Harry Dreicer in conversation with Sig Hecker
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HD: Harry Dreicer; SH: Sig Hecker

SH: **Harry, we are doing a series of interviews that capture experience of the Los Alamos people with the Russians over the years. Your contacts with the Russians date back into the 70s and 80s. But before we talk about that, I thought it would be good to talk about your personal path. Why don't you tell us again when you were born, and then, particularly, when you came to the laboratory.**

HD: Ok. Well, I was born in 1927, October 6th to be exact, in a little resort town in Germany. The reason I mention that is because we're talking about our collaboration with the Soviets, and it's important in understanding my views that people who listen to this know that my parents were Russian. In the years 1917 to 1920, the family managed to get away from the Soviet Bolshevik Revolution, and it brought them to this little resort town near Leipzig, Germany, that was the only place they could manage to find a residence. They were supposed to leave Germany and go to South America, but they had no intention of doing that for various reasons - I won't go into more details. But because they left Russia at a time of revolution and civil war that continued into what became the Soviet Union, they also left there a full family on my father's side—three brothers, two sisters, a mother and father. And for many years in the '20s and early '30s, they supported them with medicines and clothing, and there was still contact. What remains of that is a lot of anti-Communist ideology of my parents. In fact, if I don't forget, I will mention later when I took my first trip to the Soviet Union, which occurred in 1971, my father had passed away, but my mother was dreadfully afraid for me going to the Soviet Union. She was worried I would not return and would end up in a gulag.

So that was my beginning. After about twenty years in Germany, we had to run away, and we went to Italy for reasons we don't have time to describe here, which also turned out to be a difficult spot for us. We were Jews, we were stateless, none of us had passports or citizenship. It is through the help of my grandfather in Moscow, whose brother had emigrated to the US many years ago, that we managed to escape Europe in 1939 and then end up in the United States. In the United States, life was wonderful, except it was the end of the Depression still. And it wasn't easy; all of us went to work, including me at almost eleven years old. But it was a wonderful experience, and I immediately started in the New York public schools, where I learned English and began to prosper. I had less trouble with dividing in the United States than I did in Europe, so I managed to become better at mathematics and such things. I ended up in a wonderful high school, which was the Brooklyn Technical High School, and I credit it for most of what happened to me afterwards because it was so good and gave me a chance to be accepted at MIT in 1946.

At MIT, I went through the usual courses that you take to become a physicist. I was also interested in math, so I was heading for a double credit there, but it took five years. And in 1951, I finished the bachelor's degree in physics and had a lot of credits in math. What shall I say? I have been very lucky. In my last year as an undergraduate, Jerrold Zacharias, a professor in the physics department, had pity on me. I got married and needed money, obviously, and he hired me for a summer classified project that occurred at Lexington, Massachusetts, close to MIT. Its purpose was to improve anti-submarine warfare and the protection of our submarines. The worry at the time was that there would be war in Europe, that the Soviets would be attacking the Western Europeans, and that we would have again to ship all these convoys overseas, which had suffered terribly in World War II because of German submarines. So that summer I spent with a bunch of professors, people who had worked during World War II at the MIT Radiation Lab which saved us with the radar.

SH: **Yes, they did the radar.**

HD: Yes, they all did radar work there. In my senior year, I had to do a thesis. MIT Physics undergraduates had to do a thesis. And I did it with Sanborn Brown, a professor in the physics department, who had worked in the MIT Rad lab. His job had been to develop methods of protecting returning radar signals from the outgoing one. The powers were very, very different. And you had to protect the equipment, and that was done with gas discharges which protected the incoming signal from the outgoing one. So I did a bachelor's thesis in a laboratory that was still doing microwave work with gas discharges. I learned a lot, and I did a thesis. And then I went into the graduate school, and what you have to worry about with getting a PhD at MIT is getting somebody that will let you do a thesis with them. The laboratory of Sanborn Brown had a theorist professor. His name was Will Allis. He provided the theory for all the work that was done there. There were weekly seminars, and we got to know each other and all that. So I went to him and asked him for a special problem, and he gave me one. And I also went to another professor to get a special problem—an experimental special problem, because Will Allis's was theory. And that was Woody Strandberg. Woody Strandberg was a wonderful experimentalist who also was a good theorist. And he said, "Why don't you help me put together an experiment that will reduce the Doppler line width of the famous ammonia line which occurs at microwave frequencies?" He wanted me to put together a cross-field microwave absorption experiment in which, in one direction, a microwave beam, which could be tuned, was propagated. And at right angles to it, a beam of ammonia molecules was injected. Well, just by luck, I had worked as an undergraduate also in Zacharias's laboratory, and I knew that they had developed a device capable of producing intense beams. When we get through, I'll show you one of those gadgets. I kept it.

SH: Oh, you've got it? Great.

HD: So I used that approach, and that gave enough sensitivity to do the measurement. I was very lucky to have a publication in the Physical Review Letters with Woody Strandberg. And the same thing was used by Charlie Townes. It was a great disappointment to Strandberg, who actually was on the same path that he got beat. Anyways, Townes used this same approach, and the line width due to Doppler broadening was essentially eliminated. At the same time, I was working with Will Allis. And he gave me a special problem, which was to compute the electrical micro field on a test particle in a plasma. At the same time, he asked me, "Do you want to spend the summer in Los Alamos? I'm a consultant there." This was 1952. And I said no. Because I wanted to do this special problem, and I knew if I started to travel around, I would never get it done. And I was lucky—I got it done. But the next year, he asked me again. That was '53. And I said OK. And I came out to Los Alamos, my wife and I. We spent the summer there with P Division, the physics division. They were doing magnetic fusion work, and I was attached to the group which was studying the toroidal pinch in the Perhapsatron, as it was called. That was the name of the machine. And I was very lucky again because they didn't know how to determine the electron density in the pinch, and I was able to show them that, by using the microfield that I had computed the year earlier, one could essentially determine the Stark broadening of spectroscopic lines in the plasma. The spectroscopic lines were due to hydrogen or deuterium, which were not yet ionized. And so most of it was completely ionized, but there were impurities left, a small percentage. And by looking at their Stark broadening due to the microfield, they could tell what the density was.

SH: **So you're at Los Alamos for the summer, right?**

HD: Yes, and I've managed to help them measure the density of the toroidal pinch in the Perhapsatron. The division leader of P Division at that time was Jerry Kellogg, he had been a graduate student of Rabi's. Zacharias had been a graduate student of Rabi's. So anyway, they were impressed with me. And I was happy. I went back to Cambridge with Will Allis, and now we're on the thesis. Now I'm okayed to do the thesis.

SH: **When did you finish that?**

HD: The thesis was finished in '54. But actually, I didn't get a PhD until '55 because I moved my family out to Los Alamos, where I was hired.

SH: **So you got hired at Los Alamos, and you were out there in '55.**

HD: I got hired at Los Alamos in '54. And the thesis was a struggle because Will Allis wanted me to do one thing that he'd been doing using this microfield as part of it. And I determined that it couldn't be done. So I was in deep trouble until, one time, I was lucky to get the flu and to be down in bed for a week when I had a chance to read a Review of ~~a~~ Modern Physics article by Chandrasekhar, who was trying to do a kinetic theory of stars with John von Neumann, and stuff like that. And he was using a method based upon what was called the Fokker-Planck equation. So to make a long story short, when I presented the possibility of using that equation to Will Allis, he jumped on it. So I was happy, and I was on the way. And I got scooped on a portion of what we wanted to do, which was to compute the electrical conductivity. Somebody beat us to it – Lyman Spitzer and some student of his at Princeton.

The thesis' purpose was to take the usual kinetic theory of gas discharges, which is mostly non-ionized gas, and advance it to include coulomb interactions at increased ionizations. And so there are two parts to that, one is to do the conductivity of such better ionized gas, the other part is to determine the effect on ionization rates, excitation rates, recombination, you name it. All the things these people were interested in, in gas discharges. Spitzer scooped me on the electrical conductivity. So I had to switch over to doing the other part, and all we had were Fridens and Marchants to do the computations. But I did it, and I got a thesis out of it. But the Fokker-Planck approach taught me something else also, which became very useful when I got to Los Alamos, where I put everything on a computer and did it more accurately and published it. I also learned that this Fokker-Planck approach had only been used by Spitzer and his students in a perturbation sense, in which things were done by small changes. That somehow didn't help me or didn't jive with me all the way. And one day, on a nice weekend when I should have been taking my kids on a walk or something, I had an idea to do away with the small perturbation theory. And it showed me that the electrical conductivity is not a time-independent thing. That it doesn't have a steady state, and that it has a critical field above which everything, so-called runaway electrons run away to much higher energies—they just keep going, keep being accelerated. And that immediately explained to me why the Stellarator fusion experiment at Princeton was seeing x-rays due to megavolt particles hitting their chambers.

SH: It was that runaway.

HD: Yes, this was completely new and different. And I was exceedingly happy and lucky. So anyway, that's what happened. Give me another question.

SH: **First of all, that's a fantastic history. Let me fast forward to something you told me once. So now you're in Los Alamos in '55 - so that was the year of the first international conference on peaceful uses of atomic energy in Geneva.**

HD: So the second one, including fusion, that happened in '58.

SH: **The first one was in '55. But you were really not involved in that. But by the time the '58 conference came about, then Kurchatov had been at Harwell and talked about the Soviet fusion experiments. And you told me, then, you were part of the Los Alamos contingent to go to Geneva. So tell me that story because I thought that was fascinating.**

HD: At that time, I believe Admiral Strauss (Lewis L. Strauss) was still the Atomic Energy Commission chief. And he was very intent on making a huge impact there.

He's the same fellow who was involved in the Teller-Oppenheimer thing. Anyway, he wanted to make a big splash. And he even offered a prize—I think it was like a million dollars or something—to the group that could make fusion by, I think, the autumn of '58. Anyway, he caused us to have to take equipment with us. There were three pieces of equipment that went to Geneva. One was the Perhapsatron, which I had been associated with. My job, at the time, was to measure the emission of the microwaves from the pinch plasma, which I had measured. Very strange what was going on. But anyway, the other experiment was the theta pinch, with which Los Alamos had shown thermonuclear neutrons even though it was just for microseconds. The third one was John Marshall's plasma gun. He had a device for propelling highly ionized gas. And this was a very successful show because it involved the technology for making very high currents—people were not used to making a hundred kilo amps or mega amps, and the switching required for that with capacitors and spark gaps and so on. And then they saw that, they phoned home to other laboratories, and they asked other people to quickly come and take a look at this.

SH: **You mean the Soviets?**

HD: No, there were participants from all over. The Germans, the Japanese, the Dutch.

SH: **So they came from many countries?**

HD: They came from many countries, and this was a very successful exhibit. My luck was that the conference helped me to get a name because Princeton, feeling that I had done something in explaining where the x-rays in the megavolt region came from, asked me to prepare the runaway paper, and it was to be an invited paper. And it came right after the first few – Artsimovich (Academician Lev Artsimovich), the main speakers. So that was very good for me. You're asking me to tell you about my life, so here's a chance to blow my horn. I had another success which gave me some help, which was that we used to go every three months to classified meetings at the major laboratories in the United States where magnetic fusion was being worked on. That was Livermore, Los Alamos, Oak Ridge, General Atomics, that sort of places. And in

1955, my first trip to Princeton, I gave a paper which had to do with the problem at that time of trying to do measurements on very short-lived fully ionized gases that were meaningful. I had looked at the problem of ionizing various elements inside of a black body in equilibrium and found that for example Cesium could be ionized almost completely at 2,000 - 3,000 degrees. I gave that at the Princeton classified meeting, and because it got a TID classification number it was never published. And by three years later, people had figured out how to deal with a black body. They just made two hot electrodes and put a magnetic field on, and they illuminated them with these alkali atoms, and they had a steady state fully ionized gas. And we ended up doing that at Los Alamos three years or so later, and I did lots of experiments on that using microwaves, which was my heritage from MIT. And also using radioactive atoms, which was now my heritage from Los Alamos. Potassium-42 as something you could inject and watch and follow. So we did a lot of experiments. My life was always mixed between theory and experiments. I had been hired by Los Alamos in the first place with a right to do basic plasma physics, so they allowed this as long as I brought money in from the AEC to pay for it. I had myself and a technician, and over some years we added a few staff members. And we did a lot of interesting experiments.

SH: **So coming back to Geneva, did you meet the Russians in '58? Did you actually talk to them?**

HD: Yes, let's talk about that meeting a little bit. We came there, and we were a little early because we had to set up the exhibits. But the Russians came finally. And it was a very interesting interaction. For me, I was very interested in the Russians, and everybody else also. But they were scared of us. The people who were there were very obviously scared of us and having much to do with us. When we invited them out to dinner, it caused a big problem for them, they had to go back to their hotels and hunker down with the people who had come along to watch them. And they wanted to know if we were going to a nightclub. It gave us an understanding of how difficult life was for them, coming to this meeting. None of them had been out before, except maybe Artsimovich or somebody like that. I think it was Tamm (Nobel Laureate Igor Tamm) there, and I met Artsimovich and Leontovich and also a number of other people who were among the scared group, like Kadomtsev (A.A.), Shafranov (V.D), and Sagdeev (R.Z.).

SH: **So Roald Sagdeev was among the scared group?**

HD: Yes.

SH: **Because he was quite young at the time.**

HD: He was young, and I think he was in charge of some plasma work for the space program. But it's interesting. They didn't come with machines like we did, but they did bring that little satellite they had put up - Sputnik. The model of Sputnik with its antennas. And we looked at that. So while they were mostly doing theory, very complicated theory - people like Akhiezer (A.I.), Kadomtsev, Shafranov - they were all doing theory, and very advanced, I would think, although it was difficult to understand at the moment all the things they did. But the impression I got was they were not very advanced on the experimental front. I think they had done at that time experiments on pinches and things like that, but in linear tubes. I'm not sure when they went to toroidal. But it didn't take long once they were at Geneva.

SH: **How much did you know about what the Soviets had done before you went there? How much did you know about their fusion program?**

HD: I didn't know anything, as far as I can remember.

SH: **So those names, Artsimovich, Leontovich…**

HD: They were new to me.

SH: **They were new, OK. And Andrei Sakharov, or Igor Tamm, or those guys…**

HD: You know, I might have run across Pyotr Kapitsa's name because it was known that he had worked in Britain and that he couldn't get out of the Soviet Union when he went back and stuff like that. Did I know about Tamm? Maybe, he was famous. I was trying to remember another famous name since you asked me for names, and I haven't been able to.

SH: **Evgeny Velikhov …**

HD: I didn't meet Velikhov then.

SH: **You dealt with him later.**

HD: Yes. So we made initial contact, I think it was a positive contact, because we were showing hospitality to them. We would invite them to come down to where there was a little place to buy a drink, to get a soda, to get a hot dog, and so forth. And they were always worried about going down. But that all changed with time.

SH: So that interaction was positive, and it gave you a real feel for what these people had to put up with. As you say, they were scared, they had their minders with them.

HD: Yes. And it was not altogether news to me, except I only had this feeling from my parents a long time ago, and here it was.

SH: **I've also read that the first Soviets came to Los Alamos for a visit in 1958 as well.**

HD: That sounds early to me. I don't remember that.

SH: **You don't remember any interaction with the Soviets in Los Alamos?**

HD: Oh, I do.

SH: **During those early years.**

HD: Not that early. But at some point, and I can't remember the year, they did visit. And we did things like take them up to Pajarito Mountain to show them the ski area, and they would come down and stop and take pictures that they could see of S site at the lab and things like that.

SH: **What was your recollection, how did the Russian interactions go after that? Did anyone encourage you within the lab, the DOE, the Atomic Energy Commission?**

HD: Well, in ’57, a year before we went, things got declassified officially. Here and in Great Britain. And the British came to us and visited. This may not be relevant too much. But this was the first sort-of combine, which many, many years later, has ended up in the ITER (International Tokomak Experimental Reactor) with twenty or more countries collaborating. In the ‘60s or ‘70s, there had been talk about joint programs. And when I became division leader in the later ‘70s, I also became a member of the Fusion Power Coordinating Committee in the United States which met about the U.S. program. But then there was also one established for U.S. - USSR, and it was called the Joint Fusion Power Coordinating, and it was to arrange exchanges and cooperation. And that’s when I went to Moscow and to the Soviet Union the second time. The first time was in ’71 to a conference in Kiev. We haven’t talked about that.

SH: **No, that’s what I was going to get to next.**

HD: So that’s the next thing that happened, but then, some ten years later I would say, I became involved in these joint exchange deliberations. So let’s stop in ’71. In ’71, and I put together the only thing that remains for me. (*Leaves through the conference program*)

SH: Yeah, I just took a quick look and saw that you were there. That’s the one in Kiev in October ’71. It shows that you were there.

HD: It announced, it shows that they had not had such a conference before. This is the first one in which they had invited so many people from the West and from the East and from god knows where. And it was a theory conference. And I only have this left, and it lists all the participants that they mention.

SH (*looking at the list of participants*): **By this time, Sagdeev was there. And the other names that you mention. From the U.S. side, the one that I recognize is Marshall Rosenbluth.**

HD: Yes, he was a Los Alamos guy.

SH: **In the early ‘50s, right?**

HD: Yes. He was there when I came. And he had done the M theory for the pinch. It’s called the M theory. He was married to his first wife, and she had done the computations. And there were other people - Shafranov in the Soviet Union had done a pinch theory. And my thesis advisor Will Allis was a consultant because he was supposed to explain the pinch. But his problem was that he was used to starting from low ionization, so he never got to do it. And Marshall Rosenbluth started from saying, “The whole thing’s ionized. Now what happens?”

SH: **So now you go to Kiev, and it was a plasma theory conference, the first international conference for the Soviets in plasma theory.**

HD: That I knew about.

SH: **What was that like?**

HD: Well, I don’t remember the details of the conference too much.

SH: **No, of you going. How about your feeling of going when your mother didn't want…**

HD: Oh, my mother was really scared of me going. She knew all about taking warm clothing and, my god, Kiev in November was very cold. And I arrived there from Budapest. And the Budapest airport was still under Soviet times. When I came in, it was a dreadful airport. It had one little bulb hanging from a loose wire. They took me by taxi to a hotel, and I had a good time there listening to violins while I ate my dinner. The next morning, I gathered my stuff, went back to the airport, and I couldn't get in. It was so crowded, full of people. I said, "Well, I'll never make my flight, I couldn't get in." But after a while, the pilot of my Aeroflot was coming through the crowd with a sign that said Dreicer on it. I said, "Gee, this is pretty impressive." So I ended up in Kiev in a hotel where the room was dreadfully cold. I went out to the floor mother, she spoke German from the days of World War II. And I told her my room was very cold, the window won't close. She said, "Just a minute." She came with a hammer and a nail, and she nailed that window closed. And then I found out that the towel racks were heated. They had hot water running through it.

SH: That's very common. In the Soviet Union, they ran the central heating through the towel racks.

HD: Something that was unusual in the West. So she also came back with a hot tea for me, it was all very pleasant. I did notice in my time there that there was not altogether good friendship between Kiev physicists and Soviet physicists. I'm not sure it was the physicists so much as the arrangements. It turned out the Kiev were transported in different buses than the Soviets, I mean the Moscowers.

SH: It showed.

HD: Of course, during World War II, there were big problems. The Ukrainians, some of them, were happy to have the Germans come, and Stalin never forgot that. So anyways, there was that. I spent a good deal of time then in the Soviet Union, I don't remember exactly how much. But I got to know not only how Gurevich (Academician A.V.), who had been following my paper on runaways, had tried to make it even more correct. And that was a relationship which remained to this day—if I send him an email, he will answer me.

SH: **So in that '71 meeting then, what was your sense to how advanced plasma physics was in the Soviet Union versus what we had here in the Union States?**

HD: I wish my memory were better, Sig. I can't remember, but they must have had toroidal pinches. I think, in fact, they did. And I can tell you why - because the Perhapsatron was dropped at Los Alamos when it should have been explored further. We felt that way, the people working on it. It was Jim Tuck who was in charge.

SH: **I was going to ask you, you must have worked with Jim Tuck.**

HD: Jim Tuck was intent on trying as many possible approaches as he could think of, and that, of course, with limited resources, meant that something like the Perhapsatron had to be dropped. And that was before one could fully achieve things like the Tokomak, which basically started out like that also. The pinch was unstable, and so they introduced longitudinal fields. The pinch itself is compressed by itself by magnetic fields, with a current running around. There's a good deal of stabilization by putting a longitudinal magnetic field on, and the Russians made that pretty strong. And that took some basic research that we never did and dropped. After that, we got into the other one.

SH: **The theta pinch?**

HD: The theta pinch. We got into the theta pinch business. And it ended up in a big way, which finally caused the demise almost of the division because of the oil embargo in the '70s, which caused people to sell to the Congress that we could achieve success by the 80s and 90s.

SH: In fusion.

HD: So politics has to enter any big enterprise, and, right away, I need to say that it's an enormous achievement, I think in the West and in particular in our Congress, that we have, since the '50s, made funds available for magnetic fusion. I mean this is a continuing commitment that is hard to…

SH: **Hard to justify on the basis of just that by itself. I mean, that's 1950, now we're in 2017. So, 65, 67 years.**

HD: Yes, and of course this has to do with the way our system works, getting budgets for different things. And the Atomic Energy Commission people who work on this are aware of all that, so they will hand out money in a way that will increase it. For instance, they made Princeton the biggest laboratory that they dealt with. Then they found out afterwards that they also tend to be controlled by the biggest laboratory. This is the way our…

SH: That's the way it works.

HD: In 1977 when I became division leader (Controlled Thermonuclear Research – CTR Division), I accepted the job because I wanted to see if I could actually work with people. I had not done that. I had been doing basic work and working myself with very few people. And it was, well you can ask, "Was it a mistake or not?" But when you come to a fork in the road, you've got to take one or the other, and that's it. So I learned a lot from doing that, from dealing with people, from dealing with Washington. I was amazed that Domenici wanted to see me, a senator. But he did, not just because I was a voter - it was partly because I was a voter. But it was because he was working on behalf of our laboratory and labs in general. So I had to deal with the House, with the Senate, with DOE itself, with the other laboratories. You name it. I learned a lot. But I had to let go of my basic work, and, as it turned out, the DOE made sure that by the time Kintner was in charge... I don't know if you ever knew him?

SH: Ed?

HD: Ed Kintner. He made sure that I would do my work by having them kill off my group, which, you know... We had a nice group, a nice experiment, we were doing fine work, and bringing in enough money to pay for it. And when we didn't have enough money and it came to the end of the year and the division leader was Fred Riebe at the time, asked people to spend money, I went out and got special insulators that I could return to the stock room in the next fiscal year and get full credit for. So you do in life what you have to without killing other people.

SH: **As you say, that's a decision you have to make somewhere along the way. I had to go through similar decisions. You do get to a point in your career where you do think that the next step is to go help other people out. That's basically what you do. So you became division leader late '70s?**

HD: Yes, something like that.

SH: **So when did you go back to Russia? Did you go more often then?**

HD: Then as a member of this Fusion Power Coordinating Committee, I went back in '81, '82, and '84. I think in '82 it was not as a member of that committee. It was acting on a personal invitation from Velikhov, who wanted me to see a little more of touristic Russia. And so I went with my wife, and it was at the invitation of the Academy of Sciences. And I stayed at their hotel at Moscow, and I saw Velikhov. And I think that's the time I gave the message from Agnew (Los Alamos Director Harold Agnew) about…

SH: **Tell me about that message. So Harold knows you're going to go, he looks you up, and he says…**

HD: "I want you to take this message for him." And I did, and that was that. And Velikhov took it as if nothing happened.

SH: **So the message, from what I remember you told me, that Harold said**…

HD: "We know all about it." And they [the Russians] had tried to hide it, I guess. But even I wasn't aware of it.

SH: **So what he was talking about was the 1957 Kyshtym explosion of nuclear waste, where they spread radioactivity all around. Was he referring to that, or was he also - I was thinking about this - there was a 1979 outbreak of anthrax in Sverdlovsk. Was it anthrax?**

HD: No.

SH: **It was nuclear. So they kept the '57 secret … I didn't hear about it from a Russian until Sergey Kapitza, Pyotr Kapitza's son, came to Los Alamos in like '89. He gave a talk about Russia, and he talked about the Kyshtym disaster. That's the first time I heard about it officially.**
Interesting that Harold knew – likely from intelligence sources, and he wanted to make sure the Russians knew that we knew.

HD: I had not heard about it at all.

SH: **But you told Velikhov, and Velikhov just let it roll off his back?**

HD: It's not as if he didn't hear what I said, but he didn't make any other comments about it. Velikhov was an interesting character. First of all, he was like one of these Russian bears you talk about. He was big, and when he put his arm around me, I felt like the Russian bear had me. And he would say things like "You know, if we let people out of the Soviet Union, they would all disappear. You know, we can't do that." He was very frank to me.

So, back at the lab, Harold offered me the division leadership of CTR division to get me to stay at the lab when the division's funding became endangered because of changed priorities in Washington. So I had to make this decision: do I stay doing what I'm doing, or do I try to work with people? And that's when it happened.

SH: **So at that time, you were CTR Division leader?**

HD: Yes. Fred Riebe had been Division leader for a good number of years. And I think what Agnew wanted me to do was to turn the whole thing (CTR division) off in a civilized fashion, placing good people around the lab. And we didn't end up doing that. We ended up asking for different approaches to be funded, the major one of which was the reverse field pinch. And there were others like the compact toroids, etc. And it was at that time that we tried to do imploding liner. We wanted to use the remaining theta pinch condenser bank, which Agnew, with all its wires, used to call the spaghetti factory. He didn't believe in it. That didn't help. We wanted to use that to implode onto a magnetic field and a plasma and get to ignition that way. We proposed it, and Agnew was a proponent to do this with the Soviets, namely Velikhov. When the DOE got into the nuclear part, they killed it.

SH: **Did you meet up more with Velikhov in the future?**

HD: Velikhov came to Los Alamos, and I met him each time we went over there. These trips to arrange joint programs, they happened once every two years, I remember, alternating between the U.S. and the Soviet Union. And when they occurred, we would make huge trips around the USSR to different labs. And here and there, there were our people who went there to spend time, they didn't' have an easy time, you didn't have the proper clothes in Leningrad or wherever they were, who got frozen onto seats, and stuff like that. It wasn't easy.

SH: How many people from Los Alamos, for example, were involved in these Joint Coordinating Committee trips? Were there two? Five?

HD: There was one person, one representative, that was me. But then there might be trips to meetings and other things that other people went to. My international travel really went down because I wanted other people to go. What was I going to say? When the tokamak had its success, it was not believed in the West.

SH**: So when was that?**

HD: That was in the '70s, and what they were quoting were high electron temperatures. And the West didn't believe it, so they invited people from the West to come there and do the measurements. And I remember people from the UK maybe did it - and they confirmed it. And this is partly why everybody was switching—the Princeton people switched from Stellarators to tokamaks. We couldn't do it because we had left the toroidal pinch; we were doing other things. We were doing the theta pinch.

SH: **Harry, where was that experiment conducted?**

HD: At the Kurchatov Institute.

SH: **Oh, it was at the Kurchatov.**

HD: Yes, we used to visit the Kurchatov.

SH: **Was that under Velikhov's direction?**

HD: At that time, I think it was. I think Artsimovich had passed away. He was in charge in the beginning of this program, and Velikhov took over… The Geneva conference which was a personal disappointment almost overwhelmed me. Even though I had a good time with runaways, I had been working on the effect of synchrotron radiation on fusion reactors, and I got beat to it by a Soviet guy named Trubnikov.

SH: Oh, is that right?

HD: Yes. He knew more about how to do computations with photon transport than I did, and it turned out it was one of the subjects we couldn't get to in one of my courses. I never learned anything, so I couldn't do it very well. I knew it was a worry, but I couldn't put everything in. So anyway, I met Trubnikov, among other things. And Leontovich, who gave a talk—by the way, he spoke German, so he and I could speak quite a lot. And there were several, there was another guy, and I can't remember his name, who spoke German, a well-known physicist. When it comes down to speaking to people, there was a guy and his son, both of whom were called Akhiezer. They were Ukrainian, and I think they were working in Kharkov. I still remember one time when I raised the name—what's his name? The guy who did the Soviet H-bomb?

SH: **Sakharov**.

HD: Sakharov. I asked him about Sakharov, and he said, "Don't mention the name of the devil." And he turned around, looking to see if anybody had heard this. So, you know, there were still scary times there over certain things.

SH: **This was in '58?**

HD: No.

SH: This was later.

HD: This was later, after Sakharov became unpopular.

SH: **1980s. So, Harry, let me first ask: when you said you went to these Joint Coordinating Committee meetings, did we actually do joint experiments with the Russians or Soviets?**

HD: My impression is that the answer is no. We couldn't get…

SH: **It was just too difficult.**

HD: But it was still good to get together and to discuss what's happening. And every time you got together, a major meeting occurred between these coordinating committees on both sides. People were brought in to give papers and so forth. There was a lot of information exchanged.

SH: **In terms of the question that I asked, how was the American program affected by Soviet? Well a lot, right? When they had the tokamak…**

HD: It was a lot. It was the tokamak, yes. And the switch was painful because more money came into the program, and Kintner didn't quite know what to do with it. And so he parked it in the mirror program at Livermore.

SH: Oh, really?

HD: Yes. And he supported that long after there was any hope for mirrors. And I say this not because I was after their money or anything, but people knew this and thought of it that way.

SH: **But the money came because of the Soviet advances with the tokamak, right?**

HD: The money came because there had been an oil embargo…

SH: Ah, yes.

HD: And the Congress reacted to it. And Representative McCormack I think that was his name, was he from Oregon or Washington, I think he was. He wanted a piece of the action, and he wanted to make machines in his State that would make 14 MEV neutrons for study of materials.

SH: Yes, I think that was up. That was called the FFTF up in Hanford.

HD: Yes.

SH: **I've gone through basically all the questions. Harry thanks for a wonderful interview. What a life and what a career of accomplishments. Thanks for sharing with us.**