William Lester transcript

Robert Bergman: I'm Robert Bergman, a professor emeritus of chemistry at UC Berkeley. And I'm pleased to be able to interview Professor William Lester, who is professor emeritus in chemistry as well, and furthermore an old friend. We've been in the department for a long time together. So I'm going to start the interview by first introducing Bill, and asking him a little bit about growing up in his early years. One of the nice things about this is that we both grew up in Chicago about the same time. And so I'd like to start, Bill, by asking you to say a little bit about your early years in Chicago, the influence of your parents and grandparents, and basically how you got started with your interest in chemistry.

William Lester: Oh, OK. Yes I'm from the South Side of Chicago, which I think differs from you, Bob.

Bergman: Actually, I was on the South Side too. We were in very different environments.

Lester: OK. So basically I started in Woodlawn. That's *60 hundred* or so *south*; *{NOTE CHANGE}* this is slightly southwest of the University of Chicago. And growing up there was fascinating, and in retrospect it was interesting to perceive my parents and grandparents, in the sense that there was a real class distinction. On my mother's side there was college education and on my father's side not at all; high school was the terminus.

Growing up in the neighborhood we *did {NOTE CHANGE}*-- because of the residential segregation of the city of Chicago, I went to an all-black elementary school for the first three years, that was integrated in the sense of some white teachers and also a white principal at that institution. It was about third grade that it was determined that they really needed to move me out of the bedroom, which I shared with three sisters, because I was getting older. So we moved up to a project. The project was wonderful in a sense it had an extra little bedroom, which was mine.

Bergman: So this was a housing project -- it was outside of the city?

Lester: It was a housing project, no, no, in the city. Princeton Park, 9300 or so -- well it went from 90th to 95th. For those who know the *city-Wentworth to Harvard-[NOTE CHANGE}* Princeton Park was what it was called. The school for the district was one called Gillespie. Gillespie was totally black -- teachers and students -- and it was a wonderful experience. These teachers were really concerned about us. And I make a distinction there with respect to McCosh, which was the name of the school that I'd gone to previously. So that, in some senses, was the way it was. Oh, it should be mentioned that we were in a district for which the high school had been all white until Princeton Park was built.

Bergman: That was Calumet?

Lester: Calumet High School, yes, yes. And so I was about the third or fourth cadre of black students to go there. And that was a fascinating high-school experience. Almost all classes I'd be the only black student in them. But going back to this aspect of interest in chemistry -- oh that started with radio, listening to the radio commercials: "Better things for better living through chemistry." And then the term "through chemistry" *{NOTE CHANGE}* was dropped when chemistry got a bad reputation. And so it was not until my senior year of high school that I actually had a chemistry course; I had physics as a junior. And did well; we used to

have these competitions where you stand up, like a spelling bee, and the instructor would ask questions and if you missed them you sat down. So I used to win those on occasion.

Bergman: So I know that that's when your interest in sports was sort of -- either started or continued, right?

Lester: Oh well, my interest in sports -- in particular basketball, but football as well -- began in Princeton Park. We moved out there -- I must have arrived out there by the age of eight or nine, something of that sort. And the way it was configured -- that is, the development -- there was a large central area where you could play all kinds of sports. The local fire department would flood the central field and we could ice skate in the wintertime, so we'd play hockey, with brooms and sticks and all that sort of things.

Bergman: I seem to remember that you could skate on the Midway.

Lester: You could skate on the Midway, but that was way south -- north. It was actually way north of where we were. And so I skated on the Midway when I was in college. But the early years no, it was on this field in Princeton Park.

Bergman: OK. So let's talk a little bit about later on. I know that you had this interesting series of experiences that finally led you to be educated at the University of Chicago.

Lester: Oh yes, yes, yes. Absolutely. When I applied to college -- nearing, of course, completion of my senior year in high school -- every place I applied to that was a good four-year institution, I was not accepted into. And I was in the top, oh, 20 of a class of 370 students that graduated. And so my alternative, had I not gotten a scholarship, was the city colleges of the city of Chicago.

Bergman: So do you think there was a racial issue that ...

Lester: I have no idea. But let me expand on how the University of Chicago came about. Because through the normal application process, I got *nothing, but* it happened to be that *{NOTECHANGE}* there was a librarian at the high school who had endowed a scholarship to the University of Chicago.

Bergman: This was Victoria, was that her name?

Lester: Victoria Adams was the name of the librarian and of course Victoria A. Adams, as I recall, the name of the scholarship. And there was a committee -- high-school campus committee.

Bergman: Was that based on good progress in history?

Lester: Well, let me, let me talk about that for a moment. We did take a test -- supposedly in history. This test focused totally on the Franco-Prussian War. Now what did I know about that? Remains to be seen. So it is clear to me that this depended on what this small committee -- I think it was three of them, three teachers -- decided who should be awarded it. But it had been dormant for a number of years. And so three students of my class were awarded the scholarship -- a white girl, a white guy, and myself. And so we went off to the University of Chicago. The white girl decided, after a year, the social life at the University of Chicago was

the pits. And so she went off to the University of Illinois, Champaign-Urbana. The guy wanted to be a thespian, an actor, so he went off to UCLA.

But for me this was the only game in town. This is by far the best education I could get, 'cause my folks couldn't afford to send me. My father had us solidly ensconced in the lower middle class. He had passed the civil-service exam in the mid '30s, and was a mail carrier. And that's something, by the way, that I'm very familiar with, because in about my junior year of college I worked in the same place that summer. Dad said "This is where I work. Make sure you do a good job here." "Yes, Dad, got it, got it, got it." But there's another story there in that respect. And so, actually that summer I made a lot of money. I threw a case, for those unfamiliar, carried the case, then drove collection afterwards. And so I was on the clock maybe 10 to 12 hours a day. So it was very profitable. So a good experience, let me put it that way.

Bergman: So talk a little bit about what happened after that.

Lester: After being admitted to the university, it was an eye opener. To begin with, the first year, I had an English course -- and this is in the College of the University Chicago. Of course there is the college and then the specialties college. It led to a PhB; that is a bachelor's of philosophy in what we used to say, in nothing. Because what could you do? Go into the divisions -- chemistry, physics, mathematics and so forth. And in, but in that English course in the college, the professor went around the class asking us where we came from. And so it was very interesting to learn about all these places internationally where fellow students came from. So that was an eye opener. And then my second year took Chemistry 1 -- finally got to that and so forth....

Bergman: Who did you take it from?

Lester: Oh, what was his name? He was the secretary of the department. So I can't offhand recall his name. But the other aspect associated with my first year was I went out for at the basketball team. And the University of Chicago's basketball team had the record of one win and 46 losses over the previous three years. And so we went out -- mostly freshmen and one senior who went out for the basketball team. The senior became the captain. I fortunately became a starter that year, to go through the undergraduate experience in basketball. The second year I broke the scoring record which existed for x number of years. I can't remember what x is -- broke it twice before I finished. And in that respect the single-game record that I set was 42, broken in the three-point era. And when I was inducted into the Athletics Hall of Fame at the University Chicago in 2004, the emcee was a guy from ESPN, who said that I would have been in the mid 50s, had the three-point rule existed back when I played. But to this day I still have two records standing: most field goals in a game (19) and season average (25 and a half). And I have some plaques associated with my induction into the Hall of Fame.

So truly the most positive aspect of my undergraduate experience was basketball. Chemistry important and so on, but I have to back up in terms of the chemistry. Why? Because as a senior in high school, a buddy said they were hiring over at the University of Chicago. So I went over to the employment office and they said "Well, what can you do? Can you type?" "Oh, I just finished my junior year." And so they gave me a typing test on the spot. I did 32 words a minute, but no mistakes. They said "You're slow, but you're accurate. We have two jobs: cleaning monkey cages in the medical school at 88 cents an hour, or typing in physics at $1.09." So off I go to physics and what do I find? Professor Robert *Mulliken, {NOTE*

CHANGE} who as you well know won the Nobel Prize some 15 years after I started working there. I had no influence on that. And then it was an exposure to theory. In other words, I was over there to type reports to the funding agency, which was the Office of Naval Research. And I was doing this on an electric typewriter, an IBM electric typewriter before Selectric. This was a typewriter with interchangeable keys -- the Greek alphabet, mathematical symbols and all this sort of thing....

Bergman: You had to switch them manually?

Lester: Absolutely and we had -- they developed a large box, where we put keys so you know which ones are in at a given point.

Bergman: Sounds like slow work.

Lester: Oh, hey, you pay the price. And I retain some of the reports I typed and others did at that time, and I enjoyed showing those to new graduate students *who would {NOTE CHANGE}* come to the group. "This is the way it used to be and this is what we did."

Bergman: So let's talk a little bit about, you know, what happened after that -- graduate school and so on..

Lester: Oh yeah. OK. Because of basketball, you might say, I did not blaze the top like I did in high school. But I did well. But there again, when I applied to graduate schools -- similar to my experience in applying to undergraduate schools -- no success.

Bergman: It was harder to get in those days. You know, I applied to Stanford, didn't get in. So maybe that was a general experience.

Lester: Well, I don't know, since that was my only experience. But the then-chair of Chemistry at Chicago was Henry Taube...

Bergman: Another Nobel ...

Lester: Who later on -- maybe I helped these folks along the path, I don't know. But in any event he said "Well, Lester, we'll let you stay." Which was not generally done, to stay for an advanced degree at an institution of that quality, if you had your undergrad degree there. I said "absolutely." So I stayed for a masters. My master's project was under the tutelage of a guy name of Stuart Rice. I know you've heard that name. He was, at that point, a fresh-faced new professor. And so he was actually very helpful in getting me admitted for doctoral studies at Washington University in St. Louis.

Bergman: Was there an issue of not going on to a Ph.D. at Chicago? They wanted you to go somewhere else, since you'd done ....

Lester: Oh yeah, yeah. They let me stay for a master's and "That's it." That sort of thing. So, no, a Ph.D. was not in the cards for the University of Chicago.

Bergman: So then you went to Washington University?

Lester: Went to Wash. U. I should add that in Christmas midterms, associated with that

particular academic year, I got married. I hadn't planned on getting married. But before I left for St. Louis my then girlfriend said "Well, what about me?" And I said "I have no money. I just have a teaching assistantship."

Bergman: That was Rochelle?

Lester: That was Rochelle. And so she said "Well I'll work," which she had been a social worker in Chicago. So we did the calculation and said "Yeah, that will work." So we got married that Christmas, and so forth. "So forth" means that within a couple of months she was pregnant. So much for diaphragms and their adequacy -- because I have had two diaphragm children before I finished my doctorate, let me say this. But in any event, at Wash U I went to work for a guy named Weissman. I don't know if you knew Weissman. I'm trying to think of his name -- Sam Weismann. Sam Weissman was one of the experts in ESR [electronic-spin resonance] -- that was his primary thing, and was primarily research, [electronic-spin resonance, *for {NOTE CHANGE}* molecules, yeah. *{OK}*

Lester: And so I couldn't and so I called the folks at Catholic University. The reason being that a research associate with *Mulliken {NOTE CHANGE}* -- and I was with that operation, in terms of working throughout my undergraduate years -- had said if you want to get a degree dealing with *molecular orbital* theory in quantum mechanics and so forth in a chemistry department, there *{NOTE CHANGE}*were very few. Because *Mulliken {NOTE CHANGE}*was in the physics department, of course, as I mentioned early on. And so he said "At Catholic, where I got my degree in chemistry, a Ph.D., that would be a good place to go." Oh, OK. So I had applied to them. I'd been admitted but didn't know that until after the April 15th deadline for responding to the institution you would go to. Why was it so late? Because my letter went to Dr. William A. Lester, head of student health, *at* a sanitarium in suburban Chicago. *{NOTE CHANGE}* That was real.

So I called Catholic -- upon completion of that academic year, I called Catholic. Asking if I could come. And they said yes, I could. So this I planned to do. But I also needed some income for that summer at the end of the academic year. And so I went around to three companies: Mallinckrodt Chemical, Monsanto and Tri-City Oil.

Bergman: But there was some reason you went to Catholic -- there was a certain person you wanted to work for, right?

Lester: Oh yeah, it was the research director for -- this was one of the few women in theoretic chemistry, Virginia Griffing.

Bergman: Yeah, right. So it was interesting, right? I mean she had to be one of a very small number...

Lester: Yeah, yeah, yeah, Yeah. So but that summer, when I discovered I couldn't find anything, there was a professor in chemistry, Lindsay Helmholtz. Well, as I mentioned, I called Catholic and they said "Come on." Needed employment that summer and this professor in chemistry at Wash. U., Lindsay Helmholtz, said "Well you can come in, do some experiments with me." Well mind you my focus was theory, but absolutely this paid money, so I'll do those. And the goal was to seed crystals of germanium hexafluoride with manganese to *get an ESR signal. {NOTE CHANGE}*OK? And to precipitate the fluorides out of hydrogen fluoride (which you know how caustic that stuff is). So I'm dealing with rubber gloves, in a

hood, trying to precipitate these crystals with a dopant, and then put them in an ESR spectrometer.

Bergman: Is this what convinced you to be a theorist?

Lester: Let me look at this -- let me tell you part of the basis of this -- that for a full summer I was putting beautiful crystals into the spectrometer. No signal. No signal.

Lester: I said to myself "I knew that was a driver for being in theory." So really it was a very helpful experience, but it just cemented my direction, my preference -- which I already had a strong predilection to. There was no ifs, ands or buts about that.

Bergman: OK, so let's talk about finishing your PhD.

Lester: Oh, yes, yes. So here I am in a situation where I needed to get to Washington D.C. So I bought a used car, had a U Haul and piled everything in it, with my pregnant wife -- went motoring from St. Louis to D.C. Coming in on New York Avenue, and this other family came in on Pennsylvania Avenue. Their name was Kennedy. But the Lesters, you know, we had a different -- we didn't converge, by the way, except later on. Well, let me indicate now when that was -- that because of my financial hardship, the amount of money I was receiving at Catholic, I talked to *the* fellow at Chicago who had suggested Catholic. His name was *{NOTE CHANGE}* Bernard Ransell, Dr. Bernard Ransell.

Lester: And he said "Well, they have summer internships at the Bureau of Standards. Get in touch with this fellow Morris Krauss," which I did. And he said "Well, maybe he can do something for you." So I went out there and talked to Mo, which is what we called him. Mo said "Yeah, sure come on out." And so I did, and so on. Made some nice change that summer and then back to campus to satisfy requirements. And so the following Spring he called. He said "Would you like to come out again?" I said "I have two children now; I really need to go to school as fast as I can." He said "Well, people do get their degrees, you know, from here." Oh, really?

So I went in and talked to my research advisor, Virginia Griffing, and Virginia said "Well, Bill, what do you want to do? Get an education or make money?" And for me, you know, that's no dichotomy. I needed to do both. Unfortunately -- the way life is -- she died of an aneurysm that Spring.

Bergman: Oh, too bad.

Lester: And so off I went to *the {NOTE CHANGE}* Bureau of Standards and after that summer went on a regular payroll. I finished my last two years of graduate work as a GS9.

Bergman: Somebody must have had to sign your Ph.D., then -- somebody else?

Lester: Oh yes, oh yes. Actually it was signed by a priest there, Father John Dooling, who actually taught the *statistical {NOTE CHANGE}* mechanics course at Catholic. But there was another fellow who came, who actually was the reader. And I think actually he signed, being knowledgeable -- Ying Nan Chiu. *{NOTE CHANGE}.* He was a spectroscopist, whose wife was also a scientist. He went on the faculty at Catholic and she went on the faculty at Howard University. But the main point here is that it was Morris Krauss who suggested the problem I

worked on. And that problem dealt with something called correlated Gaussians, which were new and proposed by Francis *Boys {NOTE CHANGE}* publication in 1960. And we were talking, now '61 or '62, in terms of my being ('62, I guess it was), being at the Bureau of Standards, and so on this hot problem. And by the way, it's been cited many, many times subsequently. So we did two papers associated with it.

Bergman: So was this connected with the eventual development of density functional theory, or did that come later?

Lester: Density functional theory came much later.

Bergman: No, no, but I think you said something about it being the groundwork for that.

Lester: It could have been. I can't say anything about that at all.

Bergman: OK. So let's talk about postdoc -- the University of Wisconsin, right?

Lester: Yes, yes, yes.

Bergman: So again, we were there at the same time, I think.

Lester: When did you get there?

Bergman: '63.

Lester: As a graduate student, coming as a new graduate student?

Bergman: I was really just a kid.

Lester: So I arrived in the Fall of '64 as a postdoc with Richard Bernstein.

Bergman: Sorry I didn't know you then.

Lester: Where were you? Where were you located?

Bergman: In that new -- at the time a new building?

Lester: 1112 Johnson Street? *{NOTE CHANGE}*

Bergman: I think so, yeah.

Lester: What floor?

Bergman: Sixth, maybe.

Lester: OK, I was on the second floor. That's where the Theoretical Chemistry Institute was located and Bernstein was a member of the Theoretical Chemistry Institute.

Bergman: I took kinetics from him.

Lester: Did you?

Bergman: The same time you were there. I should have known you.

Lester: Well, I'm downstairs dealing with brand new problem directions -- separate, very different from my thesis. And this is dealing with internal energy states of molecules. And as you know the Schrödinger equation -- which is the big equation that one solves, in principle, if one can, to gain information on molecular systems -- can be separated in terms of nuclear motion and electronic motion.

And so the focus here was -- that is, nuclear motion being the atoms and the molecules moving, atoms and molecules moving with respect to one another. And the game that was of interest here was to compute rotational energy transfer by collision of an atom with a diatonic molecule. And so because this was computational this meant the simplest ones you could consider. And the other part of it here -- I guess that original case was to compute potential energy surfaces. But that was -- that became a sideline that I dealt with later. And we can talk about that further. But the main point here was that it involved solving coupled differential equations systems. And in that regard that was new to me. Ramped up on it, learned a lot about that sort of thing, got to know a lot of people in the Mathematical Sciences Institute, which you may be familiar with, and was able to compute accurately the cross sections expectation values for collisions of atoms -- an atom with a single diatomic molecule.

Bergman: So you know the computer facilities, were they OK at the time?

Lester: Absolutely inadequate at --

Bergman: At Wisconsin?

Lester: At Wisconsin. As a matter of fact, used some of the computational facilities at the Bureau of Standards for what I *needed {NOTE CHANGE}* to do. Oh, but then Wisconsin -- then the university as a whole had a computer center -- in the physics department is where it was housed. And they had, at that point, Control Data Systems, CDC 3600 was the first, followed by a 6400. And two years after I left Wisconsin is when the activists blew it up. But in point of fact, within eight months of my assuming the postdoc position, I was asked to assume the position of assistant director of the Institute. And I told Bernstein, I said "Dick, you know I have a regular job; I'm on education leave." He said, "Take the job, Bill, take the job." Oh, OK -- and I thought about it. So I did.

And the first day I'm reading correspondence between the NASA program officer (it was a NASA-funded institute) and the director. Second paragraph said "You need to diversify." So bottom line, Bob, it was an affirmative-action position. The guy who held the position went off to a regular job, faculty position elsewhere. So OK. But then I gave my talk to go on the faculty. And everyone approved of it except the director of the Institute, Hirschfelder. So Dick told me. I said OK. I retained this job for a few years as the assistant director. But also became a lecturer -- not on the academic stepladder -- at Wisconsin. And then they said "Well, you know, the money's getting tight; so we're going to dissolve your position." OK. So now it's Spring of '68. American Physical Society is meeting in Berkeley, American Chemical Society in San Francisco, a week apart. And so I took my family, brought them with me to the West Coast, because I have a sister who was living in the Bay Area that we would visit for the week, and so on. And furthermore during that week visit IBM Research.

Well why IBM Research? Because there are a number of guys working there who were graduate students or postdocs at the University of Chicago when I was in working for *Mulliken*, *{NOTE CHANGE}* in what was called the Laboratory of Molecular Structure and Spectra. And so I visited them. But furthermore -- now this was in a department called Large-Scale Scientific Computations, and headed by a guy by the name of Enrico Clementi. *{NOTE CHANGE}*

Bergman: So he was really at the forefront of big computing in those days, right?

Lester: Well he hadn't as yet. Because when I met him is when he was a candidate for a position on the faculty and in the institute at Wisconsin. Why? Because the fellow who was in the office next to me, junior faculty member, and this is facts: when Hirschfelder came to see him and said "You know, my name should be on this particular paper." And the guy said "If you insist, I'm leaving; I'm going back to where I was before." So he left; he went back to Argonne National Laboratory. Then there began a search for his replacement, and fellows at all levels who I'd known -- by reputation and so forth -- *came in including Clementi {NOTE CHANGE}* who at that time was on sabbatical at the University of Chicago, because he had been there with *Mulliken {NOTE CHANGE}* [0.1] So it's a tight little loop here. And so like when he was saying "I'm coming, I'm coming, I'm coming." And then one day he said "No, I'm not coming. What do I learn after I visit -- San Jose Research Lab...? Which is the IBM facility where he was located -- that he had been using the idea of leaving IBM and going to Wisconsin as a negotiation ploy to get created the large-scale scientific computations division.

Bergman: So that's how that happened.

Lester: That's how that happened. And when I visited, he said "I want you to meet someone." Took me down the hall and introduced me to the director of the laboratory and said "We're going to hire this guy." And we get back to [name?]'s office, I say "So what are you talking about?" "I know you work from when I was a candidate in Wisconsin." I mean you can't make this up! And so I was hired there -- and by the way, I should add, I was going to make a whopping 5,000 dollars more a year. I would have all the computing resources I would need, I would be in California. So it was, you know, no brainer. So the Lesters moved in 1968 to San Jose, Calif. I should interject here that a year and a half before moving, that winter, my daughter, who is now five years of age, in the three-block walk from kindergarten to the house, suffered frostbite, because she took her mitten off.

Bergman: This was in Wisconsin?

Lester:, Madison. This was a Madison winter, for which that day the high was 11 below. I'll never forget that temperature. She came in screaming. So anyway, that was one of those things.

Bergman: So California was a pleasant change?

Lester: Yeah, yeah. And so I *continued {NOTE CHANGE}* the research I started at Wisconsin. Got a lot done, and that's when I established some of the standards by which others were able to calibrate their algorithms for the *computational approach - that sort of thing. {NOTE CHANGE}*

Bergman: OK, so let's talk about the next stage in your career. You were at IBM for quite a while, right?

Lester: Yeah. Well there's an interesting point: along the way I was -- became a candidate for the fast-track: promotion, moving up the system. And so that involved spending a year on the staff of the vice president director of research -- at that point Ralph Gomory. So we *moved {NOTE CHANGE}* to White Plains, N.Y. And so my wife and I, we enjoyed it.

Bergman: That was another IBM --

Lester: That was a separate facility. This is the headquarters of the IBM Research Division. The IBM Research Division was headquartered in Yorktown Heights, had satellite laboratories at Ruschlikon, just outside Zurich, as well as San Jose. And the Ruschlikon guys had gotten a Nobel two years earlier in physics. But in any event it became clear to me during the course of that year that I did not want to marry the corporation. I mean that it would have been a totally different path from which, you know, the science driver would not be *satisfied. We did {NOTE CHANGE}* like White Plains. So I asked *Gomory,{NOTE CHANGE}* I said "We would like to stay here." "Oh no, chemists belong in San Jose."

So when I get back to San Jose things didn't work out too well there. So it was clear to me it was time to move on now. In the meantime the National Academy of Sciences had been doing a study of the creation of something called the National Resource for Computational Chemistry -- geared towards assisting chemists across the country in being able to better use computers. And furthermore asking them the questions -- the question, rather -- of subdisciplines of chemistry. How can computation advance this particular area?

So I threw my name in the ring for that and I was selected actually. It was at a time when the general view of chemists -- you may recall this, I don't know -- who said "Computer? What do you need a computer for? If you have a question, you ask a good chemist. They could answer that."

Bergman: Especially in my area. OK. Let's talk a little bit about what happened later. Start with your interaction or running the NRCC, the National Research for Computational Chemistry ...

Lester: National Resource for *Computation in Chemistry*. *{NOTE CHANGE}*

Bergman: Yeah, and how that led to the associate directorship at LBNL [Lawrence Berkeley National Laboratory], and eventually the Berkeley faculty appointment.

Lester: Well actually the issue here is that the NRCC was under contention by various national laboratories for housing, and Lawrence Berkeley Lab won that competition and so it was sited at Lawrence Berkeley Lab. And so this meant that I would have to travel from San Jose, where I lived, to Berkeley. And I did that for a few months and came in.....

Bergman: And that was not quite as hard as it is today?

Lester: Relatively speaking. It should be impossible to even think about the concept. But in point of fact, in addition to being director of the NRCC you see the position came with being an *Associate Director {NOTE CHANGE}* at Lawrence Berkeley Laboratory (and there were

about eight *Associate Directors {NOTE CHANGE}* at that time). And my appointment began in February of 1978. The concept then of NRCC was approved in November of 1977. When I come *in {NOTE CHANGE}* what I am told is that -- this is to be a three-year experiment, by the way. That there would be one year of funding and then we would be reviewed for continuation.

And I said "Well, this makes no sense at all, because you can get nothing done in a year. You still have to hire a staff to do the sorts of things." They said "OK, we'll give you another year before we have a review." "OK, fine."

Bergman: I don't know if you remember, but that -- '78 was the year that I and Earl *Muetterties {NOTE CHANGE}* got to Berkeley, same --

Lester: Oh, really, oh, no, I'm up at LBL, you know, totally consumed by this mandate.

Bergman: We were both living in the same area.

Lester: Is that right? OK, OK.

Bergman: It was like I was following you around for your whole career. 'Cause, you know, I moved to Caltech the same time you went to IBM.

Lester: Oh really? Oh, OK. Did not appreciate that. OK. So one of the main things, of course, within NRCC, was to hold these workshops in which you'd ask the question, then bring together the various folks in these various disciplines. And so that dominated the activity. Besides hiring staff, young people to work in the various subdisciplines of *chemistry, they were {NOTE CHANGE}* to have their own research activities, which we supported and so on, and one of the guys I hired was a guy by the name of David Ceperley. David was someone that Berni Alder, internationally recognized physicist, had said "Bill, you got to hire this guy," and so forth. Well I looked at his vita and so forth. It looked good, so I did. And the reason that Berni was pushing this is that David had been carrying out research and *quantum {NOTE CHANGE}*Monte Carlo -- that is for systems which are of a quantum nature to do, also, Monte Carlo calculations. And Berni was an expert in classical Monte Carlo. And so David was carrying out research with Berni while at the same time helping to develop the cadre of programs associated with *statistical* mechanics *{NOTE CHANGE}*.

One day David came into the office and said that they had gotten a 100 percent of the correlation energy for the electron gas. This is the model system in which consist of electrons running around each other. *Correlation {NOTE CHANGE}* energy is the difference between the total energy of a system and the energy associated with something called the Hartree-*Fock {NOTE CHANGE}* Approximation, which considers each electron in the field of the N minus one electrons and does the average to get the energy. So on that basis I said, "Well, you're not going to do that -- well if the stuff is really good, you should apply to atoms and molecules.".

"But Bill, I'm a condensed-matter physicist -- though he was working in a theoretical chemistry institute, so to speak. I should say NRCC was basically a theoretical chemistry activity. And so this became the point where -- there was a fellow who had gotten his bachelor's at Berkeley, Ph.D. at Boston University, who wanted to come back. And I said "Well if you'll come back and work on this problem, I will cover those costs, so we can see

what they can do." And he did. This is Peter Reynolds. Peter subsequently, at the conclusion of NRCC, went on to become a program officer with the Navy, and then subsequently with the Army and various activities of this sort -- an outstanding scientist.

But the main point: this first atoms and molecules paper -- based upon a review of publications of citations by one of my graduate students maybe a decade and a half later -- was the most cited paper, aside from the book we wrote about mid '90s regarding research that we carried out. It was new and people were interested. The quality of the results that we got were better than anything around. So that was really a plus.

And so on that basis I changed my research direction. I said "If this stuff is that good, let me see what it can do for other atoms and *molecules,* and molecular systems. Computing *barriers to reaction, {NOTE CHANGE}* structure of molecules, all that sort of thing." So that became the focus and that in general it was well received. A lot of people started to use the technique and so forth, and it let my receiving some awards. Well, election to, well, the International Academy of Quantum Molecular Science occurred some years later. A *Fellow* of the American Physical Society, *Fellow* of the American Chemical Society, *Fellow* of the American Association for the Advancement of Science -- various things of that sort, which -- you know, you know all about this stuff.

Bergman: OK, so let's talk about your career at the University of California -- how it started.

Lester: Yes, well that dates back to when I was at IBM. Why? Because of two faculty members in theory for molecules at Berkeley -- Fritz Schaefer and Bill Miller. They sent two students down to work with me, and these students were so good I said to myself "No wonder those guys are so productive." Barbara Garrison subsequently, when she finished, went to Penn State, became *Chair* of *Chemistry* at Penn State. The guy, *Andy Raczkowski, {NOTE CHANGE}* unfortunately passed some years later. So it told me something about the quality of students, graduate students, at Berkeley and so the thought of -- when NRCC was closed after review (because the basis of continuation was that we were to be reviewed two years later) -- David Shirley, who you knew, and I agreed that this was nonsensical. What they were trying to do was just bleed us to death and that would kill us in two years. So why go through that? So it was closed. I should add that the NRCC was jointly supported by the Department of Energy and the National Science Foundation, and after our closure the Department of Energy said "Well, had we supported it by ourselves we would have continued it." But the National Science Foundation sent out questionnaires to the chemistry community, asking what the impact of NRCC had been, after a year and a *half and {NOTE CHANGE}* most folks had not heard of it. It was ridiculous.

Bergman: And Dave Shirley was lab director at the time?

Lester: No, at that time Dave, I think, was --

Bergman: Associate director?

Lester: Oh, no -- yes he was *Director* of Lawrence Berkeley Lab. He had succeeded Andy Sessler, wo was the guy who hired me a year or two earlier. Keep that in mind, for sure. Yeah, David was.

Bergman: Yeah. OK. So shortly thereafter you joined the faculty?

Lester: Yes. Shortly thereafter I joined the faculty. Well, one faculty member said he would like me to stay. I said there was only one way I would stay. And there *was consternation {NOTE CHANGE}* on that. But it was made to happen and so I became a faculty member in chemistry, full professor.

Bergman: What year was that?

Lester: It was 1981.

Bergman: And so talk a little bit about both your scientific activities at Berkeley and the other things that you did.

Lester: Oh yes, yes -- well, continued research with *quantum* Monte Carlo -- applying Monte Carlo to molecular systems and so forth. Oh, I should add in the meantime, in 1979, my son won a Regents' Scholarship and came to Berkeley and majored in Electrical Engineering and Computer Science. Finished in '83 or so. Now let's see, about the same time my daughter won an IBM scholarship*, Cary Award, {NOTE CHANGE}* and she went off to UCLA and then continuing -- oh my son, having come to Berkeley, he was involved in something called the Professional Development Program [PDP], which assists primarily students of color through the beginning course -- in this case calculus and mathematics. And I liked what they did. Primarily students worked with each other at various levels to assist each other through this demanding course. The guy who was head of it initially was Uri Treisman, who received a MacArthur for his work with PDP.

And so it was in the early '90s that I was asked to be *Associate Dean for Undergraduate Affairs* in the College of Chemistry. I accepted this, and my first thought was we should map that into the *College of Chemistry*, which was done. And so we had a College of Chemistry Scholars Program comparable to PDP in mathematics. And it was like the mid '90s when Neil Lane, who had done similar work -- who was at that point director of the National Science Foundation --asked me to come back and work with him on their diversity issues. And so I went back and I had the title Senior Fellow for Science and Engineering and Assistant to the Director for Human Resource Development. This is the longest title in the world.

Bergman: So you had to take leave from Berkeley?

Lester: Absolutely. I was not -- no, I did not take leave. There's something that's called, with the government, IPA -- *Intergovernmental {NOTE CHANGE}* Personnel Act. So I maintain my continuity with campus, so lost no time, though I was physically in Washington D.C.

Bergman: Did you still have to teach, or no?

Lester: Oh, not at all. I'm totally in Washington D.C., but I am not on leave, make the technical distinction. Because that is, you know, an issue, in in terms of people going to work for the government, in that respect. And so that was interesting. We did what we could, and so on. But Neil said "Oh well, we've got you in the *government now.*" I said -- well, after a year or two I said "No, I'm going back home." I needed to go back if I was to maintain my position at Berkeley.

Bergman: Neil became science advisor?

Lester: He became Clinton's *Science Adviser*, yes. And we would go to the Hill on occasion and it was interesting, you know, walking around the Hill, doing things, talking with the various folks. And I have some pictures, you know -- of Rochelle and I -- not with Clinton but with others in the government. So it was an interesting, broadening experience -- no question about that.

Bergman: So talk a little bit about -- I know that you've been involved with intercollegiate athletics on both the campus and national level, right?

Lester: No, this is primarily campus. I was -- every institution which engages in sports sanctioned by the NCAA, has a faculty athletics representative. And so I was asked to be *Faculty Athletics Representative*. Why? Because a fellow who was my predecessor had asked the campus to vote no confidence in Chancellor Berdahl, the chancellor at that point. And so Berdahl said "I'll have nobody work for me who does not support me." So he let Citrin *{OK}* go.

Well, in any event -- so I became that -- held that position from 1999 to 2004. But in my initial year my name appeared on the front page of *The San Francisco Chronicle*, because I had approved two guys as being *eligible and* {*NOTE CHANGE}* it turns out further...

Bergman: For like football?

Lester: Football, exactly, yes. Eligible for football. And turns out they were not. My basis of approving them was that I had talked to the professor from -- who had provided them with passing grades. I had nothing to substantiate that investigation by the Pac 10...

Bergman: So there was an academic requirement for football players?

Lester: For everybody.

Bergman: Sorry, I didn't know a lot about this.

Lester: Well, you just have to pass your courses. The point here is simply that -- well, I won't go into the this and thats. We can talk about that off scale or off line. But in point of fact there was an investigation by the PAC 10 (the Pac 12 *now) and {NOTE CHANGE}* the faculty member could not come up with data to support the grade he provided. And here I had approved them for intercollegiate athletics. The University of California Berkeley was declared institutionally irresponsible because of *moi*. So I had never had the experience of not getting the truth from my professional colleagues. So this was an eye opener. So people --

Bergman: Was there any reason -- do you think this guy was paid off, or...?

Lester: No.

Bergman: His love for football?

Lester: No, I think he was -- he wanted to be helpful in terms of diversity issues. OK. And he sort of leaned over the wrong way. …*and* Berdahl said he understood it all.

Bergman: What was the resolution to that?

Lester: But even before that, the reason that there was unhappiness by the previous faculty athletics rep, was that Berdahl had approved, I think some seven or so FTEs to ethnic studies. And that was the core of the issue that led to the problem of calling for a vote of no confidence in him and all that sort of thing.

Bergman: Let's talk a little bit about your interactions kind of in a local level in the chemistry department, maybe the physics department if you had some. I'm thinking specifically of -- you know, you had some really good colleagues in the theoretical group.

Lester: Oh yeah.

Bergman: So you could talk a little bit about your interaction with those people.

Lester: I did mention early on that Fritz Schaefer, who subsequently went to the University of Georgia, and Bill Miller sent a couple of students down to work with me when I was at IBM. Both outstanding scientists -- Fritz for electronic structure and Bill for collision dynamics that he is you know *highly* recognized for. So, you know, excellent people. My next door neighbor is David Chandler and David is an institution in its own right, as we say. Other theorists, let's see, who comes to mind? Those are really -- that's the core...

Bergman: In those days, anyway, and you had...

Lester: Oh, I should mention my fellow University of Chicago graduate, Robert Harris. Bob Harris came to Chicago by the way my last year as an undergrad. We're in class together and subsequently -- and we both had the same adviser; my master's advisor was his PhD adviser. And so he was on the faculty, of course, for a number of years before retiring some years ago now.

Lester: And I did retire some years after the normal retirement age.

Bergman: That's become not so normal anymore.

Lester: Right. Right.

And so an excellent group. I think the excellent set of deans. Bradley Moore – *C. Bradley* Moore was the dean when I went off to NSF. And I think *Clay Heathcock {NOTE CHANGE}* later became dean of the college...

Bergman: Was chair for a while.

Lester: Yes, yes, yes, yes, yes.

Bergman: So it was always a very strong group I think, right?

Lester: A strong department. Of course our -- you know where our departed is recognized internationally. Number one, folks, in case there's any question.

Bergman: I mean, you know, the theory has always been this very strong.

Lester: Very strong, very strong.

Bergman: So you mentioned you know becoming an emeritus professor. So do you have any -- tell us a little bit about what your thinking was about when you decided to become emeritus.

Lester: OK, I'd be happy to. My deciding to become -- retiring -- was after my wife passed that March.

Bergman: I'm really sorry.

Lester: I retired. And then I was asked to be professor -- to come back -- I guess Professor of the Graduate School had been created at that point. And I still had postdocs and even a few students. No, I hadn't taken a student for a couple of years because whether I could see a student through five years, which was typical in terms of my kind of research, remained to be seen. So I just dealt with postdocs up until -- well, beyond age 65.

Bergman: Did you have any concerns about retiring or was it kind of normal -- did you feel it was a normal transition?

Lester: It was. I hadn't thought about retiring until *Rochelle's {NOTE CHANGE}* demise. And that's what shifted my whole focus and so forth. But I still had interest in doing things. So that's why becoming Professor *of* the Graduate school was valuable, and I had some very good postdocs who came in after that. I mean who have gone various places. As a matter of fact, there was an undergraduate from Morehouse College who came in 2011, who was going to come to Berkeley to graduate school. Also *there was a {NOTE CHANGE}* Festschrift for me in 2007, a guy I worked with at IBM in the mid '70s. His son -- he wanted his son to come and work with me. But they were caught up in the big vacuum of this outstanding graduate student I had had, Alán Aspuru-Guzik, who was, got an appointment at Harvard. And all these guys went off to Harvard. And *Rochelle {NOTE CHANGE}* said "Well that's good -- your students, where they're going." And I'd laugh.

And so in point of fact it was just this summer that I got this e-mail from the student, James Whitfield, and we met and we talked a bit. And reading Sunday's *New York Times* about quantum dynamics and computing, Whitfield is cited in one of the paragraphs. I just sent off an e-mail yesterday, "James, I'm reading about you in the *New York Times* Business Review section." So if you look at your predecessors, who's involved know that sort of thing. So, delighted with his success. Oh, another undergraduate -- an undergraduate did very well, did some research with me, went off also to work with Alán. I mean I'm doing this counting, who did this. And he has since come back to a postdoc at LBL -- I'm not sure where he is at this point, but he was really an outstanding undergraduate student here. I mean we have very good undergraduates, but this guy was in lock stock and barrel up there. So all of this was very good. I think made possible because of the quality of students we get at this institution.

Bergman: You know you've seen a lot of people in Berkeley and rise through the ranks. If you had some advice to give to young people who are starting in theory today, you want to say a little bit about that?

Lester: Oh, I would like back up, in terms of young people not just in theory but pursuing

what they are interested in. And I hearken back to my experience at IBM being on the staff of the *Vice President and Director on* the fast track -- that I decided that it was not for me. I'm sure many people would have jumped at that opportunity. So my first advice, even before we talk about theory, but if it is theory that you want, then pursue it, and pursue it in the direction that your inclinations take you. No ifs, ands, or buts about that. Not that what others would say it was good, and so forth and so on, but that what you and your gut as you well know view to be that direction ...

Bergman: Yeah, there is interest you know -- a lot of young people want to know whether there's a practical impact for what they're doing or not, you know, or something more fundamental.

Lester: Well I would argue that there's always going to be a practical impact on good stuff, because you can't anticipate. But if you see a direction that you can advance, there will be benefits that happen, that occur, that you cannot anticipate. So I think the discussion with young people who are concerned about this is to indicate to them that if you can contribute fundamentally to an area, there will be benefits more broadly to society as a consequence of making advances in that area.

Bergman: I would certainly agree with that.

I guess one thing I'd be interested in is whether you had any sense that being an African-American had an impact -- either on your appointment or anything that happened to you later.

Lester: Well, deep down I suspect probably there was some of this, but that had nothing to do with the appointment. I think it was just general background noise, so to speak, of being an African-American. And I also don't use "African-American;" I use "black American" because I know white African Americans. And so, no, nothing out of the ordinary in that respect. It's always difficult to achieve these appointments and I was pleased. I had been asked by some members of the department to stay and I said "There's only one way I would stay." I think they were thinking of my staying at LBL and, no, I did not want to do that. In terms of doing research and that sort of thing, IBM offered a better environment and that generally regard, versus LBL. But if you have LBL and the campus, then that's a whole different ball of wax.

Bergman: My recollection was that it was George Pimentel who was sort of the prime mover in proposing you to the faculty. Does that ring a bell?

Lester: No. I met George in 1968 when I visited the campus. And prodded by people in University Hall, in the administration and I visited. And it was said that there are a lot of good people who applied. And that's why I went to, in '68, to IBM. And it was a decade later or so that I ended up coming to the faculty -- a peculiar aspect of history.

Bergman: OK.

Lester: Well, thank you Bob for taking the time to do this.

Bergman: I'm more than pleased that I could do this, Bill, because you know, I mean, we've been colleagues for all these years and there's all -- so many things that I didn't know about you and your career, and especially about the times that we were in the same places,

sometimes without knowing it. So for me it's really been a really enlightening ...

Lester: The “silos of chemistry” issue.

Bergman: That's right. I think that's a good example. So I've had a really great time and I'm more than grateful to be able to act as your interviewer.

Lester: Thank you.