THE DEPARTMENT OF ENERGY ORAL HISTORY PRESENTATION PROGRAM

OAK RIDGE, TENNESSEE

AN INTERVIEW WITH ALEX ZUCKER

FOR THE

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INTERVIEWED BY

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STOW: As we look back over the history of ORNL, Alex Zucker has to rate as one of the pillars of strength in the history. He came here in 1950 with a degree in physics and retired in the 1990s, having contributed greatly to the science and the management aspects of the Laboratory. He was acting director of Oak Ridge National Laboratory for one year and associate laboratory director for two different directorates. So, that's what we'll be talking with him about today.

Alex, you were trained as a physicist -- take us back into your early days and tell us a little bit about how you got interested in physics. Was there any particular person or event that got you started in sciences?

ZUCKER: I know exactly when I decided to become a physicist.

STOW: Okay.

ZUCKER: It was during World War II. I was in the Army.

STOW: Yes.

ZUCKER: I was in Germany and the war was winding down, and I was standing on top of a building on the Rhine River. I was on guard, and, all of a sudden, it struck me that I'm going to have to decide what I'm going to do after the war. I had been in college before studying chemical engineering, and I was pretty sure I didn't want to be a chemical engineer.

STOW: Yes.

ZUCKER: I decided at that point I would go into physics. Now, that was before the atom bomb. So, you know, the atom bomb just sort of reinforced my interest in physics. But, that was the point at which I decided on physics.

STOW: Did you know about fission at the time? Had you read of that and heard of it?

ZUCKER: No. No.

STOW: So, you didn't know about nuclear fission.

ZUCKER: They didn't tell us that. (laughs). That wasn't part of my course work in college or training in the Army.

STOW: All right. Okay. Well, just thought I'd ask.

ZUCKER: They didn't know anything about that. Well, in fact, nuclei were not very well understood at that point, and I only had elementary courses in physics.

STOW: All right.

ZUCKER: So, I really didn't know anything.

STOW: And, you did your undergraduate work where?

ZUCKER: At the University of Vermont.

STOW: All right. And, then went on to Yale for your doctorate work?

ZUCKER: Yes. Right.

STOW: Why did you pick Yale? Outstanding place obviously ...

ZUCKER: (laughs) Well, I went to an advisor at Vermont and I said, "Where should I go to graduate school?" I think he was pretty busy that day, and he said, "Well, how about Harvard, Yale, or Princeton?"

STOW: Just like that?

ZUCKER: Just like that. He said, "You can't go wrong with those three." I don't remember why I picked Yale, but Yale was a very good place for me to go.

STOW: Columbia wasn't good enough in your professor's eyes?

ZUCKER: Well, I wasn't going to go to Columbia University because my parents lived in New York, and I didn't want to mix that up.

STOW: Okay. What attracted you to Oak Ridge? Did you even know that Oak Ridge National Lab existed at that time?

ZUCKER: Oh, yes. I knew the Lab had a national reputation at that time, and I thought about working in a place that had researchers I knew were good. The Lab was very strong in electronics and detectors, and I thought these things would be useful for me.

STOW: Who were the individuals down here that you knew of?

ZUCKER: Well, P. R. Bell and Art Snell. Those were the two I seem to remember.

STOW: Famous names by today's standards.

ZUCKER: Yes.

STOW: Did you ever expect that you would spend your entire career here?

ZUCKER: No, I came down here for two years and a maximum of five. That was my plan.

STOW: Okay.

ZUCKER: And, I stayed really because I thought the opportunities for research were wonderful. You had a lot of freedom. You had equipment. You could do things you wanted to do. I didn't know of any better place.

STOW: And, you were oriented toward basic research. Applied research didn't exist back then, did it?

ZUCKER: Well, the Lab was a reactor lab -- so, in that sense, it was an applied lab. But, I was a basic nuclear physics scientist, and that's what I did for a living, so that's what I was looking for.

STOW: What where your reactions, Alex, when you came to Oak Ridge the first time and saw a town that had not evolved that far beyond an Army town?

ZUCKER: My first evening some people invited me to dinner and after dinner they said, "Come look at our sidewalks." (laughs) And, I said, "Your sidewalks?" They were very proud that they had just gotten sidewalks.

STOW: Yes.

ZUCKER: So, I knew something was different. But, I liked it. I stayed at the Guest House [later called the Alexander Inn]. Once I took a bus and said, "Take me to the end of the line." And so, I got a view of the town. That's an interesting way to see a city, by the way. You get on some public transportation, and you go as far as it goes. But, the main thing for me was that they had just built a cyclotron, which they couldn't show me ...

STOW: Yes.

ZUCKER: But, they said it was there, and I would be able to work on it.

STOW: Why couldn't they show it to you? Was it a security issue?

ZUCKER: It was at Y-12 where security is tight. It turns out that the cyclotron didn't run for another two years.

STOW: Okay. We'll come back to the cyclotron in a minute. You were married at the time that you came to Oak Ridge, right?

ZUCKER: No. I was single.

STOW: Oh, you were not. Okay. You met your wife here?
ZUCKER: Yes.

STOW: Where was she from?

ZUCKER: Joan-Ellen came here at the age of twelve in 1944 with her family.

STOW: Yes.

ZUCKER: Before that, she'd grown up in the Northeast.

STOW: But, she'd grown up in the Oak Ridge area, right?

ZUCKER: She'd gone to elementary school and high school here.

STOW: So, you came here in 1950 to work on the cyclotron. What size was the cyclotron that you came to work on initially?

ZUCKER: It was called the eighty-six-inch cyclotron. Cyclotrons were numbered by the diameter of the magnetic pole.

STOW: Yes.

ZUCKER: And, it was going to be a 20-MeV proton machine. In any event, I never worked on that.

STOW: So, you got working on a sixty-three-inch cyclotron, right?

ZUCKER: First of all, when I got here, they had nothing for me to do, because the cyclotron wasn't running. So they said, "Why don't you read some books?" And that was terrible. I was ready to quit.

STOW: Yes.

ZUCKER: I didn't come here to read books. Then, the following thing happened. There was some question about whether the hydrogen bomb, a very powerful hydrogen bomb, might ignite the atmosphere.

STOW: Yes.

ZUCKER: And, the atmosphere ignition was a nuclear reaction in which two nitrogen nuclei fuse and transmute into a carbon nucleus and an oxygen nucleus.

STOW: That's right.

ZUCKER: And that reaction liberates ten million electron volts.

STOW: All right.

ZUCKER: Now, if the atomic bomb is hot enough, the chain reaction might heat up other nitrogen nuclei in the air, causing the whole atmosphere to blow up!

STOW: Just gets going ...

ZUCKER: Right. So that's a really serious problem. Everybody thought it wouldn't happen, but we had to be really sure. So, my job then was to develop an ion source to accelerate nitrogen ions. The idea was to get two nitrogen nuclei in the lab to collide at twenty-five million electron volts and see what happened.

STOW: See if they fused ...?

ZUCKER: See what the probability was that two nitrogen nuclei would produce a carbon and an oxygen nucleus. To do that, you need nitrogen ions with a high atomic charge.

STOW: All right.

ZUCKER: So, I got the job to develop an ion source. And, that was good. I used the Y-12 calutrons as a mass spectrometer, and I built a graphite source -- and it went very well. I used the technology from the calutrons, and in about two or three months, I had an ion source of triply charged nitrogen [three positive charges per ion resulting from removal of three electrons] that produced milliamperes. They were looking for microamperes, so it worked.

STOW: Yes.

ZUCKER: Then somebody would build a cyclotron using that ion source and accelerate the nitrogen ions to about twenty five million electron volts. I remember when Alvin Weinberg came to the Electronuclear Division and said, "The Electronuclear Division's going to build that cyclotron." Then they tried to hire somebody to come and build it. I think they couldn't find anybody. But I was there. My ion source was finished. My boss, Bob Livingston, said, "Why don't you build a cyclotron." And I said, "Well, I don't know how to build a cyclotron." He said, "You worked on a cyclotron." And that's true. I worked on a cyclotron at Yale.

STOW: Yes.

ZUCKER: If you worked on a cyclotron at Yale, you really understood it because there's no technical staff. I mean, you had to do everything. And, that turned out to be a very good thing. I said, "Oh, okay. I'll build a cyclotron." And so, we took one of those magnetic gaps that was used for a calutron and put

circular pole pieces inside it to increase the field to something like fifteen kilogauss, or 1.5 tesla, and built the structure, the innards of the cyclotron. In about a year, we had a cyclotron, and it cost $150,000.
STOW: That was a lot of money back then, wasn't it? (laughs)

ZUCKER: Well, no. It was a bargain even then.

STOW: Okay.

ZUCKER: Because, you didn't even have to buy a magnet. All you have to do is make those two pole pieces. And, you had to build an RF system. We scrounged a power supply from the calutrons to run the ion source. Some parts were scrounged, and some were specially designed and built. But, three of us built it. There was a mechanical engineer, Ed Mann, who lives in Clinton now. And, there was an electrical engineer who built the RF system and the resonator. And, I went from one to the other and made sure everything fit. I got the equipment together to make the measurement.

STOW: That's fascinating. In other words, your sixty-three-inch cyclotron was born from a calutron.

ZUCKER: Yes.

STOW: And, the calutron itself was born from a cyclotron back at California, right?

ZUCKER: (laughs) Yes, right.

STOW: You brought a drawing of your cyclotron.

ZUCKER: Yes.

STOW: Tell us a little bit about that.

ZUCKER: Well, this is a drawing from the times when draftsmen really drafted. This is a cross section through the cyclotron. The cyclotron was vertical because the magnetic fields of the calutrons were horizontal. So, the magnetic field is in this direction. These two structures here are the "Ds" -- they're the electrically charged "Ds" and they oscillate -- go from plus to minus, plus to minus about five million times a second. The ion source comes in from the back. It's right there. And, this machinery here is the pump. The whole thing has to happen in a vacuum. The RF system is outside -- there's no picture of it -- and this is some kind of an imaginary experiment.

STOW: When we say sixty-three inches, what are we referring to?

ZUCKER: The pole pieces of magnet that you can just barely see over here.

STOW: In other words, across the "Ds."

ZUCKER: From about here to about there. And there are some adjustments. It was the first cyclotron that was built to accelerate heavy ions. It was the world's first heavy-ion machine.
STOW: Well, we tend to think that bigger is better, and you came here to work on an eighty-six-inch cyclotron that wasn't available -- didn't run – so you ended up building a sixty-three inch. Was this a better cyclotron than the eighty-six inch?

ZUCKER: For me, yes, of course. It was my cyclotron and I did research using it. I did measure that cross section [related to the probability of nitrogen fusion that could ignite the atmosphere].

STOW: Yes.

ZUCKER: I measured the nitrogen-nitrogen cross section. Harry Reynolds and I measured that cross section together.

STOW: Yes.

ZUCKER: And, we came up with a number that said, essentially, that the atmospheric fire was not going to happen. Then we were on our own. And, then we used the nitrogen beam accelerated to twenty-five million electron volts to do nuclear physics.

STOW: Now, I wanted to ask you what some of the relevance of this basic research was back then, or were you just doing basic research for the sake of doing basic research? Did the Atomic Energy Commission have basic applications in mind for what you were doing?

ZUCKER: No, they didn't. It was a basic instrument, and I spent a couple of years, when I first got married, explaining to Joan-Ellen what basic research was.

STOW: Yes.

ZUCKER: I would tell her and she would say, "But what is it used for?" She knows now. And, really what we did was investigate what happens when two nuclei come together.

STOW: Yes.

ZUCKER: ... two large nuclei. It had never been done. People always had cyclotrons that accelerated protons or deuterons or alpha particles -- helium nuclei -- into a large nucleus. So, it was a small projectile onto a large nucleus.

STOW: Yes.

ZUCKER: A heavy-ion machine gave us the opportunity to see what happens when two heavy nuclei collide.

STOW: Okay.

ZUCKER: So, this was a first. The field was all new, which was great in some ways because anything you did was new, and it was a paper. On the other hand, it wasn't so good because there wasn't a community that you could relate to. So, we were on our own for a few years.

STOW: So, you and Harry Reynolds produced the first beam of heavy ions, did you not?

ZUCKER: Yes, yes.

STOW: Okay. Now, that's the first time I've understood the difference between your cyclotron and the ones that E. O. Lawrence developed out in California. They took a lighter particle and slammed it into a big one out there.

ZUCKER: Yes -- right, right.

STOW: That was the basic difference, right?

ZUCKER: What you looked for -- as the science of heavy ions developed -- was reactions where two ions just sort of pass each other and something goes across ...

STOW: Yes ...

ZUCKER: ... or they fuse and then they oscillate. You look at the dynamics of the nuclear reactions, and you also produce certain residual nuclei that you can't make with light projectiles.

STOW: Let's jump a little bit in time here. Tell us about the APACHE Project. I think it was called Comanche at one time, but APACHE was the name that stuck. This evolved ultimately into the Holifield Heavy Ion Research Facility, did it not?

ZUCKER: Yes.

STOW: Can you take us through some of those steps there and your involvement in it?

ZUCKER: Well, what happens when you build a machine like a sixty-three-inch -- just when you're most productive -- you've got to stop and then invent a new machine.

STOW: (laughs) That's one of the rules of the game, huh?

ZUCKER: That's a rule. If you wait until you're tired of what you're doing, you're not going to get it. So, that's what we did. There were several iterations of heavy-ion cyclotrons --APACHE was a cyclotron and a tandem together -- and we wrote proposals to the Atomic Energy Commission to build this machine. And now, I have to tell you something about proposals.

STOW: Okay.

ZUCKER: The sixty-three-inch cyclotron had no proposals. The Oak Ridge Isochronous Cyclotron, or ORIC, was our next machine.

STOW: Okay ...

ZUCKER: When I tried to find the proposal for the ORIC in order to build Holifield, I asked my secretary to find the proposal.

STOW: Yes ...

ZUCKER: She came back and she said, "I can't find it." So I sent her back to look again. Well, it turned out the proposal was a page-and-a-half letter from Alvin Weinberg to Glenn Seaborg.

STOW: Oh, my goodness.

ZUCKER: And it said, “We would like to build this thing" -- a cyclotron -- that's ORIC -- that's a big machine -- and the building and all that, and I think the whole facility cost twelve million dollars. I didn't see the return letter, but the return letter, I think, said, "Okay."

STOW: (laughs)

ZUCKER: Then, when it came to APACHE and Holifield, we went from a page and a half to 500 pages. New proposals were this thick -- a dozen people worked on them.

STOW: Yes.

ZUCKER: And, they were no better than a page and a half, except we spent a lot more money and a lot more effort, and people stopped everything they were doing to get this new machine. So, there's no question in my mind that proposal writing is bad. But, you know, that's where we are. And, I hate to even think what the SNS [Spallation Neutron Source] proposal really looked like. Anyway, APACHE then was sort of a step beyond the sixty-three-inch. Then we finally built the ORIC.

STOW: Now, there was some relevance in there to national needs, was there not? As I recall, the APACHE project did not obviously address a national user facility or national needs, so we had to expand our thinking. Ultimately, this did grow into the Holifield accelerator that came online in the '70s.

ZUCKER: Holifield came online in about '74. Well, what happened afterwards speaks to the sociology of the nuclear physics effort in the country.

STOW: All right.

ZUCKER: And, that's true of high-energy physics as well. The decision to build [a research facility] settled down into an evaluation process. That is, people would propose things, and then there were committees to evaluate them, and then they'd put together committees that would decide what would be the next thing to build for nuclear physics.

STOW: Sure.

ZUCKER: And, the Holifield -- it was named Holifield after it was built -- became the next step. Then, after that, other machines were built. It was all done by committees.

STOW: All right. Well now, in 1972 you were named director of the ORNL National Heavy Ion Laboratory?

ZUCKER: Right.

STOW: What did that appointment involve?

ZUCKER: Well, that was my reward for getting the money. (laughs)

STOW: Okay. All right ....

ZUCKER: I was in Washington at that time.

STOW: You'd spent a two-year appointment there with the academy?

ZUCKER: I was with the National Academy of Sciences, and I was running its Environmental Science Studies Board in its environmental program.

STOW: I want to come back to that because that's a significant deviation from physics, wasn't it?

ZUCKER: Yes. That was a deviation, right. Yes -- but anyway -- I was the spokesman for what turned out to be the Holifield [Heavy Ion Research Facility at ORNL]. At the time it was termed a national heavy-ion machine.

STOW: Yes.

ZUCKER: And, so, when we got the money, then I got appointed to run that. And, this was a very tricky time because that was the time when Alvin left the Lab.

STOW: That's right.

ZUCKER: And, Art Snell [associate laboratory director for the physical sciences] left the Lab. I'll have to tell you about my return from Washington ...

STOW: Please do.

ZUCKER: In Washington, I had a very interesting time of it at the Academy, as you can imagine.

STOW: Yes.

ZUCKER: It was a very interesting job. And, I came back here and I was supposed to build this machine. And, the division I was with -- the Electronuclear Division -- had merged with the Physics Division when I came back, and I said, "I need an office." And, they said, "Well, we don't have an office really." And, I said, "Well, you know. I'm supposed to do this thing -- I need an office." So, they got me an office that looked more like a closet. It had room for a desk and a filing cabinet and a chair but probably not for me. But, that was my office. And I remember going home to Joan Ellen and saying, "We've made the biggest mistake of my life coming back here."

STOW: Yes.

ZUCKER: It was a disaster. Well, then what happened is -- they moved me to Building 4500 [home of ORNL's administration].

STOW: Okay.

ZUCKER: And then, I had an office up there in what I call Mahogany Row. First, I had an ordinary metal desk and a little chair. Then all that furniture -- arrived -- in pieces. I'd get an upholstered chair. And, people would come in and say, "Where'd you get that chair?" I'd say, "I don't know. It just came."

STOW: (laughs)

ZUCKER: Then I got a sofa. Now, you know a sofa was unheard of, for anybody.

STOW: That's true.

ZUCKER: And, people would stop in and ask, "Where'd you get that sofa?" Well, I said, "I don't know. It arrived." It's true. It just came. And, that's really how you become an associate director. They ship furniture to your office.

STOW: So, that was the announcement, huh?

ZUCKER: That was the way it was announced, right.

STOW: Well, that was in '73, and Herman Postma had come here then as ...

ZUCKER: No, this was still before Alvin left ...

STOW: Okay, before he left.

ZUCKER: And, then Floyd Culler was acting director for about a year.

STOW: That's right.

ZUCKER: And, that's when we really started building.

STOW: So, you were named associate director in 1973, and that was your first significant change from doing basic research and [carrying out] administrative duties?

ZUCKER: Well, not quite. I was associate division director in my division for about five years.

STOW: Yes. But, you still did basic research during that period?

ZUCKER: I did research, yes. But, I also managed a group of about thirty-five people. And then, of course, in Washington I had an administrative job.

STOW: Yes. The two-year appointment at the Academy ...

ZUCKER: The two years, yes ... Well, it was not a two-year appointment. I left there after two years. I had to make the decision, whether to stay there or to come here. At Oak Ridge I became both director of what turned out to be the National Heavy Ion Accelerator and associate laboratory director for the physical sciences. After about a year, I decided I couldn't do both jobs. That's when Jim Ball became director of the accelerator project.

STOW: All right. What were some of the challenges that you faced from a professional standpoint as you transitioned from where you were into the associate laboratory director position in 1973?

ZUCKER: I'd put it the other way around. I think doing physics is a lot harder than being a manager.

STOW: Okay.

ZUCKER: if you do basic research, you're always sort of at the edge. And, you don't really know what you're going to find. Sometimes you don't know how to interpret [the data you get]. It's hard ...

STOW: Yes.

ZUCKER: And, you work hard. At Holifield, we'd run around the clock. I got so I didn't get any sleep. That was hard.

STOW: But, at least the laws of physics don't change on you.

ZUCKER: Well, but physics changes.

STOW: Sure.

ZUCKER: The laws don't change, but the frontier of your science changes obviously because you're pushing on it, too. I found that being a manager is a lot easier than being a physicist. Now, that may be counter to what some other people think, but I never found being a manager particularly difficult. It can be frustrating ...

STOW: Yes.

ZUCKER: Yes. It can be unpleasant, but it has its compensations. There were two [considerations] that made me into a manager. One is that when I was doing research, I didn't want some manager to tell me what I should do next. I wanted to be my own boss. To do that, you have to do some management. So, when I became associate director of the division, I had management responsibility over the other nuclear physicists. I could help shape the program, and that was good.

STOW: Well, that's probably true in a lot of cases.

ZUCKER: That was true in my case, and, I think, it's true in other cases. Then, when you become a manager, things change. You have to get pleasure from other people's research.

STOW: True.

ZUCKER: And, I found that to be true. I worked with an awful lot of people in areas over which I was an associate director, and they did some wonderful things. That was great fun.

STOW: My goodness, you had the Solid State Division, the Chemistry Division, Analytical Chemistry Division, Metals and Ceramics Division ...

ZUCKER: Well, at various times, I think I had everything except the life sciences, because I was associate director of physical sciences. Later I was, for a few years, associate director of engineering technology sciences. When I retired, I decided I would count how many people I knew at the Lab. I tried to count how many people I knew, whose research I knew, whom I recognized. I knew what many Lab employees were doing and more or less how well they were doing it. And, that number turned out to be 400.

STOW: Whew!

ZUCKER: You can see how much fun it would be to know 400 really bright and active people. Now, that's a rewarding job!

STOW: Oh, yes. But, the mid-1970s had to be a great challenge for you and everybody else because the Atomic Energy Commission was dissolved then and became ERDA and ultimately DOE [along with the Nuclear Regulatory Commission]. There had to be challenges in there that were frustrating, I would suspect. Is that true?

ZUCKER: There were.

STOW: Can you tell me a little bit about some of those?

ZUCKER: Well, I'll tell you one. I remember how I'll always remember it. We were, even then, strong in materials.

STOW: Yes.

ZUCKER: And, one of the big problems was liquefaction and gasification of coal.

STOW; Yes.

ZUCKER: Those were actively researched at that time. And, those processes put extreme demands on materials and make them corrode.

STOW: Yes.

ZUCKER: I mean, the processes are hot and acidic and high pressure -- it’s just a mess. So, I went to see a guy in Washington who was responsible for supporting technologies for the coal program, the fossil fuel program. And, I said to him that we would be interested in work [creating or identifying materials that can withstand harsh conditions] because we have experience and people who do that. He looked at me and said, “When we want a material for coal, for fossil fuel, for liquefaction, or gasification, we will write down the specifications of what material we want, we’ll send the specs to industry, and they’ll deliver us the material.”

STOW: (laughs)

ZUCKER: I can’t be impolite, but there are two words to express what he was talking about -- and that is “an attitude.” I know one guy from Livermore who went there and said, “I’m from Livermore. I’m a contractor, and I’d like to talk with you about this and that.” And, the ERDA guy said, “Let me see your contract.”

STOW: (laughs)

ZUCKER: So, it was a strange feeling when you went to Washington and encountered this attitude. ERDA was cobbled together from a bunch of organizations and was not the most successful thing. But, you know, in the basic sciences, the heart of the bureaucracies remains the same.
STOW: True.

ZUCKER: From AEC to ERDA to DOE, they were the same people, and I must say that the people in the basic sciences in Washington were very high-quality people, whether it be nuclear physics, high-energy physics, or basic energy sciences. They were dedicated. They were hard working and open. I've had some frustrations, but I never had any problem with the class of people that they were. I think the country was lucky to have them.

STOW: Well, we still are lucky in that sense, yes. Now, it was during your watch as associate laboratory director that the High Temperature Materials Laboratory got built. Did you have your hands in that, and to what degree were you involved?

ZUCKER: Well, yes. I'll tell you how that happened. This was right after the energy crisis came about.

STOW: Yes.

ZUCKER: And, everybody in the country was building low-temperature materials labs.

STOW: All right.

ZUCKER: There's a reason for looking at materials at low temperature because the atoms don't move around much. And, I thought since energy [from fuel is produced most efficiently] at high temperatures, what about a high-temperature materials lab?

STOW: All right.

ZUCKER: So, I met with Don Stevens, who at that time was the head of the materials program in basic energy sciences, on a park bench at the Wardman Park Hotel, where the American Physical Society had its April meeting.

STOW: Okay. What year was this Alex -- approximately anyway?

ZUCKER: Well, I don't remember -- it was probably in the 1975.

STOW: Yes.

ZUCKER: And so, we sat there and talked about high-temperature materials, and he said, "Well, why don't you write me a proposal?" And, that's how it started. And, then we thrashed around at the Lab asking ourselves what this would really be. Different people had different ideas. And then began a period of "view graph" engineering, where we would make view graphs of what this would be and sell our ideas to the Lab, to the advisory committees, and to DOE. It took us ten years from the time we conceived of the High Temperature Materials Lab until it got built.

STOW: Yes. That has to be a real feather in your cap, so to speak, and something that you got just tremendous enjoyment out of, right?

ZUCKER: Well, I liked the concept, but I didn't do all the work.

STOW: No, I realize that.

ZUCKER: There were very capable people who carried the ball and developed the program and bought the equipment and all that. What I did was make sure that the Laboratory was built -- in an architectural sense -- so that it really looked like something. And, that was a battle that I could tell you about. But, I think it's a handsome building.

STOW: Oh, yes Very much so.

ZUCKER: And, it's functional inside and I had something to do with that. But, the other curious thing is that, once we built the HTML, various parts of DOE came to us ...

STOW: Yes ...

ZUCKER: ... and said, "Would you like to do this research in the High Temperature Materials Lab?" And, I said, "You know -- if we could do it ... " I said, "Sure." Well, it turns out that the people we had in the High Temperature Materials Lab were the same people in the M&C Division ...

STOW: Sure.

ZUCKER: ... or in some cases, Solid State -- but once we put this lab together and hung out a shingle that said "High Temperature Materials Lab" ...

STOW: Yes ...

ZUCKER: Work came. People came to us. The Defense Department came to us. Somehow hanging out the shingle and having a modern building and various committees made the whole materials program at the Lab prosper.

STOW: Well, it's one of the strongest programs we have.

ZUCKER: And, I didn't realize how important this “visibility" was – but it was!

STOW: Well, that's something to keep in mind as we look toward the future. We should make sure that our strengths are visible to our sponsors out there.

ZUCKER: You've got to hang out a shingle and it has to say something like "Attorney at Law." Then people come to you.

STOW: Yes. (laughs) Let's talk a little bit about the transition years from Herman Postma to Alvin Trivelpiece. You were acting laboratory director during that period of time. What were some of the challenges that you faced, at that point in your career?

ZUCKER: I had, at that point, one challenge.

STOW: Yes ...

ZUCKER: And, that was to keep the High Flux Isotope Reactor from getting shut down because HFIR was then -- and still is now -- the permanent research reactor and [neutron production] tool for a large part of the Laboratory, and it's a key part of the materials research program. I spent a lot of my time keeping the wolves away from the door. There were many people in various parts of DOE who wanted to shut it down.

STOW: For what reason?

ZUCKER: I don't know.

STOW: Okay.

ZUCKER: I mean ...

STOW: Could there have been corrosion problems and so on?

ZUCKER: Well, it was not a technical reason ...

STOW: It wasn't?

ZUCKER: No, it was not a technical reason. I don't know what the reason was, but I know that there were problems that didn't seem real. In those days when somebody found a coffee cup on the floor of the tool building connected to the HFIR that a painter had left there, the next morning that finding was on the secretary's desk. Things like that happened.

STOW: I'm not sure we've improved over the years since then. (laughs)

ZUCKER: Well, I hope we have. And, people reported that trash leaves were found under the stairs in some building. They didn't tell us that we should clean it up as we normally would do. Instead, they sent the report up to Washington as a fault.

STOW: Yes ...

ZUCKER: And, then they had the "tiger teams" coming through here ...

STOW: Oh, yes. I remember that.

ZUCKER: I mean, everybody and his brother was looking over our shoulder, and it turns out that in retrospect, some of that was justified ...

STOW: Sure.

ZUCKER: Most of it was not. And, the HFIR was just part of that whole spirit. So, that's what I did. I met with a bunch of people at seven o'clock in the morning, which is not my favorite time. And, the other thing I thought we needed was to keep our morale up. It was a hard time for the Laboratory.

STOW: It was. Yes, I remember that.

ZUCKER: Because of all those things, the people in Washington, the admirals running around telling us we didn't know what we were doing, and the nuclear business, which had kind of bit the dust, there was a major change in the emphasis of the Laboratory. Fusion was still going strong, but being deemphasized by Washington. So, many of our technologies had really gone down -- had suffered. It was a hard job to keep the spirit of the Laboratory up and going.

STOW: Yes. I remember those years.

ZUCKER: The Biology Division was being decimated. A lot of things were in trouble.

STOW: Yes. A lot of environmental, safety, and health issues and nature awareness were big back then ...

ZUCKER: Yes, some of it was appropriate, and some of it was just harassment.

STOW: You mentioned a moment ago SNS and the proposal for SNS. I want to ask you why neutrons are important to science, because SNS is one of the biggest things going now here at the Laboratory, and you were involved with ANS, I believe. You've mentioned HFIR, which is a neutron source like SNS. Why are neutrons so important to us?

ZUCKER: Because we got 'em!

STOW: (laughs) We didn't know they existed until 1932, though.

ZUCKER: Yes, well, a lot of things we didn't know. Transistors didn't exist until the '50s. Neutrons are important in several ways. I'll talk like a nuclear physicist first. Understanding nuclear decay and determining the lifetime of a neutron are keys to understanding matter. Nuclear physicists study the neutron as a nuclear particle.

STOW: Yes.

ZUCKER: But, they're mostly used now for materials research, and there are several reasons for that. They're used in the same sense that X-rays are used for materials research.

STOW: Okay.

ZUCKER: Now, X-rays are a lot cheaper. You can get a photon much more cheaply than you can get a neutron. But, neutrons have several properties that make them outstanding. The first and simplest one is that they penetrate matter to a much greater extent than X-rays. By studying tiny chunks of material with neutrons, you can deduce the whole structure. And, that's very important for polymers and biological materials and things of that sort. The second reason is that neutrons can study light atoms and molecules that contain light elements.

STOW: Yes.

ZUCKER: Like hydrogen, carbon, and nitrogen nuclei, which X-rays can't see.

STOW: They just go right on through.

ZUCKER: Yes. They don't scatter [from light elements well as do neutrons] because X-rays are scattered by heavier elements with a higher nuclear charge [lots of protons, or positive charges, in the nucleus]. So, X-rays can see heavy elements a lot better, but neutrons can also see those light elements that make up much of living matter.

STOW: Which light elements?

ZUCKER: Atoms and molecules consisting of hydrogen, oxygen, nitrogen, and here and there, a phosphorus, sulfur, or iron atom. So, that's a second reason. And, the third reason is that neutrons are sensitive to isotopes. That is, a neutron scatters completely differently from a deuteron than from a proton [heavy hydrogen vs. ordinary hydrogen]. It scatters completely differently from an oxygen-18 nucleus than from an oxygen-16 nucleus. So, what you can do is prepare your complicated molecules in such a way that they're isotopically altered. You can put the right isotope into a particular spot and then, with neutrons, you can understand the structure much more clearly and much more readily than you can with X-rays.

STOW: Good explanation ...

ZUCKER: So, those are the three major things ...

STOW: Okay.

ZUCKER: But, there's a downside. The cost of a neutron is a lot higher than the cost of a photon.

STOW: I guess it is when you look at the cost of the SNS ...

ZUCKER: Yes, yes. Right.

STOW: I want to explore a few of your thoughts on your career. You've gotten over the years a lot of accolades and awards. In 1963 you were on the delegation to the United Nations conference on the peaceful uses of atomic energy, you were on a National Academy of Sciences delegation to China in '79, and you've been on the editorial board of *Science*. I could go on and on. As you look back at the honors and awards and other related things in your career, is there anyone who really stands out as being more precious to you than any others?

ZUCKER: Well, I must be a funny guy. Honors and awards don't do much for me. They come to you for something that you've either accomplished or for the job.

STOW: Yes.

ZUCKER: Very often, you know, because you're in a certain job, you get a certain award. What I think I enjoyed most was the Guggenheim Fellowship I got, allowing me to spend a year in England ...

STOW: ... in England, yes ...

ZUCKER: And, that's been a wonderful experience, both scientifically and personally.

STOW: All right.

ZUCKER: We still have friends in England that we see, and we go to England maybe every couple of years ...

STOW: Okay.

ZUCKER: So, that probably was the most rewarding thing. As for the other things, I think the rewards are in doing those things for which you get rewarded. I mean, that's the fun thing.

STOW: Okay. Well, let me expand on that a little bit. As you look at your scientific accomplishments and your managerial accomplishments, is there any particular thing that jumps out at you and says "Hey, I'm really proud of that?"

ZUCKER: Well, I guess I am proud of the work I did with the sixty-three-inch cyclotron, because it enabled us to open up a new field of physics. Heavy-ion nuclear physics didn't exist until we came on the scene.

STOW: Okay.

ZUCKER: And, I don't say "just me" because there were four or five of us -- a small group. But, we did that, and I enjoyed that. I liked to see the heavy-ion field "blossom," and it has. For example, Brookhaven National Lab got RHIC, the Relativistic Heavy Ion Collider.

STOW: Yes.

ZUCKER: I think that was an outgrowth of what we did. And then, there are heavy-ion induced fusion experiments that keep getting done at Sandia National Laboratories. There's a lot of heavy-ion atomic physics that's still being done --- and I did about a year's worth of that on a sixty-three-inch cyclotron. So, I have fondness and sympathy for the heavy-ion field. Nuclear physics is not the field that it was when I was in it. When I was in nuclear physics, it was the frontier of physics. Now, high-energy physics is the frontier.

STOW: All right, yes ...

ZUCKER: Along with cosmology.

STOW: Yes.

ZUCKER: So, that's how things happen.

STOW: Sure. Science evolves in that way -- absolutely.

ZUCKER: And, you get more interesting results. Maybe some of those things that are discovered in high-energy physics aren't as applicable as what you learn in nuclear physics. But I can't really say that because high-energy physics gave birth to the synchrotron radiation business, which is a very important area of applied and basic research. In the end, if you ask a scientist, "Why do science?" I think they'll hem and haw and say, "I do it because it's there," or "I do it because it's beautiful," or whatever.

STOW: Yes.

ZUCKER: But, in the end, I think we all do science because we think it's going to benefit mankind. I think that's the fundamental drive. And, it can benefit mankind just by enhancing our knowledge of the world around us. Enlarging the sphere of human understanding is a benefit to humanity.

STOW: I couldn't agree more.

ZUCKER: And then, there are other benefits that have to do with economic benefits, health benefits, and so on ...

STOW: Yes ...

ZUCKER: And, I think in the end, that's the reason people do science. Science is hard to do. (laughs)

STOW: That it is. (laughs) Even a poor geologist can acknowledge that.

ZUCKER: Well, anybody that does science conscientiously knows it's hard to do.

STOW: You've rubbed elbows with a large number of famous scientists and engineers, a few of whose names you've mentioned today. Over the years of your career -- starting in 1950 or so -- is there any particular individual who stands out in your memory as someone whom you greatly admire and somebody who has influenced your career?

ZUCKER: Well, first, I think all of us who came here in the '50s were influenced by Alvin Weinberg. I think Alvin had this talent to inspire you and to make you think that what you were doing was really important. And, although I think he was largely concerned with the reactors and just sort of suffered the basic research to go along with it, nevertheless, if you were working at the Laboratory you had a feeling that you were working for Alvin and that Alvin understood and appreciated what you were doing.

STOW: Especially, if he's sitting on the front row at a division's annual information meeting asking you questions.

ZUCKER: Well, yes. That was kind of fun. But, he really cared and I remember when I was doing a spin-flip experiment on the Holifield -- he didn't know what that was all about so -- and didn't understand it, so he came over to me and said, "Explain it to me." And, I went to the blackboard and wrote stuff down and he understood -- he really understood what I was doing. And, I had a feeling that I were talking to somebody who understood me.

STOW: Well, he was a brilliant man. My goodness ...

ZUCKER: And, he worked very hard at it. So, I think that was probably a big influence on me. Another influence on me was Lars Onsager. I had three thesis advisors at Yale, and he was one of them. But, he was by all odds, the most interesting person there, and he taught me a lot about statistical mechanics. And he taught me a lot about what it takes to be a scientist. Another influential person was the late Roger

Revelle, with whom I had a lot to do when I was in Washington. He was on an Environmental Sciences Studies Board, like I was, and he probably was the smartest guy I knew.

STOW: A very influential man . . .

ZUCKER: Well, he was not just an influential man. He was a wonderful person.

STOW: Was he?

ZUCKER: I know. We had him at our house a number of times, and he would go into the kitchen when the children were eating, and he would go and take food off their plate and taste it. He was just a very ...

STOW: ... playful man?

ZUCKER: Well, he was interested in people and he really cared. I have a feeling that he was somehow connected, not just to me, but to my wife and to my children. He was probably a really outstanding scientist.

STOW: That he was. I've read his book. I think we're about out of time, Alex.

ZUCKER: All right.

STOW:. Thank you very much.

ZUCKER: Yes, you're very welcome. It's been a pleasure.

STOW: It's been a good chat.

--END OF INTERVIEW --