# Walter Loveland Oral History Interview, July 22, 2015

### Title
“Of Glenn Seaborg and Super Heavy Elements: A Nuclear Chemist Looks Back”

### Date
July 22, 2015

### Location
Valley Library, Oregon State University.

### Summary
In the interview, Loveland discusses his colorful family background and upbringing in blue-collar suburban Chicago. He also describes his earliest interests in science, his path through undergraduate and graduate studies, and those who influenced him as he made his way through his higher education, including his contacts with luminaries like Charles Coryell and John Huizenga.

From there, Loveland begins to reflect on his long association with both Oregon State University and the University of California, Berkeley. In so doing, he shares his memories of his initial impressions of OSU and Corvallis, his first contacts with Glenn Seaborg, a few initial research experiences in research, and his impressions of Seaborg as a personality. He likewise recounts his interactions with Linus Pauling as well as major figures in nuclear science at OSU, Chih Wang, John Ringle and Dale Trout among them.

Loveland next recounts his memories of the Radiation Biology program at OSU; discusses the life and career of a former student, Sister Mary Joseph Bouchard; and comments on the climate for women and people of color in the sciences at OSU and in the community at large.

Loveland's research is the next focus of the interview. In this he provides an overview of his work with super-heavy ions while also describing his research collaborations and the frequent trips to Berkeley that these collaborations demanded. He also recounts his interactions with OSU's Campus Radiation Safety Committee, his disinterest in working at the Hanford Nuclear Site, his experience of co-authoring two books with Glenn Seaborg, and hindrances to scientific advancement that he has noted as a result of denials of security clearance.

As the session nears its end, Loveland provides his thoughts on NuScale Power as well as the changes that he has seen in Chemistry and Nuclear Science at OSU. The interview concludes with Loveland's perspective on the direction that OSU is headed as it nears its 150th birthday.

Please note that this interview includes remembrances of a culture of racism and the use of racist, derogatory language toward African Americans, including the N word.

### Interviewee
Walter Loveland

### Interviewers
Chris Petersen, Linda Richards

**Website**
http://scarc.library.oregonstate.edu/oh150/loveland/

## Transcript

**Chris Petersen:** Okay, today is July 22nd, 2015 and we are in the Valley Library with Dr. Walter Loveland, radiation chemist who has a very long association with Oregon State University, closing on fifty years. And I'm joined today by Linda Richards, historian here at OSU as well, with specialty in Nuclear History. And we're going to talk to Dr. Loveland about his career at OSU for sure, but we'd like to begin at the beginning. So, you were born in suburban Chicago, is that correct?

**Walter Loveland:** Actually born physically in the city of Chicago, but my parents took me home to their first home, which was in Berwyn, Illinois.

**CP:** Was that suburban Chicago, then?

**WL:** A blue collar suburb of Chicago. It's really survived as a—it was then a Czech enclave, and now it's Hispanic. It went through an Italian phase, whatever. It's called the City of Homes; it's mostly just no factories, just homes, blue collar, everybody works in a factory. Sara Paretsky, the mystery writer, once characterized Berwyn rather negatively. She said Berwyn was a place where for a dollar you could get anybody killed.

**Linda Richards:** Oh, wow.

**WL:** So, I don't know if that was true, but whatever, yeah.

**CP:** And this is where you were raised?

**WL:** Yeah, yeah.

**LR:** Was it rough?

**WL:** The neighboring town, Cicero, had a bigger reputation from prohibition, et cetera. There were things that went on that I didn't know about other than I found out later. You would go by a certain restaurant on the way back from high school and later you'd read that that's where people were being tortured and so forth, while you were walking by the door, and so forth. There was no question that it was Chicago-tough, in the sense that when I was a small boy, I remember once my father dragged me along one night, he says "come on, you're coming with me," and I "oh, okay." So, we went over to a judge's house and he bribed the judge to get out of a traffic ticket; he said "this is the way it's handled. You don't go"—I mean you don't—Chicago at that time, and it may still be, has something called the personal property tax, and nobody pays it. And with nobody paying it, they can't enforce it. But that was the standard trick in those areas, was to, if you—my father said "you know I can't take time off work to go to a court, to pay this sort of thing," he says, "pay the judge. That's the way it's done."

My mother was a poll judge. She was a republican poll judge in Cook County, which is a tough place, and she showed me tricks where she would put a lead underneath her fingernail so she could wipe it across ballots and invalidate them. So yeah, there were some tough aspects, but I mean I enjoyed the place. It was prairie; played baseball from eight o'clock in the morning till whenever the parents summoned me back in the house, whatever.

**CP:** What was your parents' backgrounds?

**WL:** Actually, I'm finding out some more about them through a student here who apparently has access to Ancestry.com, and so I found out for the first time my father had a first wife, that I knew nothing about. But anyway, he was older. He was around in his sixties when I was born, and my mother was, she had come from a place called Prairie du Chien, Wisconsin which is a town on the Mississippi River in mid-Wisconsin. His parents had ripped him out of school when he was in eighth grade because they said that was enough schooling. And so, he ran away from home then and worked his way downriver. He told me at one point that they would grow cultured pearls on the Mississippi River and he would be—or his job was to collect them, go diving in the Mississippi to collect them. His parents had used him, or his father had used him, because he was small and they could drop him down the wells. His father was a well driller and they could drop the kid down the well, and that wasn't what he wanted to do, so he went on.

He ended up at Rock Island Arsenal in Illinois and worked there first as a janitor and then worked up to being a machinist. Then he came to Chicago and then he worked for organized crime. He was a drug runner—not drug—a whiskey runner for organized crime at that point in Chicago, Capone. And so, he and his friend were driving trucks to Kentucky back and forth, bring the booze into Chicago from Kentucky and so forth like that. And then apparently he had a first wife at that point that I knew nothing about, and I don't know if my mother ever knew either. So anyway, he met my mother; she was a farm girl from Wisconsin, Eau Claire area.

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There were six surviving children of her parents, and my mother tells it maybe there were four or five that were killed on the farm, because they had large families. And my father, when I did this ancestor thing, probably had ten or fifteen brothers or sisters, but he disowned them all because at a moderately early point in his youth, when he was in his twenties, his mother died and all of his siblings in Prairie du Chien put her into a pauper's grave, and then just completely ignored him. So, he came back to Prairie du Chien from Chicago and buried her in her appropriate place in a cemetery, and he would never talk to his brothers and sisters after that. He said no, I'm done. I have no connection with these guys. And so, I never knew them.

My mother's parents, she had three sisters that I knew, one brother that, in the way of large families, she wanted nothing to do with him, and then one brother who had lived into his teens and was killed in a threshing accident on the farm. But it was amusing to me about the brother who she didn't want any time for, because he was a drunk. He was a town drunk. And so, I remember we went up to Eau Claire one time, and walking down the street and this guy, you know, "Annie, Annie, good to see you"; [in gruff voice]: "no, I don't want to talk to you." My father was rolling with laughter. He thought this was the funniest, whatever. And one time, late, I drove my aunts and my mother someplace in that area, Eau Claire, Chippewa Falls, whatever, and one of them said "oh, Henry, Henry's living over there, do you want to go and visit him?" and [in gruff voice]: "no, no, all of us, no." So, they would just disown. It's a different style that large families, they make a judgement; you're a drunk, you're a drunk, they don't want anything to do with you.

Anyway, I had a I think—coming more to the point, I had a reasonably good education. Public schools of Illinois at that time were excellent. And I think we got a really terrific education. I went to a high school that had six thousand students in it, and so we went in shifts. I was intending, I mean if all things had worked out, to be an electrician. So, most of the students out of that high school wanted to get jobs right out of high school, and so I was a vocational technician student for getting an electrician's certificate. And it was only about my junior year or so when a guidance counselor said—or no, it was actually senior year—guidance counselor says "you know, maybe you could try to get into college. It's not so likely" and whatever, "so you could probably try," and he says "well, it's sort of strategy" and I said "well, how much is strategy?" and he said "well, we'll start applying to eastern schools, because they have the earliest deadlines, and then we'll go to the Midwestern, we'll go to the western schools and we'll try to figure out some way to pay," because I think it was ten dollars for an application, and that was pretty big money.

So, I applied initially to MIT, Union College in New York, Rensselaer, which is also in New York, and Carnegie-Mellon. And I had done well on the Westinghouse Science Talent Search and I got accepted to all these places. And I said "well, you know, I'm going to MIT." There was no choice. And so, they were going to pay my way, and so that's how I ended up at MIT. It was just the first set of colleges I applied to. So, yeah.

**CP:** Did you have an interest, I mean a strong interest, in science as a kid?

**WL:** I wanted—I thought I wanted to be an electrician, and so when I enrolled at MIT I said I wanted to be an electrical engineer, because they had a good electrical engineering curriculum. And I decided, because with the high school I'd gone to was six thousand students and it was built for more like fifteen hundred, we went to school in shifts and I was able to take courses at an attached community college. And so, I had taken general chemistry and so I just started MIT in organic chemistry, and then my sophomore year at MIT I started taking graduate classes in chemistry, and I just said well, if I'm doing okay in chemistry, why not. Why not stick with it, whatever.

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**CP:** Sounds like it came pretty natural to you.

**WL:** Eh, the first courses of organic chemistry I remember memorizing a lot of stuff and thinking oh, this is boring. But then, at MIT I met a man who really inspired my career, and that was a guy named Charles Coryell. Charles Coryell was one of the dominant figures of the nuclear chemistry field, and we only—he only lectured part of the term. It was another man, John Ervin who lectured the rest of the term, but Coryell was the most inspirational person you could think about.

I know one prominent nuclear chemist, Larry Glendenin from Argonne, had worked for Coryell during the Manhattan Project, and he said "oh here was a"—Glendenin was saying "okay, I was an analytical chemist and I came in and I met this crazy guy who had his shirttail, always shirttails flopping, never put into his pants, jumping up and down, jumped up on the desktop, started yelling," and Larry said something like "I just asked him 'when can I start?' I didn't ask about money, I didn't ask anything else," he said "when can I start?" and then Coryell said "today, of course." And so that, this was a guy who inspired people.

It turns out a year or two ago was his hundredth birthday anniversary. He died much earlier. But I had the opportunity for the American Chemical Society of arranging a symposium in honor of him and Seaborg, because they were both born the same year. They both would have been a hundred years old, had they been alive. And it turns out they were both basically born, or not born, but raised in Los Angeles. And so, we had fun exploring it and I met Julie Coryell, who was Coryell's daughter, one of his daughters, and I didn't really know, but they explained to me that part of this tremendous appeal he had for people, he was a manic depressant. If you caught him when he was high, it was unbelievably good, and we never saw the low part of his life. That wasn't something we saw.

So, that's how I got into nuclear chemistry. And I actually worked with a man named Glen Gordon as an undergraduate at MIT, and we sat down sometime during my senior year and he said "here, here's a list of places to apply to for grad school," and he said "I think you're bent would be best served—don't go—you make applications to Yale and Columbia and stuff like this and you get in," he said, "I think you'd be smarter to do something different." He said "go to Seattle, because there they have a place where chemists and physicists work together, and that's going to help you, your interests are such, and it's unusual to have a place where the chemistry department and physics department are kind of intertwined and they get along with one another and they run a laboratory, and I thought it was a great idea for no other reason I was sick of being in an all-male school. And so, I want to go to school where there are women. So, we ended up at University of Washington in Seattle.

**CP:** Had you decided by this point that you wanted to pursue an academic career?

**WL:** No. Not really. No, I think what I was—I went to grad school at the University of Washington; this was really an unusual time, because we were all well-supported. Oh, okay two hundred dollars a month, but that was a great support. We didn't have any things to do. Seattle is a great place if you have no money. I can recall having once a payday party where we took the two hundred or whatever dollars we got paid and turned it into dollar bills and put it on the ceiling in these various ropes that were across the ceiling, just attached it and everyone played around. No one worried about it, whatever. We were just poor and we could do anything we wanted to at that time.

We could, my wife and I—I met a girl, a woman in my class, the first class I T.A.'d, and I asked her out, we started dating and pretty soon the guy who was in charge of the course, and also my major professor, said "you're not grading her papers anymore, son." So, whatever. Anyway, we got married after my first year there and we were married in Seattle and we would, if a day like this had occurred, we'd go over to the arboretum, rent a canoe, canoe out into the UW Arboretum, take a dinner, eat there, canoe back. That was it, it was just a different time.

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There was an institution in Seattle called the Maple Inn Tavern, or MIT, and so the grad students would spend part of the night at the Maple Inn Tavern and say "oh yeah, I'm working at MIT tonight, yeah." So anyway, it was—but you were interested in science but I think the academic part was not at all front and center. So, I finished up in about, I don't know, I guess formally it was three years or something like that, but it was actually longer as far as the time went.

**CP:** What was your research topic during this time?

**WL:** I did research on a funny topic; it was the emission of long-range alpha particles in fission. The queer part about it was there had been a student ahead of me, Joe Coleman, who eventually went on to a nice career in the National Bureau of Standards. I'm sorry, Bell Lab, and he had stumbled into some things, but his thesis didn't make much sense. And so, it was assigned to me as the problem, to solve this problem, and the problem was that we were studying reactions induced by alpha particles and looking for alpha particles, and the ability to distinguish between accelerated alpha particles that we brought into the reaction and those emitted in the reaction was a tough one, a really tough one.

And so, we finally managed to do a certain amount of work and make—in those days there was not this flavor of let's publish as soon as possible, at least publishable units and stuff like that. So, you gathered up all your stuff into this thesis and we published the thesis, which had many different parts to it. And so, I did that and I was interested, and so I said okay, what can I do for a living? Well, one of the things I had done, I had a lot of factory jobs and stuff like that earlier on while living in Chicago, but towards the end of my time in Chicago before I went to Seattle I had spent one summer at Argonne working as a student assistant, and I thought that was pretty nice.

And so, my mother still lived in Chicago, and so we went to a post-doc in Chicago, and really a very good scientist, John Huizinga, who's now dead, but he was a National Academy member and so forth, and went ahead and spent time there, and I really thought that was wonderful. Post-doc was the greatest time because, along with grad school, I had no responsibilities. All I had to do was do science. You're—as a post-doc you were assigned maybe one or two problems that you had to spend some time on, but anything else you want to do is just fine. You could do anything you wanted. And the same was somewhat true at the University of Washington too. You could—you had your thesis topic that you pursued, but you got involved in other experiments. The accelerators at the University of Washington were run by the students, so that was a big deal. And I would have been happy to stay on at Argonne forever. As I've said to people, as a high school student in Chicago that would have been the greatest job of my life, would have been technician at Argonne.

My wife hated Chicago. Just absolutely hated the place. She was a Seattle girl, had really never been outside Seattle and she just couldn't stand the Midwest. And I think it was the first winter that we got there, it was thirty-five below zero the first day we moved into a small house in Carol Stream, Illinois, and [gasps] that was horrible. And then the gas line on the car froze on top of a railroad track and we had to push the car off and so forth before the train came through and whacked us, and oh man, she didn't have any good times for it. Then, during that first time, I think it was we bought a new car I think in the spring of that first year, and we didn't have it very long and I got hit by a train. And I survived, that was the tremendous thing. I was driving to work on a Saturday morning, which is another reason you don't go to work on Saturdays, but I still did, and apparently the crossing gates didn't work and I didn't see that it was an intersection. I was just focused on driving ahead, so I just drove up in front of a train. There were bushes, trees all along the track, so you couldn't see the train, and the only thing that would have clued you was the crossing gates, which didn't work. And so, okay, and the only thing I remember of that was people loading me in the ambulance. So, I was fine. But whatever.

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Anyway, my wife said "I'm leaving you or whatever, I'm going to back to Seattle, I want to go back to Seattle." So, I went okay, well how do I find—what do I do, I have to find a job. Well, there's no technical jobs in the northwest at that point for a nuclear person. Maybe there would have been something at Hanford, but that wasn't what I was interested. So, and really naïve thing: I wrote letters to the chairs of all the chemistry departments of the Pacific Northwest and I said "hey, I've got this background, you want to have me?" and one place, OSU, wrote back, and Chih Wang wrote back and he said "yeah, we can't give you a faculty job right now, but we'll give you a job that will be a faculty job very quickly, and would you consider coming out here?"

And so, we went out, I went on an interview here. And the job was to work as a post-doc for Roman Schmitt. And so, fine. I didn't like the idea of a second post-doc at that point very much, but okay. Roman was a very nice man and a very good scientist and he had some good things to do. And then—so I had a funny title, a research professor or something like this, and the dean of science then was not happy about this and he stripped the appointment away, said "you're a post-doc, you're not any kind of research professor," whatever. He took that away and I said "I'm leaving, I'm going someplace else." And so, I let Chih Wang and Roman know that I was looking for a job now. I wasn't going to stay as a post-doc there. It was not what I came for, and then all of the sudden the faculty appointment developed. And there was interviews and so forth, but then I got a faculty job fairly quickly. I think I spent about a year as Roman's post-doc and then I was a faculty member in chemistry.

I was naïve, I had no understanding of what goes on. The chair of the Chemistry Department was a guy named Bert Christensen. He called me up, since I was at the Radiation Center, and said "we decided to hire you and we're going to pay you such and such," and I said "oh, that's good, thanks," and he says "you don't want to talk?" "No, sounds fine, salary sounds reasonable, I don't know." Today I would have been negotiating for umpteen million dollars or something like this, but at that time no, that was just it came, that was it.

**CP:** What was your sort of initial impressions of OSU and of Corvallis at that time? This must be 1968.

**WL:** Yeah, it was a funny place. There were some really good people in chemistry at that time. Jack Decius was probably the smartest guy. Maybe one of the smartest guys I ever met was in chemistry. There were guys like Ken Hedberg, who's also very good, and there were a certain number of people; Roman was very good, Chih Wang was running a very impressive operation at the Radiation Center. As I later found out, he had gathered this money from various sources to build this building and this new laboratory and then he had decided, smartly, to stock it, because there were no nuclear people in this campus at that point. So, he reached out all around the country and brought people in like Roman, he brought him from Gulf General Atomic; he brought in Malcolm Daniels from Puerto Rico, stuff like this.

And so, he built a staff one by one to populate this center, and it was a first-rate operation. It's quite different than it is now in the sense that it involved biologists; one wing was strictly biology, one wing was chemistry, and it was really a reduced population of nuclear engineers. There was nuclear engineering, but it wasn't the dominant focus. And then over time some things happened at the radiation center and in the department of general science, which was the host for the Radiation Biology Program, and that program more or less collapsed. Individuals weren't given tenure, things happened, and so that whole wing of the Radiation Center more or less changed.

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You still see the remnants of it today; there are operating rooms, there are places where large animal carcasses could be stored, and actually were stored; there are all kinds of laboratories where there were rats, stuff like this that would occasionally escape and run up and down the corridors, and there were sheep. I remember being mortified; I think one of the first summers I was here I was enticed by Chih Wang to keep offering this course of his, Radiotracer Methods, and so I was lecturing and as I sort of listen in the back; "Baaaa! Baaaa!" sheep being led down the corridor of the Radiation Center to be experimented on at the other end of the building and "ohh, this is a cow college. This is really a cow college."

You know, I think that was it. I was looking for a way to do what I wanted to do, which was hard because the Radiation Center was not well set up for that. It had a reactor but a reactor was a passé research instrument at that time. There wasn't a lot of things you could do with a nuclear reactor beside activation houses. So, Roman had a very excellent program in trace element abundances in meteorites, and then he branched out to other things, but that's, the interest has to be in the other thing that you're analyzing, not in the technique. So, the nuclear techniques wasn't a—I fooled around a bit using neutrons from a reactor to do some things. Ah, so-so, whatever. I became interested in doing neutron activation analysis for environmental samples. We did some experiments, both trace element abundances in the water and air and then we also developed tracer techniques that used rare earths, which have high neutron capture cross sections, to do tracing. And we did a certain amount of that stuff.

After about—I got tenure and then after sometime later, not too long, I wanted to go on sabbatical leave. And I had a choice, and was I going to pursue the environmental aspects of my career, I better go and really associate with somebody good, or was there still a chance that I could do nuclear chemistry, which is really what I wanted to do. So, I wrote to a prominent scientist at Caltech and he said "come along if you want to do the environmental chemistry, I'd be happy to have you for a year." And I wrote to Berkeley and Seaborg and got a hand-signed letter back saying "yeah, we'll have you. We'll pay you." Good, that's fine.

So, and I found out later, and I sort of knew, Seaborg was a very careful man. He talked to everybody I ever knew and said "is this guy worth having? Is he going to be a trouble-maker, a bombshell, whatever?" and "no, no, no, he's fine." So, I went down there. I remember my first day in Berkeley I marched in, I was staying in a fleabag motel on the university and I thought wow, you better show up today, this is the first day of Berkeley, Seaborg knows you're there. So, I walked up to his office and we chatted a little bit, and then he says "oh, I want to take you down to the lab and meet the guys down in the lab," and I go "great."

So, I'm now introduced to various people who became actually lifelong friends, and then he said "oh, your first experiment is tonight." "Really?" and "yeah, you're running on the Bevelac, we're going to—you are going to radiate uranium tonight on the Bevelac." "Oh, really?" And I thought to myself this is terrible, how can I do this, and one of the guys there, Roland Otto said, pushed me aside and says "don't worry about it, we got it taken—we got it under control," and he says "what we're going to do is we're going to take a bottle of uranium and cut away the cap and run this relativistic heavy ion beam into the bottle of uranium, and we'll look at the products." And rather than assure me, that made me think well, this is even worse, because this is the lousiest science you could possibly imagine. Just taking a bottle off the shelf and running a relativistic heavy ion beam into it, it's dumb, you don't do experiments that way.

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So, we did and we published lots of papers on that and it all worked out. But as I say, when I first, I just had the fleabag to stay in, the motel, and I didn't know where I was going to be in Berkeley and I had an experiment that night. But that was Seaborg's style; get everybody engaged right away, don't fool around, don't waste any time. So, yeah.

**LR:** Good thing you went to his office, or you would have missed your first experiment.

**WL:** Yeah, well that's right, and at the same time it turns out we got along well. He was the world's most avid hiker and I liked to go hiking and so he found that out and so we would go, and at age sixty-five he was simply remarkable. I was thirty and he was sixty-five. And we used to go to seminars on campus and Dave Morrissey and I would up the hill back. There's a stairs that's usually called now "the Seaborg stairs" that went from his office near the top of the hill down to the Chemistry Department. They've more or less been barricaded I think, because they're considered dangerous and it's too steep and all this, but we'd come up to the top here and Dave and I would get off, walk away from the stairs right near Seaborg's office, and we'd excuse ourselves and go over to the john and towel off, because we were covered with sweat. Seaborg was in a suit and tie and didn't break a sweat at all. He was really physically remarkable.

It finally caught up with him; he died at an ACS meeting in Boston, effectively died, because he was so adamant about getting exercise. He walked up and down a set of back stairs, had a stroke while he was walking up and down several flights of stairs and then no one saw, because it was in a back portion of, I don't know if it was at a hotel or the convention center. That really hurt him, and so then basically he came back to California. There were lots of comments about people having meaningful conversations with him at that point. I don't believe any of them, I don't think they're true. Al Ghiorso, who we knew fairly well, said he went to see Seaborg a couple times and he said there was nothing there, there was no recognition of anybody, so yeah.

**CP:** He's somebody that you obviously knew well and he was very important to your career. I'm interested in knowing a little bit more about him just as a man.

**WL:** Seaborg?

**CP:** Yeah.

**WL:** He was a remark—to me, I met him at age sixty-five; he was a remarkable person as an administrator. I've never seen administrators good as he was. He was completely even tempered. He knew details about things that at times he would express some anger about things that were going on, but for the most part he was absolutely calm, absolutely that way. I had heard that when it was earlier in his career when he was a Manhattan Project director, when he was first doing Nobel Laureate type research in Berkeley, he was much tougher. Stories of a grad student went and got married and he didn't ask Seaborg's permission and Seaborg fired him. But he didn't do that, and so forth, things of that character.

But I knew him as very even-handed. He had the rare ability of actually following this oft-quoted idea that you never touch a piece of paper more than once. Pick it up, make a decision, think about it and then leave it alone, don't go back and agonize over it. Just do it once. And that was a remarkable administrative trait, that you didn't go back and constantly circulate around something; you actually did it. He had a feel for chemistry that was remarkable. He would attribute that, actually, to a good friend of his, Stan Thompson, a [in German], this feel for chemistry.

I remember Dave Morrissey and I were doing experiments that year, first sabbatical year I spent at Berkeley, and we would irradiate all sorts of metal foils and Seaborg said "I want you to irradiate tantalum." Yeah, okay. Well, alright." No

one was irradiating tantalum. All the rest of the world was gold, uranium, stuff like this. Well, we irradiated tantalum. And Seaborg came around three times a day every day. First thing in the morning he came to the lab and he said "what did you do for me last night?" come around at noon, sometimes go to lunch with us; "what have you done for me this morning?" five o'clock came by; "what are you going to do for me tonight?" Those are his words, "what are you going to do for me tonight?"

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Saturday mornings he and his wife would show up for football, Cal football, big whatchamacallit, supporter of Cal athletics. He'd been the whatever, the Pac-12 or 10 conference was there, he was the Cal representative to that organization and he was absolutely devoted to sports. He was very proud of the fact that his name, Seaborg, was an anagram for "go Bears." So, whenever he had—he had been the chancellor at Berkeley, so whenever he walked into the stadium they would play "Hail the Chancellor" and so forth, the band, whatever. But anyway, he and his wife would stop by and he'd just say to us "what are you going to do for me this afternoon?" And "going to the game" was not the right answer [laughs]. So, and he'd come back after the game, and he said "what's happened during the game?" and I'd say "well, I heard some cheering out the window," and he said "that wasn't important," and he would then simply ask what had happened in the lab.

So, he knew all the time. He had these 3x5 index cards that he constantly carried and wrote notes. They were as—I interrupted the story about our tantalum experiment, because every day he'd come by and when he'd come by he'd haul out the 3x5 cards and write down what was going on. Well, Morrissey and I would offer all kinds of excuses, nonsense excuses as to why we weren't analyzing this tantalum data, and they'd all go on these 3x5 cards. Later we found out, unfortunately, they all went into his memoirs. It was unfiltered memoirs; it was all these 3x5 cards, and so you could—all this nonsense we would hand him went into the 3x5 cards, went into the memoirs. So finally, with respect to the tantalum, I said to Dave "we're never going to get around this, he's never going to forget the tantalum, let's go ahead and analyze it."

And so I did; it turns out it was remarkable. It was the easiest experiments we'd ever done, everything fell together extremely well and we were never sure how we figured that out, that this was the remarkable material that when you did the kinds of experiments we were doing, the answers fell right out. They weren't hard to get; they were easy and you made significant progress much faster with this foil than you would with gold or uranium or anything else. It was just—he'd had another student many years ago looking at tantalum for other reasons, but we really thought he just had a feel, just had a feel for chemistry, and you saw this, and a lot of times he would have problems that he would develop for people, and he'd suggest something that yeah, and it's right.

And there's a picture of him, an archival picture from Berkeley where he's making some gestures and you can see in his handwriting on the board is a nuclear reaction written. That turns out to be one of the most important nuclear reactions ever in the discovery of heavy elements. And at that time no one knew it, but it was in his handwriting on the board. So, he had a feel for what was important. He was a—I really enjoyed it, I enjoyed all the times I spent with him. He certainly helped my career tremendously. It's very daunting when you're in his office talking to him and it's Senator Kennedy on the phone interrupting and so forth like this. And he knew all the presidents of the United States. He would get calls from the White House, stuff like that, so yeah, it was a big deal. He was remarkable in that sense.

**LR:** Well, one of the things I had wanted to ask, if you remember the—I'm not sure if it's the American Chemical Society anniversary or what it was, but Seaborg, when he was president of the American Chemical Society, didn't he have Kennedy come and speak to the group and it was—

**WL:** It could have been, he was very good at trying to get these guys. He was proud, I think he was the service—in the service of eleven presidents that he had actually served as a science advisor for, and that story would probably be right, because Seaborg had been Kennedy's secretary of energy at the time, or head of the atomic energy commission. So yeah, I can believe it, and he—

[0:40:09]

**LR:** Yeah, they said ten thousand people came in San Francisco.

**WL:** Yeah.

**LR:** And I just wondered, wondered if you were there, or...

**WL:** No, I wouldn't have known Seaborg. I first met Seaborg in 1976 where Kennedy was long gone. So, he had Kennedy out, there are pictures of Kennedy in Berkeley in effect christening what's called the 88-Inch Cyclotron. So, he got the president to come out and do that, and that was something important. His probably best relationship he had with any president with was with Lyndon Johnson. He apparently had Johnson's full trust and he would say that Johnson would invite him to the ranch to just have conversations at the ranch. And he—I don't know if they ever went shooting or whatever on the ranch, but he got along well enough with Johnson and so forth, and he apparently would try at times to mimic some of the things—this is Seaborg—that he had picked up from the politicians.

The first thing I noticed when I met him was the grab [mimics grabbing a person at his side with one arm], the political grab that brings you next to a politician. They all know that. And he's tall and most good politicians are tall, so they stick out in a crowd, and he knew those tricks. And while I was in Berkeley, every so often if we'd go hiking, he would do a Lyndon-like trick and he'd say "Walt, why don't you come over to the house tonight, we'll have root beer." So, we'd sit out on the porch, much as he had sat out on a porch at Johnson's ranch in Texas, have root beer. And that's what Johnson would do for him. So yeah.

**LR:** So, on a more serious note, in a way, I wondered if he ever talked to you about how he felt about nuclear weapons.

**WL:** No, formally not. We used to go to lunch with him; we pushed him very hard on how Israel got the bomb and he was very tight-mouthed about that. He did not want—it was pretty obvious that we'd given Israel a lot of the information; he wouldn't fess up to it. He just kept that silent. I think that he probably had the same feelings of a lot of people in that era, that this was a fast way to end World War II. And a number of people, I mean you talk to anybody who was in the military in World War II, they were very happy that the war ended, and the probable loss of life in an invasion of Japan would have been horrible. And so, use of that weapon—I don't think he was ever really in any way feeling bad about the use of nuclear weapons in Japan. He did have a period where he didn't go to some scientific meetings, and that concerned what was happening to Oppenheimer, stuff like that, but directly he never spoke of it.

The things that when I met him, which was later in life, he was very angered by security aspects. So, a lot of his memoirs, which he really thought very highly, of were seized and taken away from him. In his house he had books that he had written, diaries, he was a diarist. Every day he'd write long passages in the diary. A lot of them were not so important, like his golf scores and stuff like that, but a lot of things, he wanted to record things, and he—they were all seized by the government, and he was very angry about that. I think he was a gung-ho guy, straight arrow when it came to nuclear policy, nuclear weapons, whatever. He was proud of his role in negotiating the Comprehensive Test-Ban Treaty, because he was the appropriate person of the Atomic Energy Commission when the Comprehensive Test-Ban Treaty was organized and then brought to—and he was proud of that. He thought that was a major accomplishment and he talked a lot about the negotiations with the Russians, who are very tough guys. But I don't think he ever felt—well, he certainly supported the idea of Comprehensive Test-Ban Treaty and the stepping down of nuclear arsenal, yeah.

**CP:** I have some people I want to ask you about, and then I know that Linda's got several questions that she wants to ask, and we'll kind of turn it over to her for a little while. One person who is connected with both Seaborg and Charles Coryell is Linus Pauling. Did you ever cross paths with Pauling?

**WL:** Only when he visited Oregon State. He was a dynamic force. He used to come back periodically. I think the campus here were very smart, as I'd been told, basically, to ask for his wife's memoirs and his wife's papers first. That was the thing which was a smart thing to do. The faint impressions I had of Pauling was he was a physical vital force. I was once in a meeting in Boston where he was speaking and he had attracted a huge audience and they clogged the main corridor down to the front of the room, and he just [mimics elbowing people out of the way] pounded them out of the way to get down to the front.

[0:45:49]

When they had Pauling awards here he would come and he'd sit on the front row, and the Pauling medal here is in some ways thought of as an audition for a Nobel Prize journey. So, several people who wanted the Nobel Prize would start getting this award and then seeing where it led. Pauling would have an exchange with these people, and Pauling was remarkable because he would know bond energies and distances all in his head. It was all in his head. I mean, he was a very, very smart guy, very forceful, whatever.

I don't think of the—Seaborg and Coryell probably came to an accommodation with one another, particularly later in Coryell's life. During World War II, they were bitter enemies. I don't know, I've never felt it so much from Seaborg's side, but from Coryell's side. I read Coryell's memoirs, which were published in the year that we did this symposium on Coryell, and he was connected with a lot of people in Washington University and St. Louis, and he said when he first got the job of being head of the Oak Ridge part of the Manhattan Project, several of his friends visited him and they said "Glenn Seaborg is a son of a bitch, and you need to know that. You need to know he's a son of a bitch." And so they were, you know, they weren't good friends at all. They were competitors, in some way, of what was going on at Oak Ridge and what was going on in Chicago. What going on in Chicago was probably more important and ultimately had more influence on science.

Coryell did manage to discover an element in the course of World War II, element Promethium, 61. So, that was an important discovery, but what went on in Oak Ridge was more or less routine work on the nuclear properties of the fission fragments. What Glenn Seaborg was doing in Chicago was trying to make new elements, along with the isolation of plutonium. So yeah, they were rivals. Now, they had this parallel development, which was very fascinating, because they'd both—Seaborg was particularly poor as a kid and Coryell was not—the family was not that wealthy. I found it interesting that they both developed in suburban Los Angeles. Seaborg grew up in Watts and Coryell grew up in Alhambra. They both attended university obviously, because of their age, at approximately at the same time. Seaborg went to UCLA first and then to Berkeley; Coryell went directly to Caltech and had met several prominent science—he met Pauling. And so, he was Pauling's student in that sense, so they had that connection.

And early on, Coryell was much more prominent than Seaborg, I think, because of his work with hemoglobin; Pauling and Coryell did this work with hemoglobin that got them in a lot of fame and so forth, justifiably. Seaborg's progress was he was a personal research assistant to G. N. Lewis, and so the work was okay but it wasn't extremely flashy, and finally G. N. Lewis said to him, he said "Seaborg, I think I'm wasting your time, why don't you do what you want to do?"

**CP:** Yeah, another person important to Pauling, G. N. Lewis. Three names of OSU people; these are all folks whose papers we have and I'm interested in just getting some sort of institutional memory on these three individuals, and the first one we've talked about a bit, Chih Wang.

[0:49:54]

**WL:** Yeah. Chih Wang was really a visionary in many respects. He had come from really a tough background. He had been captured during World War II, he was a prisoner of war, he survived that, came to OSU, got a degree in organic chemistry with Bert Christensen, but he had his interest in radioactivity, and he tried to pursue a work with radiotracers, but he was also an entrepreneur. He always told me that one out of every three proposals would be successful, so you just had to generate a certain number of proposals. And he was very, very good at being successful in the political environment of the state of Oregon. A lot of his time was spent influencing state legislators. They were all brought to the Radiation Center. One of his tricks was to put them on the top of the reactor and pulse the reactor, which gives a flash of blue Cherenkov radiation, scares the living wits out of these people because they think they've been killed, but they're all go to say "yes sir, we're going to have nuclear power then, whatever you want, you've got it."

And so, he carefully, assiduously courted these people, and that's why he had so much success getting grants and building this out of nothing, and there really was nothing there on campus, and he built this center. The center is incredibly well-designed. Not very many people know it. The people in the building know it, at least those who are sort of alert, but the tricks you find in the building; there are tunnels in between the different laboratories where all the utilities are. You rarely see buildings that are that smart. So, you can work on utilities and all this without digging up concrete or anything like this. Tremendous. Electrical cords are in the ceiling, they're dropped down. The building had a saltwater aquarium on one side that not many people know about, that had those huge walk-in refrigerators where they stored whale carcasses,

elephant carcasses, stuff like this. It was a remarkable building, it had all these things plus the nuclear reactor, which he then developed.

He really got interested in the reactor. When I came, the requirement was that all the people who came to the Radiation Center as staff had to use the reactor. You had to convince him, at least, whether it was true or not that you were going to use the reactor. Sometime after I got here there was an initiative to bring in high-quality faculty to chemistry, and we got one in nuclear chemistry, which was Darrah Thomas from Princeton, and he—Darrah had to tell him he was going to use the reactor, and both of us were sort of smiling, because I knew he knew he wasn't going to use the reactor at all. But that was the words he had to say to Chih Wang, say "oh yeah, I'm going to use the reactor." Sure you are, yeah, yeah. And so, Chih Wang built a special laboratory for him and everything, and he was very, very good.

Towards the end of his life he became bitter and he did some nasty things. He was not the kind of administrator of Seaborg where nothing fazed him. He got mad about some things and he did some nasty things, and I heard some of them. When he wrote this textbook on radiotracer methods, Dave Willis was chair of General Science and Chih was, what do call it, director of the Radiation Center, and Dave had been Chih's student and his PhD thesis had been to write this book. Well the book, by the time I got on campus the book was horribly out of date and it wasn't all that good in many areas. And so, they engaged me to help rewrite the book, and so I started doing it and finally they left me alone and I just rewrote the book, but once a week or once every two weeks we'd have a meeting in Chih's office and we'd go over the book and then these guys would start talking about General Science and campus politics, and sometimes they'd excuse me and sometimes not, and I heard stuff that I just said oh no, this is terrible, this is they've screwed some people, no question. They got mad at somebody because he was divorced; that was a no-no. Out, out, threw him off the faculty because he was divorced.

Campus was very provisional. That's not the right word, it was very...

**CP:** Provincial?

**WL:** Provincial at that time, because common—I mean Roman Schmitt is a first-rate scientist, and some people said to me, and Chih Wang at the same time, "we hired him despite the fact he was Catholic." There were all sorts of other things; "so and so is a token Jew." They didn't do that. And of course the racial relationships at that time on campus were horrible, with the Fred Milton incident with Dee Andros, Dallas, Oregon, the hanging tree, which persisted for a long time. So, it was—

**LR:** Can you say a little bit more about that?

**WL:** The hanging tree? Well, it was in Dallas, I didn't know about much about it other than it existed for a long time, and it was a long time before they chopped it down. It was part of a—this area of Oregon was a KKK center, and so they had lynchings in earlier years, and so this tree was a remnant to that, and an incredible insensitivity, whatever that tree was persisting. I don't know when it was finally chopped down but it was very, very much later. It's just an ugly piece of Oregon history.

**CP:** Another person from OSU is John Ringle.

**WL:** Oh yeah. John was a nuclear engineer, he was—his research on campus at that time, as I recall, was attenuation of radiation in various materials; sand and stuff like that, but he had an administrative bent and he became the dean of the graduate—well, assistant dean to the graduate school, associate dean of the graduate school, and he spent all of this time being this person in the graduate school who kind of kept things going. He did a lot of the work of the graduate school at that time and did it with equal pleasantness, skill, whatever. I think sometimes in the history of graduate school here there were unusually qualified people as dean and then there were other times where there weren't so unusually qualified people as the dean of the graduate school, and then we had periods of time when you had guys like Emery Castle who were dean and they were really good, really smart, and you had other people who weren't so good and not so smart. It's a difficult job.

**CP:** And Dale Trout?

**WL:** Dale Trout I only knew peripherally. He had been an executive at General Electric; they gave him a laboratory in the Radiation Center at the end, and there's still a plaque on the wall for his x-ray machines, but at that time he was running a program with a guy named John Prince and it was centered in Waldo Hall and he was really, more or less had the role of corporate executive in a university. He had some tricks that were, I don't know where he learned them, at GE when he was a General Electric executive or whatever; the chair he would put people in where the front legs were cut shorter than the back legs so you would have difficulty sitting and you'd kind of slide and so forth; one of these half-doors into his office that had a lock on it that wasn't obvious how to open so you would fumble with it and he could come over and "oh, you simple-minded fellow. I'll open this for you." Little tricks like that. Apparently he learned as a GE executive or something, whatever. I didn't know him as a scientist at all; he was a bigshot, really bigshot. Chih used to, well, he respected him but I don't know if he was really a good friend with Dale Trout, yeah.

**LR:** I got the feeling they didn't actually get along. I was researching.

**WL:** Could have been, yeah. They were—Trout was clearly a corporate executive and he was spending his time here, yeah.

**CP:** I've got more to ask you but I want to give Linda a chance to ask some of her questions, too.

**LR:** I've piled up new ones. I want to go over the, you mentioned—because we're talking about the Radiation Center, so this kind of fits in with that. My first question that came to mind: when you were talking about how they lost the Radiation Biology program, pretty much shut that down, could you say a little bit more about that, when that was and what was going on there?

**WL:** Sure. Well, I don't know the dates, I mean they're kind of fuzzy in my mind, but I know the sequence of events. They built the program up, they had one assistant professor, Stu Nachtwey; they had a guy named Bill Ellit who also was assistant or associate professor; they had Don Kimeldorf, who was the senior radiation biologist at that point, and there had been people who were from other areas of campus who kind of worked along with them, people who are prominent now, so forth, that actually were involved in the enterprise there. They were doing semi-chemical, semi-radiation biology things.

[1:00:54]

The name Frank Moore comes to mind, Kathleen Moore's husband. They were doing research on rocket fuels. On the back corner of the Radiation Center there's—they've destroyed this facility since then, but there were all sorts of walk-in hoods, and that was Chih Wang's business too, of doing rocket fuel research, and they'd spray the rocket fuels on plants and mice and all this kind of stuff to see what was going on, but more focused on radiation biology. That was really a good idea and a good department and a smart move. Then it came unraveled, and it came unraveled with Stu Nachtwey. Stu Nachtwey was the guy who got divorced, and basically during that time while I was writing the book and having weekly meetings with Dave Willis and Chih Wang, they discussed what they were going to do about Nachtwey, and basically they decided to get rid of him. So, it was, you know, there was—

**LR:** So, my hunch was that there was this tension in Radiobiology between the biologists and the physicists, in my mind, right, and that doesn't sound that this is that story at all.

**WL:** No, no, no, no, no. As I knew the story, it was Stu Nachtwey got crosswise of Dave Willis and Chih Wang, who were fairly straight-laced and they did not approve of his behavior at all.

**CP:** And so that ends the program?

**WL:** That ultimately ended the program, because they gave, they fired Nachtwey. He went off to another career in NASA, as I recall. And Bill Ellit was kind of isolated at that point. He had, I think he had come from the EPA or some federal thing and he was a fairly good radiation biologist, but once he saw what happened to Nachtwey he said no, and he—and I remember, he said to me, he says "if you were any good, you wouldn't be here." That was his remarks to me. He said "if you were any good, you'd get out of here." And Kimeldorf then was left by himself and he had a heart attack, I think somewhere along in there, and so nobody did anything particular with respect to Kimeldorf, but eventually he just decided

to move away. He had been a chief of the Naval Radiation Laboratory in San Francisco and he was very good, and he had lots of grant money and stuff like this. It was a very well-funded operation.

The anecdotal thing I remember, I think it was Kimeldorf had the experiments with all the rats, and occasionally the rats would escape and people would be running up and down the corridors of the Radiation Center trying to catch these rats, whenever periodically.

**LR:** Another person who I thought, I wasn't sure if you would have remembered this, but you have already talked about how you observed race relations, for example, when you were at OSU, and I wondered a lot about women who were in the Radiation Center at the time, and there was an oral history interview where someone mentioned that there had been a nun?

**WL:** Yeah, she was my student.

**LR:** Really? She was your student?

**WL:** Yeah.

**LR:** So her name, Mary Joseph Bouchard, is that correct?

**WL:** Yes, Sister Mary Joseph Bouchard.

**LR:** Do you know anything about her, can you tell me anything about her?

**WL:** Oh yeah, I know a lot about her. First of all, she had come—there was a program in the United States at that time called Academic Year Institutes. So, they'd bring in people from smaller schools, and I think she was teaching in New England, I don't remember the school she was teaching at, but she came here for one of these Academic Year Institutes where they'd spend a year doing research and supposedly getting all charged up and go back to their school, and she didn't go back to her school. She stayed on to work with Malcolm Daniels in Radiation Chemistry, and she and Daniels didn't get along. And so, I would see her at night in the corridors of the Radiation Center and she was crying, just crying and literally bawling and I'd "Sister, what's wrong?" and she'd "oh, I can't deal with that man, I [inaudible]," and I said "well Sister, I know this goes against your beliefs, but you need a divorce," and she said "well, who am I going to work for?" I says "I will have you as a student."

[1:05:20]

So, she did and she worked with me. She did a reasonable thesis; she did calculations, there was no lab work, and she did them at the EPA lab across the street from the Radiation Center because they had enormous computation facilities that they would give us at night. We could use them at night. And so, she would go over there every night and run so many jobs, and this was the days of punched cards and all this sort of stuff, and okay, she would do this and whatever and I'd tease her. A lot, because I'd say "wow Sister," she'd show up, because it was mostly in the evening she would do the work and she'd show up and I'd say "wow, what did you do? You spent your day by the swimming pool swilling beer all day long?" and: [mimics unintelligible angry response]. And she took it fairly good naturedly.

Unfortunately, what happened was she had been a nurse and she knew disease as well as anybody, and while she was here she had clear-cut signs of colorectal cancer. She ignored them. I used to fume, because any money I gave her went to the church. She took an absolute vow of poverty, minimal existence. A lot of people stepped in to help her. She lived on Kings, finally, in an apartment run by a Chemistry Department employee. I think they probably gave her special treatment. Everybody was trying to help her out, but any money that went, went back to the order in Maine. And so, she ignored her cancer and eventually she left here, she got a job with Boston University teaching chemistry and she did things that only a Sister Mary could ever have done.

Students weren't too bright, some of the boys aren't too bright; she used to refer to them as her "little Jew boys" and she'd say "I come up to these little Jew boys and I grab them by the cheek and I shake their cheek and I say "you are so stupid, but I love you," and get away with it. She'd done a little of that here when she was a T.A., because she would tell students they're stupid, but she said "I love you, it's okay." And she was a remarkable individual.

The colorectal cancer did occur. I visited her two or three times in Boston, but each time she was worse and worse, and she died then. And that was probably preventable, because she knew it was going on and she said "I don't want to spend the money on me, spend it on something else." So, at the end of her life she had friends in Boston who were basically taking care of everything for her, paying her bills, doing everything so she would at least have some modicum of existence, and I think there were others from Corvallis who came to visit her, who would spend a day or two visiting her, talking to her, whatever.

**LR:** Just one more question about her, and that's did she ever explain how she had gotten led to nuclear chemistry?

**WL:** Well, it was just that she had come to work in Radiation Chemistry with Malcolm, and then—

**LR:** But why? Why does a nun...

**WL:** Oh, well she—it was the science influence. Look, she came for the Academic Year Institute, which was clearly nuclear oriented. Chih Wang had these institutes, and they were—they'd all hook up with some faculty member, and Malcolm actually had three or four students in those things, and I would never had a student in those things. Those were mostly chemical skills, and this was chemistry. So, she was interested in chemistry, she was a chemist and she taught chemistry, whatever, so she was a chemist. And what Malcolm did was chemistry, it was very good chemistry. And so, there were other students who worked with Malcolm at that time. He had ultimately, well, at his funeral last year I said something like "Malcolm didn't suffer fools gladly, and therefore he didn't do much work with graduate students here."

**LR:** Okay. So, it wasn't like a man/woman thing, it was just...

**WL:** [Shakes head] no.

**LR:** Okay.

**WL:** Oh yeah, I kidded her a lot, but no.

**LR:** So, what was the climate for women in science at that time at OSU when you were around?

[1:10:03]

**WL:** So, I'm probably oblivious to what was going on. The Chemistry Department was ultimately part of the dirty dozen, the twelve largest chemistry departments in the United States who didn't have a woman. And it was an attitude not that so much anyone disliked women or disliked women scientists, it's just that there was not any interest in the idea of making a hire that had a diversity aim. In fact, there was just violent opposition to any of that sort of stuff. So, occasionally there would be a woman who would compete for a job and they would—she would always lose.

And then the dean came along, I think it was Fred Horne, and said "you're not getting any more positions in this department until you start hiring women, period. That's the end of it. You can yap all you want about competitions and grading and people, you're not going to have any more faculty in this department until you hire women. That's it, I am telling you. You're next one or two hires are going to be women, and that's it. That's the end of the story." And so, they did. They hired, I think Wei Kong was brought in on that time, and oh, I'm not sure of all the others who came. One woman came, was unsuccessful. There was always this claim that chemistry had women because they had spouses. Lise Hedberg was a scientist working with Ken. But it's a gap, it's still a gap. No matter how you look around the Chemistry Department, there's an unusual male-to-female ratio, very high, because these kind of targeted hires are still an anathema to the Chemistry Department.

And it would have taken, I think it would have if—I don't think there's been a dean for a long time, but it would have taken someone like Fred Horne who simply said "that's the way it's going to be. You're not going to get any more faculty unless you do this." And the same policy could, frankly, hold to change the racial mix of the Chemistry Department, too.

**LR:** Yeah, I was going to say. So yeah, it probably...

**WL:** There, for a long time in science here, there was only one black person, Wil Gamble, and he used to refer to himself publically as a "show nigger" of the department, and it was because they treated him that way. Every time there were visitors to campus, Wil was invited along.

**LR:** They probably made sure he was in the pictures, too.

**WL:** [Nods affirmatively]...yep.

**LR:** So, what did you think when—I just want to go back to that incident with the football players, I guess, and then I promise I'll let Chris have a turn [laughs]. What did you think about all that? The walkout and--

**WL:** I didn't have much to think, I guess, at that. I mean, Andros was a funny guy, I mean everyone criticized him as the Great Pumpkin or something like this, and the walkout of the football players didn't—I didn't really understand much about it and wasn't particularly committed to it at that point. It seemed to me, I guess it—in retrospect, there were so many other areas of discrimination in this campus that whatever happened there was minor. I do recall the fallout out of that; the barbershop which still exists here in Corvallis, Mel's, the guy throwing away the scissors when he cut the black man's hair, making a public ceremony of throwing away all the equipment he'd used to cut the guy's hair.

**LR:** I don't know if we know that story. Can you say a little bit more about it?

**WL:** There was an old-time barber, he had worked in the MU when there was a full barbershop in the MU and he then opened his own private shop. His name was Mel. That shop still exists, it has no relationship to the old Mel, it's near Rite Aid, in that area, and there's still this barbershop called Mel's and basically it's sport's oriented. There's another barbershop on 3rd Avenue in Corvallis not far from the Mac store that's also sports oriented. They're not university oriented at all. They have sometimes had some absolutely horrible things in their window, stuff like that. That's still an issue. When you've had black coaches for basketball or stuff like that, it's still an issue. It's still an issue here. And it's much better, obviously, than it used to be, but this is not Eugene, in any sense of the word.

[1:15:27]

**LR:** So, how did you know about the KKK here? You just kind of found that out?

**WL:** Well, people have told me, and there—you have to just read the history records of Western Oregon to know that this was a place where the KKK had a dominant presence. And as I say, testimonies of this. And I think when I first came here it was really much, much worse, when you could talk openly about hiring a Catholic, that that was a big deal. That's different, that's strange. I didn't understand at all. I couldn't believe it, I—

**LR:** Those kinds of politics, you hadn't grown up that way?

**WL:** No, it just, well I'd never heard of it. I mean, I grew up in a community where there was a strong Catholic church, but I absolutely wouldn't have ever thought that this was something that, you know, people made any difference. When I went to MIT was the first time I had a lot of contact with Jewish guys. So what? It didn't make any difference to me. I didn't care or even know the difference. I guess I was oblivious to a lot of this. And then I still don't understand a lot of it yet.

**CP:** Well, I'd like to talk a bit more about your work.

**WL:** Sure.

**CP:** So, my notes say 1978, that was about when the super heavy ions stuff started, is that correct?

**WL:** Yes, it was starting roughly at that time. When I went to Berkeley in '76 there was starting to be an effort to do super heavy element experiments in Berkeley. Seaborg had come back from the Atomic Energy Commission, reestablished his position at the University of California as a senior professor, and he'd gotten the money and so forth to hire some groups and he brought in a scientist named Louie Ensen [?] from Sweden, and they started a program, really a low-level, primitive program, but it was oriented in super heavy elements. There was attempts to do experiments at the Super HI-

LAC at Berkeley. They mostly had the result of not finding anything, and that was understandable because the sensitivity of those experiments was absolutely off at least two or three orders of magnitude from what you ultimately had to find. Maybe the sensitivity, in terms of cross sections, was cross sections the order of ten to the minus thirty-four square centimeters ultimately. When those same experiments were done successfully, they were done at the level of ten to the minus thirty-six square centimeters. So, they were two orders of magnitude off, and there was no way they could compete.

And so, there had been a shift in nuclear science. Elements 93-106 were basically U.S. property. They were discovered by American scientists working, largely, at Berkeley. There are occasional things that happened like the Coryell discovery of promethium in World War II and so forth, but it was a Berkeley business. But people, for physical reasons, ran out of steam around Element 106, because it was thought that the half-lives of these nucleoids would be too short to be detected experimentally; the half-lives were decreasing steeply with increasing atomic number.

And then Yuri Oganessian made this postulate that if you did these reactions involving lead and bismuth nuclei reacting with anything else that was appropriate to get to what your product wanted to be, that you could do these reactions at very low excitation energies, so-called cold fusion, and you'd be successful. And so, elements 107 through 109, maybe 110, were discovered using these techniques. Berkeley had no part in this. This was all done in Germany. We tried at Berkeley; Al Ghiorso led the experiment, forty days of beam time at Berkeley to try to make element 110, which ultimately became known as Darmstadtium.

[1:20:02]

It was a strange culture at Berkeley. People didn't like these experiments. And so, Al was a revered guy from all of the elements he discovered. He probably discovered more elements than Seaborg, ultimately. But he didn't have political skills, he was—took every opportunity he could to stick his finger in the eyes of the administration of the laboratory. He was a bitter opponent of the Vietnam War and he always referred to God "she", and that annoyed a lot of the people in Berkeley because of that reference. So, he was stripped of a lot of equipment and he didn't get much of a chance until they finally said "okay, we're going to give you a chance to do the 110 experiment." I was part of the experiment and he probably made the first atom of Element 110, but the equipment was bad, it was broken and some critical pieces of the equipment sort of malfunctioned at the time of the one atom being made. So, we had an involved process of a year or two in which we tried to demonstration that we had made the first atom of 110.

It was probably right, and Peter Armbruster finally told me that yeah, he believed we'd made 110 first. But Germans made 110 with copious quantities using a different reaction and a more favorable reaction, and there was no doubt that they were the first ones to really come with convincing evidence for the synthesis of 110. The Ghiorso finding of the one atom was probably right, but it wasn't convincing in the sense of the German work, and so it's perfectly reasonable that the Germans are given credit for discovering Element 110. And that was a cold fusion reaction.

Now beyond that, it was difficult to make progress, and so one thought in some ways one was having real problems. It turned out later that this cold fusion path was pushed further to make Elements 111 and 112, but it ultimately probably died at Element 113 where there was a successful probable synthesis of Element 113 by the Japanese, but they had to run their accelerator for five hundred and fifty-three days to get three counts. And it's a remarkable story of human courage, perseverance, fortitude, whatever, but it's more or less the end of the line. Nobody wanted to do that ever again. And so, the Russians now, again, Oganessian leading the Russians now directly said "well, we'll do a kind of reaction known as hot fusion where we take actinide targets and we bombard them with lighter ions. We form nuclei that are highly excited, so we're going to lose a lot of nuclei due to fission, but at the same time the ions will fuse, which they were really not fusing in the cold fusion, and ultimately when you got to 113.

And so, one has successfully synthesized the elements 113, 14, 15, 16, 17, 18 using hot fusion reactions, mostly driven with calcium-48 projectiles; some of the later ones other projectiles. The bottom seems to have dropped out at Element 119 and Element 120 where nobody has made them and the preliminary upper limits now are also in the so-called FeMtobarn region, which means you make one atom a year, and that's tough work. So, that's where the field lies now. The only thing that people are looking for, the Russians are building a dedicated facility, extremely high beam currents, and maybe it will be possible to go further, but there are some doubts. Maybe this is a physical limit. Certainly we know at some point the periodic table will come to an end. That's not suddenly you stop discovering elements, but the periodic

table, which is based on the chemical properties of the elements, may become useless because the elements that you make will have such similar chemical properties that the concept of a periodic table is useless.

**CP:** As you were contributing to this field over the course of the years, was this mostly an outgrowth of your collaborations in Berkeley, or was—

**WL:** Yeah, well that got me into the heavy element business, was at Berkeley. Initially Seaborg had pushed us off in a direction of highest energy particles, the GeV per nucleon stuff with the Bevelac, and we followed that a while and then we went down to what's called intermediate energy reactions, because they were—we could measure much easier and better data about them and it was, we got much better results. And then eventually we driveled back down to the low energies again, and then we'd always been interested, but I hadn't been part of that group at Berkeley that was doing those experiments. Seaborg and Morrissey and I wrote an article I think in 1978 in Science where we had seen enough of the experiments that were unsuccessful that we said to the world "look, there was a large effort to discover super heavy elements in nature," we said "that was dumb," because we already knew enough about the half-lives about these things that they were not going to have half-lives in the order of the age of universe.

[1:25:55]

And so, we wrote an article to that effect and it still occasionally comes around. We have some people today who are still trying to do this finding of things in nature, and sometimes I get their papers to referee and I remind them of what we published in '78 and they say "oh yeah, but you know it's fun to look" and so forth. "We don't think we're going to be successful but we got the time" and on and on and so forth. So, okay.

**CP:** So, what was your work pattern like? Were you making trips down in the summer time, or?

**WL:** No, no, no, when I was working, all the time I worked with Seaborg I would go down on weekends, every weekend, spend at Berkeley.

**CP:** From Corvallis?

**WL:** Yeah. And then I would go when there'd be breaks in the school year; sometimes there'd be a lull in classes, I'd just go down there, yeah. So, sometimes I would just take off a quarter here, teach more courses one quarter and then have a quarter where I didn't teach and go down to Berkeley and live in some fleabag apartment or sort of like this.

**LR:** [Laughs] Still in the fleabag apartment?

**WL:** Well, they weren't—I mean the one I had during sabbatical leave turned out to be very good, it was a wonderful mother-in-law apartment in a fancy area of Berkeley, the Alameda, so forth. Other times I remember places I would stay in, places that the key element was a water bed that went [mimics water sloshing around] all the time and various and sundry places. Staying at motels at—well, the first time I went down there I stayed in a motel called the Pink Flamingo and I got robbed there one night and I couldn't believe it. It was such a dive, because you would get knocks on the door at night with prostitutes coming around, saying "can we help you out?" and "no, no, no, no." But I remember one time—you probably don't need this story but I'll tell it anyway—we went out and went out drinking and came back that night; I woke up in the morning and my pants, where are my pants? My pants are gone. Really? What am I going to do? And I go this can't be, was I so drunk last night that I took off my pants somewhere in San Francisco and came home naked? I don't understand this, what was going on? So, I dial the motel office and I said "I don't know how to explain this but I think somebody robbed my pants," and the lady said "yeah, several rooms were broken into last night and people's goods were stolen." I said "my pants?" she says "yes, I have your pants." They were found in the dumpster, and all the guys did, they—

**LR:** Took your wallet out.

**WL:** Yeah, they took the wallet but they didn't take the credit cards. We were so happy. They took all the money out of the wallet, sure, but I got my pants out of the dumpster at this motel in Berkeley. And after that, I didn't stay at that motel. But I still this day, I still don't know. And there were two or three other people who had the same experience; you woke

up in the morning and stuff was gone and you never knew it. They somehow got in the door of the rooms, and maybe they had a key, who knows, and worked there.

**LR:** I have some abrupt other things that are completely different, but I've been wondering about them. So, I just thought this might be my chance to ask you. I wanted to know if you had any interactions with the on-campus Radiation Safety Committee or the safety officer, or how you felt about radiation safety and maybe even the discipline itself?

**WL:** Yeah. Well okay, so the campus department has always been pretty innocuous, and kind of out-classed by the radiation center's own Health Physics people. Rainier Farmer was a nice guy, but he wasn't, he didn't have much control except some of the stuff that was going on campus. The Radiation Center is a domain of its own, of itself, and it has always had the high-quality people of Radiation Health Physics and so forth. My first encounters were more or less bitter with Art Johnson. He was the prevailing guy and he and I fought all the time.

[1:30:14]

**LR:** Really?

**WL:** Oh yeah, because he was bound and determined that I was going to follow all kinds of rules and I didn't, I'm just not of the mind of a person who follows rules, and so we were always fighting, always, you know, one another battles. One time he and some of his associates thought they wanted to get rid of me and they did something fairly stupid; they established a surveillance of me, so everywhere I went in the building people took notes of what I was doing, and if I didn't do something exactly right they wrote it down. And so, they had a meeting of the Campus Radiation Safety Committee to kick me off campus, effectively take away my license, whatever, kick me off campus, and they wrote to this long, you know, "so-and-so professor stepped in the wrong place" and something like this, and the people in the committee were academic types and they said "what the hell is this crap? You put a faculty member under surveillance for months at a time and you think that was a good idea?" and the health physicist went just [makes throwing out motion] "don't come back with this crap, and don't ever do it to anyone else." So, that stopped that. At that point they knew that in some ways I had the longevity, because they had just completely mistaken what they could do. So yeah, okay.

**LR:** Was it a safety issue?

**WL:** No, it was an issue because I was contemptuous of them. I didn't have any respect for them at all, I didn't think they were intellectually very good and so okay, I didn't pay attention to them. And ultimately what—a lot of success I had was I did experiments elsewhere.

**LR:** Outside the center?

**WL:** Yeah, outside the campus, so I didn't have to deal with these people at all. And other places were very different. Much different.

**LR:** So, when you're working on the heavy elements and the ions, I mean is that highly, is there a lot of alpha radiation, is there a lot of—

**WL:** Well sure, there's all kinds of radiation, you know how to deal with it, you establish some reasonable protocols and you can work safely. You may, at some time, you may not have the right color lab coat on, because some people have color-coded lab coats or something like this. Well, that's not really important. The color of your lab coat is not important. And things have changed here. That military-like culture is gone. A lot of the people were from the military; they're gone. And so, now you have a different safety culture on this campus, at least for the radiation safety, which is "okay, you're going to do your experiment, how can we make you do it safely, how can we help you do it safely?" which is the appropriate culture, instead of "these are the rules and regulations, you have to bend your procedures to fit these rules." And that was the big fight I had with Art Johnson and with everyone else; it was rules first, science second, and then eventually that switched, yeah.

**LR:** Well, that's interesting. Glad we got to talk about that. And then you had mentioned you weren't interested in working in Hanford; why not?

**WL:** It's an awful pl—

**LR:** Wasn't that where all the action was?

**WL:** No. First of all, it's an awful place to live, first of all, and secondly what they really came to by the seventies or so forth is the work done there was technological, applied, not forefront science by any means. I mean, there were good people there. I recently went to a meeting in Hawaii in which one of those people, who I knew fairly well, recounted his career. He had just retired. Most of the things they were concerned about were industrial issues related to national security. It was all related to national security, and that wasn't something that interested me. I was more interested in basic science. There wasn't a lot of basic science at Hanford or anything like this. The good science labs were Berkeley, Los Alamos, Argonne, Brookhaven, stuff like this.

**LR:** And how was it working on the books that you did with Seaborg?

[1:35:10]

**WL:** It was, he was a remarkable proofreader, as well as a contributor. We'd sort of agree on an outline and I write it up and give it to him, give him chapters or pieces and then he'd [mimics crossing items out on paper]. And I'm not sure, his greatest proofreader was his wife. She was an English major, and she was Lawrence's secretary. And so, between the two of them they'd proofread things and come back all chalked up, but mostly finding different wordings. And we would have long discussions over more subtle points. In one of our books, I don't remember whether it was a textbook, but it might have been this Elements Beyond Uranium, I made a statement in a chapter on weapons about the size of a weapon, and he said "you can't say that," and I said "why not?" and he says "look, that's classified information," and I said "but it's obvious, you can do that, so forth," and he says "that's not good enough. You can't say you figured it out because you're smart, that's not important. So, you got to find some other way."

And so, at that point, Ken Krane in Physics had this textbook published and I looked at it and I said this is it. He had gone to the museum in Los Alamos and put one of his tennis shoes on one of the bombs. Therefore I didn't need to establish any dimensions; I didn't even do a calculation of the mean free path of the neutron in the weapon. I said "look at this picture, it's got a foot and it has the weapon," and Seaborg said "okay, we're done. That's fine, whatever. So, you're okay." Once you see that, because otherwise he said "you're giving a dimension out which is classified." And this kind of fight with classification occurred even a few years ago. We started doing experiments at the Los Alamos Neutron Science Center, LANSCE facility, and we produced a paper on the energy release in the fission of U-235. Well gee, you know, this had to be known, but there was nothing in the unclassified literature.

So, we wrote this big paper up and submitted it for publication and a month went by and the referee returned it and said "I can't referee it." Happened a second time; "can't referee it." Third time; "can't referee it," and we said okay, what's going on, this has got to be a problem. So, it's got to be a classification issue, so how do we deal with it? There's something called the X-Archives at Los Alamos where you can post papers and have them appear internationally. We took our paper and we posted it there, and within a week or so the patent manuscript was approved for publication, because we had broken the classification scheme. It was a dumb classification scheme, because you couldn't convince yourself that bombs were built without knowing the energy release. Made no sense. You couldn't run a reactor if you didn't know this information, but it wasn't in the unclassified literature.

And we continuously fought about this since then, because our colleagues at Los Alamos were supposed to get this stuff declassified and they obviously didn't, and so we were left figuring out how to break it, and so we now know it's the idea they understand quite clearly, is that we will publish this stuff in the open literature, and regardless of what they had said. The only thing that's happened that is bizarre is when you apply for beam time at Los Alamos they have these program advisory meetings, PACs, and we found out work moved from Science to Defense Applications, and it turned out, it's completely bizarre; there were students in the room, and partway through the last one that I was at, they announced they were moving the whole session to another building, which was classified. And I didn't care, because I had done my talk. The students were sitting there and they said "well we, we're not—we don't have Q clearance, we don't have clearance to go up," and the guy running it is scratching his head: "you can call." So, they had this bizarre situation where these graduation students called in maybe a block away to another building, because they weren't allowed in the building, called in to describe their talk and give their talk on the phone.

**LR:** Wow. It sounds like the 1930s all over again.

[1:40:04]

**WL:** It's very hard, and that's what we learned from Seaborg; it's very hard to break the classification schemes.

**LR:** Have you seen it get better since you started in your career?

**WL:** No. It appears what, the only way you can break the—now it appears this trick of publishing things in the archives is a way of breaking them. But no. And I have, you know, occasionally people ask you to be on a review panel for a national laboratory and they'll stop a discussion and say "these people here aren't cleared, we'll continue in another room with those who have clearance." One of the most egregious things that happened to me some years ago, I was giving a talk in Livermore, which is a weapons lab, and so outside my talk there was a big sign, a red sign saying "speaker is not cleared," like you were unclean. And then, in the course of giving my seminar, I was asked some questions and I started to give answers to the question and a senior guy jumped up and he said "this man is not cleared to discuss this data." So, the subject had to be stopped.

**LR:** It seems like it would go against your, perceived as basic science.

**WL:** It's against common sense. Yeah, it doesn't—but that's why the classified world lives very differently, and...

**LR:** Would you say it impedes science?

**WL:** Oh, sure it impedes science, and it's funny because some of this classified research takes on things that actually people will find very interesting. I participated in a classified review, although I wasn't told the classified aspects of the review, of something called the Feynman conjecture, which I'd never heard of before, but something that Feynman had said of the Manhattan Project in World War II, and these people were exploring the implications of it for national security, and that was very interesting, but never would you think about it, yeah.

**CP:** I want to ask you about one of the more high profile projects that's come out of the radiation sciences here at OSU in the last several years, is NuScale, your thoughts on that enterprise as it's developed?

**WL:** Well, Jose Reyes is a very, very smart guy, very talented. He's in a unique position, having been in the Nuclear Regulation Commission, to try to do the kind of work he's doing, because that is important. They have a tough road ahead of them. The idea of modulized nuclear reactors certainly is something which has some safety benefits that they can document. One of my colleagues pointed out that there may not—they may not have the optimum place to run these reactors, or haven't thought enough about it, maybe. These reactors run at lower temperatures, because they're smaller. The Carnot efficiencies of the reactors are not what they are in the larger machines. One of the colleagues of mine said "one of the places that this kind of reactor could be extremely useful is in European cities where you have heated water." They circulate heated water from a central facility, and that heated water is used to heat houses and stuff like that. This is the perfect type of reactor to do that sort of thing, because instead of generating the kinds of steam cycles they have now, the heated water cycle might be a better thing because they can do with reduced Carnot efficiency.

But as it says, I mean they're working forward, everything appears to be straightforward. They've had ups and downs in their business with funding, they're obviously in a serious configuration with the government, because the government has, I believe, given a significant grant to Babcock and Wilcox or one of their competitors. They have associated themselves with the Fluor company, which is a tough energy company; they're not a fuzzy, friendly company, they're tough guys. Wish them all the good luck in the world, I hope they're successful. It's very complex, I don't know. It seems like they've been at it for an awfully long time, they've had ups and downs, so forth. I hope they're on an upcycle, hope that they're successful in getting that federal demonstration project, because at that point, I mean okay, they've done it. Now, there are other people who think of other designs of reactors that aren't based on uranium fuel cycles, stuff like this, thorium fuel cycle, and people think this is an important way to go forward, takes care of the waste problem in a different way because there's no plutonium waste, or as these devices will still have plutonium based.

[1:45:18]

It's hard to say, from the point of view of nuclear power is stalled. It has been stalled really since Three Mile Island in the United States. And well, it's a tougher business and waste disposal issues now; apparently the people in Arizona, New Mexico, are more interested now in having waste disposal facilities there, but for a long time it was the "not in my backyard" syndrome; nobody wanted waste disposal. That affects other things: medical use of radium nucleoids, et cetera.

**LR:** When you were talking it made me remember about they were setting up Trojan about the time you—right after you first came.

**WL:** Right.

**LR:** I mean, probably it started before you came, but they had a speakers bureau where they'd send out like different nuclear engineers, and I was just wondering if you remember any of that or were a part of that.

**WL:** Okay, so the Radiation Center, Chih Wang ran a summer program for high school teachers to try to promote nuclear energy, and Chih said "you're going to run it" to me. And so, I did run it for some years. It was an interesting experience, because we brought in these school teachers, mostly from this region of the United States. We wined and dined them and gave them every opportunity to have good times; I'm taking them out for pizza and stuff like this and arranging guys like John Ringle and so forth, gave talks and whatever and we schmoozed with these guys and they went out to be more or less advocates in the classrooms, and we taught them some skills so they could do experiments for their students, et cetera.

I remember one case where at the end, you know, we were going to give them academic credits no matter what, but at the end of the thing we gave them some sort of an exam to try to validate this process, and I had to look at these exams. And I always remember some guy from Onalaska, Washington, right where the John Birch Society sign is on I-5, and I thought this was a joke at first, but he wrote this long essay on "the purity of our essence," Dr. Strangelove movie phrase, he was serious about fluoridation, everything was related to fluoridation, and this was all [taps head], guy's a nut job. But he was teaching high school in Onalaska. I've always remembered, whatever comes out of Onalaska, Washington I'm a little worried about [laughs].

**CP:** Well, I have some sort of closing questions, I want to make sure Linda, have you touched everything you...?

**LR:** Well, I'm good for now, yeah. Thank you.

**CP:** Okay, good. You've been here a long time and I'm interested in just sort of your perspective on how Chemistry and Nuclear Science have evolved at this university and sort of where we're at right now.

**WL:** Well that, actually there's two subjects in that; one is how Chemistry has evolved and one is how Nuclear Science has evolved. They're different. Chemistry, when I first came here, was relatively strong. I could look on the walls of Gilbert Hall and see pictures of the Chemistry Department in the thirties and forties, and it was huge. A hundred faculty in Chemistry, and when I came the faculty was strong and good. There were programs in the National Science Foundation called Centers of Excellence and OSU won them. And it stopped happening and there was just a long, long, long slide. Some of it was internal decisions to change faculty positions into TAs, that the mission was teaching and not research.

Well, I think the low point occurred a few years ago when Chemistry went down to something like ten faculty, tenure track. It had always had—it had kept hiring people as temporary people to general chemistry and organic chemistry, but the research end of the house came down to ten people, or something like that. I don't know the exact number; it was the order of ten, that was the worst part. It's built back up now, so it's sort of of the order of twenty, maybe a little bit less. It's way, way, way from what it used to be, and it's not a program. If you teach four thousand undergraduates a year, which is roughly what Chemistry teaches, this is become a bottom tier department. We used to be in the top third of the Chemistry Departments in the United States; we're now in the bottom half, there's no question about that. And I've seen that slide.

[1:50:36]

I don't think it was done externally to Chemistry; I think in some ways Chemistry did it to itself by decisions relating to the role of research versus the role of teaching. And so, we do a lot of teaching, we do a lot of it well, we probably do better than when I first came. When I first came, every faculty member taught general chemistry. No question about it. Some of us weren't all that good, some of us were good, whatever, it happened. It was just a cycle. Every faculty member

from Jack Decius down to the last assistant professor taught general chemistry. That's gone. There are these hired guys, specialists, some have more longevity, some are yearly appointments; come in, teach, move on. The research aspect I think has probably been lost in the instruction.

Maybe there—I can say that I've seen the quality of the textbooks used in the course go [makes motion and sound of plummeting], down. The intellectual level is down, seriously down. The Honors College students, I've taught them off and on for some years; they're getting worse every year. They're not as smart as they used to be. I don't know where they are, at least the ones I saw, they really aren't very high quality. If I compare them, say—I know this isn't a fair comparison, but if I compare the general chemistry class of Berkeley or MIT or any of these other places that you see on the web now—they're all web-available—there's just no comparison. The first lecture of general chemistry at MIT is the Schrödinger equation, solutions of the Schrödinger equation. They're dealing with a much more talented group of students than we are.

Now, nuclear science is a different story. Nuclear science was mostly nuclear engineers at the time, and well, the Radiation Center had science, significant science, and that went away. So, it became strictly an engineering group. Chih Wang was interested in being an engineering group. He brought in some very distinguished nuclear engineers, Bernie Spinrad, mostly. He was really, really, really good. But I don't think Chih could ever live with the idea that he would step aside as a chemist and let Bernie Spinrad run the show. That wasn't apparently what he wanted. Bernie left ultimately, went to Iowa State, I think, or something like that. But he was the best intellectual engineer that you ever saw here, and a nationally known figure, internationally known figure.

Since then there's been developments. The Health Physics group has evolved, they're really much better than they ever were in the old days. The number of students in nuclear engineering is incredible, especially if you consider the number of new reactors that are built every year and have a hundred, hundred and fifty students in graduate school there. They do some of that by a trick that other departments won't do, in that these students are self-supporting, for the most part. A lot of them are part of Ecampus type instruction, but the numbers are huge. And they've had a successful program; people have been entrepreneurial and smart and really developed lots of areas of research. I mean the part that's driven by Jose has been outstanding, there have been a whole lot of younger faculty who've come along on his coattails and succeeded, have many projects going. The Radiation Center is a constantly growing enterprise. I don't, can't figure out what all of these engineers that I see every day are doing, building new pieces onto the building, et cetera, but it's a viable thing. It's not the same institution, the reactor doesn't drive it. The reactor is somewhat anomalous at this point, but okay. As long as they keep the reactor going it's probably fine, but yeah, it's been a very, very successful department.

They're trying to expand out now; Medical Physics, that's a logical expansion. They stubbed their toes a little bit about that, because it's a big development that you need a lot of effort and money to put in to that to be successful. And they have a cooperation with the hospital in Portland Oregon, Health Sciences University, but there's still not enough people, at least in my opinion. We don't have a critical mass and they need to bring in those people as fast as they can to have a full-blown Medical Physics program, which is quite serious. They made the right affiliation to do it with the institution in Portland instead of doing it with one of the local hospitals, that's clear.

[1:55:49]

I think their trajectory is upward. I'm surprised they haven't been able to really get the donor to expand the Radiation Center and those temporary buildings still on the side of it. They have all the plans for the addition to the Radiation Center. The university has done so much fundraising, and these, the Schudes [?] I thought had made commitments to make significant buildings out there, but that's the only thing missing, is more buildings, more space, et cetera. But yeah.

**CP:** Well, sort of in that vein, the last thing I want to ask you about is where you see OSU as being positioned as it heads towards the 150th birthday.

**WL:** I don't know that. I mean, Ed Ray has been a fund-raiser type president, a guy who wants the place to grow in every way, and he's reflecting what he did at Ohio State, the OSU. Anyway, he's been extremely successful. I don't think we've had a president who's been that successful at fundraising and who's put that priority on fundraising. We had presidents who were strange individuals. Robert MacVicar comes to mind, a man who used to go around every time there was a tavern opening anywhere near campus and testify against the tavern. I can recall submitting travel bills for a thing, and he would sign them personally; the president of the institution signing a travel voucher [taps head]. Small town stuff and

didn't have anything. One of his predecessors was a guy named Jensen, who was a high-quality from the Atomic Energy Commission and so forth, and really a topnotch person.

Other people, I speak kindly of Paul Risser, although I have lots of stories from people whose interactions with the unions, et cetera, were awful. But I have a certain guy who we thought, one arm, who typed one-finger, all these memos, stuff like this. But Ray is driving the institution. I'm a little surprised at the backlash he's getting from the community. It seems completely unnecessary. Parking is a three-dimensional problem in the rest of the world. Treating it as two-dimensional seems so incredibly provincial you wonder, and this is why I sometimes worry about OSU, because you see these provincial things come in. Nobody in the world treats parking as a two-dimensional problem. It's always a three-dimensional problem. The University of Washington is incredibly successful by going down underneath their quadrangles, where all their parking is. The faculty drive to work, park underneath their office and take the elevator up to their office. It can't be that hard. Yes, it's more expensive, but you've solved that problem.

I mean, getting hung up on this—and I understand Ray's tendency of telling the people in Bend, also, you know: don't bother us, kids, we're going ahead, we're doing this kind of stuff. And that kind of force, I don't know how long he wants to stay. At some point he's probably going to flip it in. He says he's flipping it in at some age, but I don't know. But it's not, as far as OSU goes, they figured out what it is to raise real money; now they have to sort of figure out how do they support things which are flashy: nanoscience, these kinds of things, which will help the institution grow. And how do they make sure there's a core of science, humanities, other things that are much better than they are now?

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OSU's weakness has been it Liberal Arts, they're terrible and they're not really anywhere near being good. Yes, there are people who are very good at Liberal Arts, but historically it's been a backwater of the institution. That has to develop, and really develop significantly if they want to be a first-rate institution. Science has faltered. There have been some great things in science, but there are problems, internal problems right now, serious internal problems. If you have a conversation with the faculty who've been around here a while, you'll see a lot of raised eyebrows that there's—something isn't working right now. So, you don't see the sciences growing and leading this, whatever.

So, there's great promise, the money really looks good, the buildings look good, that they're rebuilding things. They're figuring out how to build all the new buildings that most campuses already have. What do you do with the antique buildings? Well, I guess eventually you tear them down, but that—you have to cross a lot of those bridges, the fact that you have some of the town battling you, and most of the town who are battling you are retired OSU people who somehow want to freeze things in time.

So, I don't have a final answer to where OSU's going. I don't spend all that much time about it. I'm sort of in the position of saying okay, what do I want to do in the next five or ten years? You know, I'm old, what do I want to do, what do I want to get accomplished, how do I do it? I spend much more time raising money than I ever had in my life, spending hour after hour after hour writing proposals for money. They're successful, surprisingly successful, but that's not what I signed up for, is to write proposals for money all the time. So yeah, I spend a lot of time worrying about money. That's not the best. Yeah, whatever. So yeah, that's my answer.

**CP:** Well Dr. Loveland, I'm sure I speak for Linda when I say this has been a very valuable contribution to our project, and I really appreciate it, thank you very much.

**WL:** Thank you.

**LR:** That was fascinating, yeah.

**WL:** Oh, good.

[2:02:25]