter.

ZIERLER: Axions have proved to be quite stubborn in that regard.

SIKIVIE: It's amazing. So, the topic of the lecture I could give at the APS April meeting -- actually it was canceled by the pandemic, but I gave it online. It's just amazing, everything that axions has touched. It is all over the place. Nuclear physics, atomic physics, cosmology. It just touches everything. Even condensed matter physics, I'm told, has axions.

ZIERLER: Pierre, why would this particular discipline proceed on the basis of a negative assumption, where the impetus was to rule out axions? Why would the assumption or the intellectual tradition not be yes, definitely axions, and here's why. Can you explain that context?

SIKIVIE: Well, I think that you always have a give and take. In the best cases, where theorists invent something, and then it has to be tested, and then it's ruled out, or it is not. That's what happened with the axion. The original model was ruled out, but theorists invented this invisible axion. So, now, can you test that? Well, you know, it's a give and take, back and forth between theory and experiment. We have not seen axions. So, if axions are seen, then it will be ruled out. Sorry, ruled in, excuse me.

ZIERLER: Where is the axion haloscope in all of this? In other words, your invention of the haloscope, is the impetus to find axions or to demonstrate that they're not there?

SIKIVIE: So, I tell this also in the lecture how that came about. I'm grateful for the opportunity to teach the core courses in graduate school. I taught E&M, and then, I had asked to visit Harvard for a month. They said yes, why don't you come, and so forth. Going there, I was looking at a paper by Sidney Coleman on monopoles, and I realized that there are some interactions between monopoles and axions. So, what I did is I started to write down how electrodynamics is modified when you have an axion. So, with Maxwell's equations for the electromagnetic field, but the axion is also a light field that can be around. It's very light. So, I just wrote these equations. Then, I thought, well, people say the axion is invisible. But they just say it's very weakly coupled, so it's invisible. Let me calculate how invisible, and that's actually how it came about. I started to cook up schemes to make it visible. Well, I can tell you in more detail, because I remember very well. I was having the problem of producing and detecting axions, right? At some point, I remember this very well, I was thinking of nuclear explosions underground producing axions. There was going to be a detector -- maybe a few axions which went through it. It was invisible, for sure, until I realized actually, you don't need to make these explosions because you have the sun. The sun produces a lot of axions. It can be like 10^14 axions per centimeter squared per second. Really huge number. So, I was kind of, you know, making these explosions that were completely useless. Hardly produced any axions, while the sun is sitting right there producing axions. And it never occurred to me. So, then, it occurred to me that actually you don't have to produce axions. The axions are already there. Not only are they produced by the sun, but they're also there as dark matter. So, I switched my focus upon detecting axions already there.

ZIERLER: Pierre, at what point do you realize that the experiments at Florida with the haloscope are not going to be sufficient, and that ultimately, you're going to have to take this elsewhere for what obviously is going to become ADMX?

SIKIVIE: This also happened at a very particular moment, at a conference in Brookhaven. The first person to actually do the experiment is a guy called Adrian Melissinos. He was at Rochester and had a collaboration. He did a lot of really innovative experiments, and he did the axion experiment. He did it, and then we did it, too. But he did it a little bit earlier. When we had our results, they were short by a factor of a thousand, or something, in sensitivity. There was a conference in Brookhaven, and a lot of people who were interested were there, and we all sort of expected that it would fall to Adrian, and Adrian would say, "Well, now we do the next big experiment." We all turned to Adrian, and Adrian said, "No, I'm not going to do it. It's too difficult." That's basically what happened. Then, we were in the doldrums. Is that how you say that nothing happened?

ZIERLER: Yeah.

SIKIVIE: But we did get a big experiment, and basically what happened is that at the time, Karl van Bibber was at Livermore. There are some big magnets in Livermore, because they confine the plasma in fusion experiments. There are some magnets lying around in Livermore, and Karl said, "Let's use these big magnets in Livermore." So, we went to look at these really big magnets lying around in some parking lot, and okay, so let's talk about what we could do with these magnets. But it's difficult to use these magnets. They had not been used for a long time. They need a power supply, and so forth. Karl went down the street, and there was a company called Wang NMR. You know, Mr. Wang building NMR magnets. Karl went to talk to them and said, "What would you charge to make a magnet?" And also, Michael Turner was very supportive. Michael is very persuasive. I don't know if you heard of him, but he's very good at convincing people, usually. He has good taste, so he convinced the lab to just shell out money for this magnet from Wang NMR. Actually, Wang didn't even charge that much. I think the magnet cost something like $300 or $400 thousand. It was not a fortune, and I think it's basically money from Livermore Lab. So, then the experiment was established at Livermore, and Karl worked on it, and a bunch of students, and a bunch of people, and also Wolfgang Stoeffl. Do you know him?

ZIERLER: Yeah.

SIKIVIE: You know Wolfgang Stoeffl?

ZIERLER: Yeah.

SIKIVIE: How do you know Wolfgang Stoeffl?

ZIERLER: This is what I do, Pierre. I talk to physicists all day long. This is my job.

SIKIVIE: Alright. I think he is a very careful guy and built the experiment. I'm not saying it was just him, but he built the experiment at Livermore, and then it attracted more people, including Leslie Rosenberg. Leslie Rosenberg got a job at Seattle, Washington, and after a while decides it's too difficult to travel back and forth -- Leslie became basically the main spokesperson. He wanted the experiment moved to Seattle, and that's where it is at the moment.

ZIERLER: Pierre, given why it made so much sense for the experiment to transfer to Livermore, I'm curious, there are so many what ifs with regard to had the SSC been built to completion. For you, if the SSC had gone through, would that have been relevant for axion research in your mind?

SIKIVIE: When was the SSC canceled? It was the mid '90s, wasn't it?

ZIERLER: I mean, the death of it is really completed in '93, '94, but in its planning stages, '85 to '89, this is the time when the promise of its completion was really pretty strong at that point.

SIKIVIE: I think that I had switched over to dark matter and cosmology, a little bit like Bernard. You mentioned Bernard Sadoulet, right? Bernard also made this transition, and he was very much in the midst of particle physics with UA1. He was on UA1, right? And he decided to make this transition. He made this transition quite abruptly because he was at the top of the field with UA1. I think, at the time, it was looked at a little bit with suspicion, not because of the people involved, but -- let's forget about dark matter. Let's talk about neutrinos. People had been pushing for these proton decay experiments, like the experiment by Michigan, Pennsylvania, Wisconsin. These became the big neutrino detectors. These people were pioneers, I think, because they pushed money away from accelerator physics, and they had to fight very hard to make it happen. I think Bernard had the same struggle -- Bernard is very persuasive and was very influential in making this field grow. But I think for a while, it was sort of considered in competition with accelerator physics. We had very little money to start with. We had to scrounge. Now, this is changed. There are big dark matter experiments now, but at the time, we had to really struggle to get money.

ZIERLER: Pierre, in what ways did ADMX really come into its own by the time the operation was transferred to Livermore that might not have been possible had it stayed in its current form in Florida?

SIKIVIE: Well, the main reason ADMX moved-- it could have been in Florida. We just didn't have the resources. We didn't have the magnet. So, in a way, I'm grateful to Karl for making it happen in Livermore. Karl made it happen by getting this magnet from Wang NMR. How would we have been able to do it? I mean, I don't think the DOE was going to give us $400 thousand for a magnet. It's sort of an accident. At the time Livermore had a pretty active role in cosmology because of the MACHO search. Remember? There was a MACHO search at Livermore. The main person, I forget his name now, was at Livermore. [The name is Charles Alcock.] But they had other interests in dark matter. So, there was kind of an accident, and Karl, I think, had a lot of influence in the lab. He was an influential person and he could convince people to make this happen. He got the money from the lab, as far as I know. I don't know who else paid for the magnet. Once a magnet is there -- I like to tell this to axion searchers -- get yourself a big magnet. It's like having a lot of clout. If you have a big magnet, then things will accrete onto it. It's like your real estate, you know? Big magnet.

ZIERLER: Pierre, another question about optimism. Given the fact that dark matter, to this day, remains a mystery, to go back all the way to the early 1980s, what was the optimism, or what was the excitement with regard to your field of research, with regard to axions, where there was the sense that there was a real possibility that the mystery of dark matter might be solved through the axion experiments?

SIKIVIE: Yeah, so, when we built the axion detector, I think there was a real sense of something exciting -- you have a new window here, and something big may happen. Of course, it didn't happen. But we were not really sensitive, so nothing really should have happened. In my mind, the motivation has never disappeared. There's always been good reason to think that the axion haloscope can find axions. It's just been extremely slow, and well, I don't know, extremely slow.

I guess, slow compared to the WIMP searches. The WIMP searches grew very fast, and have become very big, and actually very successful in ruling out WIMPs. Relatively speaking, the axion searches are just beginning. They're just beginning to rule out parameter space, and they have a long way to go. So, in a way, the motivation has never disappeared. It has always remained the same. It's just been postponed.

ZIERLER: What had been some of the feedback mechanisms, both experimentally and theoretically, over the years, that have given you a good sense of whether or not ADMX has been on the right track?

SIKIVIE: Well, ADMX, I think is on the right track. For me to explain why I think so would be pretty longwinded. You could say ADMX is just a shot in the dark, because it can look at a particular window in frequency, and you have to go higher, and you have to go lower. Well, first, people have ideas how to go higher, and how to go lower. There's competition. At Yale, there's this HAYSTACK experiment. There are experiments in Korea. There are experiments in Germany at DESY, and so forth. I think the fact that it has taken a lot of time should not be held against the idea. You know, you have to think that the Higgs was proposed in '64, whenever it was, and it was discovered 50 years later.

ZIERLER: However, with the Higgs, there's the theoretical basis that it's always there. It's just a matter of at what energies are we going to see it? Is that a fair comparison with the axion, or is it not?

SIKIVIE: I think it's a fair comparison.

ZIERLER: So, that begs the question, what are the theoretical underpinnings where we can say the same thing? We know it's there, it's just a matter of developing the right experiment to prove it.

SIKIVIE: The thing with the Higgs is that you knew it had to be roughly between 100 GeV and 1000 GeV. It had to be. You could actually guess it was probably going to be in the lower part of this range, maybe below 200 GeV if you did some radiative corrections in the standard model. The trouble with the axion is you don't know the mass. So, the mass can be all over the place. There are limits on the mass, but these are limits from astrophysics and cosmology. And you know it's not that strongly constrained. People will always find some way to go around a limit from cosmology. On the other hand, I think that when people go around limits, they usually make it more complicated. You know that usually in physics it is the simplest version of new ideas that actually turn out to be correct. This is the way I read physics. Difficulty is not in finding some complicated model. The difficulty is getting the right idea. Usually, when you have the right idea, it's something like that. I saw this when I was a graduate student with the standard model, because Weinberg had the idea -- actually, Glashow before, even -- that the model should be SU(2) x U(1). Once you have the idea that it should be a broken gauge theory, SU(2) x U(1) is just an example. But it is sort of the straightforward example. Many people made other examples, including my collaborators and I. But the thing that turned out to be correct is the simple version of this new idea. So, maybe you could hope that the axion is going to be something similar. You have these axions, they are the dark matter because of this realignment mechanism. Maybe there are variations that people cook up, because people need to write papers, right? They need to get the next job, so they make variations.

ZIERLER: Pierre, in what ways have development and advances in QCD, have they been relevant in axion research over the past 30-40 years?

SIKIVIE: Well, of course, QCD itself is one of the original drivers for axion physics. But once QCD was established, it doesn't have that much more influence, I think. It did have influence because the strong CP problem can also be solved by having not an axion, but a zero quark mass. This is not my field, but people who do QCD lattice calculations claim to be able to show that a quark mass cannot be zero. So, that's an important influence. Makes the axion actually more necessary. Another influence is that people do lattice calculations of the axion mass as a function of temperature. If you do it analytically, you have various approximations. In the lattice you can do it, in principle, correctly. But you know, the lattice also has its limitations. It has limitations of resolution and so forth. But in principle, you could have the lattice calculations tell you the axion mass as a function of temperature in the early universe. That is useful, and some people claim that this has determined the axion mass more accurately. But, okay, there are still too many other things that you don't know. It's like you know one thing maybe more accurately, but there are still many other uncertainties. So, the big picture doesn't really settle the issue of what is the axion mass.

ZIERLER: Pierre, as you know, perhaps better than anyone else, given the fact that axion physics originate in a modification of Maxwell's equations, that might suggest to an outsider that there's something more fundamental about axions. It begs the question, why were people not thinking about axions 30-40 years earlier? 1983 seems to be rather late in the game to make this realization that Maxwell's equations can be modified to give us the possibility of axions.

SIKIVIE: Yeah, I think that is a very good comment, yes. Why not? I don't know. But you're right, because Maxwell's equations is the equations for a massless vector field. Of course, the generalization is just some other light bosonic field. A light boson field would be the natural thing to think of. So, you're right, actually. If you'd just follow that logic, you would then just say, well, let's imagine there's a light boson. It almost certainly has the coupling to the electromagnetic field that is characteristic of axions. You would then have what's called axion electrodynamics. That's right. Yes, in principle, it could have just been thought of like that. Right. But that doesn't necessarily happen. Maybe someone did, actually. I don't know.

ZIERLER: Given that axion physics in two years will be approaching its own 40th anniversary, given the fact that you are so optimistic that, as you say, axions is really where it is. This is something that you strongly believe this is where fundamental discovery is coming. Do you see that trajectory generally stable from the early 1980s? Has the field experienced ebbs and flows in terms of optimism, in terms of people working on it, in terms of funding and collaboration?

SIKIVIE: David, I should say, if you asked me 15 years ago, I would have said, "I have no idea where the axions are that they're talking about." It's only ten years ago or so that I claimed to have an argument that the dark matter must be axions, at least in part. This has to do with Bose-Einstein condensation and is not widely accepted. Actually, no one accepts it, I think, pretty much. It doesn't bother me. I think it is correct. So, if you ask me, this is what I'm saying. I think the dark matter must at least in part be axions or an axion-like particle. Maybe not the original axion, but something that behaves like it. This has to do with Bose-Einstein condensation and is a very long argument. So, since that time 10-12 years ago, I've not wavered along this line, but before that I had no idea, honestly. If people found in, say, 2005, that it’s WIMPs, and there's no axions, I'd say, "Well, sure. That's the way it is." Now, if you ask me today, even if they found WIMPs, I would say, "It cannot be the whole story." That's what I would say today. But you are just asking me personally. If you're asking the whole community, there has been a very big change, because people did not find WIMPs. The WIMP people have built very big detectors, and actually, they deserve a lot of credit for the achievement, because their limits have been improving all the time. They've looked for WIMPs with higher and higher sensitivity and they never found anything. So, they did a great job. For them, the unfortunate result is that now axions have become more credible. This has had a lot of influence in the community. And justifiably. It should have influence. But I always thought that axions should be taken seriously as a candidate. So, I don't like when people say, "I found the dark matter. It's 100%." Because there's no way you can show you have 100% of the dark matter. If you find, say, MACHOs, you should not say the dark matter is MACHOs, because actually, the dark matter could be many forms. If I challenge you, at best you can say the fraction of dark matter you found is maybe 70%, or maybe it could be even 95%. But if you establish a lower bound that it is 5% of the dark matter, that's very interesting. It's not the percentage that matters. What matters is whether you find anything at all. Right? You know what I mean?

ZIERLER: This is to say, Pierre, that perhaps there's no LIGO of dark matter. There's no one experiment that will conclusively and with great satisfaction say this is dark matter, and that's all that dark matter is.

SIKIVIE: That's right. That's correct, yes. Dark matter could be an endless question. There always may be some additional dark matter that we don't know about. Even if we know most of it, there's still maybe some more that we don't know.

ZIERLER: Pierre, have advances in black hole research been relevant and important for axion physics?

SIKIVIE: Well, psychologically, I think it is helpful because, again, here we see fundamental work being done with a new tool. Actually, it took a very long time. The idea of this laser interferometer for gravitational waves is 50 years old, I think, or something like that. It took a very long time, but now it turns out to be a very powerful tool. So, that I think is beautiful because it's a new tool, and axions potentially could provide a tool like that. I think it makes people think -- okay, accelerator physics has been the golden road to new physics, right? You discover so many things. But it is just a tool. It's not a rule of physics that everything is going to be found with accelerators. That's not a one of the laws of physics, right? The more tools you have, the better.

ZIERKER: Pierre, just to bring the conversation and the research up to the present, given that you're so bullish, you're so optimistic about axions today, where is the field, and what's so promising about what's going on now at the University of Washington, for example?

SIKIVIE: Well, I think, thanks to the persuasiveness of people like Leslie Rosenberg and others -- I'm very unpersuasive. When I ask for money for an experiment, I'm sure to tell out of honesty every reason they should have doubts in giving me the money. So, I actually get nothing.

ZIERLER: Maybe that's a caricature trait of Belgians.

SIKIVIE: There is something to that. I think Belgians don't have this tradition of salesmanship I find elsewhere. Anyway, thanks to people like Leslie, there is funding. Leslie has made the point very forcefully that axions are a very plausible dark matter candidate. There is an experiment possible, and that should be funded, and actually, the DOE has listened to him. So, you know, the DOE has funded this generation 2 axion experiment, and it's part of its portfolio of projects that it has been looking after. So, that has been a very positive thing. In addition, I think people all over the world have started to look for ways to search for axions as dark matter. There's a very big effort in South Korea called CAPP, the Center for Axion and Precision Physics. They have many cavities that are being built. There is an experiment at Yale, there are experiments at DESY. So, you know, there's a sort of sociological movement in the direction of axion physics. It's something very interesting to do, to think about. So, the result is that there are many more conferences and so forth, and opportunities to discuss and socialize and so forth.

ZIERLER: Pierre, now that our conversation has worked up to the present, I'd like to ask, for the last part of our talk, a few broadly retrospective questions about your career, and then some looking to the future. One thing we haven't covered yet is your career as a teacher to undergraduates, and a mentor to graduate students. So first, I'd like to ask, over the years, and particularly the transition from when you were hired at Florida on the basis of your expertise in theoretical particle physics to the transition more to astrophysics and cosmology. What have been some of the most rewarding classes for you to teach undergraduates with all of your areas of expertise?

SIKIVIE: Well, to undergraduates I basically taught quantum mechanics, and I taught thermal physics, and for a while, I taught in the big auditorium. The big auditorium did not suit me very well. I tend to be uncomfortable in front of hundreds of people. I'm actually very comfortable talking to one person. Even two people, I find this too difficult. You first asked undergraduates. So, I really value teaching. Not so much because I like it. I mean, I do like teaching alright, but I really do like the opportunity to review the laws of physics. So, this did not happen very much with the undergraduate quantum mechanics course, but I did teach also thermal physics, which I had not thought about much since my own undergraduate days. But I found it was very interesting. I was not very happy to be given this course, thermal physics. And I don't think I was that popular with the students because I tend to worry much about the logic of thermal physics, and it is somewhat murky at times. Undergraduates don't care that much about the logic of it. Thermal physics is sort of an introductory course to physics for undergraduates, so they want someone who will really capture their interests. Not someone who is going to worry about what does it really mean precisely, you know? But for me, I liked the opportunity to review the topic. Actually, I learned a lot of stuff I'd forgotten. I told you, in E&M, teaching that course was very useful for me.

ZIERLER: Back to basics.

SIKIVIE: Reviewing all the basics of doing all these problems, because when I asked how one would use electromagnetics to find axions, I got all the tricks. I knew all the tricks because I'd done all the problems. So, that was very useful. Then, classical mechanics is good. I don't think I learned much new there, but quantum mechanics, you can never think too long about quantum mechanics. So, that, I think, has really been rewarding, to teach quantum mechanics for many semesters. Probably more than I should be entitled to. But I find it really interesting. Then, there's quantum field theory. That's such a difficult topic, you also have to spend an enormous amount of time on it, so teaching it is good for you.

ZIERLER: Pierre, surveying your career as a graduate advisor, I wonder if you might reflect on your own unorthodox experiences at Yale, in terms of wanting to leave without having a fully formed thesis. In what ways, for better or worse, has your personal experience as a graduate student informed the kind of graduate advisor you are to your graduate students?

SIKIVIE: I insist upon seeing the students every week, because I don't want them to be in a corner, not sure what they're doing. I'm not sure it's better, actually, because I think when I was a graduate student, it was upon me to come up with something. In many ways, that may be a better education. It doesn't, maybe, produce all the results immediately, but it forces one to really be original, and be your own person. So, I'm not sure. I'm a more conventional advisor in many ways. I see the students every week and ask them what they are up to. I look after them. I am not essentially a teacher. I don't think I did physics because I wanted to teach the stuff. I think it's people's own business to learn physics. If they're interested, they can just learn it. I'm more interested in research, by far. Much happier working on my own. But I like to teach. I like to be clear. I like the process of clarifying some topic to such an extent that a student can ask me pretty much anything about it, and I know the answer.

ZIERLER: Pierre, given that it was so recent, let me take the opportunity to congratulate you for winning the Sakurai Prize last year.

SIKIVIE: It was a very big honor for me. I was really delighted, yes.

ZIERLER: I'd like to ask specifically, because so much of axion research is hypothetical, or there's the promise for what it might achieve in the future, to what extent was this recognition a metaphor that a critical mass in the physics community has come around to recognizing it's important? Given your stature within the axion community, and the honor and recognition of the Sakurai Prize, how might you put all of these things together in your mind?

SIKIVIE: Well, you know, it's difficult to know exactly how the community thinks. You know, you win a prize for a number of reasons. One of them is that you have a colleague who's willing to take the time to promote the idea that you should win the prize. I know I was helped by my colleagues in Florida, for sure. But also, the idea of a theorist having proposed experiments, experimentalists like that because you give them the opportunity to do new experiments. The axion has not been found, so in a way the prize was given just for the idea that you might find axions in a particular way. But that's not unusual for the Sakurai Prize. Unfortunately, particle physics has not had that many ideas recently that are really proven to be correct. So, the same is true about axions. The prize is only given for showing that they might be discovered. I think it's true that the prize was given at a time when axions got sufficient recognition. It probably would not have been possible a while ago. They might say, well, this is not sufficiently established. But, you know, Peccei and Quinn were given the Sakurai Prize, and I think the main reason was the Peccei-Quinn symmetry, and that was not established. So, the Sakurai Prize is often given, I think, for things that are not really established to be reality but have had sufficient influence. It would be better to be able to say it is reality. But the Sakurai Prize, for me, felt very good. I was very happy, honestly.

ZIERLER: Absolutely. Well, Pierre, for my last question, looking forward, I'll save the big one for last, and that is, one of the things that's so fascinating intellectually in hearing about all the different approaches in the search for dark matter, it's really remarkable how many different kinds of physicists and physics are involved in this effort. Really, a broad array. So, I wonder if you might situate your expertise in axion physics within that broader collaboration of all kinds of physicists searching for the dark matter, what does axion physics have to offer this broader effort, and even more broadly conceived than that, what do axions, if the research goes where you hope it will, where you assume it will, what does that tell us more fundamentally about the nature of the universe and its origins?

SIKIVIE: Well, I think I already touched upon the fact that axions have this sort of amazing property to have connections to so many different areas of physics. So, one of the reasons it’s been very rewarding for me is it's given me the opportunity to learn about these different things. It almost has forced me to learn a lot of things. Cosmology, of course, is the most obvious thing, because I didn't know cosmology before. But more recently, Bose-Einstein condensation. When I was a graduate student, I was taught about Bose-Einstein condensation, but I really understood it very superficially. But I do think it's really relevant to axion cosmology, so now I understand it much better. So, I've learned that, too. Axions, somehow, give the opportunity, if you are curious about the properties, to force you to learn various things. In that respect, you would think it almost should exist just merely because of the richness of the physics of it. Very few things have this property. So, the future, I don't want to say what is the future, but I already mentioned my conviction that part of the dark matter is axions. So, in principle, you can find them, and when you find them, it will be a very powerful tool, like LIGO. Actually, I think it will be at least as good as LIGO.

ZIERLER: I hope so. That would be very exciting indeed.

SIKIVIE: Yeah, it hasn't happened, but let's hope it might.

ZIERLER: Let's hope. Pierre, it's been a great pleasure spending this time with you. Thank you so much for doing this. I really appreciate it.

SIKIVIE: David, I really appreciate you doing the interview, and I appreciate that you were actually so well prepared. I don't have to explain many things. You already know about them.

ZIERLER: It's my pleasure.