THE DEPARTMENT OF ENERGY ORAL HISTORY PRESENTATION PROGRAM

OAK RIDGE, TENNESSEE

AN INTERVIEW WITH DR. ELLISON TAYLOR

AND DR. ROBERT HOLMBERG

FOR THE OAK RIDGE NATIONAL LABORATORY

ORAL HISTORY PROJECT

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STOW: Today, we will be talking with Ellison Taylor, a long-time director of the Chemistry Division at Oak Ridge National Laboratory, and Robert Holmberg, a chemist in the Chemistry Division, who did a lot of different things.

Bob, let's start with you. What is your background and what brought you to Oak Ridge?

HOLMBERG: Well, I always like to say I've never had a honest job. I've worked for the Manhattan District and its successors all my life.

STOW: (laughs) Careful. We won't edit that out. (laughter)

HOLMBERG: All right. When I was an undergraduate at Iowa State College in those days getting my degree in chemistry, about ten or twelve of us were interviewed by what we called a "big time operator" -- all together from a place called the Metallurgical Laboratories of the University of Chicago. He couldn't tell us what they were doing. It was secret. But it had to do with energy and things like that. It wasn't hard to put this together. I had read at least two articles in magazines like *Collier's* or *Saturday Evening Post* on fission. And I said, "That's what they're doing – nuclear energy." So I said, "That's a good thing to apply to for a job." And, of course, we could use our professors for references. And, they said, "Bob, we're doing the same thing here. Why don't you come work for us?" And, of course, I chose that rather than going into the wilds of Chicago. And, I've been on the project ever since. I got drafted shortly after I started work at Ames (Iowa) – I had a vigorous four days of basic training, was sent back to Chicago, where I wired my mother to send me civilian clothes, and came back to Ames in civilian clothes, but as a buck private in the Army. And, eventually, the project slowed down there and they sent me to Oak Ridge. In Oak Ridge, I was assigned to work at the Castle on the Hill for Doc Jerry Cole. This was the Manhattan District Research Division at that time. It was a fairly small division and, I worked for him. He decided to leave shortly afterwards, and as he was leaving I said, "Offer me a job, Jerry, so I can get out of the Army." And, he did that and I've been at ORNL for forty years.

STOW: (laughs) What were your first impressions of Oak Ridge as a city and a place to live?

HOLMBERG: Well, I came here as a GI -- we had a barracks area at the time, so it was still a little muddy and it was still primitive by today's standards. We lived in small rooms in dormitories that you shared with other people. It was a young man's town and a young woman's town. So, it was a great place to live for young singles.

STOW: And, were you a single man at the time?

HOLMBERG: I was a single man.

STOW: Ellison, were you a single man?

TAYLOR: No, I was married and had one child.

STOW: What did your wife think of Oak Ridge as a place to live?

TAYLOR: Not very much. (laughter). The chief thing I remember in detail about it was that she went out one night finally by herself, thinking she could drive around and manage without me along, and she got lost coming back because all the houses looked alike. She couldn't tell when she was home.

STOW: Was it a cemesto house?

TAYLOR: Yeah, we got a cemesto house. We were lucky. We really didn't have any hardship.

STOW: Was it a -- an A, B, C or do you recall?

TAYLOR: It was a B.

STOW: A B house.

TAYLOR: We were entitled to B, because we had one child.

STOW: All right. What was your first job when you got here, Ellison? You were in the Chemistry Division, but what sort of technical work did you do first?

TAYLOR: Well, the Laboratory was working on what later became the Homogeneous Reactor, which had to be decontaminated as a result of its radiation. This was a reactor that had a water solution of some sort of uranium compound.

STOW: Was that a breeder reactor?

TAYLOR: No, it was much more elementary than that. The goal was to make a reactor with the uranium in solution or in a slurry. Irradiation of water decomposes it, producing hydrogen and oxygen. That poses a problem for a reactor, because if you have hydrogen and oxygen all in one place, you could have an explosive mixture. And that's not a good thing in any sort of operation, particularly if you have a reactor that you're running with all sorts of things going on around it. So, they wanted to develop a catalyst that would recombine the oxygen and hydrogen into water, more or less, as fast as it was formed, so that they only had negligible pressure of this explosive gas above the solution reactor. And, since I had had some experience developing catalysts, that was what I was supposed to do. So, my job was to set up some equipment to make some catalysts and try them out on a small scale to see if and how they might work for this. One man was hired before I came to ORNL to work on it with me. He had been working over at the S-50 plant, which was the Liquid Thermal Diffusion Plant for separating the uranium isotopes. The people at the Lab didn't know what sort of program they ought to have in catalysis, so they were waiting for me to get here and so was he. And he was getting kind of nervous and wasn't very happy that I'd delayed so long in getting here. But, we finally got started and worked for a little while on that. And then, it became obvious that there were other ways to solve this gas problem, so the catalyst approach was dropped. We were going to make what is called a heterogeneous catalyst -- that is, a solid that would just sit in the liquid and do its thing. It turned out that it was much more effective to use a homogeneous catalyst -- something like copper or copper salt dissolved in the water, which was certainly an effective catalyst and much easier to manage in a solution reactor of that sort. So, our catalyst program kind of dissolved. But, we immediately set out thinking of things that we might do in the way of catalysis relating to radiation. And, until he went back to college to get a degree the next spring, we tried some preliminary experiments to see what the reactor did with catalysts.

STOW: Bob, what was the first job that you were assigned when you got here?

HOLMBERG: My first job was in the Army and there I worked for Jerry Cole.

STOW: I understand.

HOLMBERG: Primarily, I was doing some work that I was unqualified for and S-50 was brought up.

TAYLOR: Oh my ...

HOLMBERG: I was supposed to write a section of the Smyth Report describing the S-50 plant. I had about two months to do that and by that time, the Smyth Report came out. Jerry left -- offered me a job at the Lab and I got out here in July of 1946 and went immediately to work for Kurt Kraus in plutonium chemistry. It was an interesting experience because I don't recall ever having been trained in microchemistry manipulations and the dangers or hazards of plutonium. It was just a common thing. We worked in open hoods and ...

STOW: But, the danger of plutonium was well known going back to the early 1940s.

HOLMBERG: Yes.

STOW: Was it a safe work environment, do you think?

HOLMBERG: I thought it was a safe work environment. We used fairly large amounts of plutonium. I once had a test tube in my open hood with a hundred milligrams of plutonium in the sixth valence state that I was using for some experiments. It's my understanding in the later days that that was more plutonium than Oak Ridge National Laboratory would allow.

STOW: We may have that much spread around on old lab benches now. (laughs)

HOLMBERG: One of the big problems at that time was that nobody had cars – only a few people had cars. And, we took buses and there was a 440 bus. In the course of our work, we'd get a few counts of plutonium. So we had to get our hands clean before going home. We had hand counters. You have to scrub your hands with soap and water and, if that didn't get it all off, you tried dipping your hands in hydrochloric acid and following it up by dipping them in sodium hydroxide. There was a whole formula that had been passed down to me on how to clean your hands and get them count free.

STOW: Probably would not be acceptable in today's environment, I don't think.

HOLMBERG: (laughs) I don't think it would be acceptable.

STOW: What about issues related to disposal of nuclear waste out of the laboratories, and so on? Was there much consideration given to that in 1945 when you got here, Ellison? Or a year later in '46 ... ?

TAYLOR: Yes, it seemed to me that it was just exactly what you needed to do. You kept the waste in a separate place. You didn't spread it around. There were hot drains in some of the sinks, which led to containment -- not into the regular sewers. The stuff that went into sewers passed through some sort of processing before it ended up in storage tanks. And, from the viewpoint of those days and from the practical necessities of doing these things in a finite amount of time, it impressed me from the beginning as just about the right level of precautions.

STOW: Good. In looking back at personnel records for the years when you fellows initially came, there are some names that jump out at me -- individuals who ended up with fairly prestigious careers in the chemical sciences elsewhere. George Boyd, Harrison Brown, Merlin Peterson ... but they went on elsewhere and were employed in other laboratories. Lyle Borst was another one. Can you recall any interactions with those people? And, what kept you guys here?

TAYLOR: Well, the fact that it was a job kept me here.

STOW: Yes.

TAYLOR: Also, that it was interesting. Yes, I knew most of those people, some pretty well, some not so well. Harrison Brown, for instance, was the assistant director of the Chemistry Division when I came here, and he was the person I talked to most often in the early days when I was trying to set up a program in catalysis for their reactor. And, he and I had lots of discussions about where we could get people and how much money we could spend to buy equipment for this new thing. He soon became pretty busy in the effort that people at the Laboratory were making to be sure that atomic energy -- later called nuclear energy -- was used in a reasonable fashion. In the beginning, a number of people at the Laboratory and a few from K-25, and perhaps some from Y-12, got together and decided they ought to promote the idea among the general public in an educational manner that nuclear energy was here to stay. There wasn't anything we could do about it, and there was no way that we were going to keep it from other countries that might want to use it against us. And so, we'd better be getting on with some sort of arrangements worldwide to make sure that it was only used peacefully. And, an organization was formed that eventually had the acronym AORES -- Association of Oak Ridge Engineers and Scientists. This was not a work-related job; it was an after- hours job, although many of the people, some of whom you've mentioned, did a good deal of telephoning during the day to people at other AEC sites in Chicago, Los Alamos, and so on. This organization sent out lecturers to places that were interested in it, to talk about the problems that atomic energy was going to pose for the world. We had, I think, three principles that we espoused. I can think of only two of them. One was that there was no defense against nuclear energy. If somebody else had a bomb and decided to drop it on us, there really wasn't any defense. It has too massive an effect. The second was that there was no way of keeping it secret. Any country with any scientific establishment could go ahead and make an atomic bomb. So, it wasn't going to be any use in trying to keep it secret. It wasn't going to be our secret for very long. And so, we espoused the idea that the world should get together and decide what to do with this new type of energy in a peaceful way. So, we sent out groups of two or three people to universities or other places that had an interest in this and gave public lectures, trying to drum up support. That was the chief non-work activity, I think, that occupied a great many people at the Laboratory. Among them, Harrison Brown and Charles Coryell were really the most moving spirits among us. Charles was legendary for his tremendous use of the telephone, much of it for technical reasons. All of us had to call around to various other places, particularly to Chicago because of the interrelation of the research, but some of this, of course, carried over to things like seeing what they were doing in Chicago concerning this kind of "advertising" about the properties of nuclear energy for the security of the world.

STOW: In 1947 the Atomic Energy Commission came in to run the Oak Ridge facilities, as well as Los Alamos and the other facilities. Did you see any changes that could be directly tied to the presence of the Atomic Energy Commission? Bob, any ideas there?

HOLMBERG: Certainly, not at my level. At that time, I was pretty much laboratory-oriented, and we had gone through the problem of losing Chicago as our contractor ...

TAYLOR: That came almost a year after the AEC came in. Monsanto came in January of 1947 as the Lab contractor, I believe. During all that year, they had already decided on the University of Chicago as a new contractor for the Laboratory to replace Monsanto, which had operated it since 1945. Before that, DuPont was the operating contractor, although under the University of Chicago. But now, in 1947, Chicago was going to take it over as soon as they could find a new director for the Laboratory to replace Martin Whitaker, who had been the director of it from the beginning. And, that didn't go well. AEC people talked to a good many prominent scientists -- mostly, if not, exclusively physicists -- and brought them here for interviews. The candidates met the people at the Laboratory, who had dinners for them and showed them around the Laboratory. None of them wanted to take over the directorship. And, in the end, toward the end of 1947, the AEC settled on having an interim director, Warren Johnson, who had gone back to the University of Chicago Chemistry Department, from his position during the war as director of the Chemistry Division here. He was the first director of the Chemistry Division. So that was the situation toward the end of 1947. Chicago was still going to be the operating contractor, and we were going to have an interim director, Warren Johnson. And, the AEC was still going to find a permanent director of great distinction who would run the Laboratory. But, then the AEC gave up -- shortly before Christmas in 1947 – and decided to take an easier way out. They would have Union Carbide operate the Laboratory. Carbide was already the operator for K-25, which had the largest organization. Most people in Oak Ridge were already used to the city and knew something about the Laboratory. So, in December of '47, the AEC announced that Union Carbide would be the new contractor and announced some other changes that struck deeply at the rather vague plans we had for the future of the Laboratory. The Lab was planning on two or three different kinds of reactors that it wanted to build and expected that the Laboratory would be a site, if not the chief center, for the reactors in the AEC complex. AEC had decided at that point that they would move all that to the Argonne Laboratory in Chicago, the successor to the Metallurgical Laboratory after the war ...

STOW: Yes.

TAYLOR: ... and that would leave to Oak Ridge National Laboratory the work on development of waste treatment of nuclear fuel for extracting plutonium and fission products – namely, a chemical processing laboratory. Well, this didn't fit at all into the ideas that we had had for what we might make of the Laboratory, and so there was great unhappiness. On Christmas Eve and New Year's Eve, at a whole series of parties, people got together to bemoan the newest developments and make nasty remarks about the AEC and Union Carbide. At the time Clark Center was the president of Union Carbide's office in Oak Ridge. All of this turned out for the best in the end, I think. Our negative impressions of Union Carbide had been based mostly on lack of knowledge. A number of their lesser administrators who thought Carbide might take over the Laboratory made snide remarks such as, "Well, we'll straighten out that country club." And, since they were an industrial chemical firm, we had some idea that they might have pretty severe ideas about how to run a research laboratory. So, we didn't like the idea and character of Carbide. I'll put most of it in the laps of Clark Center, who it turns out, really knew how to run a research laboratory from the position that he held relative to it. And, in the end, he was responsible, perhaps as much as anybody, for the success of the Laboratory with the respect to AEC, which got things straightened out so that we did participate in reactor programs. We did keep up a basic research program, and evolved into, more or less, the Laboratory we are today.

STOW: Yes. Alvin Weinberg is very complimentary of Clark Center.

TAYLOR: Yes ...

STOW: ... and points out that he was instrumental in saving the Lab within two years after "Black Christmas."

TAYLOR: Yes. A great many people left the Laboratory. Ones that could find other jobs left for them. It was a black period.

STOW: Bob?

HOLMBERG: Ellison left out one important point.

STOW: Yes.

HOLMBERG: I didn't know Ellison very well. He worked at the other end of the building.

TAYLOR: Yes, the chemistry building was rather long -- there were three wings. I worked in the west wing. He worked in the east wing ...

STOW: Okay.

TAYLOR: ... and the administrators were up front in the middle wing.

HOLMBERG: But, Ellison became famous during this "Black Friday" by writing a poem, which he did not allude to in this ...

TAYLOR: Well ...

HOLMBERG: ... and so, after that, everybody knew Ellison Taylor.

STOW: Is that the "Deck the Halls" poem?

HOLMBERG: Yes.

TAYLOR: Yes, but I didn't draft it ... actually, the person that started it was Frank Miles. I came to the party when they were already working on it, and Frank Miles and Jim Standy and I finished it. And Alvin Weinberg's memory of it was somewhat fallible. He thought the language we used was somewhat worse than what we actually did. We just said ... one of the lines was "AEC has screwed us clearly." Well, he didn't remember it just that way.

HOLMBERG: (laughs)

STOW: I think he published it somewhat differently in his book.
TAYLOR: Yes, he did. (laughter)

STOW: What was your morale like at that time Bob? I mean, you were kind of down in the trenches I guess, but ...

HOLMBERG: I was down in the trenches ... it wasn't that personal. I, like everybody else, didn't like the idea of Carbide coming in, but there was little I could do about it. And so, I just continued my work life.

STOW: Well, Jerry Cole continued as division director for chemistry, I understand, and Ellison, you were assistant division director.

TAYLOR: Yes. He made me assistant division director in the late spring or early summer of 1946, after he'd been here for three or four months, and I'd been here for half-a-year. I think he wanted somebody to help him. There had been a rather formal administration in the division at that time, like all the rest of the Laboratory. The Chemistry Division had a director, an assistant director, section chiefs, assistant section chiefs or associates, and then group leaders. So, there was this rather large organization with maybe more managers than troops. And, Jerry simplified that by fiat. As a result, we had just the division director and group leaders. But he wanted to get an assistant. Of course, the logical thing would have been to get one of the few section chiefs that stayed, but I think he thought that there probably would be a certain amount of jealousy if he appointed one of them. So he picked me. He asked me if I would be assistant director perhaps because I was the oldest of the new people that came here. Well, at the time, I wasn't all that interested in it, but I decided it would be a good idea to accept, because it would give me a fallback position if the research I was vaguely planning didn't pan out. I could fall back on the fact that I had some administrative work to do. So I said, "Well, yes I'll do it, if I can spend half my time doing research. I'll do administration in the morning and research in the afternoon, or whatever way it works." So, he said, "Sure, fine." And so, just very informally, I picked that up and I kept that up, at least in my mind all the rest of my career. I don't think anybody else really thought about it much, but I always felt to myself that I was doing a half-time job, and I was to have half-time to work on research.

STOW: I doubt one could do that in today's environment.

TAYLOR: I doubt it very much. One can hardly do full-time research as a division director.

STOW: Well, let's touch on a few things that went on during the 1950s. Bob, what did your job evolve into during the 1950s, after you'd been here for five or six years?

HOLMBERG: Well, I was still with Kurt Kraus for five or six years...

STOW: Were you?

HOLMBERG: We had moved into the 4500 Building. Plutonium chemistry was gone. I was still working on heavy metal chemistry. I was also going to school at the University of Tennessee and doing coursework leading to my Ph.D. degree. My original intentions were never to stay here ... (laughs)

STOW: Here you are ...

HOLMBERG: Here I am. Eventually, I changed from working for Kurt Kraus in solution chemistry to working with Ralph Livingston on electron-spin resonance on which I did my thesis.

STOW: Yes. And, in 1951, Ellison, I think, you hired Sheldon Datz to come work for you, did you not?

TAYLOR: Yes. I had hired him originally as a dishwasher in my group at Columbia University. He had just graduated from high school in New York City and was looking for a job. And he was bright and wanted the job, so he came on. He didn't tum out to be a very good dishwasher because we found so many other things that he could do, and I formed a very good opinion of his abilities

-- both experimental and mental. Then he was drafted into the Navy. After the war, he went to Columbia and earned a bachelor's degree and a master's degree and was looking for a job. I hired him for the research I had already started on trying to use molecular beams to study mechanisms of chemical reactions.

STOW: Well, did that work lead, at least in part, to his getting the Fermi Award?

TAYLOR: Well, it was part of it, yes. Although I'm pretty sure if that's all he had done, he might have qualified for other sorts of awards, but not for the Fermi Award, because the molecular beam work doesn't have any particular relevance to nuclear energy, which the Fermi Award was started to recognize. But yes, that was his major accomplishment in that field, both with me and later with other people that he got hold of.

STOW: Let me spin off of your association with Sheldon for a moment. Now that each of you look back over your careers at ORNL and originally Clinton Laboratories, you've rubbed elbows with a large number of individuals in the chemical sciences and in the physical sciences, that have turned out to be quite well renowned individuals in their own rights. Can you identify one or two people that you really have admired as coworkers or peers here at ORNL that you'd like to mention?

TAYLOR: Yes, well, I'd go back to the two people Bob worked for. Kurt Kraus, first ...

HOLMBERG: Yes.

TAYLOR: … and Ralph Livingston, second. Bob's told you a little bit about his work with Kurt. Now, he hasn't emphasized Kurt's impact on chemical science, but it was very great. He was an extraordinary inorganic chemist -- he had all sorts of ideas. He understood experiment and theory and he knew how to use good people. He produced a tremendous amount of research that revolutionized various parts of chemistry, including things that are extremely useful in industrial separation processes using ion exchange. And, for a number of years, he was the head of our water research project, whose main practical goal was producing potable water from the sea by nuclear energy. Reaching this goal required a great deal of chemical engineering and all sorts of research and development. The other person Bob worked for more recently was Ralph Livingston, one of the most extraordinary people we had at the Laboratory. Just to make a general statement, I used to remark to people sometimes that we surely had made a great splash in science with the second team from the Laboratory. All the people with the greatest reputations, who had done the most spectacular work during the war, had returned to their universities, or wherever they came from, leaving us with people who had smaller jobs, less publicity, and fewer accomplishments during the war. But, with people like Kurt and Ralph Livingston, the Chemistry Division produced research that, at that time, I think matched what was being done at any of the great universities in the country. Ralph Livingston, in particular, was just a genius at taking new physical techniques, such as electron-spin resonance and pure quadrupole spectroscopy, and applying them to chemical problems. Ralph and I worked briefly together on electron-spin resonance. The technique of electron-spin resonance had just been invented. Ralph immediately saw its applications to various problems we had at the laboratory and built all the apparatus to do this. I had some samples at the time that I had been irradiating at very low, liquid nitrogen temperatures with gamma rays, to see if I could find out something about the free radicals that were formed there. And, Ralph said, "Look, let me look at some of these samples to see if I can identify any free radicals." But I expected these free radicals to be produced during irradiation of materials with gamma rays and other ionizing radiation. And, about the first sample that I gave him came up with unmistakable evidence that hydrogen atoms were formed -- neutral hydrogen atoms, not molecules -- frozen in the liquid in which I'd produced them, sulfuric acid. And, starting with there, he went on to a whole series of research findings detailing what things happen during irradiation, particularly in water and aqueous solutions, which are very important to reactors, for instance. They are also very important in biology, since most of a person is water and irradiation effects are dominated by what happens in the aqueous solutions we're made of.

STOW: Ellison, you may want to come back to that in a moment as an accomplishment of your life. I wanted to ask each of you to reflect on your careers here at ORNL and identify the one or two greatest accomplishments that you think you made to science, politics, the well-being of the Laboratory or your profession. Bob, let me start with you.

HOLMBERG: Well, that's a hard one. I think probably my best studies were the studies of single crystals ...

TAYLOR: Oh, those beautiful crystals you grew! Huge crystals!

STOW: All right.

HOLMBERG: And studying the anisotropic spectra ... electron-spin resonance that we had to identify free radicals in crystals.

STOW: These were crystals of what?

HOLMBERG: Oh, any crystal I could grow could be studied this way. For quite some time we worked on single crystals of hydrogen peroxide. We always seemed to work with chemicals that were nominally dangerous, but really weren't.

STOW: What has the impact of that work been?

HOLMBERG: Well, we tried to find the OH radical. We did not find it definitively. But, that kind of search in hydrogen peroxide was a fascinating experiment. We worked with hydrogen peroxide and deuterium peroxide, with deuterium substituting for hydrogen.

STOW: I can tell you enjoyed that work because you light up as you talk about it. That's great.

HOLMBERG: Yes, I did.

STOW: Ellison, what about you -- what would your answer be to that question?

TA YLOR: Well, the most important scientific experiments that I had a part in was the work I did with Sheldon Datz on using molecular beams to study chemical reaction mechanisms. That really revolutionized the study of chemical reactions. In fact, the name of the field, which at the time was chemical kinetics, has now become chemical dynamics. And, you can follow the utmost details of chemical reactions between gases by this technique. And, if I had any success at all as an administrator, I would say that the most important thing I think I know is that you do your best administration if you don't really want to do it, if your heart is in something else. I think if you want to be in charge of something, chemical research is not the thing you ought to be in charge of.

STOW: Well, there's some good philosophy for us to think about. Do either of you have any little sideline stories you might want to pass on -- things I haven't asked you about but that might be interesting as incidents that went on behind the scenes.

HOLMBERG: I have a little story about Ellison. It's really a story about my wife and Ellison. It was when one of our small cutbacks was occurring at the Laboratory. Most employees were a little bit worried about their jobs. My wife called me at work, but I wasn't in my office. Henry Zeldes answered the phone and told my wife, "He's not there. He's talking to Ellison." And, that response panicked her ...

TAYLOR: That must have been frightening.

HOLMBERG: I got home and she said, "Well, what were you talking about?" And I said, “Volleyball."

STOW: (laughs)

HOLMBERG: Ellison and I were co-members of a volleyball team, and we were discussing strategy for the next game.

STOW: What year was this? Do you ...

TAYLOR: Oh, I don't know. Maybe '61. That was one of the worst years for downsizing. That's another reason why you shouldn't want to be an administrator -- at least in an organization in which you don't really have any control over funding and size.

STOW: Well, what caused the downsizing in 1961?

TAYLOR: Well, I think that one was related to the loss of the Molten Salt Reactor. I could be wrong on the date, but we had a big effort in molten salt chemistry to back up the Molten Salt Reactor. Anyway, the chemistry funding got removed from that project or the aircraft reactor project, which led to the Molten Salt Reactor. But anyway, there were two or three or four instances like that in which we suddenly had to drop a number of people.

STOW: During the Molten Salt Reactor days, there was a good bit of work done in the Metallurgy Division on development of high-temperature metals that would resist corrosion by the fluoride-bearing salts.

TAYLOR: Yes.

STOW: What was the collaboration like between chemistry and metallurgy?

TA YLOR: Well, by that time, the chemical work relating to that was done in a separate division. Because so much of the work was specialized, we decided it would be better to have another division called the Reactor Chemistry Division.

STOW: All right.

TAYLOR: And so, the people in the Chemistry Division who had been working on molten salt chemistry moved into the Reactor Chemistry Division. But this new division’s chemists had very close collaborations with metallurgists, particularly on "in pile radiation studies." If you had a

molten salt mixture you wanted to use as a solvent for uranium in a reactor, you needed to study it in actual radiation fields to see how it would react with various metals and alloys. The Reactor Chemistry Division and the Metallurgy Division did this sort of thing jointly.
STOW: As we wind down here, let's step away from the work that either of you specifically did in the Chemistry Division and look at the entire division. What would you say were the two greatest accomplishments within the Chemistry Division during your tenures here? Bob, do you have any insights on that?

HOLMBERG: Well, other than what Ellison has already mentioned, I worked with Kurt Kraus and Ralph Livingston. I also think we ought to mention the work of Henri Levy on the diffraction X-ray and neutron diffraction work.

TAYLOR: You know, Levy's work was one of the division's most solid accomplishments. It has long had a continuing influence. Calculating the results of a diffraction experiment is a monstrous task that is slow and tedious by hand. The calculation, when done by computers with the proper program, goes pretty well. The Laboratory staff led by Levy developed the best programs for helping researchers interpret their diffraction data. As computers improved further, they were used for controlling diffraction apparatus. You had to move the components of the apparatus around to pick up the reflections of X rays or neutrons from various planes of the crystals you were looking at. And, computers were an obvious way to do this. The programs that ran these computers were developed at ORNL and distributed very widely. Another important thing I think we did, mostly in-house, was educational work in collaboration with what was first called the Oak Ridge Institute for Nuclear Studies and later Oak Ridge Associated Universities. We were able to bring university professors here for a year at a time to study some particular field in which they were interested, such as diffraction or the use of carbon-14 in organic chemistry, for example. We would bring undergraduate students here for the summer, so they could get some idea of what research was like in a large laboratory. And, we would bring graduate students here to do research on materials that weren't available elsewhere, or with equipment that wasn't available at their home university. So, perhaps the most enduring result of the Laboratory's existence in terms of chemistry has been its educational impact on institutions all over the country and all over the world.

STOW: Well, there's certainly been an impact by the Chemistry Division and by both of you fine gentlemen who've taken time out of your schedules to join us here today. We want to thank you very much for coming here and spending some time looking back in history. Thanks a lot, Ellison and Bob.

TAYLOR: Well, thank you very much for having us, and it was interesting to talk to you about it.

HOLMBERG: Yes.

TAYLOR: What else would you think of to ask me?

STOW: Ellison, when I look at your name, something in the back of my mind is associated with some work that you did at one point on a "polywater doodle?" Can you explain that to us?

TAYLOR: Okay, if I can stop laughing and crying. Well, if you want to know how this came about, then I'll tell you, okay? Back in the early 1960s, a Russian in some little place [Nikolai Fedyakin of Kostroma, Russia] thought he had found a new form of water. If he would leave some fine capillary tubing in a wet atmosphere, he'd later find little drops of liquid in the tubing. And, he thought the drops had properties different from those of ordinary water. So, he took his discovery to a famous Russian physical chemist in Moscow named Boris Derjaguin, who took this idea and repeated this simple experiment. He found that, if you put this little capillary tubing in a moist atmosphere, after a few days or a week or so, little drops of water would condense in the tubing, except they didn't act like ordinary water. Their viscosity was much higher. He'd measure that by blowing it along the tubing, back and forth, to determine the drops' flow rate. Also, the vapor pressure was different -- it wasn't like water in evaporation rate. The freezing point and boiling point were different from those of water. So, he started a big research program on this new kind of water. He should have known better, but he got carried away. These findings eventually got re-evaluated by some chemists in Great Britain. They found similar differences in properties and some of them got all excited over their findings. Finally, interest in the new water came to this country. It began to get popular attention in publications such as the *New York Times* and *Chemical and Engineering News*. Chemists started talking a lot about it. They asked, 'What is this all about?' You must have heard about it.

HOLMBERG: Oh, yes.

TAYLOR: Well, it struck me as obvious what it must be. I thought, "Well, it's obviously just ordinary water that's condensing on little pieces of impurities -- maybe little crystals of salt inside the tubing." And, that'll give you a solution that has properties different from those of pure water. But since there was so much talk about the Russian work, I thought, "Well, I'll try it -- if it is something, it'd be interesting to know about it." So, I repeated this experiment, went away for a weekend, and came back to my little capillary. I saw the tubing had some drops in it, and I experimented on them in the way Derjaguin did. But, I persuaded myself that this indeed was just a solution of little impurities that happened to be in the tube. Now, Max Bredig and I had been talking about the Russian work, and we agreed it was just crazy. If water solutions form like this, why wasn't all water "anomalous water," as this was first called? But then, in *Science* magazine there appeared a big article by a well-respected spectroscopist who had taken infrared spectra of little samples of supposed anomalous water and found indeed it wasn't ordinary water. The authors called it "polywater" and offered theories as to how it formed. Oh! Hurrah! We heard that all over the place. Well, Max and I decided to get a little more serious about it. And Max started trying to repeat the spectroscopic experiments. And, I wrote up what I had done and we submitted our paper to *Science*, but our paper was rejected. The editors said they already had received some papers that proved that polywater wasn't so. So, we forgot about it. But, the colloidal section of the American Chemical Society had a meeting on the subject in Pennsylvania, and we both went to it and listened to the authors of the papers. I presented our little paper and came back. We discussed the ACS meeting and then presented a little seminar here about the polywater controversy. And, Bobby Lyon, who was the editor of the *Oak Ridge National Laboratory Review*, asked me to write a little article about it. So, I did. After I finished, I gave it to her, and she noticed that I had given the article this title: "Two Years with the Wrong Water." Because it had been about two years since I had started thinking about this, and it obviously wasn't the water almost everybody else thought it was. My title was based on a sort of a joke, because there was an old country-like song titled "Thirty Years with the Wrong Woman." So, I thought "Two Years with the Wrong Water" would make a catchy title.

STOW: (laughs)

TAYLOR: But, Bobby thought of a better one. She suggested the title "The Great Polywater Doodle."

HOLMBERG: (laughs)

TAYLOR: Well, now, that's what I'm famous for at the Laboratory, I guess. I'll go down in history as the man who wrote the article on "The Great Polywater Doodle." (laughter)

STOW: Well, your reputation is much more than just the Polywater Doodle fiasco. We thank you guys today for coming over and spending time with us. You've made significant major contributions to Oak Ridge National Laboratory and to the Chemistry Division, more specifically. And, we thank you for taking the time to share some of those historic thoughts with us.

TAYLOR: Well, thank you, and remind us not to fall off the step here when we get up.

STOW: I will do that.

HOLMBERG: I'll hold your hand.

--END OF INTERVIEW ---