9531

Smithsonian Videohistory Program

Manhattan Project
Session Six
Collection Division 2: Oak Ridge

Stanley Goldberg, Interviewer

March 4, 1987

Banic, George M., Jr.

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Larson, Clarence

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Livingston, Robert S.

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12-23-87
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Robert S. Livingston
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Manhattan Project: S-6

The Manhattan Project: Session Six

Calutron Design and Operation

Interview with
George M. Banic, Jr., John M. Googin, Chris P. Keim,
Clarence Larson, Robert S. Livingston

March 4, 1987
in Oak Ridge, Tennessee

by Stanley Goldberg
Interviewer

for the Smithsonian Institution Videohistory Program

NAME LIST

RU 9531
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01:37:33:00 [Interview begins; Banic, Googin, Keim, Larson, and Livingston are seated around two tables in a V-formation with Goldberg seated at the open angle in the Kennedy Maxwell Studio in Oakridge, Tennessee]

GOLDBERG: Well, why don't we begin. We're going to be talking today about electromagnetic separation, which was the effective process for separating Uranium-235 from 238. But in fact, there were a whole variety of different kinds of separation processes proposed, and this was the last one that people thought about. This was in Berkeley, California [University of California, Berkeley], I guess, where it first originated, and you were there, Dr. Livingston, weren't you?

LIVINGSTON: I didn't start on the project the very first day that it started, but I did start the day after Pearl Harbor, which was in December, 1941. Some work had already been done. The 37-inch cyclotron in the old wooden building at the Radiation Lab had been dismantled from the cyclotron and some new guts had been put in it to make a mass spectrograph. It was actually running as a mass spectrograph, and the electromagnetic processes were really based on scaling up the mass spectrograph, the scientific instrument which had been known for a long time. Ernest [Orlando] Lawrence, it was his laboratory and it was his initiative that started the project.

The question of whether a mass spectrograph could be scaled up was in scientific dispute at the time that I got on the project, and there was some theoretical work that suggested that due to space charge effects, you would never be able to get more than a small fraction of a microamperes of current. Of course, for the project to be important, you would have to get large currents in the milliampere range.

GOLDBERG: Let's stop here a minute, because not everybody that sees this is going to know what a mass spectrograph is. Why don't we say a little bit about the principles involved in this kind of separation process. We've got, in fact, a rather lovely schematic here. Okay. You talk about it. You know it far better than I do. You don't even have to see the drawing of that. But what we're doing here--well, why don't you say what it is.

01:40:29:00 [Close-up of plaque with schematic diagram of a mass spectrograph]

LIVINGSTON: Basically, to separate uranium isotopes, you need an ion source, a way to get ionized uranium atoms. You need a magnetic field, you need to accelerate those ions, and you need a collector box of some sort. The idea in this case, in the electromagnetic process, is to purify Uranium-235, which exists as a very small fraction. About seven percent of natural uranium is U_{235}, and that's the isotope that you need.

01:41:11:00 [Goldberg hands schematic plaque to Livingston]

GOLDBERG: Right. I guess the best thing to do is to give this to you, and you can aim it at that camera over there, and we'll talk about the real hardware as we go along. I mean, there's a lot of chemistry involved in this, and I hope you people aren't just going to let him get away with making mistakes. [Laughter]

LARSON: Bob's done a wonderful job of setting the stage here, and we'll chime in.

01:41:36:00 [Livingston indicates the ion source and the collector box on the diagram]

LIVINGSTON: In this diagram it shows the source at the bottom of the apparatus, and the collector box at the top.

01:41:53:00 [Using a pointer, Googin indicates the actual ion sources on the table]

GOOGIN: Here are the sources right here, with the charge material in the back and the vapor passageways and the slits, and then we've got a little gadget here that fits on over here that helps us ionize the thing to make the ions.

01:42:08:00 [Larson holds up the filament]

LARSON: This essentially is the filament. [Laughter]

GOOGIN: Filament. That's the filament in the light bulb that helps makes the ions.

LARSON: It's a very big filament.

LIVINGSTON: And this is the collector for the Uranium-235.

GOOGIN: Right over there.

LIVINGSTON: In the effort to get more intensity, which was the game that you were fighting, we went to two arcs in this particular machine. The lower one and the upper one you can see over there. The two arcs are not shown in this sketch. This is a schematic and it shows one.

GOLDBERG: We'll go over that in some detail later, but if you could turn that slightly. That's good. So that the camera can see it. I have a picture here of one of these things, the only one that we could find quickly. I know there are clearer ones than this, of-- I guess it was called a door that this was all built into, right?

BANIC: That is the D. Unfortunately, that is Alpha One process. That's different from this. This display or this plaque that we brought along is one that we use to explain our stable isotope separation, which is evolved from uranium. So there are differences.

LARSON: Yes, but the principle is the same.

BANIC: The door, as you call it, the D system, is for the Alpha One source.

01:43:30:00 [Close-up of a photo of the Alpha One system]

GOOGIN: That picture is Alpha One, and this, of course, is a Beta. Modified, maybe, but a Beta unit.

01:43:36:00 [Goldberg indicates the ion source, the collectors, and the magnetic field in the Alpha One photo]

GOLDBERG: If we could put that picture back on. What we're talking about here is the source is at one end of this, and the collectors are at the other end. The magnetic field is actually perpendicular to this plane here.

GOOGIN: The circle. That great circle.

GOLDBERG: And ideally, the path of the uranium ions, the charged uranium particles, is around in this semicircular path here. The magnetic field bends them into a circle.

GOOGIN: Of course, there was a lot of artistic work done on shaping the magnetic field of what is known as shims, and that's one of the particular problems.

GOLDBERG: We're going to get to that, because, in fact, I gather the problem here was, when you said "space charge effect," that the very process itself, the beginning of the process itself would cut it off.

LIVINGSTON: That was the theoretical grounds for the pessimism at the beginning, that it would be impossible to get very large currents, because the space charge effect would blow the beam up and nothing would be focused. The early work in Berkeley was really designed to look at that and find out if it was true. Ernest Lawrence was very skeptical about the fact that these theoretical predictions were so pessimistic.

GOLDBERG: When you say the early work, when was that?

LIVINGSTON: In '41 and the early part of '42. Rather rapidly, in perhaps the first month or so of working in that 37-inch cyclotron unit, we found out that these predictions were really wrong, because we were able to get the currents in the microampere range. Later on, at the plant down here, we got many milliamperes of current, and the total output of the building was measured in amperes. But down there we had just one unit running, and Ernest Lawrence came in one day when we had managed to get the current up, I think, to something like 10 microamperes, and he really whooped and hollered, because that meant that for sure the space charge effects had been far exceeded from the nearest theoretical predictions.

LARSON: I was just going to say that it's obvious to all of us, but we speak of the source, and perhaps some people who have come on this term for the first time, the source we lump together as the radium material--which happened, in this case, to be uranium tetrachloride, plus the ionization chamber there, plus the accelerating electrodes and so forth, and that's the combination of all these which we call the source--produces these uranium ions which then can travel. I think some people, when you speak of source, it's so obvious to all of us, but--I'm sorry to interrupt.

GOLDBERG: You're not interrupting at all. Not at all. In theory, this is all very neat. I mean, my impression of this--

and correct me if I'm wrong--the theory is all very neat, but, in practice, as John said, there was an awful lot of tinkering that had to be done with it before you could really get it to work. Is that a right impression or is it wrong?

LIVINGSTON: Yes. Basically, it was detailed hardware problems. You had to get everything to work simultaneously and essentially perfectly, you know, for a good process, so that you had high voltage of 40,000 volts or so on the source, which would push the ions out, repel the ions, and start them.

GOOGIN: In your drawing, we don't have it here, but there are some carbon electrodes that hang in the front of this that shape the beam and accelerate it.

01:47:54:00 [Livingston indicates the kilovolts on the source in the schematic diagram]

LIVINGSTON: This sketch here shows 35 kilovolts high positive on the source and -20 kilovolts on another electrode, and then the rest of the apparatus is at ground potential, so that the actual voltage driving these ions was about 55 or 60 kilovolts. That helped get the input up, having higher voltage. So it was a compromise between the electrical breakdown which you would get if you raised the voltage too high. Actually, there was a play of a lot of different things going on.

GOLDBERG: Now, Dr. Larson came on this project about six months after you did.

LARSON: July of 1942.

GOLDBERG: Were they already using uranium tetrachloride for this?

01:49:00:00

LARSON: They had started using uranium tetrachloride. I might point out the origin of that. When Dr. [Alfred Otto Carl] Nier of [The University of] Minnesota did his original separation to prove that it was the U_{235} isotope, he tried uranium hexafluoride first as a volatile source. He would ionize that. But he found that that was not very good, because it was so volatile, he lost all of it before he got a chance to make his measurements. So he tried uranium tetrachloride and uranium tetrabromide. He tried both of them. I've forgotten which one finally worked best, but either one would have worked. So he made his separations, which he then forwarded to [John Ray] Dunning. He did those with uranium, either tetrachloride or uranium tetrachloride.

GOLDBERG: When you say Dunning, that's John Dunning?

LARSON: John Dunning. That's right.

GOLDBERG: And where was he then?

LARSON: Well, Nier was at Minnesota. He did the separation, then shipped it to John Dunning at Columbia [University], and that's where he proved that it was the U_{235} isotope that was really the one that did the fission with slow neutrons.

GOLDBERG: Was that in 1940?

LARSON: Late '40.

LIVINGSTON: It was sometime in 1940, because the world knew all about the theoretical possibilities in 1939 from [Lise] Meitner's article. But it wasn't clear what isotope. They knew it was uranium, and so this work by Dunning nailed it down that it was U_{235}, which, fortunately or unfortunately, was the scarce isotope.

LARSON: There were two possibilities: that it could be U_{235}, which would have the ideal; or a very low cross-section, but noticeable from U_{238}. Nier and Dunning's experiment clarified the whole thing and set the stage for the separations.

01:51:22:00

GOLDBERG: You said before that there was a lot of skepticism about being able to do it this way. What was Lawrence like, that he just pushed through that?

LIVINGSTON: Well, if you know Lawrence, he was the inventor of the cyclotron. He was a very good guy at raising money for research. He had a lot of contacts and had raised money for the 60-inch cyclotron in Berkeley and was well on his way toward raising money for a still bigger cyclotron up on the hill, which later on was used, actually, for a prototype.

GOLDBERG: "Up on the hill" means? What are you referring to?

LIVINGSTON: Well, that's the phrase we used at Berkeley. The Radiation Lab had a very small side on the campus, the regular campus, close to the physics building, and so they needed to expand, and they got some property up about a mile or so away. It was up on top of a hill.

GOLDBERG: I see.

GOOGIN: Above the stadium.

LIVINGSTON: Above the stadium, yes. And they were going ahead at that time, I guess, in 1939, they were going ahead with plans to build a very big cyclotron, much larger than the 60-inch. Then, of course, in '40 we were moving closer toward being involved in the war, and in '41, after Pearl Harbor, we were in the war. So that really changed all the emphasis on things.

01:53:04:00

GOLDBERG: So the idea here was that it was the magnet of the cyclotron that was used to test the principle of making a large mass spectrograph.

LIVINGSTON: Yes, although we essentially were using this little 37-inch cyclotron, which was right close on the old campus, we did prove that you could get large currents. Even after that, though, there were still a lot of skeptics, and the reason was that all mass spectrographs operate with a very small percentage of the material that goes through the ion source, ending up in the pocket of the collector. So people would say, "Well, even if you get this big current, you can never get an appreciable amount of stuff deposited in a pocket." So the goal of the experimentalists there was to increase those quantities, and that developed during 1942. Actually, after we came to Oak Ridge [National Laboratory], it kept increasing. By the time of the war's end, we were running up to 20, and I think the record was about 25 percent of the charge bottle was deposited in the collector, which was fantastic compared to anything that had been done before.

GOOGIN: Right at the end of the war, we ran a calibration run on the best Beta operations we could get. As I remember, we got up to 27 percent of the available material in the collectors, our very best units.

LARSON: I believe that started under 1 percent at first.

LIVINGSTON: Oh, yes, it was very strong. In fact, the early collectors, the very first ones we used down in the 37-inch, almost all the material was bounced back out, because it was just a little U-shaped piece of platinum that we were hitting, and the new ions coming in would knock the old ones out, so we had to go to things like this collector box over here.

01:55:16:00 [Googin indicates with his pointer a cross-section of an actual Beta collector box at the end of his table]

GOOGIN: This cross-section here shows you the complexity of what was going on.

GOLDBERG: Hang on a second, John, before we get to that. These were designed--what John is pointing to was designed in Berkeley?

01:55:27:00 [Close-up of the Beta collector box cross-section]

LIVINGSTON: Well, some cruder versions of them were designed. This is a much more advanced model, and there was continuous work going on and improving during the operation in 1943, '44, and '45. An interesting point about the collector boxes was that Herb [Herbert Frank] York, who later went on to considerable fame, was in Oak Ridge, and his job was to design better collector boxes, and he did a terrific job on it.

GOLDBERG: Go ahead.

01:56:00:00 [Googin traces the path of a beam through the slits into the box]

GOOGIN: The focal area of the beam is right at these slits in this model right here, and this is the smaller radius, so this is the 235 pocket, and this is the 238 back here. So you tried to put the focal area of the beam right at this slit, and the side beams are coming in various directions so that this is the only point where you have a terribly concentrated plasma right there, and then it diverges back into here and hits these surfaces. This is designed to make them bounce around without going back out. This one is designed with a whole series of things called footscrapers, actually, these pieces of graphite stacked along in this direction at an angle, so that the beam would hit and ricochet back and forth on bouncing ions and the sputtering carbon would end up taking out the energy from the beam without, again, bouncing back out. We ended up making a series of complex uranium carbides, as well as having some uranium metal deposited on the inside of these two chambers. This is what separated the two isotopes. We had another little isotope we had a problem with known as Uranium-234, which used to bounce off this surface here and return back into the recycle.

BANIC: John, while you've got the receiver there, while you have your plaque, let me hold this up a minute.

GOOGIN: It really bounced off and went right down here.

01:57:38:00 [Camera moves from the actual Beta collector to the schematic diagram as Banic explains the beam path]

BANIC: It may be a little confusing, because if you look at the source itself, as Dr. Livingston has pointed out, you see a good straight line with an exit source there, almost two or three centimeters by six centimeters. But when you look at the receiver itself, you notice you have a curved surface, the one that Stanley's holding, and it is curved and also at an angle of 45 degrees. As Dr. Livingston has pointed, the beam is accelerated through the source, through the exit system and through the electrode system, travels through the magnetic field and gets bent. You would expect, like in cases of mass spectrometers, which is primarily an assay machine, a nice straight line focused at the collectors so you can determine mass assays and whatnot. But as Dr. Livingston also pointed out, the idea here is production, get as much material as you can. This is where a couple of fellows from Berkeley, also the way [inaudible] came in. They designed a set of magnetic shims, and it's probably as much in importance in making the thing work as possible. If you look at the plaque again, you'll notice that the beam looks like a lunar shape, like a crescent moon. If you notice, in order to get as much out of this machine as possible, you open up the exit slits as much as possible to get as much material out to accelerate. Unfortunately, not everything comes out normal to the exit slit. Some of it will come up at an upward angle, some at a lower angle, which we refer to as positive and negative angles of divergence. The unfortunate thing is that these beams have to be brought back together when they reach the collector, because the idea is to get them into the pockets, as John has talked about. That is where these two gentlemen who developed the set of magnetic shims, which look just like a couple of airplane wings slapped up against the magnetic pole face. Now, the magnetic pole face, the whole thing is the vacuum chamber itself. The whole thing, of course, operates in a vacuum.

GOLDBERG: I want to talk about that in a while.

BANIC: Okay. So what happens is that these magnetic shims bent at a negative angle, those dropping down more than they did the upper ones, so, as a result, they refocused. So you can see now, instead of having a small ribbon of beam, you've got a large quantity of material coming out, and if you can refocus it up here at the receiver, you've increased the quantity of the machine itself. The unfortunate drawback is that when this aberration, due to the magnetic shims, gives you a curved pocket or receiver, you see, the angular curve, and, also, unfortunately focuses on a 45-degree plane. This meant a little more difficulty in the actual construction of the components in the design of that type of material, which was one of the functions that we were involved in in the so-called powder plant.

LIVINGSTON: An interesting background point on that magnetic shimming was that the first proposal for this came from [J.] Robert Oppenheimer, who was associated to some extent in the early days of the electromagnetic process there at Berkeley, and he did some back-of-the-envelope calculations. I think it came out of the very first theoretical work that the receiver plane would need to be 45 degrees.

02:01:02:00 [Camera focuses on Beta collector]

GOLDBERG: So the fact that the face slopes upward and people should understand that, the model that John was pointing to is cut and sectioned, but the face, actually--I don't know if you can get a picture of this--but the face is actually like this, and it slopes back at a 45-degree angle. That's the angle you're talking about.

KEIM: An interesting thing about those receivers and collectors, they gave them African names. For instance, Ubangi. They took different tribes in Africa where they use different distortion of the lips and made big mouths, and so they used those different types of nomenclature to name these collectors.

GOLDBERG: We won't ask who did that. [Laughter]

KEIM: I expect Herb York had something to do with it and Bob [Robert Lyster] Thornton.

LIVINGSTON: So anyway, the main reason that we got these high efficiencies that I was talking about was because of this plus or minus 15 degrees of beam, or 13 degrees of beam that could be focused due to the shims. So it all tied together.

GOLDBERG: Right. But a lot of it just, when it came out, didn't get ionized at all; it just came out as UCl_4, presumably.

GOOGIN: Most of it got ionized one way or another, but a lot of it was in side beams and complex ionized species that splattered all over the inside of the unit. The beam that was actually collected was the U+ beam, and the U++ and the UCl-pluses and all those other things were not collected; they ended up hitting baffles and the sides of the unit and what have you.

LIVINGSTON: But the secret was that adding all those other ions up, which were every possible ion was made and charge they were made in the ion source, but up to 25 percent of it got to be U+ and got transmitted safely and got retained by the collector box.

KEIM: While we're talking about retention, those Uranium-235 ions, positively charged atoms, went into that pocket with lots of energy, and there was a certain amount of bouncing back. So at one time, we worked on a grill in front of those entrances to slow down, decelerate the ions. Well, it worked fine. It increased the retention up to a point, but the high energy ions hitting those carbon strips of the grill soon burned them through, and so we had that benefit only for a short time.

But I remember one night, late at night, General [Leslie R.] Groves came into the pilot plant, and we were running one of those experiments. He asked everyone in the pilot plant how successful this is. "Would you use it in the next Beta production building?"

And we said, "No, it's not ready."

He said, "Well, I have to make a decision by 8:00 o'clock tomorrow morning." And the next morning, he decided to use the tried and true collectors which had been used in the previous Beta collection buildings.

GOLDBERG: Did he know you?

KEIM: Yes. Yes, he and E.O. Lawrence came around all hours of the day and night. We were used to seeing them come. We felt we were pretty well acquainted with them. I could tell you another one on E.O. Lawrence.

GOLDBERG: Please do.

02:04:55:00

KEIM: We were talking about the Beta production, the sources. We were running one night, and very contentedly receiving Uranium-235 into the collector pockets, and not a lot of sparking, not a lot of interruption. E.O. Lawrence came in, and he said, "Is this the best that will do?" And he sat down at the controls. He started adjusting the voltages and adjusting the temperature of the oven to get more uranium out. He increased that production tremendously, but at the same time, a lot of fireworks were going on inside the vacuum chamber. Things settled down, and he got up and he said, "See? We want to get all we can out of this production, and this is the way you do it." And he walked out of the building. In about five minutes, everything literally blew up inside the tank. The insulators were destroyed. Everything went bad, but he had proved a point. We were taking it easy on ourselves, because we knew if we increased that too much, the experiment would break down and we'd have to pull out the unit and put in new, and that was rather monotonous and tedious. So we kept them running as long as we could. But his idea--and he was right--is to continue to try to get the maximum production. So we changed our whole attitude after that.

GOOGIN: The units were expendable, just part of the chemistry of the process. Just plug 'em in. [Laughter]

KEIM: They were expendable.

GOLDBERG: You were talking about the fact that you put those grids on because they bounce up. There was the other problem, too, that you dealt with.

GOLDBERG: Initially, when the uranium ions hit the pocket, they got imbedded in the material of the pocket.

LARSON: Now, it was in the Alpha process. Here you notice those pockets are built of carbon.

GOLDBERG: Right.

LARSON: But in the Alpha process, they were built of stainless steel. In the so-called footscrapers that you have, where. . .

02:07:13:00

GOLDBERG: Let's stop a minute here and sort out what an Alpha and a Beta are.

LARSON: I think that's important. We did go rather fast there. Because in order to be successful, the product had to be up eighty-five to ninety percent U_{235}. This could not be reached in just one process, because the first time you did this, you got between U_{235} content of twelve to fifteen percent. Then you had to take the output of--we called that the Alpha process. You had to take the output of that, which was a small percentage of that amount of uranium. You take the output of that and put it into another like but smaller unit called the Beta unit, which we have right here, and that brought it up to 85 to 90 percent, which was satisfactory insofar as a weapon is concerned. So there are two steps there. The sources are pretty much the same, I believe, but the collectors were quite different. They were quite different.

GOLDBERG: Let me put this photograph up. I'll show it to you, and you'll recognize it. It's one of Ed Wescott's photographs.

GOOGIN: That's one of the round and round tracks. We had square ones, too, and rectangular ones.

GOLDBERG: Those came later, the rectangular ones.

GOOGIN: Right.

02:08:49:00 [Close-up of photo of an Alpha track; Goldberg indicates magnetic fields]

GOLDBERG: So we have here a track. It's called an Alpha track. The magnetic fields are perpendicular to. . .

LIVINGSTON: The magnetic fields are horizontal to the ground.

GOLDBERG: Yes, parallel to the ground.

LIVINGSTON: Pole tips are vertical.

KEIM: Parallel to the ground. Yes.

LIVINGSTON: And a magnet had never been built that way before, I don't think. I know years later we used some of these magnets to make cyclotrons, and so we, naturally, made vertical cyclotrons, and nobody had ever done that before. It works just as well.

KEIM: This combination began being called a calutron. How many calutrons were in this racetrack?

BANIC: Ninety-six.

KEIM: Ninety-six.

GOLDBERG: So there were ninety-six in this one. In the end, how many were there altogether?

LIVINGSTON: There were five buildings.

GOLDBERG: Five buildings.

LIVINGSTON: Of Alpha. And four buildings of Beta. There were seventy-two per building in the Beta units, and ninety-six times two in the Alpha units.

02:09:56:00

GOLDBERG: Why was it built in an oval? You could have built it--you didn't have to do it in oval.

LIVINGSTON: Well, that's right. And later on, they found out that that oval made quite a bit of problems, so that the final Alpha version, the Alpha Four and Alpha Five, were built in rectangular.

LARSON: We still call them racetracks although we no longer have the oval.

BANIC: This is an unusual situation. This is what we called Alpha One. It was a cold source, completely different configuration than what we're talking about here. The source and all--we looked at the sketch and, as Dr. Livingston pointed out, we accelerated at +35,000 volts, extracted a focus of adjustable -20,000. The sources were all at ground potential on these two tracks. I think there were about five hundred of these and four hundred of what we called the Alpha Two, and I know since I've been involved mostly in Beta, there's 288 Betas. [Laughter] But the source was at ground potential, and the liner and the receiver collector ran at the high potential.

GOLDBERG: The liner was. . .

BANIC: The liner that you showed on that first photograph that you referred to, the door or the D.

GOLDBERG: This here.

BANIC: That's right. And that operated at -35 or 49 kilovolts, and the receivers were actually inside that liner, because they had to run at high voltage also. It's just a reversal. This presented a lot of problems with designing building insulators and being able to maintain voltages inside of vacuums. The system there was that on a cold source, you started with your -40KV, and then as you accelerated and extracted, you had to keep adding on to that. By going to the Beta process or the Alpha Two process, you could start at a ground potential and accelerate at a +40, and focus and extract at a -40. So you see, you're not adding stair-stepping voltages, so to speak; you can work on either side of a ground potential. That was the Alpha Two process, of which there were four hundred-and-some units, and that is the same process that the Beta is, what we refer to as the hot source.

LIVINGSTON: On the design of the magnets that you were sort of asking about, there was kind of a naive assumption that you would save a lot of iron if you had no return yoke. The magnetic path has to be complete around the thing, and so their idea was we just fold it around, and then there's no return yoke anywhere. In the Beta units, they did have a great big chunk of iron at each end to return the magnetic path, and they found out from working with the Beta that that was really a pretty good way to do it, and it got rid of all this crazy angles that you had in these end tanks.

GOLDBERG: You mean as you went around the curve.

LIVINGSTON: But there was more iron required for that, so that was really the offsetting issue.

02:12:53:00

GOLDBERG: Let's get back to this question of the pockets and the Beta. The Alpha pockets were not carbon; they were stainless steel.

LARSON: Stainless steel, with stainless steel footscrapers, so to speak. They were at an angle so that when the beam hit them, they wouldn't bounce right back out, but would bounce at an angle and be collected on these. So that was quite different from these, which were all made of carbon, and you could recover the material by just burning the carbon.

02:13:32:00 [Close-up of actual alpha pocket at end of table near Larson]

GOLDBERG: So you just burned the carbon away, and you'd be left with uranium.

LARSON: Yes, that's right. But in the Alpha stage, the assumption was that when this uranium hit the so-called footscrapers in the U_{235} pocket, which would give you about 15 percent U_{235}, very valuable material, it would all be there in this pocket. The receivers were quite a bit bigger, almost twice as big as these.

GOLDBERG: Why don't you trade receivers there. You've got the source next to you. You want to talk about the receiver, right?

LARSON: Yes.

GOOGIN: We'll just bring the little receiver over here.

02:14:30:00 [Googin hands Larson the cross-section of the Beta receiver]

LARSON: This is the so-called Beta receiver. But the principle is the same. The U_{235} comes over here and it's separated from the U_{238}, and you don't care about the U_{238}. You let that go wherever it wants to.

GOOGIN: They bounce to the back of them.

LARSON: But the U_{235} comes in here and all these angles, as we've pointed out before, are correct, so that the U_{235} stays right inside here and is burned off.

In the Alpha, the receiver is roughly--it's the same principle, only the pockets were made, instead of being made of carbon, were made of stainless steel. The angles were furnished by what we called footscrapers. So when we did this experiment first out at Berkeley, we just put a stainless steel piece in the beam at a collector spot, and then I can remember bringing that back over into the laboratory. Dr. [Martin David] Kamen, the discoverer of Carbon 14, he was doing this experiment, and he showed this slight coating of the uranium isotopes on the stainless steel piece, and he said, "Now all we need to do is just take nitric acid," and it came out and there was a nice yellow solution there. The problem was that by the time we got into production, the energies were somewhat higher, and really a substantial portion buried itself into the stainless steel and made it sort of a uranium stainless alloy. [Laughter] Nitric acid just would not touch that, so when we got into production, I think it should be mentioned that these pockets are monitored with meters, so you can calculate by the amount of--it's really almost a kilometer--you can calculate how much material ought to be in there from the electrical readings. Then when we came to separate it out, we found out that less than half of it got dissolved out, and the rest was buried in the stainless.

So the question came up then, how to get this out. In my thesis work, I happened to be doing some work on electrochemistry, so I was quite familiar with even such simple things as electroplating, so that we decided to coat the stainless steel insides and the footscrapers, electroplate a layer of copper.

GOLDBERG: When you say "we," who decided that?

LARSON: Well, we were doing some experiments on this, and I had a very fine electrical engineer working with me, and there were a few others. It was a cooperative effort, everybody coming back with a distressing point that we were getting only half the material out. So we came up--I think I came up originally with the idea of copper plating this. And then nitric acid, it would bury in the copper. Nitric acid dissolves copper very easily and leaves the stainless untouched. So we then decided to copper plate all of the Alpha receivers. And so I told Lawrence and Lawrence said, "How long will it take?"

I said, "Oh, we ought to be, if we put high enough priority, we ought to be able to move the equipment and the electroplating equipment in probably a couple of weeks."

And Lawrence said, "I want these things being plated tomorrow!" And sure enough, we worked twenty-four hours a day, and we didn't have the proper types of vessels, so we took the so-called Alverine sinks that we had, very resistant, and made electroplating equipment. It just happened that the electrical engineer we had was very familiar with electroplating. He knew all about it. So we were able to start almost the next day with that, and then we were able to get essentially the same amount back that the meter said we ought to--not quite.

GOLDBERG: When was that?

LARSON: Oh, it was early in 1944, as I remember it. In January, or maybe even in '43.

GOLDBERG: You were working on it for about a year?

02:19:43:00

LARSON: No. When did the first track start up? It was in November?

LIVINGSTON: November 11th.

LARSON: So it must have been about November of '43, so it was almost immediately this problem came about. Then, of course, this seemed like a very good solution, because we then got the Uranium-235 into solution, unfortunately mixed up with a lot of copper. Now that changed the chemistry a lot, but in our chemistry group, we had done work on extraction, and so instead of trying to get it out by the usual precipitation methods, we used ether extraction at first, and later on found much better extracting agents. But when you first used ether, it did the job fine, but we had to stand by with fire extinguishers [Laughter] to put the fires out as they came. But the problem did clarify itself.

GOOGIN: It was very nice with the copper, because all you had to do was evaporate to a concentrated solution of copper nitrate, and that made the extraction go much better.

LARSON: It just happened that as luck would have it, the addition of the copper was a benefit.

GOOGIN: Right. [Laughter]

LARSON: It made the extraction go better. So something worked the right way.

GOLDBERG: Both of you started in Berkeley, so you really knew what was going on right from the beginning. You actually travelled back and forth between Oak Ridge and Berkeley?

02:21:37:00

LIVINGSTON: In my case, I was hired on to Tennessee Eastman [Corporation] on exactly forty-four years ago today, March 4, 1943. So I became a Tennessee Eastman employee and continued to work pretty much on the same kinds of things I'd been doing at

Berkeley. But by March 4, the designs were very far along, and what was needed then was to have a lot of technical supervision over the production lines of the sources, which were Westinghouse [Electric and Manufacturing Company] in Pittsburgh, and I spent a couple of months up in Boston, from May and June of '43, talking with Stone & Webster [Engineering Corporation], who was designing the plant, and trying to help them figure out the vacuum pumping problems of a giant plant that nobody had ever built before. Then I came to Oak Ridge in the summer of '43.

LARSON: It was right about the same time, maybe a couple of weeks later, that I also joined Tennessee Eastman.

GOLDBERG: You came from California, too?

LARSON: Well, actually, I stayed right there at Berkeley, same as Bob, and continued to work until July of '43, when I came to Oak Ridge. I don't know when you came, but you were going back and forth.

LIVINGSTON: By the way, Tennessee Eastman came into the project--I don't know exactly when, but probably around the first of 1943, but they started sending people back to Berkeley in, I think, January or February of '43. We got hoards of really highly qualified people who were, I think, big shots, pretty much technical big shots in either Tennessee Eastman or Eastman Kodak [Company]. We had these little bitty prototype units out there, and we were supposed to take these people on as crew members and show them how they worked and what sort of stuff they were getting into.

GOLDBERG: So that they would understand?

LIVINGSTON: Yes.

GOLDBERG: Chris, you started here.

02:24:01:00

KEIM: I heard about Oak Ridge on Christmas Day 1943. I was living in Pittsburgh at that time, and the next day I came to Oak Ridge and was given a tour through Y-12. I had an idea of what was going on, but it didn't take me long to decide in my own mind that it was atomic energy. Then I came back with Tennessee Eastman in February 1944, rather a latecomer. And I went into the pilot plant. I stayed in the pilot plant for about fifteen years. Every minute of it was exciting, because, of course, during the war years, we were concerned with process improvement of the equipment used. Then George will remember we were talking about the path of these ions from the source to the collector. We noticed on the inside of the stainless steel liner a lot of extraneous paths, semi-circular. We deduced those might be chlorine, they might be copper, they might be iron. So one night, late at night, when nobody knew about it, we adjusted the magnetic fields, we put in some copper charge material, and we collected the isotopes of copper. That started a stable isotopes program.

When the Atomic Energy Commission came into authority, Bob [Robert Fox] Bacher came by one day, and he said, "How long will it take you to go through the periodic chart?" Fifty elements, about 225 isotopes.

Very quickly, we estimated we could do one a month. We said, "About twelve years, we can go through."

He said, "Do it! And how much are you spending each year?"

We said, "About $2 million."

He said, "That's not bad. If anybody ever tries to stop this program, wherever I am, you let us know, and we'll come to bat for you." The program continues to this day, and that's the program George is involved in.

GOLDBERG: So you're still using the same tracks, is that right, the Beta?

02:26:29:00

BANIC: In March of '44, we talked to some people. I was with General Electric [Company] in Schnectady, New York, at the time. They needed people in Oak Ridge, Tennessee, who had some equipment with experience in high voltage power supply. I'd never been south of Pittsburgh, Pennsylvania, so I said, "Well, I believe I'll go down south and see what's going on down there."

I talked to a friend of mine who was from North Dakota, who had gone to school at Ames, and he said, "Well, let's go down." Because this was just supposed to be a temporary thing. So we went.

So I went into the pilot plant in the first production building at Oak Ridge, and that's the only place I worked until we shut it down in 1975. Well, in the meantime, the only building which is left is one of the Beta buildings on the Beta tracks that had been used by Dr. Livingston's group, the electronuclear. They had done research. Of course, all the time that production was under way, our process improvement group was trying to do things--the fracturing of the high voltage bushings, the insulators was quite a problem. So we had a group that was working on that. Of course, we tried to build the equipment for the experiments, to help these people as much as possible.

The demand for the isotopes, I should say in 1946, was the first official Copper 63-65 separation, and there are samples around as people have retired. Leon--we gave him a little plaque with a sample, which we thought was nice to give people when they retired, a sample of the first stable isotope that had been separated. It was in the Oak Ridge National Laboratory calutrons.

But as I mentioned the program grew from there, and we needed more equipment. The people who said, "Well, we'll give you a little assistance," so we thought about taking the Alpha building. The pilot plant I'm talking about is two Beta units, two Alpha units. So we thought about taking the Alpha unit and just laying it on its side and adding two more separators. It was a little difficult job to turn a 464-ton magnet on its side, but we were prepared to do it. I like to never have got it stopped, because in the meantime, why, Dr. Livingston's electronuclear group had moved to their cyclotron 6000 area situation. Fusion energy, which was Sherwood in 1958, which developed. . .

GOLDBERG: Project Sherwood.

BANIC: Project Sherwood, which developed into thermonuclear and now fusion energy, they were in the building also. They expanded to the point where they had their own building. So that meant the Beta three building was available, so instead of expanding the pilot plant, why, in '58, we had the authority to expand into 924-3, which is the Beta building. So we kept it. We got it into operation--strictly stable isotopes now--and continued our pilot plant in 9731.

Well, in 1975, because of budgetary limitations--the problem seems to bug everyone--we had to give up our pilot plant and concentrated all our efforts in the Beta building with the 72 separator. We have a wealth of separating equipment. We don't run them all, never have, but we have run as many as thirty. We converted the building.

GOLDBERG: The same equipment that you built during the war.

BANIC: Same equipment. I'm pointing out a little comment here. When you walk in, you look at the building, and the tracks look the same. Well, you don't change a four-thousand-ton magnet very much, the appearance of it, anyway. So Dr. Nier, we mentioned from the University of Minnesota, came and he looked at this equipment twenty-eight years or so ago, but it had already been in operation twenty years. Somebody commented to him that it still looks the same. He says, "Well, maybe sometimes you do something right the first time." [Laughter]

But the equipment, what you're looking at here is uranium equipment.

Our stable isotope program, which maybe if we have time, we can talk some about, the equipment doesn't look like this. Everything is graphite. We're dealing with much smaller quantities, as most everyone knows. As the gaseous diffusion plant came on line, they were able to offer fifteen percent equipment, so that meant Alpha was shut down, and as they went to ninety percent, Beta was shut down.

GOLDBERG: We're going to talk about gaseous diffusion in a moment.

BANIC: At other times, yes.

02:30:53:00

LARSON: If I could digress here a moment, I think that Dr. Googin has had some very interesting experiences over on the Alpha chemistry side, and I think before we leave the Alpha-uranium separation, if I can just give you a couple minutes of introduction here to this. At Berkeley we worked on preparing uranium tetrachloride, because we knew that that was what essentially near established that that was the best way to go, and some other of the. . .

GOLDBERG: Let's be clear about that. It's the best way to go because you can heat it and it becomes a gas without being a liquid.

LARSON: That's right. Also, the rate of flow, you need to have the flow of the uranium vapor at about the right rate. You could use, in theory, uranium hexafluoride, but as Nier found out, it gets away from you. And it turns out that as you heat the uranium tetrachloride by adjusting the heaters, you can adjust the flow and so forth. But that meant we had to prepare a lot of uranium tetrachloride, and we did the original experiments at Berkeley. There were two general methods. One was to take uranium trioxide and heat it under pressure with carbon tetrachloride, which made uranium pantachloride plus a lot of phosgene. Under pressure. The other method was a so-called vapor phase method, whereby you would put the uranium along a long tube, and then first you would reduce it with hydrogen to uranium dioxide and then lead carbon tetrachloride over it. You'd make uranium tetrachloride. So it turns out for mass production for Alpha, the high pressure method was the best one, and that's what John has had a lot of experience with. [Laughter]

GOLDBERG: We're going to turn to that.

LARSON: But as I say, I didn't want to get into the Beta. There are some interesting things that John has to say about the Alpha.

GOLDBERG: We certainly do want to talk about this chemistry. When did you come, John?

02:33:45:00

GOOGIN: Came in May of 1944. I was one of the Johnny-come-latelys in the group here. It turned out, however, that the original articles in The New York Times in 1939 impressed me that this might be a project that had a future, so I had adjusted my undergraduate curriculum to specialize in things like advanced inorganic chemistry and modern physics and that sort of thing. In about February of 1944, I asked my major professor if there was any interesting work in the country that I might get involved in.

GOLDBERG: Who was that?

GOOGIN: Dr. Walter Lawrence. Not the Lawrence--another Lawrence, at Bates College. He was the head of the department of chemistry in Lewiston, Maine. Bates College is a small liberal arts school. But he said there were a couple of projects in the country; there was one in Columbia, and there was one in Washington, and there was one in Tennessee. Like George, I said, "I've been living in the state of Maine for a long time. I think I'll go south." So I put an application in to Eastman Kodak in Rochester, and they came back and said they had a job in Tennessee that I could have. I waited around home for a month while the Army decided that I was 4F, and therefore not a prime candidate for the infantry.

02:35:16:00 [Begin U-Matic Tape 2 of 4]

So I came to Tennessee. They introduced me to this plant, and it turned out that in just a quick tour of the plant, they had been doing everything I had thought they ought to be doing, really. So I was really prepared, by pure accident, absolute accident, to do the job that they had assigned me. That might be because somebody was knowledgeable, but it was no knowledge on my part.

GOLDBERG: So you came directly from undergraduate.

GOOGIN: Undergraduate. I got my degree in chemistry and physics, and I came directly to Oak Ridge as an undergraduate, just a graduate. I signed in at the princely salary of ninety-five cents an hour. [Laughter] In Oak Ridge, Tennessee, in 1944.

GOLDBERG: You both were on the ground floor of this, and you were designing it, so you really knew it all.

GOOGIN: Right.

GOLDBERG: You didn't.

GOOGIN: Except that by accident, I had already been studying it.

GOLDBERG: Could you pretty much figure out like Chris?

GOOGIN: We had a little tour of the plant, and everything was in code. They brought me back to the office and asked me what I thought they were doing, so I said, "Well, you're processing a heavy metal, it's probably uranium, and you should be making an atomic bomb." [Laughter] So they put me to work in the Production Division, Alpha chemical production, as sort of the process chemist assigned to production rather than to development. One of those little arguments you always have is who improves the process. [Laughter] So I was in a little laboratory. They put me to work with a chemist they already had there, and we worked together on improving the Alpha chemical process. Of course, as Clarence has pointed out, and as had been pointed out previously, the units didn't really recover a whole lot of the uranium that you fed to them, so you had a cycle that went round and round and round, where to start with, ninety percent of what you sent out as product through the track buildings came back to you as waste to be reprocessed back into tetrachloride.

GOLDBERG: When you read the literature, this is the stuff that they call "gunk."

02:37:37:00

GOOGIN: Now, the liquid that came off these machines was what we called gunk. If you've looked at the buildings, they have these wash lines where they dismantle the units and clean up the excess uranium tetrachloride and other compounds that are scattered all over the inside, and the residue in the charge bottle, and it all ends up as a nitric acid-hydrochloric acid mixture, which is rather mean. That is known as gunk, and it's got corrosion from copper and stainless steel and a tad of something else, tungsten, a little tantalum, and anything else you can think of. All they want you to do is very expeditiously convert it all back to chemically pure uranium tetrachloride that doesn't outgas too much and run it right back in the charge bottles, back into the units. So that a big portion of the expense and problem in the calutron process, of course, is the chemistry associated with it. It was my job initially to try and keep increasing production in the small plant and helping design small additions or large additions to keep up with production as they kept adding track buildings. [Laughter] They'd say, "How about getting some more feed?" Our problem then was to keep redoing the chemistry to increase production and keep up purity and install new equipment. We ran production up by basically an order of magnitude in about a year from where we started when I first came in.

GOLDBERG: The problem here is this, that the ore that you're dealing with, when it comes in. . .

02:39:35:00

GOOGIN: We brought in fairly pure material. There was another set of industries established, of course, to get the uranium ore out of the ground and concentrate it and to make relatively pure uranium oxide, UO_3. Mallinckrodt Chemistry in St. Louis did a lot of this.

GOLDBERG: So it came in as UO_3.

GOOGIN: Came in as UO_3.

LARSON: And it was really very pure, primarily because in the process that Mallinckrodt used, they dissolved it in nitric acid.

GOOGIN: Ether extraction.

LARSON: Then they did ether extraction, and when it came out, it was 99.9 percent pure. Very pure material. It was contaminated by--the nitrates weren't quite all, but it was a small problem.

GOLDBERG: Why don't you have it come in as UCl_4, as uranium tetrachloride?

GOOGIN: See, the problem was, of course, to get the raw material, but then we had to recycle inside the plant so many times. If you'd shipped all the gunk one thousand miles to get it processed and get the oxide back, you'd have. . .

GOLDBERG: No. I asked a different question. Why didn't you have--who was it that. . .

KEIM: Mallinckrodt.

GOLDBERG: Mallinckrodt. Why didn't you have Mallinckrodt send it in as uranium tetrachloride?

GOOGIN: Like I was saying, we had to have a plant locally to make the tetrachloride, so it was an argument of how you'd like to ship it. Uranium tetrachloride is a very hydroscopic material that you have to send in fairly interesting tight packages in order to keep it as a tetrachloride. Mallinckrodt did send some material to Harshaw, and Harshaw did make some tetrachloride and we did send some in. But basically, on the site, we had to make tetrachloride, so there's not much point in buying it, because we had to make ten times as much as we were buying. So a small increment of our capacity was the only argument that was involved.

To satisfy the needs of these folks, we had to take this gunk solution, and we started out with very basic precipitation chemistry based on--any chemist in the crowd interested in the complexing of uranium with a carbonate solution, ammonium carbonate--to keep it in the uranium in solution and precipitate most everything else except those things that complex with ammonia. And then we destroyed the carbonate in solution with nitric acid and reprecipitated the uranium in uranium salts, in this case, ammonium diuranate, which doesn't really exist, but it has a name anyway, and we fielded that off and washed it. [Laughter] Then we dissolved that back up in nitric acid and reprecipitated the uranium with hydrogen peroxide to manufacture uranium peroxide, which gave us really a very elegant three-step purification cycle, so the oxide we made was really chemically pure.

GOLDBERG: So uranium peroxide is UO_4, right?

GOOGIN: It's UO_4 precipitate, which we calcined it back to UO_3, and it makes a nice high-surface area oxide that reacts very well with carbon tet.

LARSON: In fact, if you make it fine enough, it'll even do it at atmospheric pressure slowly.

GOOGIN: Slowly. [Laughter]

GOLDBERG: Sounds like you guys should have been cooks, not. . .

GOOGIN: Well, of course! Chemistry is just like cooking. The housewife doesn't know how much chemistry she is running; otherwise, she'd be scared! [Laughter]

LARSON: As you go through the other projects--you didn't get into it very much yesterday, I guess, because you didn't have many of Glen [Theodore] Seaborg's people there--but they had their problems also, and they had to choose between several things. We went through the same thing. We had to choose between several different approaches.

GOOGIN: Because this is where we started in the chemistry. After we chlorinated in these big pressure vessels. . .

GOLDBERG: So you chlorinated under pressure.

GOOGIN: Under pressure from carbon tet. Then that would vent the phosgene off through big chemical scrubbers.

02:43:57:00

GOLDBERG: Let's stop there. Before, you said that nitric acid and sulfuric acid together was mean stuff.

GOOGIN: Hydrochloric acid.

GOLDBERG: Hydrochloric was mean stuff.

GOOGIN: It's known as dilute aqua regia.

LARSON: Dissolved gold.

GOLDBERG: Dissolved gold. If that's mean stuff, what do you call phosgene?

GOOGIN: Oh, phosgene is a fairly innocuous liquid. It vaporizes pretty much, but reacts nicely with caustic soda so you can get away with it.

GOLDBERG: But it's very dangerous material.

02:44:25:00

GOOGIN: Oh, it's very dangerous. It's known as war gas. And what the folks used to do is they used to send me little containers of carbon tet saturated with phosgene to analyze in Erlenmeyer flasks, and it would sit on my desk and warm up, then the stopper would go "blip...blip...blip," and out would come a little bubble of phosgene. Since I was working in a laboratory that was halfway between the ammonium carbonate step and the phosgene scrubber step, there was sort of an interface here when we had a leak, where there would be a cloud of reaction product in the air, and I had to wear a gas mask a fair percentage of the time. All of our people that operated in the high pressure phosgene-containing area had to have immediate access to gas masks. We had at least one fatality where a person didn't understand that the solution of carbon tet might be saturated with phosgene, and broke a flange on the line to do some maintenance work and got a face full of phosgene and carbon tetrachloride. So that it was very dangerous, really.

Now, that pentachloride that came out of those carbon tet reactions under pressure was then decomposed and resublimed to get a purer product in the sublimation units, where we put in the impure product in a vacuum system, heated it up in trays, and collected pure tetrachloride on condensers. And the condensers would then be stripped and the material be put into charge bottles to go back to the track.

So when I got there, that's basically the chemistry that was going on. Of course, my job was to improve its performance. We eventually eliminated most of the steps and, with Clarence's group's help, we went to what was known as the direct peroxide precipitation. Hydrogen peroxide is an interesting reagent that enjoys decomposing itself while you're using it, and the decomposition is catalyzed by the same corrosion products as we get off those units.

LARSON: Iron particularly.

GOOGIN: Iron and copper, especially, in combination. Right. So here we had an unstable reagent we were trying to do a precipitation with and get uranium directly from this gunk. We succeeded in doing that by some fairly elegant combinations of temperature control, composition control, rates of addition. . .

LARSON: The pH control.

GOOGIN: The pH control. So that we could, in fact, bypass most of the steps in the process and get a lot more production out of a given set of equipment.

KEIM: John, you keep making reference to gunk. We have to differentiate between "gunk" and "crud." [Laughter]

GOLDBERG: Go ahead.

KEIM: When we opened up the electromagnetic separators, the interior of the vacuum chamber and the liner was just full of crud. [Laughter]

GOLDBERG: This would be the uranium chloride, the uranium tetrachloride and impurities that hadn't made it either into one pocket or the other pocket.

LARSON: It would be some chloride, some oxides, iron, some oxides of practically every element in the periodic table.

KEIM: John would take the crud and put it in the solution and call it gunk. [Laughter]

02:48:10:00

LARSON: But I think that if you read the histories of the Manhattan Project, you won't find, with the exception of [Stephane] Groueff's book, he goes into it in fair detail, but you won't find much reference to the chemistry of this. So the question comes up, "Why did we run into these problems which weren't solved before?" In the first place, there was less than ten months, you know, before you had to freeze. You had to develop the process.

GOLDBERG: By "freeze," you mean. . .

GOOGIN: Engineering freeze.

LARSON: Engineering freeze.

GOLDBERG: So you had to fix the design.

LARSON: Fix the design.

GOOGIN: Had to start somewhere.

LARSON: Very rapidly. So there were quite a large number of unknowns there, and as usual among chemists, there were quite a number of different opinions as to how to go about it. If you read Glenn Seaborg's books on that, you'll find out there was some controversies over on the plutonium project also. But we had a controversy there, and there was another process that was developed primarily for the Beta recycle, but it was based on an oxalate separation, which was a very fine separation providing you had absolutely pure uranium. When you came to get some of these gunk solutions, the process really fell all apart, so to speak.

I found that uranium and the rare earths--of course, that includes plutonium and neptunium and so forth--have a very interesting--they are almost the only element in the periodic table that's precipitated by hydrogen peroxide. Now, that was in the literature but not very well noticed. And when the people first tried it out, as Dr. Googin mentioned, the first thing, you put the hydrogen peroxide in or the precipitation would come down beautifully, but you let it set for five minutes and it would start to decompose and then foam over the beaker, and you'd lose it. So it was discarded.

Then I remembered from some work I'd done in isolating natural products in organic chemistry, which you had to do them in cold rooms, you had to keep it cold because of the decomposition of organic. It had never been done in inorganic chemistry before, but I thought, "Well, let's try cooling it down." And that solved the problem, you might say, almost overnight. The direct precipitation, if you kept it cooled, and we had jacketed vessels, and you used pH control, so we had to change the process of the Beta thing almost overnight. There was one discouraging part. It did not filter very well, the peroxide, but it did centrifuge quite well. So we needed centrifuges in a hurry to do this particular operation in the Beta section. The Sharples Company [Sharples Corporation], who made most of the centrifuges, they were doing a lot of work for the Navy, because they used centrifuges. We took the whole output of the Sharples Company, flown in on bombers to Oak Ridge or Knoxville, and we were in operation very quickly. That, of course, because in the Alpha process it was not nearly as critical, but in the Beta process with this very valuable material, you know, we nominally assigned a value of one thousand dollars a gram.

GOLDBERG: Because it was already partially. . .

LARSON: Because it had been through the Alpha process, through that very complex. . .

GOLDBERG: Partially enriched.

LARSON: And also, you wanted to get it back into the process fast. So you couldn't go through a very complex chemical system, but this direct peroxide precipitation worked beautifully.

02:52:46:00

GOOGIN: An interesting little sidelight to that, because Clarence was having trouble convincing some of his colleagues that this would work. So one day he came up to my laboratory, because he knew I was working on peroxide for the Alpha chemistry, and he says, "Googin, how about you just running a few experiments cold to see how well we can do?" [Laughter]

LARSON: It worked out beautifully the first time.

GOOGIN: In the Alpha cycle, we weren't trying to use the centrifuges. We were using the centrifuges already, but we would prefer to use ordinary rotary filters.

KEIM: The importance of chemistry was evident all the time. George and I, involved in just the mechanics of separating the isotope, we just took it for granted.

GOOGIN: Right.

KEIM: However, in the stable isotopes, when it came to separating the rare earths, we had these ten or twelve rare earth elements, and we wanted to separate their isotopes. We had a chemist in charge, Boyd Weaver, who worked out the process of separating those rare earths in groups so that we could successfully separate their isotopes. And I often think that is something we were inclined to take for granted and maybe overlook, but it was a tremendous achievement.

GOOGIN: These physicists took a lot for granted, actually. [Laughter]

KEIM: It was a tremendous achievement, and he deserves a lot of credit for having done that.

LARSON: Well, I should tell you a story about E.O. Lawrence in this connection. When I first came on the project, there were only a comparatively few chemists, because Lawrence and the other physicists had the idea that, "After all, we're only going to need seventy-five pounds of this, and you can do all this in beakers if necessary."

GOLDBERG: You were only going to need seventy-five pounds? Oh, to make. . .

LARSON: To make a bomb. And so, you know, "That's not much. You can do it in beakers if necessary," you know. I mean, that was his general idea. As a matter of fact, since Lawrence, in his early days, was a very avid amateur radio operator, and I happened to be also. I had mentioned it and he used to give me some radioactive isotopes for the cyclotron occasionally, and so we had occasional--and so he said, "You know, we've got a few chemistry problems, but when you're finished with these, with your electronics knowledge, then you can go over on to something else." [Laughter] But as John well knows, we never quite got to the point of those chemistry problems.

GOOGIN: Of course, we had several cycles running in Oak Ridge. We had the Alpha cycle feed the big machines, we had the Alpha product chemistry to make the feed for the smaller machines, and we had some of the chemistry to take the finished product to make it into tetrafluoride, to ship it to Los Alamos to be made into metal.

GOLDBERG: And that's the form it was shipped in--uranium tetrafluoride?

GOOGIN: Tetrafluoride.

BANIC: Occasionally, they would come up with a compound and send it to us at the pilot plant just to run it through the calutron to see if it would work. So we did a lot of charge evaluation for them also.

GOOGIN: So we did actually turn out the green salt, so-called. And that's one of the problems with using hexafluoride as feed for a calutron. When you start trying to go from the hexavalent material all the way down to a metal beam, you do leave some intermediate compounds along the way, one of which is green salt that precipitates all over everything. So you do have a problem starting with a hexavalent uranium.

GOLDBERG: Let's take a break. We've gone now for a long time.

GOOGIN: We've only gotten warmed up. [Laughter]

GOLDBERG: Well, I want to make sure you stay warm.

KEIM: We have to stop and reload. [Laughter]

GOOGIN: We'll take off the mikes and throw them on the floor.
02:57:58:00 [Pause in Tape]

03:21:22:00 [Interview resumes]
GOLDBERG: Let's back up a little. We've gone through an awful lot in a very short time.

GOOGIN: Of course!

GOLDBERG: I want to put some scale on it--how big the project was, how fast it went up. In the break, we were talking about the length of time it took from the time that Lawrence first got the idea. I date this, looking at the Archives [National Archives], as being very late, being in late 1941, for the time that they were actually breaking ground to build the first Alpha unit--I guess it was an Alpha building that was built first--from the time that building was finished and you actually began operating in it.

LIVINGSTON: I think the site was selected in late 1942, and I don't know the exact date when they started construction of the Y-12 plant, but it was in early 1943 sometime. When I arrived in Oak Ridge, which was July of 1943, there was a vast amount of construction going on at Y-12, but there was nothing but an administrative building that you could use right then in July. So we divided our time between that administrative building and some space up in the townsite.

The building 9201-1, was the first building under construction, so it must have started in January or February, somewhere along there. They tried to turn it on in October, I believe it was, and there were a lot of shorts in the magnet, much to their dismay. All the bigshots were there, you know, for the turning on, and the thing was a complete flop because you couldn't get the magnetic coils to hold any voltage. So there was a frantic effort to clean up the oil system, which was full of water and junk and all kinds of things, but a magnificent effort was made on that during October and early November, and they got it all cleaned out, dried out, and ready to go. So it was mid-November--November 11th, I believe--when they actually turned it on and cranked on their first unit.

03:23:59:00

GOLDBERG: Let's stop there. What are we using the oil for? Why is there oil?

LIVINGSTON: The oil was to cool the magnets. The magnets were made of silver, you know, that had been borrowed from the Treasury Department.

GOLDBERG: Something we'll talk about, yes.

LIVINGSTON: Because there was just too much demand for copper and other things. I guess that's an interesting story. I don't really know the details of it. But there's a lot of power that goes into the magnets, and that has to be taken out through a heat exchanger using oil. You couldn't use water because water would short out the magnets. In fact, water was shorting out the magnets in that first test. They had to dry out the oil, dry out the pipes first.

GOLDBERG: Where did the water come from?

LIVINGSTON: Well, it's just high humidity I think, and condensation. There weren't any water leaks into the system, but there was a lot of water in there through water vapor. Then there was some amount of carelessness, because the welders who were doing the job had not really been properly briefed on how careful you have to be and how much cleanliness was required, so there were a lot of kind of scandalous things, like boots and gloves and all sorts of things which they found after they went back in. So it was necessary to clean all that out, get rid of the water vapor, and have the oil in clean condition. Once that happened, then the magnet worked fine.

GOLDBERG: How much power?

BANIC: The magnets themselves were 5000 amperes. This is the amount of current of 350 volts, which is a sizeable block of power, and that's just for one track.

GOLDBERG: Each track.

BANIC: Each track.

GOLDBERG: How many amperes--5000?

BANIC: Five thousand.

03:26:05:00

KEIM: Didn't they say that Oak Ridge used more power than Manhattan Island?

LIVINGSTON: "Five times the state of Ohio" and things like that.

GOOGIN: I don't quite remember, but the total Y-12 installed power was a few hundred megawatts only, just a few hundred.

LARSON: What was the generator size in that?

KEIM: Some were 5000, some were 8000 amperes at 350 volts.

GOOGIN: Our system, I think, was about 350 megawatts, some such number.

GOLDBERG: Did they take this out of the grid from TVA?

LIVINGSTON: Yes. That was one reason they picked this area, because there was ample power from the Norris Dam.

GOLDBERG: But there was also a huge power station here, was there not? Didn't they build a big power station?

LIVINGSTON: They built a power station for K-25.

GOLDBERG: For K-25.

KEIM: They built a coal-powered station at Kingston, and it was fed directly to gaseous diffusion.

BANIC: That was 1600 megawatts in five units.

GOOGIN: But we started K-25 by building a power plant for it.

LARSON: Actually, there was a power plant that was apparently ordered to go someplace, some municipal system in Illinois, and they just took that power plant and put it into K-25.

GOLDBERG: It got diverted from. . .

LARSON: It got diverted, but that was only about 350,000 or something like that. It wasn't very big as we speak today. It was a big one for that day. That was one of the biggest plants of the day, but today we go up to three or four times that.

GOLDBERG: You mentioned silver.

03:27:51:00

BANIC: I'll let you in on a little of that, since that was one of the things we got involved in, in the removal. As Dr. Livingston mentioned, copper was a very strategic war material. You didn't want to use that up in making windings. So we wound up with about half the Alpha buildings and two of the Beta buildings, one or two of the Beta buildings, using silver windings. There was something like 15,000 tons of the Treasury Department's silver. When they went to Washington to request the silver, as I understand it, the caretaker said, "Well, how much silver do you need?"

"Fifteen-thousand tons."

"Sir, we measure our silver in troy ounces." Needless to say, somebody pulled a few strings, and the silver was made available. It's a very safe depository. It's in a quarter-inch steel casing. Once they were fabricated and the coils sealed, it's as safe a place to store as you can. Then when the plant was shut down, why, all but about 2,000 tons was sent back to Philadelphia for resmelting and putting back in the Treasury Department's ingots. We kept the rest of them as spare parts for whatever we might happen to need.

Of course, every year, somebody in what was then AEC [Atomic Energy Commission] had to write a justification to request keeping the silver in Y-12. Well, this got to be old after a while, so they finally let the 2,000 tons go back, and then the only thing that was left was 67 tons, which were in Beta coils and the Alpha coils in 9731, the pilot plant. We kept those. Here again, every year somebody had to--I guess, Dr. Keim, you probably wrote a lot of these memos, too, to the Treasury Department, requesting that we continue this operation. There's not a safer place to keep it, really.

But the time came when it became necessary to return all the silver, so in 1970, the Beta process--Beta was no problem, because we had enough Beta spares that we'd saved when they dismantled some of the Beta protection buildings, so we were able to replace the silver coils that were Beta with copper coil. However, all the Alpha coils were gone. Well, the Alpha coils wouldn't have done us any good anyway, because the pilot plant, what we called our XAX, our Alpha process, they were specially designed coils. So we actually had to fabricate some coils for that, which we did. In 1970, we had the construction company come in, and they removed all the silver, sent it back to the Treasury Department, and we operated on copper after that.

There was a difference in the size of the coils. One thing that happened previous to this--and we still kid some about it--we needed another motor generator set. The two that were operating in 9731, the pilot plant, had been manufactured somewhere along 1903.

KEIM: They had been used in the sugar cane. . .

BANIC: Sugar cane mill or something like this, and they were available, so they were installed. We had a little difficulty with one of them. We couldn't quite reach the magnetic field we wanted in the XAX, so we found through the "used car" equipment circulars, we found one that was in the upper peninsula of Michigan. It was one that belonged to Henry Ford when they were using wood in their auto and truck bodies, and this is the lumber mill that he'd used for his automobile manufacturing. Of course, that had long been shut down, but Henry Ford kept it because it was his personal toy. He would go up there and play with this thing, up near the Michigan College of Mines, way in the upper peninsula, a good getaway.

The thing about it that was real interesting, when we went up there and saw that equipment, it was just like it was new. The end bells of rotating equipment were chrome plated, they had beautiful scroll work painted on the stationary parts, and it looked like something, I guess, that you would almost put in your museum after you decorated it real well. But, of course, Henry Ford had since died, and the heirs decided that it was time to dismantle the plant, that they weren't interested in it. So the generators were available.

KEIM: They kept a caretaker at those motor generators turning them over periodically so they wouldn't set, and they were most beautiful when we saw them.

BANIC: They were quite the machines.

03:32:29:00

GOLDBERG: You needed the motor generators because. . .

BANIC: I wanted to increase the capacity of the XAX machine.

GOLDBERG: Right. But you needed the motor generators initially because the current. . .

BANIC: They furnished the current to excite the magnet.

GOLDBERG: But it was a direct current.

LARSON: It had to be direct current.

BANIC: It had to be direct current, that's right, because it was a DC magnet.

GOLDBERG: So the motor was an alternating current.

BANIC: It was an alternating current motor and it had a DC generator that furnished the power to the system.

GOLDBERG: How big are those?

BANIC: These were relatively small compared to what we're talking about, the Alpha building. These are, say, 300-500 kilowatts. All we needed for that was, say, 1200 amperes, 250 volts, which is big for a lot of people but small for people that are in this kind of business. So when we brought this equipment--well, before we were able to get it, we requested Purchasing to buy these machines for us, they said, "The government and Carbide [Union Carbide Corporation] isn't in the process of buying junk equipment."

I said, "That's not junk equipment." I said, "Not only that, we need it. We can have it in less than a month. We know how to install it; it comes ready to go." Now, if we placed an order for a new machine back in the late forties or early fifties, I forget when it was, you had maybe a two year lead time before somebody could set up to build up your machine. I said, "This is something we need right now." So we went up and we inspected it, we checked it electrically and all, and it turned out it looked like a pretty good machine, and we brought it back. It served its purpose very well. In fact, this is the lead-in to the silver story. When we converted from the silver coils to the copper coils for the Beta process, the copper coils required a lot more current than the existing generator would put out, because they were a different winding configuration. So here we had the machine already installed in just no time at all. We cross-connected and we were back in operation, and the Treasury Department had every bit of their silver back now.

03:34:28:00

KEIM: George, I remember how those silver coils in the pilot plant were replaced. When A.J. Mettler knocked out the ends of the buildings and reached in with our crane and lifted them out and then lifted the new ones back in, how did you get that motor generator into the basement of that building?

BANIC: It was put in before the vacuum equipment was put in.

KEIM: No, I mean the Henry Ford.

BANIC: We didn't put it in the basement; we put it in the building next door, 9736.

KEIM: That's right.

BANIC: We had a special building, which was a fabrication-type building where some of these things were made and graphite pockets were made. So we installed it over there and bussed over to it. But the replacing the coils was, as mentioned, knocking out the end of a building. These coils weighed in excess of 40 tons, which to pick up and move around in the air was quite a task. So Mettler had a brand-new crane which had a 120-ton capacity, and they tried it out on our system replacing these coils.

KEIM: But you know, when they were doing that, it was interesting. They'd lift those coils out, those coils weighing 20 tons or more, they'd set them on a piece of plywood, and they used Ivory Soap flakes. Then they'd use a high lift, and they pushed them along the plywood, didn't have to lift them. I asked them, I said, "Why do you use Ivory Soap flakes?"

They said, "We've tried them all, and those are the only ones which work."

GOLDBERG: They used it as a lubricant.

BANIC: Used it as a lubricant to slide those coils out.

KEIM: They were a most clever group of operators.

GOLDBERG: None of you were involved in the silver coming, I take it.

LARSON: I think practically all of that was wound at Allis-Chalmers [Manufacturing Company], wasn't it?

GOLDBERG: So that the silver was taken. . .

LARSON: The silver was in there.

GOLDBERG: So the silver was taken from Fort Knox, extruded into wire, melted and. . .

BANIC: Into wire which is three inches wide and four-tenths of an inch thick. [Laughter]

GOLDBERG: So it's really a wire ribbon.

KEIM: Isn't the silver depository at West Point? I believe it is.

BANIC: Once it went to Philadelphia, why. . .

GOLDBERG: It didn't matter, because it all ended up at Oak Ridge, right?

KEIM: Rather interestingly, you were talking about the water in the oil. Real early in the project, some of these silver-wound coils began to short out, and the reason they shorted out, they had spacers between the windings made of masonite. And before the oil was cleaned up from water, the masonite would absorb water, and gradually it would char and char, and after a few months, we had almost pure carbon between the windings, and then they'd short out. I remember we drilled a hole in one of them, in XAX, the Alpha tank, to look in to see what was going on. We didn't have fiberglass optics then, either, but we did the best we could. We had a little hole about an inch in diameter, and all the time that hole was open, a guard stood there twenty-four hours a day. Why, I don't know.

LARSON: To keep you from stealing the silver. [Laughter] I think, as a matter of perspective, translating this tons into ounces, I think it was about 600 million ounces, roughly, which, in those days, a dollar an ounce, silver was really cheap, that was about six hundred million dollars. Now, when silver got up to ten dollars an ounce, that was six billion dollars of silver. It went higher than that. It was about $3 billion worth of silver at today's prices was in that.

03:38:32:00

GOLDBERG: Were any of you involved in returning it, in the actual return of it to the Treasury?

GOOGIN: Not directly.

BANIC: I watched them dismantle the coils and shear it in liquid strips. This is an interesting story, too, because we were very careful. We didn't cut it; we sheared it to keep the amount of waste to a minimum, stacked it on pallets with guards around, put it in trucks, and shipped it.

KEIM: No sawdust.

BANIC: No sawdust. [Laughter] However, when these pallets came back from the Treasury Department and the smelting program, where they put it back, there was splattered silver all over everywhere, so we were really disgusted. After we took great pains to make sure we returned it all, they weren't too careful how they cleaned it up at the other end. We have photographs of that.

LARSON: Yes. I remember when that took place. At that particular time, I think I was superintendent of Y-12, and we went through that. I was always afraid, you know, they were going to find a couple of million dollars missing or something, but no, it was all accounted for very well.

GOLDBERG: You were involved in that?

GOOGIN: Just peripherally, in helping them decide how we were going to do it in Y-12, to make sure we didn't--like the shearing operation, make sure we didn't wear too much away in the handling process. One of our biggest problems was because we felt that they had kept pretty good records, was to get it back into the same record base without indicating any serious loss.

BANIC: See, these coils, particularly the Beta side--I'm not too familiar with the details of the Alpha coils--the Beta coil, as I mentioned, the winding is a ribbon three inches wide, four-tenths of an inch thick and over 4,000 feet of it in one coil. You had almost a mile of that, so how do you get this out? So they devised a big turntable, kept the top off, and then they just fed the thing through a set of shears and just kept it going on automatic process, shearing it into little three-foot strips. Just handling that much in one piece, I can imagine the problem that Allis-Chalmers had winding the coils in the first place and not getting electrical faults.

03:40:45:00

GOLDBERG: Let's go back. That was quite a day, wasn't it, when those coils shorted out? Let's look at some of the details of that.

LIVINGSTON: Well, I've never seen so many perplexed big shots in my life as when that--everybody had very high expectations, you know, because they'd been working furiously for eight or so months to get everything ready, and everything was ready, they thought. So a lot of people came into town for that. Ernest Lawrence was the one that I had most of my interaction with, and he was the most crestfallen person I've about ever seen. He's not one given to being crestfallen; he's a buoyant optimist, normally. But the answer in those days was, "If something goes wrong, work like crazy and get it fixed." So they turned everything upside down to get the things fixed, and they had a few days, I think, or a day or two to diagnose it to find out really what was wrong, and they rather quickly found out. Then it was a massive cleanup effort.

GOLDBERG: Was it at Chalmers that this had happened, or was it just sitting around waiting?

LIVINGSTON: It was sitting around waiting. It's a massive piping system, if you look at the piping diagrams of the business--six-inch pipes running all over that racetrack.

GOLDBERG: With oil.

LIVINGSTON: Yes.

KEIM: And through that time of year, in this area it's about one hundred percent humidity.

GOOGIN: One-hundred percent humidity every night.

KEIM: Relative humidity.

LIVINGSTON: And this was in the summertime when they were doing it all, finishing the welding and so on, and the humidity is very high most of the time. If they had known enough, of course, they could have put the piping on a vacuum system for a while to dry it out before they put the oil in, but they simply--none of us, I grew up in California, and most of the other people that were the scientists had grown up in places or worked in places where we didn't have this furious humidity. In fact, you never see one hundred percent humidity in California except when it's raining, which is not that much. So that was the major unexpected thing that happened that was bad, was getting the oil going. The rest of the--well, there were lots of other detail problems, but I think all of them, as far as I know, had been anticipated, and had shown up on the pilot units and on the XAX unit and so on. I didn't actually participate in the startup of that building, because I was furiously working on Beta, trying to get it ready to go. I don't know. Did you?

LARSON: But the depression was really deep all over the whole plant, because it looked for a while there that this might be real delays, not a couple of weeks, but maybe months. Of course, as you pointed out, there were things like the shoes and the boots and the slag as well as the moisture, and it was a real mess there.

GOOGIN: Well, they actually found some of that sort of thing at K-25, too.

LARSON: Yes, that's right.

BANIC: Not unique to us. [Laughter]

GOOGIN: Not unique to it.

03:44:25:00

LARSON: I think it's interesting, from the standpoint of perspective, that each one of the major installations in the Manhattan Project had some sort of crisis like that. Of course, you're familiar with the xenon poisoning of the Hanford reactors, when it looked like they wouldn't--and if they weren't designed to put additional uranium in, they probably wouldn't have got it going during the war. So that was one of these terrible things comparable to the Y-12. Then, of course, at Los Alamos, when they found the spontaneous fission rate was so high that it looked like they wouldn't be able to make the plutonium bomb work, that was another crisis. Y-12 had its magnet crisis. So it was interesting; all of them had.

GOLDBERG: When you say everybody was there, General Groves was there?

LIVINGSTON: Groves and [Kenneth David] Nichols.

GOLDBERG: Then Colonel Nichols.

LIVINGSTON: And Ernest Lawrence. I'm not sure who else.

GOOGIN: I wasn't there. It's blurry to me. It's before my time. [Laughter]

GOLDBERG: Before your time, too?

GOOGIN: Yes. Six months before my time.

LIVINGSTON: It was intended to be a celebration, you know. [Laughter] And they just misjudged the situation. The switch had never been thrown before.

GOOGIN: Well, let us say we still have humidity problems on our projects to this day in East Tennessee, and nothing has changed in that regard. [Laughter]

KEIM: But we owe a lot to the vacuum experts like Doc Norman and others, who were successful in solving a lot of those problems involving high vacuum, the pumps and the conditions inside the tanks.

03:46:20:00 [Goldberg hands a photo of the pumping system in the Y-12 building to Livingston to pass to the other participants]

GOLDBERG: I'll show you a photograph. That's the pump floor?

KEIM: Looks like it, yes.

GOLDBERG: In Y-12. Why don't I pass it around and then we'll put it up on there, so you'll all know what it is. I think that's the next thing that I'd like to turn to is the vacuum system and the pumps. We have the drawing here if we have to refer to it.

GOOGIN: Of course, it isn't just pumps that you see there. There's lots of other equipment.

LIVINGSTON: The pumping system consisted of mechanical pumps, then diffusion pumps, oil diffusion pumps, and then big pipes that went to the tanks.

GOLDBERG: But this wasn't just an ordinary high-vacuum pumping. This was a unique system in some ways, wasn't it?

03:47:12:00 [Close-up of pumping system photo]

LIVINGSTON: It was very unique because it was so much bigger. It was bigger by 100 times or more than anything that had ever been done before. Back in Berkeley, we had 16-inch oil diffusion pumps. We had a couple of them that we had made, and nobody else in the world had ever made a diffusion pump that was that big.

GOLDBERG: Sixteen inches is--what is it a measure of?

LIVINGSTON: It's the diameter of the main body of the diffusion pump.

GOLDBERG: And the diffusion pump works by--uses oil.

GOOGIN: It's a kinetic pump that uses a deflected stream of oil vapor to entrain and move gases along.

GOLDBERG: Drag the gases down with them.

GOOGIN: And then condenses out the oil and traps the gas at a high pressure.

LIVINGSTON: Basically, you heat the oil vapor and it circulates and traps the oil. It will work in a region of from--well, you start with a mechanical pump and that goes down to a millimeter or so.

GOOGIN: 50 microns.

LIVINGSTON: Microns. And then to get the kind of vacuum that you need for the system, which is 10^{-3} to 10^{-4} millimeters, the oil diffusion pump will do that. There were mercury diffusion pumps, too, but they're really messy and they can't expand. It's expensive to expand.

LARSON: For small vacuum systems, mercury is quite good, but for this, it wouldn't be.

GOLDBERG: How big were these pumps?

LIVINGSTON: Some were twenty inches.

KEIM: Twenty inches for the Alphas and 42, I think, or something like that for the--twenty inches for the Beta and forty-two for the Alphas. They're tremendous. Ten to twelve feet high, forty inches diameter.

LIVINGSTON: I'm not sure what the Alpha One. . .

KEIM: Alpha One was the same as the Beta; they were the twenty inch.

LIVINGSTON: Of course, as we got more experience, since no one had ever built these things on this scale before, you only had to go on theory that they would work. You had to build some, and none had actually been built as large as ones we were talking about until they brought them down here to install them.

GOOGIN: You used two-stage diffusion pumps, multi-stage inside and pumps in series, as well, to get everything to work.

03:49:43:00

GOLDBERG: In one track, how many of these pumps would you have?

LIVINGSTON: In Beta, there were two pumps. I'm not real sure.

BANIC: Two pumps per separator.

LIVINGSTON: Per separator. So that was twice the number of tanks or pumps. Then Alpha may have been more than two.

BANIC: The Alpha One was two, but when we went to Alpha Three, Alpha One, which is Alpha One and Two buildings--it's always been a confusing thing--the Alpha Two process was

Alpha Three, Four, and Five buildings, that's when the large pump, the 30-inch or 40-inch diameter, probably 30-inch is more like it, we used three of those per separator, had one on each side and one hanging at the bottom. Those are the two sizes, and then each one of these is backed up. The 20-inch was backed up by an 8-inch. I forget, it's been so long since I've seen the Alpha, but it was backed up by a smaller diffusion pump also.

KEIM: You needed three, was it, just for the volume, not necessarily to get the low volume.

BANIC: No, nothing ran lower than, let's say, .1 or .05 microns pressure, but the Alpha Two, which was a four-arc, as compared to this two-arc model we have here, see, the Alpha Two is twice as big, which means eight times the volume, twice in every direction. So that means you had an awful lot of gases that you had to remove. The Alpha One and the Betas were pretty much trapped into where you could put the pumps. The picture that you showed of the Alpha One track had separators back to back. The racetrack had openings from the front that you installed the D or the doors that you referred to, which was one separator, and then from the inside you installed another separator upside down because of the way the magnetic field configuration could go. So that is how you were able to get ninety-six separators in one racetrack. So in doing that, then you were able to drop four pumps at the bottom.

But when they built the Alpha Two track, they couldn't do this because the volume got even bigger, so what they did, they built the vacuum chamber with a pump on each side and one hanging down at the bottom, since they could no longer have separators on the back side. So now you had the back side to put a pump, the front side to put a pump, and also a manifold dropping down into the basement, so to speak. You had room for another pump and they made these three the large pump, the 30-incher, the thirty-two, whatever the size was.

LARSON: And then all these pumps were backed up by tracks also.

BANIC: That's right.

LARSON: And was that liquid nitrogen?

BANIC: Liquid nitrogen.

LIVINGSTON: 714 is the word for it.

GOOGIN: Yes, I was kidding him earlier and called it L-28, to see whether he would respond. [Laughter] L-28 was K- 25 and 714 was Y-12. [Laughter]

GOLDBERG: This was the code.

BANIC: This was the code. Everything had a code.

LIVINGSTON: You were not allowed to use any word such as "arc." It was a "J." But the potential, yes, that was an arc. That's what you meant when you said "J." And the meters were all labeled "J current," "J voltage," on the cubicles.

BANIC: "E" receiver. The beam that went into the receiver was a "Q"; the magnetic field was an "F." Everything had a code.

LIVINGSTON: The ion source was "M," and you had the "M" potential.

GOLDBERG: What was the "M?"

LIVINGSTON: "M" was for the ion source, at least in Beta, that's what it was.

GOLDBERG: And magnetic field?

BANIC: Was "H."

LIVINGSTON: Well, that was. . .

GOOGIN: Which comes awful close, yes.

BANIC: [Laughter] Yes, which comes awful close, yes.

GOOGIN: Now, in the chemistry cycle, we had, of course, numbers. Uranium is 720. And 703--is that peroxide? Or 707 is ammonia.

LARSON: Something like that.

GOOGIN: And 727 is ammonium diurinate, and 723 is uranium peroxide. [Laughter]

03:53:53:00

GOLDBERG: You knew what all these things were. Who made up this code?

LARSON: That, frankly, I don't know. Those codes just sprung up.

LIVINGSTON: There was some group in charge of security at the plant, some highly intellectual group that decided what all things you had to code, which amounted to everything that could be identified by a common name. They were not wanting inadvertently to let out the main features of the electromagnetic process. In fact, it was quite difficult after the war and after the secrecy ended for us to revert, having used for years all those crazy names. It felt like you were breaking security, you know if you used the real names.

GOOGIN: Uranium was also called Tube Alloy if it was not too highly enriched and alloy if it was highly enriched.

GOLDBERG: The code that the people at Hanford and who worked at X-10 in Chicago used a different code.

GOOGIN: Right. And as I say, K-25 had a different code from Y-12.

LIVINGSTON: Each project was compartmentalized rather highly, and there was a very great effort to keep the scientists in one area from comingling at a technical level.

GOOGIN: I had a small problem in the early days. I had to write a report every week of the progress I was making, but when I finished writing it, I couldn't get it back because it was classified higher than I was allowed to have. So I had to keep a few notes now and then. [Laughter]

GOLDBERG: Did they really do that? They classified it above your. . .

GOOGIN: Yes, they did. That happened.

LARSON: Quite often.

LIVINGSTON: The normal classification was "Secret" for almost everything. Production figures were "Top Secret," and then a lot of stuff was "Confidential," which was just components and things like that.

03:55:58:00

KEIM: I have never heard the explanation why the electromagnetic separation plant is Y-12, gaseous diffusion is K-25, and Oak Ridge National Laboratory. . .

LARSON: X-10.

KEIM: X-10.

GOOGIN: Well, you have to recognize that S-2 is right next to Y-12.

KEIM: That's right. [Laughter]

GOOGIN: And S-50 is down at K-25.

03:56:22:00 [Pause in Tape]

[Begin U-Matic Tape 3 of 4]

[Begin VHS Tape 2 of 2]

01:00:32:00 [Interview resumes]

GOOGIN: So they were jumbled coordinates on a map.

GOLDBERG: Well, if there were those jumbled coordinates, I've never seen them, and they've never been made public.

GOOGIN: No. But you see, S-50, as I say, is right next to Y-12. It's hard to get next to it without having some degree of. . . [Laughter]

LARSON: And of course, at least in the very early days, I don't think we used it too much at Y-12, but I remember at Berkeley it was the Tube Alloy project, and we spoke of the isotopes for a while there as being "magnesium" and "aluminum" and "sodium" for the

234. So for a while there, you spoke of the U_{235} isotope as "magnesium," and U_{238} was "aluminum." But that didn't carry over to Y-12. We abandoned that fairly early. I think originally the British came up with those designations, because I think the British used the term Tube Alloy Project.

GOOGIN: It got more complicated, actually, in the chemistry business, because we insisted on having 720, which was normal uranium, 820, which was the product from S-50, and 920, which was the slightly enriched product from K-25, feeding these things into the factory simultaneously. So we had three separate isotopically enriched production lines going simultaneously, trying to keep everything straight by recoding the code. [Laughter]

LIVINGSTON: I was in on the naming of the beams for the meters that the cubicle operators would look at. The first proposal was to call the beams "P" and "Q." We got so much vulgarity out of P and Q that we dropped it and went to Q and R for the two beams.

GOLDBERG: That is for the. . .

LIVINGSTON: For the U_{235} and the U_{238}.

GOLDBERG: So the two beams in the arc.

LIVINGSTON: And the operator had to have a label on her meter for what it was she was trying to maximize on her machine or on the cubicle.

LARSON: The meters were labeled "Q" and "R."

BANIC: Still are. [Laughs]

01:03:00:00

GOOGIN: See, he made a little comment there. He said "she" ran the cubicle. It turns out that "she" ran Y-12. Essentially all of the chemical operators were women, as well as many of the operators in the tracks. [Laughter]

01:03:20:00 [Close-up of photo of female operators stationed at their meter cubicles]

KEIM: And the training of those girls, to me, was almost a miracle. I stepped up to one of those girls one day at the production building, and the meters were just as steady as could be and high productivity. I said, "That's beautiful. Tell me, what did you do before you came to Y-12?"

She said, "I lived out here on the farm and I can't even run the radio at home." But they trained these people to perfection. I think it was remarkable.

01:03:56:00

GOLDBERG: Were any of you involved in the training?

LIVINGSTON: I was involved in supervising the training or the preparation of the training for the Beta process, but I didn't have much hands-on knowledge of it, because a fellow named Fred [Frederick Thomas] Howard was doing that for us. But I talked a lot to him about what they needed to know. Back in Berkeley, before I went on to the Tennessee Eastman payroll, we had a lot of arguments and debates about many things. Lawrence came by--I think it was in December of 1942--and said, "We have about decided to go ahead with the plan. Are you guys sure that you can get ten milliamps out of these arcs? Are you sure that sparking isn't going to kill us?" and things like that. Of course, we weren't really sure, but we knew that we could duplicate those things day after day in labs, so we said, "Yes."

But the question of cubicle operators or people operating, nobody had ever operated one of those units except a Ph.D. in physics or an M.S. in engineering at Berkeley. So it was more or less an act of faith that we could find people who could run the cubicles. I personally didn't spend any time worrying about that. I guess there were so many other, more concrete things to worry about at the time.

But I was the senior staff person for John Rogers on the Beta process so, basically, I had to worry about all the technical stuff as the buildings started up. The thing that was most gratifying--well, we started up later doing the Alpha building. They already had a number of the units running in Alpha One, anyway. We started up Beta One. We had one technical person or two on the shift, and a shift foreman, a shift supervisor. Those people had already had some practice in units at the XBX side, and some of them had been to Berkeley and run units out there. So there were a corps of people who had really run units before, and the thing was, by example, people had had some training in terms of what meters they were going to be looking at and what you need to do with the meters and what the names of the meters were, and the object of the game was to get a lot of Q and a lot of R and to have the ratio of Q and R in a certain range, and the arc current needed to be optimized. To optimize those Q and R meter readings, you needed to adjust the temperature rheostat sensitively. You must not overheat, and if you did, everything would go to pieces. In spite of what Ernest Lawrence said, you would destroy a lot of production if you didn't have good judgment.

Well, those women operators, who were mostly high school graduates, some of them had never done anything before, as Chris mentioned. After the initial shock of walking into the building--and I had seen girls break into tears, just walking into the building and seeing all those giant pumps and cranes and noise and everything. It was so new, they hadn't seen it. But once they got adjusted to that and once they got over the shock, they could sit in front of that cubicle and be the most patient person in the world. With really rather a small amount of supervision, they really did the electromagnetic process.

01:31:03:00 [Close-up of photo again]

KEIM: This is indicative of the progress that was made. That picture is interesting, because if that had been taken a few months earlier, there would have been one operator at every cubicle. But here we see a lot of vacant chairs, because it progressed to where one operator could operate more than one cubicle--two, three, and even four, I expect, in the production buildings.

GOLDBERG: What was your job at that point? Did you deal with the. . .

KEIM: No, I was in the pilot plant all the time.

BANIC: It's interesting, also, the metering and whatnot. As you can imagine, as the enrichment of the V increased, as we got better and better stuff in K-25, so naturally that's going to affect how the ratio of the Q to R, the 238 to 235, is going to change. So the electrical groups would go through there and they'd just put shunts on some of the meters, so as far as the girls were concerned, the meters always read the same ratio. They had set them for that. As I say, as the quality increased, we would just shunt one of the meters so they would always read the same thing for them.

GOLDBERG: It would read the same, but it was actually measuring. . .

BANIC: But it was actually different. That way there wasn't that confusion. Say a ratio reading of 20 or 30 to 1, and all of a sudden it's reading 20 or 30 to 5, 20 or 30 to 6, it would be real confusing. So if we just put a shunt on the smaller meter so that that could be taken as a factor automatically, they always adjusted the knobs, they were told, until the meters read so and so. They didn't know what the actual purity of the feed material was. Didn't care, didn't need to know.

GOLDBERG: They didn't even know it was a matter of purity, did they?

BANIC: That's right. [Laughter]

GOLDBERG: When they had something wrong, they had to get in touch with somebody who did know or knew something about the. . .

KEIM: There were supervisors up and down that aisle.

01:32:57:00 [Goldberg points to the supervisor in the photo]
GOLDBERG: So there's a man way in the back here. He's the supervisor?

KEIM: Isn't that right?

BANIC: He was what was called a start-up crew, a technical supervisor.

01:33:06:00
LIVINGSTON: Actually, the start-up sequence was, first of all, you had to get a vacuum, a good vacuum. So there was nothing much to do until the pumps did their work. This took some amount of time. Then after you got a suitable vacuum, you had to establish voltage on your unit, because there was no point in turning any vapor on or striking an arc or anything until you had a unit that would hold voltage. So that was a kind of specialized deal, breaking a unit into new voltage. It didn't take very long, provided you had a tight vacuum system and everything was working properly.

Then the next thing you would do would be to start heating up the furnace that was going to thaw out your uranium tetrachloride, and you would strike an arc by heating up this filament that's kicking around here somewhere. Then all of this took I don't know how long, but it would take some appreciable amount of time to get a cubicle started up. But once it was started, it would run, and if this was done with enough skill and courage and understanding, it would run for two or three days until you exhausted the charge. Of course, you had three shifts going all the time, so one person would work on it for eight hours, and then a new crew would come in and work the next shift, and so on.

You had all the units in the building, if the building was running well, all the units would be either turned on or in the process of being started up or in the process of being terminated. It was desirable to spread out both the start-ups and the terminations, if you could wangle it, so that your charge came out right and so on. So basically, it was a pretty smooth-flowing operation, and you had specialists to do special things. You had an installation crew to install the unit. The cubicle operator didn't really need to worry about that. When the unit was ready to start up, she needed to know what to do and in what order to do it.

Start-up was more difficult than just running, because just running, all you really had to do was watch the meters. It was desired to optimize a maximum Q, the product stream, which, in the early days, started out at a few milliamperes and later on was up close to 100 milliamperes per arc. This was a result of people being trained, of units getting more reliable and so on.

01:36:26:00

GOOGIN: Eastman Kodak had a rigorous management scheme, since they were an old-line company. I can still remember, right in the middle of the war, being scheduled to take management training, and I took it, no matter what, and to take courses in statistics so I could analyze what I was doing better, and I took it, no matter what. [Laughter] So they brought in these young supervisors, potential supervisors, and technical people, and never gave up on the training all the time.

KEIM: Some of us, after we came to Oak Ridge and had not been cleared, sat in management training seminars for ten days or more until military intelligence had cleared us, so we could go into the Y-12 area.

LIVINGSTON: In 1943 was not the most advanced management compared to today, but I thought Eastman Kodak--or Tennessee Eastman and Eastman Kodak--had very advanced management plants. We always were doing merit ratings on our operators and establishing salaries. With all those operators, you had a big problem, because you had to confer with a lot of people. That was all done very conscientiously, and the people were really paid for the skill of the work that they did. That extended, among others, to the cubicle operators.

KEIM: Mentioning Eastman Kodak, John mentioned a while ago he had an application at Eastman Kodak. I had finished my graduate work as a physical chemist. I had an application in at Eastman Kodak. I'm sure that one reason I received a telephone call on Christmas Day in 1943, that they were going through their files, picking up some of these old applications. And I wasn't surprised because Eastman Kodak had that reputation of being so meticulous on their personnel affairs.

GOOGIN: Of course, they were safety conscious too, right from day one.

GOLDBERG: A lot of the stuff we've been talking about so far is very dangerous.

GOOGIN: It can be.

GOLDBERG: We laugh and joke about it, but phosgene is not pleasant stuff.

GOOGIN: No, it's not pleasant, even though I made a little light of it because once you understand it, you can get along with it. But it's not pleasant stuff, not in tonnage quantities.

01:39:11:00

GOLDBERG: At the time you were hired, your clearance wasn't high enough so that you were supposed to know what was. .

GOOGIN: See, I wasn't supposed to know. I wasn't even supposed to know such a project was possible. So when I came in, I was already in violation, you see. But we had a security system of compartmentalization. Each badge had a set of letters on it, and you could go through certain gates and certain gates only, and you could only get certain classes of documents and what have you, because of the security system.

One thing that used to happen to me occasionally was the security checkers would come around and ask how we're doing. So I would tell them about how well the tracks were doing. They said, "You're not supposed to know that."

I said, "How can you keep me from knowing that? Here I am processing all the material that goes through the tracks and running the material balance, and pretty quick I can tell about what they're doing." [Laughter]

So you had these problems of logic in the security system, but there was a real effort to keep it compartmentalized. Of course, when you're in the chemistry business and handling all of the feed, and some of your buddies are handling all of the product, pretty soon it becomes pretty obvious what's going on.

KEIM: And it was obvious that throughout the plant and elsewhere there were military intelligence personnel working along beside us. I particularly remember the chauffeurs and chauffeurettes. They were obviously military intelligence, because I know of several cases where people came to Oak Ridge to be interviewed, and in the conversation from the airport to Oak Ridge, they were evaluated. I know in two cases they weren't even interviewed the next morning; they were taken right back to the airport.

GOOGIN: I told George on the way out one of the little stories about being involved in this problem. Of course, we used to work a lot, as everybody said. In fact, some weeks we worked two shifts a day. We were doing that one time, working on the old iron horse vibrating chlorinator--remember him?

LARSON: Oh, yes, I remember that monstrosity. [Laughter]

GOOGIN: A buddy and I were working on that late one night and decided to catch the early bus home to the dormitory, so we got out at 10:30 instead of 11:00 o'clock. There were two of us on the bus. We were talking a little bit in code, you know. We were reacting the 723 with 754 and it wasn't going too well and all that. So in a couple of days we got invited to the security office. It turned out that the bus driver apparently was an FBI checker. The security man gave us a little lecture. Of course, we were kind of young upstarts at the time, as Clarence will tell you. After, we had a little argument about who was doing the most good for the country. The man in the security side of the house said, "Googin," he says, "I've got a proposition for you. You shut up and you stay shut up, or else I'm going to put you in the Army, I'm going to send you to the South Pacific, and I'm going to censor your mail in and out." [Uproarious laughter]

LIVINGSTON: In the early days of the project, we actually had counterintelligence out in Berkeley while I was still working for the Radiation Lab out there. We were all invited into a big conference room one day, all the people working on the project. And this guy that we had never seen or heard of before walked in and told about all the information that he had been able to pick up at the University Faculty Club about what we were doing. I personally wasn't involved in it, because I didn't use the Faculty Club much or any, but a lot of the people did. There were two or three people who had been a little bit indiscreet about their conversations. The easiest way to handle all that for me, personally, was just never talk at all when you're outside, about anything technical.

LARSON: The security was very tight.

LIVINGSTON: On the whole, I think we did pretty well out there.

01:43:48:00

GOOGIN: We had a small problem, known as Al Slack. Al Slack worked in building 9202, where I worked. Al Slack was the shift superintendent of that building. And he was a real nice guy, used to come down to my desk to talk about how well we were doing and "What do you think the product's going to look like when we get this bomb built?" and all that. It turned out right after the war, the FBI came to talk to me, did I know Al Slack. The next question was, "Did you know he was a Russian agent?"

"No, I didn't know he was a Russian agent." But Al Slack was a member of Harry Gold's operation out of Philadelphia, the same apparatus that was managing Klaus Fuchs at Los Alamos. We could never prove that he had ever been in contact while he was at Y-12, but they could prove he had been in contact while he was at Holston Ordnance Works. So he got a jail term and didn't get anything more serious. So we had security problems, and I worked for a Russian spy in Y-12.

LARSON: Of course, we don't know how many were there that were not caught. That was a problem throughout the whole thing.

KEIM: We all had individual experiences with security. We were talking at the break; one of the experiences that stood out very, very powerfully to me, on one of the trips to Berkeley, I transferred from the Oakland Interurban to the K-car. As I got on the K-car, the motorman looked at me, and he said, "Well, good morning, Dr. Keim. How's everything in Oak Ridge?" Fortunately, I was so shocked I couldn't answer him. I went and sat down, and I got to thinking about it. Supposing I had acknowledged I was from Oak Ridge. I don't know what the results would have been, because he was aware, obviously, to me at that time then, that he was alerted to who was coming from Oak Ridge to Berkeley, and he would feel us out and see if we were talking. Fortunately, I was tongue-tied at the time; I was shocked and didn't reply. It was best for me, obviously.

LARSON: I think with regard to the security there, there was a lot of security within each project, but they very carefully discouraged any conversations between projects, like between the Chicago-Berkeley plutonium work and Y-12 and then between X-10 and Y-12, and also on the diffusion side. That compartmentalization worked remarkably well. In fact, I accidentally ran into just a couple of little pieces of information, cross-compartmentalization. But one of them was that after I had started, I was at the College of the Pacific in chemistry and left for the Radiation Laboratory. I went back there, and Amos Alonzo Stagg was a coach, and I used to know him very well. I was talking with him. "I just came back from Chicago," he said. "All the people there that I talked to were agog about this tremendous project that was going to win the war." My gosh, I just sort of looked surprised and so on, but that was the only violation of cross-compartmentalization.

01:47:56:00

GOOGIN: There were other violations, and some of us, in fact, sort of organized a counterespionage force of our own, because to get the job done, we had to have certain bits of information. It was a lot more economic to get it from some other compartment than to do it all again. So that those compartments worked, but sometimes they didn't.

GOLDBERG: Do you have an example of that?

GOOGIN: Well, I had a hard time. I wanted to find out what was going on at the blue products laboratory of E.I. DuPont and de Nemours [and Company], because they were processing uranium. So I had to get a little bit of help to get those reports without going through official channels, because it wasn't really supposed to be, for instance, to be connected. So people took security very seriously and, of course, that was one of the arguments you always had with a security person, as I said, was that sometimes the security costs you a lot of time and money, and sometimes it didn't. But there's always that argument that goes on.

KEIM: But we knew the seriousness of what was going on.

GOOGIN: Oh, sure.

KEIM: We knew the seriousness of the war, and we respected security.

GOLDBERG: You were traveling back and forth between Berkeley and. . .

KEIM: Just occasionally. Bob, were you with me when we got on that Union Pacific Streamliner at Chicago?

LIVINGSTON: No.

KEIM: I was with someone, and they put us in the last compartment, double bedroom on the streamliner. The conductor came immediately, and he said, "Now, do you want to have your meals in your compartment?" He assumed that we would be staying in that compartment all the way. I learned later that double bedroom was reserved permanently for the Manhattan Project, and different people rode in it at different times. Some were of such importance that they didn't show their faces from Chicago to Oakland, California, and they were attended to in their compartment. A lot of travel was by train at that time.

We mentioned our badges. We didn't mention the Roman numerals, I believe. The top level was a Roman numeral V. The next level was IV, III, II, and I. I often thought, though, was that wise. If I were in espionage and I wanted to try to get information, I wouldn't bother with a I or a II; I would sure shoot for the V or the IV. We had to wear those badges all the time in Oak Ridge, in the town site and everywhere.

GOLDBERG: What number were you?

KEIM: IV.

GOLDBERG: You were a IV?

GOOGIN: No, no way. I was down at the III level. I wasn't allowed to do these things I had to do. [Laughter]

BANIC: Technical people were usually IV's. Vs had production data.

GOOGIN: I got promoted after a while.

BANIC: Those of us who just needed to do the technical work, they stopped us at IV.

KEIM: But there was an irony in that. Now, our secretaries, what were they? I or II? [Laughter]

LARSON: And they knew everything.

KEIM: They knew everything, probably more than we did in some cases.

LARSON: Yes. I always said, you know, sometimes with regard to this, if you didn't feel you could give a man a raise, you could just raise his security. It would make him just as happy. [Laughter]

GOLDBERG: At this point, let's take a break and have lunch, then come back.

LARSON: Fine.

KEIM: Oh, it's fun.

GOOGIN: He's got us wound up here.

GOLDBERG: I didn't do anything.

01:52:30:00 [Pause in Tape]

[Begin U-Matic Tape 4 of 4]

01:53:14:00 [Interview resumes]

GOOGIN: And if you make a small mistake along your career, over and out.

KEIM: But just two weeks ago, I heard a Bechtel [Corporation] executive say they lost an $80 million contract and Japan got it, but he said, "We're not out completely, because we're subcontractors." [Laughter]

GOLDBERG: Was any of the centrifuge work done at all during the war here?

GOOGIN: No. We stored the machines here. We had the old machines that were built, part of them in storage in K-25. They may still be there. They disbanded that project, and they sent the residue to Oak Ridge for storage.

GOLDBERG: Do you know when they dismantled it, do you have any idea?

GOOGIN: It was 1943, 1944, 1945.

GOLDBERG: Do you think there's a possibility that the old rotors are down in K-25?

GOOGIN: Well, they used to be. Those machines were two-ended machines. In other words, they were not the Zippy pattern, but the two-ended machines. And the bearing systems on the ends and the feeds on the ends were pretty complex gadgets. Of course, that's one of the reasons they didn't succeed, is they tried almost too much all at once with a small project. But they actually got those machines up to running. They got the supercritical machine running, and they got 80 percent of the expected theory separative work out of those machines.

LARSON: Paul [Ross] Vanstrum probably would know.

GOOGIN: You might want to talk to--who are you going to see at K-25?

GOLDBERG: Paul Huber, Paul Vanstrum and Parsons.

GOOGIN: Van may know. Jim Parsons. Okay. They may know whether they're still there or not. We used to have them on a display down there, showing the evolution of the centrifuge. But whether it's available or not, I don't know.

GOLDBERG: That project stopped very suddenly.

GOOGIN: Yes and no. We've got a great big plant up here if you want to finish.

GOLDBERG: That plant, yes. That's recent. I mean during the war, it stopped very suddenly.

GOOGIN: Yes, it stopped very suddenly because the resources were all put on the other projects, and it wasn't revised until we heard from Zippy what was going on in the Soviet Union. Mr. Zippy was the key, really, in discovering simple ways to make machines.

GOLDBERG: That was Jesse [Wakefield] Beams' project during the war.

GOOGIN: Right, and also after the war when it restarted. The Zippy configuration--Zippy came to this country after the Soviets graduated him with honors and a prize and had cooled him off for a few years. Then he came to this country and reproduced some of his designs at the University of Virginia, which turned out to be, again, the basic design as far as concept is concerned, of the machines that we just finished building.

01:56:28:00

GOLDBERG: Let's go back. I wanted to pick up on something we talked about before lunch. Last night we talked to a number of operators--that is, the people you were talking about, women--not a number; two, in fact--and one of the trainers. They also told us about the code, but one of the things that they talked about, one of them said, anyhow, that the magnetic field was not associated with H, but with Z.

KEIM: With Z. You're right. That's right.

GOLDBERG: And they talked about losing the Z. So they didn't know what that meant, but if they lost the Z. . .

LIVINGSTON: That's correct. H would not be a proper code.

KEIM: H is it!

GOOGIN: H is what it is.

LARSON: But "C" is "H." [Laughter]

GOOGIN: That's why when he was calling it "H," I told him I didn't think that was too much of a code. [Laughter]

GOLDBERG: What would you do, for example, if you had to go on the floor for any reason and you had contact with these people? You had to know that code intimately as well.

GOOGIN: Oh, yes!

KEIM: Everybody knew the same code.

GOOGIN: We never used anything else. You wrote documents in code; you didn't write them in ordinary English.

LIVINGSTON: Because of the mental processes, you can't jump back and forth; you just had to go to the code and use it as if it was the only word there was for that.

GOOGIN: Even lots of [Office of] I[nternational] S[ecurity] A[ffairs] reports were written in code. They weren't written in chemical symbols. If they ran an equation, you know, it was "720 + 707 = 727." I mean, that's the way it was. [Laughter]

LARSON: It took a long time to drop the letter T and substitute the U, for instance.

GOLDBERG: For. . .

GOOGIN: Tube Alloy.

LARSON: Uranium. It would always be T_3O_8 or TCL_4.

GOOGIN: Right.

BANIC: The code still exists. In our stable isotope enrichment program program, those of us who have been with it a long time, a filament is still a K, and ion beams are still Q's. I mean, we get into trouble when it comes time to write reports. Somebody has to almost go through there and transliterate back again. A filament will be a K to me until the day I die. [Laughter]

GOOGIN: Right. So when I came in, I told him he had a K.

BANIC: Got a K. The J arc will always be a J.

KEIM: When I came into Y-12, I was in a training class, and some of my questions were being answered as to what it was all about, at least as far as physics was concerned, not in precise terms, but my deductions were confirmed. One day, the instructor used the term "Tubealloy" and I said, "What's that?"

He said, "I'll see you after class." After class, he said, "Now, you're not to ask questions. We tell you what you are to know, but don't ask." That was fair enough. I didn't from then on.

01:59:38:00

LIVINGSTON: I was thinking about something else that Chris said this morning about General Groves looking at some new development and deciding not to put it in the tracks. That reminded me of an interesting tension that existed throughout the process, the question of improvements versus the stability of the hold units running. One of my jobs was really to encourage the introduction of new technology, but in a safe and sane way. I used to make periodic trips to the assembly line in Pittsburgh, where the new sources were being assembled, to see that they were doing it according to the latest specifications, and so on. It turned out that there were a few new developments--the addition of a special geometry of an accelerating electrode that really did produce a dramatic increase in current. We had a discussion about it, and they decided that there was no way that you could put that thing into the production line without screwing up the schedule and everything else. So what they did instead of that, they hired Stone & Webster [Engineering Corporation] to set up a special shop assembly line, and every brand-new Westinghouse [Electric and Manufacturing Company] unit, after that was set up, would be uncrated and taken down to that assembly line of Stone & Webster's, and they would put all the latest gadgets on it so that by this method, we could put on units that represented the best and the latest technology, and it didn't disturb the line at all, although at first thought, I thought that was kind of a silly way to do it, but in practice, it actually was a good way to do it. That enabled us to jump ahead and speed up the increase in output of the units rather dramatically.

GOLDBERG: How would an innovation like that come about? Is that a matter of theory, or simply a matter of fiddling around with these things?

LIVINGSTON: Well, it was a cross between those two things. There were some people that were thinking about it from a theoretical point of view, and there were others who were using the empirical approach. This was divided up between people who were still working in Berkeley, between a group of Berkeley people that were in the 9204-3 building, and the process improvement group, under--was it under Harv[ard Leslie] Hull then or later?

KEIM: Yes, but Ben Hall--later, Duane [Campbell] Sewell was pilot plant superintendent.

LIVINGSTON: Duane Sewell later was the secretary of. . .

GOLDBERG: Assistant secretary.

LARSON: Assistant secretary.

GOOGIN: He worked at Lawrence Livermore Laboratory for a long time.

02:03:00:00

LIVINGSTON: So there were a number of ways that new things could get into the system. There were a number of people paying attention to things. The U.S.E.D. group, the one that Herb York worked in, was very hot on getting better E-boxes or collector boxes, and they came up with a number of new designs. It was easy to put those into the system, because all you had to do was get a place that could fabricate graphite, make up the new unit, and screw it onto the mounting plate of the system. So some things were easier than others.

My first job was being technical advisor to the Eastman Kodak head of the Beta process, and then later on, as they opened building One, Two, Three, and Four, he asked me to go and run a process in Building Three. So I had about a year's experience in Beta Three. Since I really had grown up with the technology and thought I understood what was important, I decided to try to show the other three buildings what you could do if you really got all your people oriented so they could do it.

What we were gunning for was efficiency. That number between 15 and 25 percent of the fraction of the bottle that got up in the E pocket, and enrichment. We had a production schedule. Our productivity was shown on a chart that John Rogers got and a few other people, and we all got to see it, and our building did come up above the others. Bob Thornton, who was running the U.S.E.D. group at the time, said, "I don't believe that." It was a couple of points, like 17 percent average instead of 15 percent. Well, I knew it was right, but it was because we used to heckle the operators all the time to keep their units cold. One thing I chided my wife about that she didn't bring up last night was that she said, when she worked in the Alpha Building, just before spot check time, the supervisor or the technical person would come around and crank her rheostat on the temperature up, which would raise the throughput a little bit, but would cause great havoc ten minutes later.

LARSON: And probably even decrease the efficiency.

LIVINGSTON: Yes. That really made her mad, but she was only a cubicle operator, and she couldn't do anything about it. That was the last thing in the world the supervisors in our building would do, because we would have jumped on them with two feet. The technical people and the supervisor were always persuading the people to try to run the units cold, because if you run them cold, there's a better chance of achieving the high efficiency and also better separation.

KEIM: We had a procedure we followed in the pilot plant as well as we could, that there would be those coming by, particularly higher level people, who would like to sit down at the cubicles and experiment with them. We had a rule we mentioned every now and then, that whoever sits down at a cubicle leaves it operating no worse than when they sat down, preferably better. And we followed that, and most people respected it. When they sat down, they knew that if they messed that up, that they were going to have to sit there and straighten it out.

Another thing I learned from General Groves: How he did as much as he did, only he would know, but when he'd come into our building-- and he was always accompanied by an aide-- when the time was up, the aide would look at his watch, and he'd say to General Groves, "We have to go." And he'd go. He was scheduled precisely, and he followed the schedule which had been set down.

Later, in the early sixties, I was responsible for a lot of visitors in the Oak Ridge National Laboratory, and I remembered what General Groves had done, because if you take visitors into certain areas, everybody likes to talk about their work and they're proud of it, and they'd keep the visitors all day if they can. I even had to say to Dr. [Alvin Martin] Weinberg sometimes, "It's time. We have to go." And I'd take the visitor out the door. I don't think I would have learned that unless I'd seen it done effectively with General Groves. It's something we have to follow.

02:08:48:00

GOLDBERG: Let's go back to the chemistry just a little bit, because there are a couple of things I wanted to pick up on. This morning, when we were talking about recycling, there are two or three recycling phases. One of them is when you have what you call "the crud."

GOOGIN: Which became "the gunk." [Laughter]

GOLDBERG: Which became the gunk. To make the crud into the gunk, you had to somehow get it out of the door. The whole big apparatus--can we get a picture here? This one will do. I don't have the photographs, but I've seen them. It looked to me like you had specially built devices for moving these things around intact, is that right?

GOOGIN: Yes.

LARSON: That's right.

KEIM: They had to be non-magnetic because of the magnetic field. In the early days, I remember when I came into the pilot plant in early 1944, we had tools which were magnetic, we had plates which fit over the vacuum opening after we'd taken a source out, and we put these plates over to keep the moisture from going in. Those were magnetic, and you'd walk into the field with those plates, and it'd take it right away from you. Too bad if you had your fingers in it. It was only by experiment and a little bit of time that those became non-magnetic plates, the tools became non-magnetic.

GOOGIN: And the girls lost all their hairpins. [Laughter]

KEIM: Yes, and we had mechanics--I had some in 9731, the pilot plant, who would not go into the magnetic field, because they'd feel it tugging at their keys in their pocket, and they were afraid.

LARSON: It didn't do their watches any good, either.

KEIM: No, it didn't. I was mentioning this morning, one of the Army men came to us in the pilot plant one day and said, "A lot of people are going to a jeweler in Knoxville and getting their watches demagnetized. For two or three minutes' work, he's charging them $3.00. He's doing a big business. Can we do anything about it?"

I said, "Sure. In every stockroom, we can demagnetize watches. That's no trick."

He said, "Well, let's do it." And overnight, that lucrative business for this jeweler in Knoxville was shut off. A little late.

LARSON: Quite a breach of security, too.

KEIM: He got to talking too much, and the military intelligence picked that up. But the funny thing to this story is that about in 1955, a jeweler from Knoxville set up a jewelry store in Oak Ridge, and we were visiting on the street one day, and he said he decided to come to Oak Ridge because he thought there would be a lot more business, because he had demagnetized a lot of watches in Knoxville, but overnight, he said, "It all shut off." And he didn't know what happened. I was able to explain to him what had happened. That, to me, was a very interesting development, but it was so true that these magnetized watches--the whole magnetic field was interesting. If you walked on top of those tracks, on top of those magnets, and you had heavy-soled shoes with lots of nails in them, it was just like walking on flypaper.

02:12:40:00

GOOGIN: We had other kind of jewelry problems. Most people don't realize it--Clarence mentioned it earlier--that we did use the oxalate precipitation to recover uranium. The oxalate is in the four-valent state. In the back end of building 9202, we had a production line where we used mercury cathode electrolysis cells to reduce uranium to the 3-4 state, where we blew it back with air to the four state and precipitated it with oxalate to recover uranium from some of our salvage operations. So that we had a whole roomful of these cells that were running on mercury cathodes. I had a fairly steady business at my laboratory, because the ladies would bring up their gold jewelry, and I'd put it in my vacuum and get rid of the mercury again. Now, after you've done that two or three times, the jewelry isn't very good anymore. So the history of mercury in Y-12, not only in McCloud gauges, but in chemical processing, goes all the way back to the initial days of the operation.

LARSON: And a lot of the analytical work was based on electrolysis of uranium, back to the plus-four state, and then titrated and so forth.

GOOGIN: Right.

LARSON: There must have been thousands of those cells in various places.

GOOGIN: So the chemistry cycle was more complex than appears even in some of the textbooks.

GOLDBERG: You would have to take one of those doors, the whole unit, and just. . .

GOOGIN: They had these manipulators, and you can see them nicely in George's present building, where the unit was on a semi-automatic machine that was stopped by microswitches. You'd start it, it would stop at the right position when it came to a switch, so that the machine could be easily inverted, rotated, dismantled, and washed off, and the solution collected in big funnels.

LARSON: As I remember, we'd take them to a separate area.

GOOGIN: Yes, we had a separate area.

LARSON: This is a separate area, and then they would be placed in position over the proper type of that, and then cleaned down and so forth. You recovered as much as you could from those. Sort of a messy job, to say the least.

LIVINGSTON: The vehicles all had wheels on them, and you could just wheel it over.

GOOGIN: We had these old Edison cell electrically-driven transporters that carried the things around.

BANIC: Still have them.

GOOGIN: Still have 'em. [Laughter] Still the same old Edison cells.

02:15:28:00

BANIC: Dr. Keim was talking about the magnetic field. The story goes--and I have no doubt of its authenticity--we had people come who were discharged from the Army, came back and got jobs in Y-12, and were assigned to operations. This one particular fellow notified his supervisor that whenever he worked at the track, he was hurting inside. Well, we began to wonder and ask him a little of his past experience. We found out that he had been in the Army and had been wounded and had shrapnel in him. So here's a fellow who's almost ready to be stabbed to death from the inside. Needless to say, he was found a job somewhere else. The magnetic field was pulling on the shrapnel particles.

GOOGIN: Iron fragments inside his body.

KEIM: We say 9731--that's the pilot plant. I remember one day we had a high-level visitor, and we had them take off their watches and put them on the supervisor's desk so they could walk into the magnetic field. This visitor from England, when he went back to get his watch, it was gone. That was horribly embarrassing. It happened that there was construction work going on in the chemical area at that time.

GOOGIN: The chemical area was responsible. [Laughter]

BANIC: But this visitor had to go to the airport and catch a plane. We apologized, and he went on. Just on a hunch, I went to the supervisor, the foreman of that work gang, and told him what had happened. He said, "I'll get your watch for you." In about ten minutes, here he came with the watch!

I said, "Where did you get it?"

He says, "I only promised you I would get it and wouldn't tell you how I got it." We were able to send a courier to the airport, give our visitor his watch before he got on the plane. [Laughter]

GOOGIN: As you were talking earlier, thievery in government operations, even in highly secure ones, is not an unknown art. Some numbers of small tools and bits and pieces of this and that did escape through our gates. [Laughter]

BANIC: I'm sure of that.

02:17:54:00

LARSON: Yes, of course, I should tell the story. This fits in with security, because we ultimately had polygraph examinations for certain special situations.

GOOGIN: Like myself.

LARSON: I had to take it on a routine basis.

GOLDBERG: When was that?

LARSON: Let's see. What year?

GOOGIN: That would be about '46, '47.
Right after the war.

LARSON: After the war. But then we would be asked, "Do you know of any spills that have not been reported?" "Do you know of any diversions of material?" and so forth.

GOOGIN: Any security violations that had not been reported.

LARSON: That's right. We definitely did not use it for anything like theft of pliers or drills or anything else--just for security reasons.

GOOGIN: Of course, there are tales that go with that.

LARSON: There's a large number. But the amazing thing was that in spite of the fact we assured everybody that this would never be used for petty theft and so on, only for security, the use of Kleenex and things like that just plummeted. [Laughter] Everybody knew that we'd never use it for petty theft. Nevertheless, it did have a reaction there.

KEIM: We needed a refrigerator in the pilot plant during the war, so we put in a requisition for one refrigerator. They were hard to get. It went to Tennessee Eastman procurement, and they doubled it. Then it went to the Army, and they doubled it. We got four refrigerators, so we used one for the original purpose, for storage of chemicals; we used one for lunch bags; and we had two we could negotiate around the area to trade. [Laughter]

GOLDBERG: You were telling the story before about the. . .

KEIM: The vise-grip pliers?

LARSON: Yes.

KEIM: I hadn't been in Y-12 very long, and I observed in the mechanical servicing that we had a lot of mechanics, and one would be tightening a nut, and the other would be holding the bolt. I asked if they'd ever heard of the vise-grip pliers. "No, what's that?" I explained it to them. Then I went to procurement in Y-12 and told them about it, and how much manpower we could save by the vise-grip pliers. I knew the history of the vise-grip pliers, because I knew the farmer who had invented it and has made a very fine business out of it for his family. So Y-12 procurement ordered vise-grip pliers. In about three weeks, here came a boxcar-load of all sizes of vise-grip pliers and went into every mechanical servicing area. They would have been a very worthy investment, but I'm sure they had to order another boxcar load, because they were only about that long, and they could go into the lunch pails very easily, and probably did.

LARSON: Yes. As far as the polygraph was concerned, it did lead to some disclosures of people who had taken samples of uranium, and that would show up.

GOOGIN: A friend of mine went through that experience. He had been working in the analytical business, and we had some of these plating operations, where we plated out uranium for analysis as the oxide and it made gorgeous colors. He'd taken some of these things home because they were nice decorations. He got worried while he was on the polygraph, and said, "Gee, I'm lying every time." So he finally brought them back.

The next time he went through the polygraph, the operator laid back and said, "Brought 'em back, didn't you?" [Laughter]

02:22:23:00

LARSON: Yes. Finally, the upshot of this, this gave us a lot of grief, because a lot of it was comparatively minor. There was no theft of enriched material or anything else like that. So we never did discover any spies. But you know, Congress heard about this, and so I had to go up to Washington and get grilled, you know, on "Are we sure this isn't going directly to Russia, this sample of uranium?" and so forth. It really caused us an awful lot of problems.

So I said, "We'll continue this program if every other installation in the Atomic Energy Commission institutes the same thing, but we don't want to be unique." Of course, nobody else would institute it, so we dropped it.

GOOGIN: I used to have fun with the polygraph man, because I'd say, "Now, I'm going to answer all your questions truthfully."

And he'd ask these one-sided questions, like, "Do you know of any security violations?"

And I'd say, "Yes."

"Do you know of any uranium that has left the plant?"

And I'd say, "Yes." And on we'd go.

He's say, "What are we going to do about you?"

I said, "If you'll ask sensible questions, I'll give you sensible answers." [Laughter]

LARSON: But those are some miscellaneous things. Did you cover all that you wanted as far as that uranium? We were on the subject.

02:23:55:00

GOLDBERG: No, we'll go back to it. We talked a little bit about the physical problem of getting the stuff out of the units so that you could reprocess it. But there was a whole other process that had to be going on near the end of the war or near the time that we were getting ready to manufacture the first bombs in Los Alamos. That is the feed material then changed, did it not? Because you were now able to get stuff from K-25 and from. . .

GOOGIN: We started getting uranium hexafluoride from a place known as S-50, the thermal diffusion plant, in late '44.

GOLDBERG: This thermal diffusion plant, you know something about that. Why don't you tell us something about it.

GOOGIN: Well, I think it was under a Navy contract.

LARSON: The work was done at the Naval Research Laboratories under Phil [Hauge] Abelson. He was the one who, incidentally, happened to be the inventor of the process for making UF_6, the basic patent on all UF_6 productions.

GOLDBERG: Uranium hexafluoride.

LARSON: Phil Abelson was the inventor of that. From the time. . .

06:25:53:00 [Pause in Tape]
[Begin U-Matic Tape 4 of 4]

06:26:00:00 [Interview resumes]

GOOGIN: And we designed that facility to operate at 3.5 percent, where we have to have worry about the criticality problem at every step, and we had a very complex system of both safe geometry, limited mass, limited size of vessels, and an interlock system of basically switches, again, so if all the switches weren't in the right direction, you could not transfer things from place to place. But we never put that in operation, because when we succeeded at the end of the war without ever having to use the Alpha cycle at a critically dangerous level of enrichment. K-25 was coming along fast enough at the end of the war, so there was decided to allow it to drift on up to 30 percent--20, 30 percent--and that uranium hexafluoride was received in Y-12 and put directly into the Beta chemical recycle process. And we shut down Alpha and kept running Beta for a while as a topping facility for the diffusion plant. As they kept getting along even better, why, then, of course they ran the thing up to 90-odd percent at K-25, at which time we shut down Beta.

We have a lot of interesting happenings that go along with that, because as this inventory of isotopes out there with a strange assay that is high in Uranium-235 and low in Uranium- 234 that nobody seems to remember we ever made. [Laughter]

06:27:42:00

GOLDBERG: Well, when you got to that point, that is, when you started building this processing plant in which you wanted to ensure against criticality, at that point did you know what you were doing? I mean, were you supposed to know what you were doing?

GOOGIN: We were discussing that. There had been some words passed about this. Y-12 was equipped to handle critically sensitive amounts of material before it started, not after, because obviously, as soon as we were successful, we were processing weapons-grade material out of Y-12 as the green salt, and we had to do all of the operations at the top product level, at top product conditions, and we had to do all of the intermediate work with many, many tons, literally, of uranium at 12, 13, 14, 15 percent. So the whole cycle, as soon as it became close to equilibrium, which took us all of a few months, everything was being done under classical criticality control. It had been designed that way to start with.

LARSON: You referred to the visits of [Richard Phillips] Feynman, and then also, I think, [Emilio Gino] Segre came there and reviewed that. The main thing was there that they had done those criticality experiments at Los Alamos, but because of security and so forth and so on, there were very few people that got the actual numbers. I think we could have made it a lot easier if we were given the exact numbers from Los Alamos.

GOOGIN: Yes, that would have helped a little.

GOLDBERG: So you knew about criticality.

GOOGIN: Oh, day one! We knew about criticality before we started building the plant.

LARSON: Oh, yes.

GOOGIN: I knew about criticality when I came there, and I hadn't even been there. [Laughter]

LARSON: That's right. At least for myself, I thought the critical mass was larger, however. I was a little reckless, you might say, with regard to that. The criticality turned out to be less than I thought it was going to be.

GOLDBERG: Especially in solution.

LARSON: Oh, yes.

GOOGIN: Because that's very sensitive to the measured parameters you had to deal with, and we didn't have the right measured parameters at all times, but we were concerned about the problem.

06:30:12:00

GOLDBERG: Green salt--is that UF_4?

GOOGIN: Yes, UF_4.

GOLDBERG: And that's how you shipped it?

GOOGIN: That's how it was shipped to Los Alamos. They reduced it to the metal.

GOLDBERG: Is there some reason why you chose UF_4, say, rather than a nitrate or something?

GOOGIN: Well, the basic reason for that is that that's what feeds the metal reduction bomb, and it's easy to ship, and all of the salvage associated with the process up to that point can be kept internal to reprocess, and you can do the purifications and all and turn out a product that's immediately useful to the customer.

LARSON: And UF_6 is not very hydroscopic either.

GOOGIN: UF_4.

LARSON: I mean UF_4.

GOOGIN: UF_4 is very easy to ship.

LARSON: I'm sorry. UF_4 is easy to ship, easy to handle.

GOOGIN: It's an inert green powder.

LARSON: And they did the reduction with pure calcium or pure magnesium.

GOOGIN: Basically, they used the calcium-iodine reduction.

06:31:07:00

GOLDBERG: How did you ship it? Were you responsible for shipping it?

GOOGIN: I don't remember. Two fellows I went to school with actually did all this. So it was sort of a spy system. [Laughter]

LARSON: I was responsible for shipping the original uranium nitrate out, on which they did the criticality experiments, but then it got into our production, a very orderly process.

GOOGIN: Right. They had little metal containers with lids that bolted down. They'd load it up, ship it out in safe geometry containers and all of that.

LARSON: The operations were made, actually, in gold bolts.

GOOGIN: Platinum.

LARSON: Gold and platinum equipment.

LARSON: Because that was inert to fluorides or HF and so on. So the reduction of the final product, to UF_4, it was fairly clean-cut. Very little problems, as I remember.

GOOGIN: You have to remember that you sent us these carbons all loaded up with anything else that might have been there, including the ash and the original carbons, and we took out the material and we calcined it and we burnt all the carbons down to nothing, and left some ashes. Then we leached all of this with nitric acid to make a nitrate solution, and it went through a purification, and we used at various times various things, but most of the time we just used peroxide precipitation to make the purifications, and then take that oxide and convert it to UO_2 and to UF_4 for shipment. So it depended exactly what time of history you talk about. But when we were running 30 percent feed into Y- 12, the processing of these things was really a pretty big business, and we created a whole new building known as 9212 to do it. During the war, that processing was done at the top story of a building known as 9206 and was sort of hidden away. Downstairs in 9206, they ran the Beta recycle, and upstairs in 9206 they ran the product.

KEIM: Mention here is made of uranyl nitrate. Occasionally reports would come through when "uranyl" would be embarrassingly misspelled. [Laughter]

LARSON: That's right. [Laughter]

GOOGIN: Right. [Laughter]

GOOGIN: So that the rumors that we didn't understand are exaggerated, because we operated the place safely all during the war with enriched material that could have gone critical if we'd made a mistake.

06:33:50:00

GOLDBERG: But was it mailed? Did a special courier come?

GOOGIN: Oh, a courier.

LARSON: Oh, yes.

GOOGIN: In fact, we had several buildings as intermediate transshipment points. You will find, buried out in the back hills of Oak Ridge, a farmhouse with a silo on it that looks like it's a farmhouse with a silo on it, but it wasn't. It was a storage and transshipment point with guns coming out of the top of the silo. Eventually, we built a special building in Y-12 as the product got bigger and bigger, to store it inside the Y-12 fence and ship directly from a special building, and kept what happened there separate from the rest of the facility.

GOLDBERG: At that point, coming down near the time when the raid was made on Hiroshima, did you know that you were sending the final product down to. . .

GOOGIN: Oh, yes. Oh, yes. It was the big campaign.

LARSON: That's right.

GOOGIN: Oh, Lord. We squeezed everything out of everything we could get our hands on.

LARSON: Every corner, everything else was scraped.

KEIM: General Groves used to call us together periodically for his morale talks. His favorite saying was, "The use of your product is in the offing." I remember that just as clear.

Then after that push in the early summer of 1945, yes, after that push of production had ended, I was having lunch with one of the Westinghouse men, and I just casually remarked, "Well, I expect the product is on its way."

And he looked at me and he said, "It's there." That was only a few days afterwards, after the production. He said, "It's there." And he said, "As you know, President [Harry S.] Truman is at Potsdam. By the time he gets back, it will be used." And I was surprised to get all that information from a Westinghouse salesman, really, that he knew.

GOLDBERG: That's amazing.

KEIM: He knew. He had the information.

GOOGIN: When the test shot went off, of course, we sort of recognized some of the cover stories in the newspaper. When you have an explosion, a small explosion in what was supposed to be a munitions dump out in New Mexico that lights up the whole Western United States, it's a little bit difficult for us to be logical about that conclusion. So we kind of suspected that things were going pretty well.

GOLDBERG: But you didn't know at that time whether that was uranium or plutonium?

GOOGIN: Well, some of us did, but I didn't know. At my level, we didn't know. Then they told us about how successful the test had been soon after it went off.

06:36:48:00

KEIM: Speaking of the Alamogordo test, it was interesting. We knew something important was going to happen about that time, and just a day or two after that test, E[rnest] O[rlando] Lawrence came to Y-12, and down in our conference room in the pilot plant, he had a little meeting. He was so optimistic and so different about all he talked about, what he was going to do after the war. So we knew something had happened.

LARSON: That's right. As a matter of fact, General Groves came down to Y-12 about that same time. Probably he and Lawrence were together, but anyhow, we gave him a rather small lunch, it might have been 25 or 30 people, of us. General Groves, with all the problems we had at Y-12, always had a sour face anytime he'd come around, but this time he was all smiles and said, "We are now assured of the success of our enterprise." We really couldn't quite understand, although we could guess, of course. He wouldn't carry it any further, but you could tell more from his face than you could from what he said. There was something really giving us that optimism.

06:38:18:00

GOLDBERG: Now let's turn to the day--that is, August sixth.

LARSON: Oh, yes.

GOLDBERG: What happened in Y-12?

GOOGIN: I wasn't there.

LARSON: You were out of town?

GOOGIN: After the big push, I said I probably could relax, so I decided to start my vacation. So the last week in July, the first week in August, I was on vacation, and I was in the Washington airport, waiting for an airplane, when I saw the newspaper headline. [Laughter]

KEIM: Well, I'll tell you. It found me in the pilot plant, all at once, in the morning, the announcement was made here over the PA system. The radio was turned on, and here the announcers were talking about Oak Ridge and what was being done in Oak Ridge, and things which, to all of us, had been deep secrets. I immediately got on the telephone and called my supervisor, and I said, "Hey, what's going on here? We're hearing things that we're not supposed to be talking about."

And he said, "You can talk about it now. It's all out."

I remember in the pilot plant, we just shut down. Ben Hall was superintendent of the pilot plant, and he said, "Come on, let's go home. I think our wives need us worse," because they were just learning for the first time what was going on.

LARSON: I think everybody has an interesting story, just like, "Where were you on Pearl Harbor Day?" type of story, you know. I can remember in the laboratory, one of the men burst into the office and said, "They dropped the bomb!"

I said, "Did it go off?" I mean, just unconscious. All the way through the war, I was afraid it was going to fizzle. I mean, I was very much afraid of the fizzle effect.

KEIM: This was the Uranium-235 bomb.

LARSON: That's right.

KEIM: It had never been tested.

GOOGIN: That was the easy one, though.

LARSON: But I never could quite be certain that they could get those pieces together fast enough so that it wouldn't just sort of fizzle. I think last night there was some discussion, somebody mentioned that they thought that the news came at night.

LIVINGSTON: That's not right.

LARSON: No, it was in the morning.

LIVINGSTON: I was in the mezzanine in 9204-3. I got a telephone call, I believe from my wife. She said, "It's on the radio that they've dropped the bomb in Hiroshima." Then pretty soon, the message started coming from all directions. I don't recall that we did anything very much, except we were kind of awestruck by it all. I wasn't aware of any particular reaction from people in the building. I might not remember that at this stage.

LARSON: I think a lot of us were sort of taken aback. Here we had not even mentioned the word or given any hint whatsoever, and then in a matter of seconds, the whole situation had turned.

GOOGIN: When I saw those newspaper pictures in Washington of the Alamogordo tests, pictures that they used for the announcement, I was really taken aback. Here it is!

06:42:10:00

BANIC: And they had the Smyth Report ready almost immediately after that.

KEIM: A mimeographed Smyth Report came out just within a day or two.

LARSON: I always lose things, but fortunately, I still have my mimeographed one. It's still available.

GOOGIN: I had a Smyth Report, but then I thought Senator [Albert Arnold] Gore needed it to read, and he's never sent it back. [Laughter]

BANIC: I must have been working evenings that week, because I know it was in the morning, because I was on my way to lunch, just getting off the bus on the way to lunch, prior to just going to work that afternoon. People in the cafeteria, of course, caused quite a stir, particularly because most people had no idea what it was. As I recall, I just went ahead and ate and went to work like I was supposed to, to continue the program.

GOOGIN: We didn't know whether that was going to be enough.

BANIC: That's right.

KEIM: I said we went home, Ben Hall and I. We went home a little before lunch, had a leisurely lunch, and came back afterwards.

GOLDBERG: You have your copy of the Smyth Report with you?

06:43:23:00 [Banic places his bound copy of the Smyth Report on the table]

BANIC: Yes, I brought it with me here.

GOOGIN: I've got to write another letter to Senator Gore.

06:43:31:00 [Banic opens the report]

BANIC: It's kind of worn. It's been around a while, you see, but it means a lot. To tell you how far back it goes, the address is our friends', George Banic, Jr., RD #5, Greenville, Pennsylvania. That's how temporary we thought that job in Oak Ridge was going to be. [Laughter]

KEIM: You were here for the duration.

BANIC: Yes. It was real interesting, as I say, when it first came out. It means a lot to me. Dr. Keim has one also. Those of us who were in the pilot plant at the time, we all get the Smyth Report, naturally, and those of us connected with it autographed it. Everybody has a book, you know, a carry-over from the old school days. It's interesting to read. Years and years went by that I hadn't even looked at it, and the time I brought it out was when Stephane Grouett, who wrote The Manhattan Project, came to visit and interview us for information for his book. I got it out, and I said, "Well, golly," and I started reading through there. I see here's Dr. Keim's name, Dr. Larson's wife, Jane, she was a mathematician, she was there when I first came, and Dr. Livingston was gone. He was already out of the pilot plant and into the Beta production building. Those people that were connected with the product, Bob Compton, Chauncey Starr, who's gone on to General Dynamics and up from there. One thing is really interesting here. We were pretty young at the time. Murray Beevis, who was one of our electrical engineers, and one of the fellows who's still in Oak Ridge who's no longer with this program, in fact, he's retired now, a fellow by the name of Dismu, and he got married, so we all went to the wedding. For some reason or other, Murray Beevis' father was in town.

KEIM: He was a professor at Ohio State [University].

BANIC: He was the President of Ohio State.

KEIM: President. That's right.

06:45:30:00 [Close-up of signatures on the inside cover of Banic's copy of the Smyth report]

BANIC: We were talking there and just making conversation, and one of the fellows went up to him. It's amazing how you can take a bunch of people and just sit and talk forever, but you put them in a Sunday suit and send them to a wedding or something, and you can't even think of their names, you know, and stand around. But anyway, we were making conversation. One of the fellows went up to him and said, "I understand you're in the schoolteaching business."

"Well, I guess you might say that. I'm President of Ohio State." [Laughter]

LARSON: I just happened to notice there the name Anne Bishop.

BANIC: That's right.

LARSON: She went on to get her M.D. degree and now is head of the pediatrics department at Johns Hopkins.

GOLDBERG: Everybody that was there signed each other's books.

BANIC: As much as we could. It's an interesting memento.

GOLDBERG: It sure is. Let's show the title page.

06:46:22:00 [Close-up of the title page of the Smyth Report]

KEIM: We were talking about Chauncey Starr. Chauncey was in the pilot plant after the Berkeley group moved in, Chauncey Starr and Ben Hall. Chauncey and I used to sit and argue all day long. Then about the time the day was over, I'd find we were agreeing. I said to Chauncey, "Why have we argued all day? Because we agree!"

And he said, "It's my policy to get a debate going. I agreed with you all day long, but I wanted to bring out any argument, the facts." That was one of his techniques. It's a good technique, really.

GOOGIN: There's always been a lot of discussion about how much value that book is to other people.

GOLDBERG: The Smyth Report.

GOOGIN: Yes.

GOLDBERG: Were you surprised at what was in it?

GOOGIN: Of course, I was surprised. I was flabbergasted.

KEIM: Because all the sites are in there.

GOOGIN: Yes. So some of our folks, over the years, have asked the Soviets how much that might have been useful to them. Some of them have said, "Invaluable," but from the point of view of decision-making, not necessarily the technology. They've said, "You could always sell if the Americans did it that way, and that's the way to do it."

GOLDBERG: It might not be accurate, purposefully accurate.

GOOGIN: As far as the techniques go in there, of course, they're accurate, but they're not in enough detail for you to solve the technical problem. But they said that the decision-making that's represented there was very useful for them making their own decisions.

06:48:12:00

KEIM: The fact that that book and the mimeographed copy, especially, came out so quickly made an impression on me, because it indicated how well planned the whole project had been, every aspect. That wasn't put together in just a few days.

GOOGIN: Well, one thing that impressed me is we actually sat down and wrote a history as best we could. I mean, they haven't done that since. [Laughter]

LARSON: That was an amazing one. I had occasion to introduce [Henry DeWolf] Smyth--I gave a speech down at Oak Ridge once, and I said, "I regard your book as being probably the most significant book in science history since The Origin of Species." I think it certainly was a significant book. I don't know of anything else that compares with it.

GOLDBERG: Anybody want to add some last words? Thank you very much.

GOOGIN: You're welcome.

LARSON: Okay. Very good.

GOOGIN: Now we get a taxi?

LARSON: No, we have to walk. [Laughter]

BANIC: If they would collect their toys and go home, huh?

GOOGIN: Right.

GOLDBERG: You guys ought to be tired.

KEIM: I'm going to Oak Ridge. I have room.

BANIC: Well, I have to wait for Ed to come, because he's going to take this stuff back to Y-12.

GOOGIN: I can go back with you, C.P., if you're going that way. You're going to leave me at Y-12 gate?

KEIM: No, I can't get to Y-12.

GOOGIN: You can get into it.

KEIM: I can leave you at Orkney Lane, Orkney Circle.

GOOGIN: [Laughter] I better wait for Ed. [Laughter]

06:50:02:00 [End of Session Six]

[End of U-Matic Tape 4 of 4]

[End of VHS Tape 2 of 2]