CHEMICAL HERITAGE FOUNDATION

FRED W. MCLAFFERTY

Transcript of Interviews
Conducted by

Michael A. Grayson

at

Cornell University
Ithaca, New York

on

22 and 23 January 2007

(With Subsequent Corrections and Additions)

ACKNOWLEDGMENT

This oral history is one in a series initiated by the Chemical Heritage Foundation on behalf of the American Society for Mass Spectrometry. The series documents the personal perspectives of individuals related to the advancement of mass spectrometric instrumentation, and records the human dimensions of the growth of mass spectrometry in academic, industrial, and governmental laboratories during the twentieth century.

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FRED W. MCLAFFERTY

1923 Born in Evanston, Illinois on 11 May

Education

1943 B.S., University of Nebraska, Chemistry
1947 M.S., University of Nebraska
1950 Ph.D., Cornell University

Professional Experience

University of Iowa
1950 Post-Doctorate under Ralph Shriner

Dow Chemical Company
1950-1956 In charge of mass spectrometry and gas chromatography
1956-1964 First Director of Eastern Research Laboratory, Basic Research

Purdue University
1964-1968 Professor of Chemistry

Cornell University
1968-present Peter J. W. Debye Professor of Chemistry (Emeritus)

Honors

1972 American Chemical Society Award in Chemical Instrumentation
1975 Spectroscopy Society of Pittsburgh Award
1981 American Chemical Society Award in Analytical Chemistry
1983 Honorary DSc degree, University of Nebraska
1984 New York Section- American Chemical Society Nichols Gold Medal
1985 International Mass Spectrometry Society J. J. Thomson Gold Medal
1985 Cincinnati Section- American Chemical Society Oesper Award
1985 The Association of Analytical Chemistry Award
1986 East Tennessee Section- American Chemical Society S. C. Lind Award
1987 Ohio State University W. L. Evans Award
1987 Honorary DSc degree, The University of Liege
1989 University of Naples Gold Medal
1989 American Chemical Society Award in Mass Spectrometry

1992	Royal Society of Chemists Robert Boyle Gold Medal
1994	Pioneer in Analytical Instrumentation Award
1995	Honorary DSc degree, Purdue University
1996	American Institute of Chemistry Chemical Pioneer Award
1997	Utrecht University J. M. Bijvoet Medal
1999	Czech Academy of Sciences J. Heyrovsky Medal
2000	Italian Chemical Society G. Natta Gold Medal
2001	Swedish Chemical Society Torbern Bergman Medal
2003	American Society of Mass Spectrometry Award for Distinguished Contributions to Mass Spectrometry
2004	French Chemical Society Lavoisier Medal
2006	International Association of Protein Structure Analysis and Proteomics Pehr Edman Award

ABSTRACT

Fred W. McLafferty's oral history begins with a discussion of his family's history of education and his early life in Nebraska during the Great Depression. Sparked by a high school chemistry class, McLafferty decided to pursue the subject at the University of Nebraska. Because his undergraduate career coincided with World War II, McLafferty entered an accelerated degree program and enlisted in the war. After months of combat, he returned for graduate work at Nebraska, where he earned his Master's degree and published papers as an analytical chemist. After moving to Cornell University to pursue his doctorate degree, his interest shifted to organic chemistry and his work on organofluorine compounds began. In 1950, after completing his degree, McLafferty entered industry at the Dow Chemical Company in Michigan, where he was introduced to mass spectrometry. There, McLafferty and Roland Gohlke helped develop instrumentation and gas chromatography-mass spectrometry. After several years, McLafferty was sent by Dow to Boston, Massachusetts to direct its new research lab. There he worked on patents and the McLafferty rearrangements in mass spectra correlations and utilized time-of-flight. In his oral history, McLafferty speaks often of the community and meetings of mass spectrometrists, and how he has collaborated and interacted with this community in the past fifty years. In 1964 he left Dow for an academic position at Purdue University, where he created a new research program. He continued his collaboration with Gohlke and also started collaborating with Klaus Biemann on topics such as collisional activation and gas chromatography. While at Purdue, McLafferty consulted for companies like Dow and Hitachi, and began securing grant money for research. After four years at Purdue University, he became Peter J. W. Debye Professor of Chemistry at Cornell University. McLafferty discusses his longtime position at Cornell University, which has allowed him both to publish landmark works and to develop techniques like electron capture dissociation and top down proteomics, and his most recent research work, which has included published papers on the use of ammonium tartrate and succinate in electrospray solution. McLafferty concludes his interview by discussing his impressions and remembrances of his long list of peers.

INTERVIEWER

Michael A. Grayson is a member of the Mass Spectrometry Research Resource at Washington University in St. Louis. He received his B.S. degree in physics from St. Louis University in 1963 and his M.S. in physics from the University of Missouri at Rolla in 1965. He is the author of over forty-five papers in the scientific literature. Before joining the Research Resource, he was a staff scientist at McDonnell Douglas Research Laboratory. While completing his undergraduate and graduate education, he worked at Monsanto Company in St. Louis, where he learned the art and science of mass spectrometry. Grayson is a member of the American Society for Mass Spectrometry [ASMS], and has served many different positions within that organization. He has served on the Board of Trustees of CHF and is currently a member of CHF's Heritage Council. He currently pursues his interest in the history of mass spectrometry by recording oral histories, assisting in the collection of papers, and researching the early history of the field.

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INTERVIEWER: **Michael A. Grayson**

LOCATION: **Ithaca, New York**

DATE: **22 January 2007**

GRAYSON: So, I'm going to start off saying that this is Mike Grayson. I'm in Ithaca. On January 22, year 2007, and I'm interviewing Fred McLafferty, who is a mass spectroscopist of note. What we're going to do is fill in some blanks. We already have some oral history by Carsten Reinhardt and other material on Fred. There's also a nice video history that Cornell has which is available on the web. So, I'm going to try and pick up things that these other interviews had as well as get some other insights into Fred's career.

McLAFFERTY: I'm Fred McLafferty and I'm delighted that we have Mike Grayson here, even though we had to use such a flimsy excuse to do it!

GRAYSON: Yes. Well, I don't think it's a flimsy excuse. If we run across names I'll want to write down the spelling of them. We do that in the order that they occur in the recording because then when the transcriptionist hears the name they'll be able to refer to the spelling. And, I'd like to remind you to try and date things with at least years. When we talk about events let's try and get the year that they occurred, so we have things dated. So actually, one of the first things I would like to find out is where were you born?

McLAFFERTY: Evanston, Illinois. Nineteen twenty-three, May 11th.

GRAYSON: I don't know if you know this, but the Cornell website that has your bio ---

McLAFFERTY: But, it doesn't say where?

GRAYSON: And, it says, Born May 11th in" [laughter] and a dash. So, I thought I'd go ahead and get that information out of the way.

McLAFFERTY: And I think Carl Djerassi's birthday is the next month.

GRAYSON: Oh really?

McLAFFERTY: So, I think he's very young.

GRAYSON: Oh, okay. [laughter] The same year I assume?

McLAFFERTY: Same year.

GRAYSON: Yes. I did look at the video interview that Cornell had done and I was interested in the fact that in that interview you mentioned that both your paternal and maternal grandmothers had gone to school, had gone to college. Is that correct or did I misunderstand what was said there?

McLAFFERTY: Yes. They'd all gone to college. Right. One time when our son Joel was applying to Harvard, my mother was there. He said, "It says here, 'Did I have any relatives that went to Harvard?'" Both my wife and I said, "No." My mother said, "No, that's not true. You're descended from the second president of Harvard." So, that's according to my mother. I could look it up. Why are you interested?

GRAYSON: Well, I mean, this would have been the late 1800s, right, for your grandparents, grandmothers?

McLAFFERTY: Yes.

GRAYSON: And were they from the East or the Midwest?

McLAFFERTY: Both—well my grandfather McLafferty and his wife were from El Paso, Illinois, and they did not go to college. He came earlier to Nebraska. He was Fred McLafferty also, and so I have a teaching certificate for Fred McLafferty dated 1877 in Omaha, Nebraska.

Okay. So, my, my mother's mother, Julia Lowry, was from Chicago and she had gone to college. Her brother was assistant superintendent of Chicago Public Schools. Her husband, my grandfather, was from Springfield, Ohio and he had gone to Antioch College.

GRAYSON: Okay, looking back at some of the material that I've seen in the past it was unusual for women to go to college and get a college degree, even into the 1920s, teens and 1920s it was not common. And so, I just wanted to know their educational background. Obviously, education was an important part of your heritage?

McLAFFERTY: My mother's parents came to Nebraska and first lived in a sod house and they were married in 1883 when she came. So, they were Nebraskans for a long time after.

GRAYSON: So, now with regard to your immediate family, your mother and father, your parents, were they in an intellectual environment? What did your dad do for a living?

McLAFFERTY: Well, my father was a civil engineer and they met each other at the University of Nebraska. And, my wife's father was a dentist, and her parents met at the University of Nebraska. [laughter] I met my wife at the University of Nebraska. So, there's the -- that's a real intellectual background.

GRAYSON: Yes. [laughter] Do you have any brothers and sisters?

McLAFFERTY: I have a brother. I have an older sister who lives in Peoria, Illinois and she got her bachelor's degree and taught school, and has four boys. And, I have a younger brother who lives in Orangeburg, South Carolina, who is a CPA. But just last month I went down there to see him get his PhD at age seventy-nine. Because all of his children, he has three children with PhDs, and his wife died a few years ago and he decided he would get his PhD. So, don't ask me where all of that came from.

GRAYSON: So, I'm just trying to get a sense of the intellectual climate in your family growing up.

McLAFFERTY: Well, to go back one more step on my mother's side, my grandfather's name was Joseph Warren Keifer, Jr., and his father, Joseph Warren Keifer, Sr. was a Major General in the Union Army in the Civil War at age twenty-nine, and then became Speaker of the House of Representatives in Washington. And so, at least my mother regaled us with stories of sleeping at the White House and things like that. And, whether that inspired us—the Depression was hard on everybody in Nebraska including our families. And so, let's say my growing-up years I don't remember anything greatly elegant. But, the only time I met my great grandfather was down on the farm. My mother was born in Bostwick, Nebraska.

GRAYSON: What was the, how's that?

McLAFFERTY: Bostwick, Nebraska. It had two hundred people when I was a kid and hardly has twenty now. But, great grandfather came and all the grandchildren came to meet him, and he gave us each a twenty-dollar gold piece, and that was really something. And when we started to put it in our pockets our parents said, "Oh, let's put them on the top of the piano so everybody can enjoy it." And guess what? Next morning they weren't there. [laughter] And, we never found out where they went, except it was the height of the Depression. [laughter]

GRAYSON: Hmm. So, this would have been what, 1929, 1930?

McLAFFERTY: This would be around 1929 or 1930.

GRAYSON: Okay.

McLAFFERTY: Yeah. I can still remember it, so I must have been seven years old or something.

GRAYSON: So, was there any person in your early education background that kind of got you interested in science in general or chemistry in particular?

McLAFFERTY: Oh, my high school chemistry teacher was great.

GRAYSON: You remember the name?

McLAFFERTY: I can always remember his name, but now I can't.

GRAYSON: Well, we'll, you know—

McLAFFERTY: If I remember it I'll tell it to you. It was J. H. Dorsey.

GRAYSON: So, this is high school in what city?

McLAFFERTY: In Omaha. Omaha North High School. And, this teacher was great and I loved chemistry and I decided I'd do chemistry. In those days everybody went to the University of Nebraska. You didn't worry about which college to go to or anything like that.

GRAYSON: You just went there?

McLAFFERTY: Yes, that's, everybody. My sister went there, my brother went there, and everybody I knew went there. And I found, I didn't even know until I got down there that I had to choose between chemistry and chemical engineering. I was so totally naïve when I got there and I took chemistry, and well I've been in chemistry ever since.

GRAYSON: So, in your chemistry in high school did you have a year, like a junior-year course, or did you have several years of chemistry with this professor?

McLAFFERTY: Oh, in those days physics was the junior-year course, science course, and chemistry was the senior-year science course. And, you had mathematics and things like that. I enjoyed all of those things, but the chemistry, somehow, I really liked.

GRAYSON: So, the teacher really kind of turned you on to the whole thing, chemistry?

McLAFFERTY: Yes, and I did have a chemistry set in the basement and my neighborhood friends and I concocted things. But it was the chemistry teacher in high school that really did it.

GRAYSON: Interesting. If you do get to the Chemical Heritage Foundation they have a collection of old chemistry sets.

McLAFFERTY: Oh, do they? [laughter] I'd love to see it.

GRAYSON: So you might, you might be able to find your old chemistry set there.

McLAFFERTY: Oh on top of that the old University of Omaha was just a few blocks from where we lived in Omaha and, in fact, we went by it everyday to go to Lothrup grade school, and in those days they used to throw the old stuff out the window or out the door. So, outside of

the building there would be glass, and rubber stoppers, and things like that, just lying there on the ground. [laughter]

GRAYSON: Great environmental protection type thing?

McLAFFERTY: Great environmental protection. But, we thought it was great, you know. You'd get a rubber stopper with a glass tube sticking through it. This really increased your chemistry research prospects greatly. So, I remember that.

GRAYSON: So then, in Nebraska you decided to go into chemistry. Chemical engineering was not a consideration. And so, when did you enter the school in Nebraska? It was in—

McLAFFERTY: Nineteen forty. September 1940.

GRAYSON: And then, let's see, America was—where were we with the war in Germany at that time? Were we involved with—

McLAFFERTY: September—December 7, 1941 was Pearl Harbor.

GRAYSON: Okay. Pearl Harbor.

McLAFFERTY: And so, Pearl Harbor came when I was a sophomore. And in fact, organic chemistry was sophomore level and my teaching assistant was Bob Alberty. He has been in the National Academy many, many years. He was at the University of Wisconsin as a professor and then he went to MIT as provost, I think, or dean of Science or something like that. And he and his wife, who was at the University of Nebraska then, are good friends of not only my wife but especially her sister, and so we see them. And so anyway, people like that were an influence early on.

GRAYSON: So, this was like the lab instructor --

McLAFFERTY: A lab instructor. He got his masters degree at the University of Nebraska, as I remember. So this would have been like 1941.

GRAYSON: The curriculum there was pretty much standard for that time, where you have an introductory chemistry course? Did you do any inorganic? I guess you had some inorganic?

McLAFFERTY: And we had analytical with fumes—it was on the first floor and we had to have the windows open when the fumes got so bad. [laughter]

GRAYSON: So, you didn't have hoods in these labs?

McLAFFERTY: In the organic lab. I remember that there was a fire in the sink at least every, [laughter] at least every lab period.

GRAYSON: So, how big were these classes?

McLAFFERTY: Oh gee, I don't remember. The lecture hall was pretty big. I think there were a few hundred people in the general chemistry class.

GRAYSON: Typical And I guess chemistry was, at Nebraska it was always an important subject because of the relationship to the agricultural industry?

McLAFFERTY: That's right. The Ag college actually, I think, had its, I don't know if it had its own, how much of its own chemistry it had. It was out in the suburbs and so you couldn't walk from the Ag campus, like you can at Cornell. Of course Cornell's medical school is in New York City, so it's even worse to walk there and you're, at Wash. U. (Washington University in St Louis) you're very lucky.

GRAYSON: So in a lot of schools today chemistry, inorganic chemistry, these courses are prerequisite for medical degrees to go into medicine, but I guess at Nebraska at that time it was kind of a prerequisite for going into the agricultural area for a lot of students?

McLAFFERTY: Well, the University of Nebraska was a land-grant college and certainly the Ag school was a very important part of it. And yes; what career choices did people have? That's a good question. I guess we didn't see much of that because everybody went to the war. So we didn't deal with that part of career choices. Yes, that's such a big thing for students now, and should be. But that wasn't something that people even kind of thought about at that time.

GRAYSON: Before we go on, I just want to—can you clarify what is meant by a land-grant college?

McLAFFERTY: Oh, the Morrill Land Grant Act of 1865—

GRAYSON: Is that M-O-R-E-L?

McLAFFERTY: M-O-R-R-I-L-L. In fact, Cornell is the land-grant college of the state of New York. Each of the states was authorized by the federal government to start a college. I don't know whether they called it "college" or "university," and it had to have mechanic arts, which of course is engineering now, and agriculture, and military science. And the government gave them a land grant in order to put the college on.

GRAYSON: The place to put it?

McLAFFERTY: So, the federal government had lots of land in Nebraska and so they gave them land, not only land to put it on but land to support it. And, for Cornell, there wasn't any federal land in New York state so they gave Cornell upper Michigan land [laughter] that was worth five cents or ten cents an acre or something.

GRAYSON: I see.

McLAFFERTY: Ezra Cornell himself saved the university when they were strapped for money. He bought this land for five and ten cents and acre and set it up some way, so when they sold it they got a dollar or three dollars, an acre, which made a big difference [laughter]

GRAYSON: So, you're telling me that the federal government gave Cornell, gave the state of New York land in a different—

McLAFFERTY: In upstate Michigan.

GRAYSON: In Michigan? Now, this is—

McLAFFERTY: Upper Peninsula of Michigan.

GRAYSON: The UP? So, this is really useful for New York state [laughter] to have?

McLAFFERTY: Well every state got it, and so for Iowa there's Iowa and Iowa State, and Iowa State is the land-grant. In Nebraska, they have only the University of Nebraska and that's the land-grant college. And, I guess in Missouri, I'm not sure which is the land-grant.

GRAYSON: Probably the University of Missouri, I would guess, but I don't know.

McLAFFERTY: Well, they have to have an Ag college and an engineering college. Well in Indiana, Purdue is the land-grant college and, of course, Indiana University has arts, and things like that.

GRAYSON: So basically, he leveraged the land, the value of the land in Michigan, the UP, to buy a property here so they could have their—

McLAFFERTY: Well actually this was Ezra Cornell's farm.

GRAYSON: Oh, okay.

McLAFFERTY: Where the university is. And he gave them money besides. But, there was a big fight in the state legislature of where, in New York state the land for the university would be. And Ezra was in the legislature and he had made a lot of money on Western Union Telegraph, and so that's how it worked at Cornell. At Nebraska, I think things were simpler because the land was there and they had essentially no universities. So, this was their first university.

GRAYSON: So, that was the Merrill, Morrill Act in nineteen, eighteen—

McLAFFERTY: Eighteen sixty-five. Just at the end of the Civil War.

GRAYSON: That's kind of neat. That's an interesting part of our history as a country that I didn't realize. You hear about people speaking about land-grant colleges and you just kind of think, "Well, it means something."

McLAFFERTY: And that's because the federal government didn't give them any money but they had lots of land.

GRAYSON: Interesting. So, let's talk then a little bit about that business with the war. Okay? You came to start your college career in 1940 and my understanding is that you were able to get an accelerated completion of your bachelors degree?

McLAFFERTY: [laughter] Yes. Let's see how'd it go? Well, I'd taken—well, the land-grant college is supposed to have military education and they even had ROTC in high school, and I took ROTC at college. The war started in my sophomore year and training facilities for troops were a tremendous problem. Just having the ROTC there to train people was something. And so, they came up with this deal that if you'd been in the ROTC and would stay in that training you could enlist and they would leave you in school for a while so you could continue your education. This gets us around to the fall of 1942, so that's the start of my junior year. I enlisted in the infantry in my junior year, in September of 1942, but I got to stay on campus. And then they called us. There were lots of us who had done this same thing and they called us to active duty in April of 1943, but they actually let us finish the semester. They put us in the library building, and housed us there, and gave us uniforms and everything like that. Well, that meant that I could finish my third year. And, the university came up with a plan that if you weren't more than sixteen hours short of the hundred and twenty required for graduation you could get your bachelors degree. Well, I'd gone to summer school and I'd taken larger loads and so forth and so I had enough hours. So, I actually got a bachelors degree in three years, at the same time that we had to go to basic training in the Army. The other thing that was part of the deal was, if you go to four years of ROTC they make you a Second Lieutenant. So if you go to three years of ROTC you automatically got to go to Officer Candidate School. And, I just as well tell you the story. So, after basic training I got to go to Officer Candidate School. It's, I think, a thirteen-week school at Fort Benning, Georgia. It's pretty basic training. The very last week they called me in before the Officer's Review Board and they said, "Well, you have a very different case than we think we've ever seen and we're having a big argument about it." "Well, what's that?" He says, "Well, some of us think you look fourteen years old and that would be difficult to lead troops." And I said, "Well, what do the others think?" "They think you look thirteen." [laughter] And so, I got bounced.

GRAYSON: Ah. So you had the age and the training but you didn't have the looks?

McLAFFERTY: They also told me I had the highest IQ score, Army test score of anybody that had come in there too. [laughter] So, maybe they bounced me to save my test score. [laughter]

GRAYSON: So, I just want to back up a second. What was a typical course load? Was it like eighteen hours, nineteen hours, twenty?

McLAFFERTY: Well, I think I took eighteen hours.

GRAYSON: Eighteen hours?

McLAFFERTY: Or nineteen. Something like that. And the summer after my sophomore year when the war was on I took a laboratory organic course or something in the summer. So, that's how I got the necessary hours.

GRAYSON: So, when the OCS bounced you then that meant you went back into a noncom position?

McLAFFERTY: Well no, I was a Private First Class. Private First Class, luckily I got to train with the infantry rifle company and we then went overseas. We didn't go overseas until Thanksgiving Day 1944. And so the invasion of Europe was under way and we went in through Marseille. We got into the southern end of the Ardennes offensive, and things like that. But then of course the war was over in May of 1945.

GRAYSON: So, you saw a little less than a year, maybe eight or nine months of—

McLAFFERTY: Oh no. Just from December through April.

GRAYSON: About six months?

McLAFFERTY: I think officially our unit was 110 days in combat, and of that I spent ten days in the hospital. For anybody that thinks that's a short time, I had one Purple Heart for getting wounded. There were 193 men in my infantry rifle company, but my one Purple Heart was below average because our, our rifle company had a total of 194 Purple Hearts. So, and thirty-nine were killed in action.

GRAYSON: You had thirty-nine of that group of 193?

McLAFFERTY: Twenty percent. Oh, and incidentally they offered me what they call a "battlefield commission." I must have aged. [laughter]

GRAYSON: So—

McLAFFERTY: I turned down the battlefield commission. I knew by then what a Second Lieutenant's life expectancy was. Every single one of our Second Lieutenants were either killed or wounded.

GRAYSON: Oh, okay. So, you decided that really wasn't something you wanted?

McLAFFERTY: Well, it was a great honor but as they said about the man when they were riding him out of town on the, tarred and on a rail, "If it weren't for the honor I think I'd just as soon walk." [laughter]

GRAYSON: So, then you stayed as a Private First Class through the whole—

McLAFFERTY: No, actually the guys above me—let's see, the section leader got wounded so my squad leader got the job. Then my squad leader got wounded as section leader and so, and I became squad leader. And then when he got wounded as section leader I became section leader and so I was a staff sergeant at the end of it. And the thing about the infantry was there's a point system for getting out of the Army after the war was over, and the first people out had to have eighty-five points and I had exactly eighty-five points. And so, I was back at the University of Nebraska graduate school that fall, the fall of 1945. And, as Mike Gross has pointed out to many people, [laughter] nobody else was back from the Army. Only the crippled and the blind males were there, and so that's when I caught Tibby, my future wife. So, that was the best part of being in the Army.

GRAYSON: So, on this point system how did, I mean you were only in for, what, a hundred and some odd days but you were able to amass eighty-five points? What was, how did you do that; what counted toward points?

McLAFFERTY: Well, you got a point for every month in the United States and two points for every month overseas, but the biggest things was that you got five points for a medal. And so, I had a Combat Infantry Badge, and a Purple Heart, and five Bronze Star Medals for valor. And, that adds, that's where the points came from.

GRAYSON: So, this was an exceptional event for most people for that short a period of time to have acquired that many combat commendations?

McLAFFERTY: Yes.

GRAYSON: So, what did you do [laughter] to get all that stuff, Fred?

McLAFFERTY: I got lucky. [laughter] I got lucky. Well, [sigh] did you want to hear one story? There's one I tell my grandchildren about the rest of them I don't tell anybody about. [laughter] My best buddy was Don Woods and Don was just a year younger than I but he'd gone several years to the University of Oklahoma and we were interested in the same things, so we hung out together. As I say, he was very conscientious though. Different than I am. [laughter] And, we had had a real tough attack one day trying to push the Germans out of the land in front of the Siegfried Line and, we were back and we'd sort of quit for the day and it was getting dusk, and we were sitting around, I don't know what, and this Major that we didn't know came up with a medic. Medics had red crosses on their helmets. And, he says, "We've got this problem. This guy in your company got shot in the attack and when we had to pull back he's out there and the medics have gone out to treat him and get him and the Germans have decided to shoot medics today. We've already lost one medic." And, this guy who was with him said, he had a hole in his shirt, "So, as soon as it gets dark we've got to sneak out there, with a stretcher, and bring him back." And he says, "I need two volunteers." And, this Don Woods jumps up and says, "Sir, we will volunteer." [laughter] So, we went out and we got the guy, and the Germans were, somehow they knew we'd be out there and so they were targeting any noise. And on the way back we made some noise and so we got a lot of artillery thrown in on us and Don got a piece of shrapnel in his arm, and I came through fine. I'm a lucky guy. And so, the Major was very pleased we'd done this and put us in for Silver Stars. Don really wasn't badly hurt but it turned out that for years he had trouble with his arm where, the skin hadn't really been penetrated too much, but it damaged his tendons and stuff. Anyway, he got a Purple Heart for that, as he should. Well then, Don had applied for West Point and while we were in the front lines his appointment came through. He didn't tell anybody and I saw the next letter that came through a couple of weeks later saying, "Didn't you get the letter?" I saw him opening it and saw the return address was a very official letter and, of course, I insisted on reading it. And I said, "What are you still doing here?" "Well, I can't leave my buddies." And I, of course, spoke up loudly so everybody within hearing could hear what the story was and I said, "How many of you guys would stay here if you got one of these?" And everyone said, "Hell no! We'd be out of here." So, we shoved him out of there. Anyway, when he gets back to West Point that summer, when you're just starting at West Point you're a plebe, or a rookie, or whatever it is. They really treat you like a pledge or something. But he wasn't there but a week or two and the General called a parade for the whole outfit and then he announced, "The parade is to award medals." And Don got the Silver Star and the Purple Heart in front of all these guys. He never got harassed.

GRAYSON: No, he'd had real harassment in the war.

McLAFFERTY: Well, my stories are very long, but—

GRAYSON: No. No. No.

McLAFFERTY: But, that's the one I tell about. So anyway, Don got the Silver Star but I got demoted to the Bronze Star.

GRAYSON: Well, he volunteered you. [laughter]

McLAFFERTY: I've never complained but I know that's what the answer would be, that I didn't volunteer.

GRAYSON: So, you were able to bring back this wounded fellow? I mean, he survived?

McLAFFERTY: I never found out if he survived.

GRAYSON: But he was alive when you got to him and got him back out?

McLAFFERTY: Yes, but he was in terrible shape. But, but we did get him back. We volunteered.

GRAYSON: Yes. [laughter] You volunteered. So, what led to your Purple Heart? That's a commendation for wounded in action, right?

McLAFFERTY: Yeah. I was next to an eighty-eight shell that went off and blew out my ears and put some shrapnel in me.

GRAYSON: So, your luck had run out on that one?

McLAFFERTY: Well, ten days in the hospital. A million-dollar wound is where you are wounded badly enough that they have to ship you back to the States, but you recover. And mine was a thousand-dollar wound. [laughter] I got ten days off.

GRAYSON: So, you went back into combat then after your ten days?

McLAFFERTY: Went back into combat. It was after I got back into combat that,—yeah, I think I was a squad leader when I got wounded, and then sort of the second day I was back my former squad leader, who was the section leader got wounded then I was the section leader, Staff Sergeant for the rest of the war, for the last few weeks of the war.

GRAYSON: So, you were able to come back immediately then? Immediately after the war had ended were you able to return immediately?

McLAFFERTY: Yeah, of course the war had to end in Japan.

GRAYSON: Right. I mean, a lot of, my understanding is a lot of the guys in Europe were looking, not forward to, but looking at the possibility of having to go to the Pacific?

McLAFFERTY: Oh yes. We were already an occupation force that summer in Europe and we were headed for Japan. Our unit was headed for Japan.

GRAYSON: So, since you had a sufficient number of points you were able to go home?

McLAFFERTY: And then, as soon as the war was over—actually, I sailed out, I've forgotten, fairly early in September and then, of course, the war wasn't officially over until like September 3rd. And, I think I was discharged like the third of October, or something. And it turned out the University of Nebraska was glad to accept me as a grad student. I didn't know any chemistry. I didn't remember the difference between benzene and acetone.

GRAYSON: At that point in your life, in the life of a young person, the interruption in your education is considered to be something of an inconvenience. How did people view the fact that the war was going to infringe on their education? I mean a lot of people probably had to stop in the middle of their college education to go off to war and typically at that age you're not terribly excited about interrupting your studies?

McLAFFERTY: Well, the interesting part about the wars we have now and the people that have to go; World War II was entirely different. Nebraska in World War II, let's put it that way, I think it was that way all over the country. But, the girls would ask you "Why are you in the university?"

GRAYSON: "Why aren't you in uniform?"

McLAFFERTY: "Aren't you eighteen?" This is what you did. And, for me to enlist in the infantry, I thought, "Oh, I was on the rifle team in college. So, that's the only reason they let me in the infantry." (You know, it's a very exclusive club. Hardly anybody can get in. [laughter])

GRAYSON: But you get shot at a lot?

McLAFFERTY: Me and a few million other guys. [laughter]

GRAYSON: Yeah. Right.

McLAFFERTY: But, the thing about the career -- when you asked about careers that was a very interesting question for me because I can't remember even ever thinking about a career during that time. Or being interrupted in my career, because the war was such a huge part of everyday life and everybody's conversation, and the radio, and everything else that you—and there were draft boards, and there were, scrap drives, and victory gardens, and you just planned on going off to war.

GRAYSON: Well—

McLAFFERTY: Yeah, I never thought of having my career interrupted.

GRAYSON: My sense, because I was born in 1941 so I have no real true [laughter] experience of it, but my sense is that it was a time when the entire country was mobilized? I mean, everybody.

McLAFFERTY: Sure.

GRAYSON: I know my dad used to talk about—he was a traveling salesman and they rationed tires, they rationed butter, they rationed copper, they rationed gas, they rationed everything for the war effort. And I think it's probably incomprehensible for people in this generation, this age now, to understand how completely involved the whole country was in the war. Before I leave the war I've got one question I want to ask you. Did you ever serve near Dresden in Germany

McLAFFERTY: Dresden?

GRAYSON: Yeah, Dresden.

McLAFFERTY: No. [laughter] We, no, I've been to Dresden since then.

GRAYSON: Because, you're familiar with someone who did serve in Dresden, I think?

McLAFFERTY: What?

GRAYSON: You're familiar with someone who did serve close to Dresden?

McLAFFERTY: Who is that?

GRAYSON: Klaus Biemann.

McLAFFERTY: Oh, Klaus. Yeah. I forgot that. I didn't realize it. I guess I didn't know that.

GRAYSON: He, well it's in the literature and it's also in the interview I did. When the war ended he was in Dresden.

McLAFFERTY: In, uniform?

GRAYSON: In uniform.

McLAFFERTY: Oh.

GRAYSON: And when it was obvious that the war was over, he essentially walked back to Innsbruck, hiked back to Innsbruck from Dresden to get back to his family. So, he was on the other side.

McLAFFERTY: Oh, I've never heard that story.

GRAYSON: Yeah.

McLAFFERTY: I'm glad you told me. He never told me.

GRAYSON: So, I think we can move forward then from there. You went back to Nebraska and then relearned your chemistry?

McLAFFERTY: Relearned my chemistry. [laughter]

GRAYSON: And, you graduated?

McLAFFERTY: Took it. I was an analytical chemist.

GRAYSON: Analytical chemist? So you went in, it says you graduated as an analytical chemist. Let's see, you took a masters degree there?

McLAFFERTY: Yeah. And the interesting thing is, the great analytical chemist, well the only analytical chemist in the National Academy until Charlie Reilley got in in the 1970s was Kolthoff.

GRAYSON: How do you spell that?

McLAFFERTY: University of—K-O-L-T-H-O-F-F.

GRAYSON: Okay.

McLAFFERTY: Initials are I-M.

GRAYSON: Uhm-hmm.

McLAFFERTY: Isaac M. Kolthoff. But, he was, everybody called him "Pete."

GRAYSON: Pete?

McLAFFERTY: Anyway, Pete Kolthoff was a bachelor and lived in Minnesota, and I remember visiting him at age ninety-five when he was living in the student union. But, anyway I got my masters degree with Professor Armand Pagel, P-A-G-E-L, and Pagel got his PhD from Kolthoff and Sandell, and the famous old analytical chemical quantitative analysis textbook was Kolthoff and Sandell. And so, I felt like I was a real analytical chemist. And I got two papers out of my masters degree and they were both published in *Analytical Chemistry*.

GRAYSON: Hmm. Okay. This would have been in probably, what, late 1940s, mid 1940s?

McLAFFERTY: Late 1940s.

GRAYSON: Late 1940s?

McLAFFERTY: Forty-eight or forty-nine. Something like that.

GRAYSON: Okay. Use of Tributylphosphate for Extracting Organic Acids From Aqueous Solutions?

McLAFFERTY: Ha. Ha. Ha. Ha. Ha. Oh.

GRAYSON: Yeah. In *Analytical Chemistry*.

McLAFFERTY: Sounds exciting doesn't it?

GRAYSON: Oh yes. Yes. Well, you've got to start somewhere. [laughter] So, you got your masters degree and you decided to go to Cornell, right, for your PhD?

McLAFFERTY: You need all the story? Well, I was still pretty naïve and so I talked to Tibby about it and we decided that—

GRAYSON: Now, you'd gotten married when?

McLAFFERTY: Oh, in, oh that's right. I didn't get married then. I got married in 1948. So. that was after. I graduated in June of 1947 with a masters degree and we got married in February of 1948. Because she graduated in January of 1948.

GRAYSON: And what did she study?

McLAFFERTY: Bacteriology.

GRAYSON: Oh wow. Okay.

McLAFFERTY: Yes. Anyway, I decided that Cornell was far east, as far as Nebraska's concerned, but not totally far east, and so I applied to Cornell. The chair of the department there, Cliff Hamilton, was a friend of Jack Johnson, who was the big organic chemist here. (Aside "They named the chemistry building after him.) Anyway, the chair stopped me in the hall and said, "Well where are you going to grad school?" And I said, "Oh, Cornell." He says, "Oh, have you had an offer?" I said, "Well, actually I sent the application in months before and I haven't heard anything." "Well, where else did you apply?" I said, "Oh, I didn't apply anyplace else." [laughter] And, he rolled his eyes. [laughter] But, apparently he got on the phone and within a few days I had an offer from Cornell. [laughter]

GRAYSON: So—

McLAFFERTY: Things still worked. .

GRAYSON: Yeah, well, you know, the old network. But obviously he wouldn't have done this for anybody. I mean, call up his buddy at Cornell. So, he must have been impressed with you.

McLAFFERTY: Well, in fact I got a research position on a Navy research grant with a guy I worked for here, which in those days there weren't a lot of grants, which was a great help since I had the G.I. Bill.

GRAYSON: So, you were able to use G.I. Bill funds for your education then, for a while?

McLAFFERTY: Yes, Tibby worked too. Well, I tell you, we were living high.

GRAYSON: Yeah.

McLAFFERTY: Got a car. [laughter]

GRAYSON: Yeah, that must have been something. So, you ended up coming to Cornell, what, in nineteen forty—

McLAFFERTY: Forty-seven.

GRAYSON: Seven? (McLafferty: Yes.) Forty-seven.

McLAFFERTY: Fall of forty-seven.

GRAYSON: In the fall of 1947. And then your thesis work was on fluoro?

McLAFFERTY: Organofluorine compounds.

GRAYSON: Organofluorine?

McLAFFERTY: Chlorofluoro compounds.

GRAYSON: What was the interest in those at that time?

McLAFFERTY: Oh, my thesis advisor, William T. Miller—

GRAYSON: That's M-I-L-L-E-R?

McLAFFERTY: Just Miller, like in Miller. William T. Miller ran a lab on the Manhattan Project and he invented the lubricants that were used in the uranium hexafluoride diffusion plant that would stand up—uranium hexafluoride is almost as corrosive as fluorine itself-- and these fluids would stand up to that for vacuum pumps, and greases. And so, it was an entirely different field of chemistry and I thought it was fascinating. It was especially good in that he used what I thought were modern instruments. He had multi-point recorders and we put thermocouples into our distillation columns. Oh, I thought that was really sexy. We could plot out a freezing point curve to see the purity. We can plot that out on the recorder. Oh man, I thought that was exciting.

GRAYSON: So, you were able to use a lot of your analytical chemistry training from masters in your work here?

McLAFFERTY: You know, analytical is how you define it. I didn't dare tell him I was doing analytical chemistry. I mean he, he was an organic chemist. But after I got out I found it easier for me to call myself an analytical chemist. Well, they didn't trust me to teach organic chemistry or physical chemistry so it's much easier to call myself an analytical chemist.

GRAYSON: Okay. So, you basically went through the program at Cornell in a pretty normal amount of time? Three years was about typical for getting a PhD degree?

McLAFFERTY: Well actually, I finished in the fall of 1949. Two, I was there two years and two months. It didn't seem like it was unusual for me to get out then, but maybe he wanted to get rid of me. [laughter]

GRAYSON: Yeah. Well, I think you progressed pretty rapidly. And so, this is like 1949?

McLAFFERTY: Yeah. In 1947 I came here and 1949—my degree is actually 1950 because we left here in November of 1949. And, Tibby always likes to joke, to say she had the car packed and, well you saw where we walked in those steps down there, she had the car sitting there and my PhD exam was in the first room inside there. And as soon as I shook people's hands I walked down the steps and got in the car and we were out of town in ten minutes. [laughter] So anxious to get rid of Ithaca, but nineteen years later we came back. [laughter]

GRAYSON: Yeah. So, a few things happened in that intervening period. And I understand you tried to get a job in academia for starters when you got out of graduate school.

McLAFFERTY: Yeah. Actually, I did a postdoc at Iowa, with Ralph Shriner. Back in those days—S-H-R-I-N-E-R.

GRAYSON: N? Or—

McLAFFERTY: Shriner.

GRAYSON: Shriner.

McLAFFERTY: S-H-R-I-N-E-R. Ralph Shriner. And, there was a famous organic analytical book called—"Qualitative Organic Analysis", or something like that. Anyway, it's where you identify molecules by making derivatives and getting their melting points and seeing what color they are, and so forth. And, I didn't work on that with Shriner but he could be called an analytical as well as an organic chemist, and he was in this same famous Roger Adams School of Organic Chemistry out of University of Illinois that all of these other people were from.

GRAYSON: So, what is the Roger Adams School?

McLAFFERTY: Well, organic chemistry was, was not as important—well, maybe that's not true, but physical chemists looked down their noses at organic chemists. But then, Roger Adams became department chair at the University of Illinois and got some very bright young organic chemists there and he then sort of dominated the graduate school scene. He had a pipeline to DuPont and so he could place his PhDs anyplace. DuPont had to take them, but he could place them in universities. And so, Nebraska had one of his students. Cornell had one of his students. Shriner was on the faculty there and then went to Indiana and then went to Iowa, and the university organic chemistry departments were populated with Illinois organic chemists.

GRAYSON: So he kind of moved organic chemistry up a notch in the—

McLAFFERTY: Oh, he did. He did indeed. And, they still have great organic chemistry at Illinois. But there are others now I guess. There are others.

GRAYSON: We were getting ready to look for a position.

McLAFFERTY: Oh, that's right. So I thought, "While I have a postdoctorate. I'm going to do academe."

GRAYSON: Was that a one-year appointment as a postdoc?

McLAFFERTY: That was a one-year appointment. We went there December of 1949. And so, I was looking at academic positions in the fall of 1950. And, I think I only had one interview and maybe I didn't get an offer there. [laughter] But, I went to the ACS meeting, the fall ACS meeting, and went through industrial job hunting, and gee I had all sorts of interviews. And, when I went to Dow to interview at the organic lab they said, "Oh, you'll spend the afternoon in the organic lab but in the morning you're going to interview in the spectroscopy lab." "Well, I'm not a spectroscopist." "Well, that's all right. There's an opening in mass spectrometry." " I've never seen a mass spectrometer." "Well, that's where you're going." And so this Vic Caldecourt—you got to write his name down. Caldecourt, C-A-L-D-E-C-O-U-R-T, Victor Caldecourt had a mass spectrometer, a Westinghouse mass spectrometer, and was building a second mass spectrometer like the Westinghouse, and he was terrific with electronics and instrumentation, and he had to do all the analytical, and interpreting spectra, and everything else. And he said, "Well, we just need a chemist to do this stuff and it's really very simple." I said, "Well, I've never seen it before." "Well, look and see." And he showed me this mass spectrum of normal butane. He said, "You see here there's a peak at fifty-eight." He says, "It's C_4H_{10}. Of course there's got to be a peak at fifty-eight. You see this peak at forty-three? Well, that's, because it's lost fifteen, the methyl group's fifteen. And see the twenty-nine? Well, that's half of the molecule." He says, "See how simple it is?" I said, "Yeah." [laughter] I said—and so, well anyway, to make a long story short I took the job. And, all my friends, when they'd say, "Oh, where are you going to work?" And I'd say, "Oh, I'm going to do mass spectrometry at Dow." "Mass spectrometry?" [laughter] You should have heard them yell. "Oh, you dumb guy." And, I did get an offer from the organic place, and I've forgotten what I was going to do, and I'd tell them and they'd say, "Oh, that really sounds exciting." [laughter] "Well, I'm going to do mass spectrometry."

GRAYSON: Let me back up just a second, when you did look for academic positions; obviously you sought more than one? I mean, for graduate school you applied to Cornell and

that was it, [laughter] but when you started looking for positions in academia you sent out more than one application?

McLAFFERTY: Yeah. I did, but Ralph Shriner wasn't a great help. I mean, if I have a student and he wants to be in academe there are all sorts of things I do. And Ralph Shriner was absolutely a wonderful person. I mean, apartments were very hard to find in 1950 when we went there, so we lived with them for six weeks. Bless them. Anyway, I wrote to places that had ads in *C&E News.* I've forgotten what I did. Let's say it was not a well-planned attack, but—the other thing was there happened to be time. Well, my excuses are there were jobs, but let's face it if you don't get a job offer there's another reason.

GRAYSON: Oh, well, but you know—

McLAFFERTY: But, I think the only place that I interviewed was Knox College in western Illinois. And, one of my colleagues here that I actually do research with is Barbara Baird, who went to Knox College, and so once in a while I remind her of my only other academic interview. [laughter] She likes to kid me about, "Well, I'm glad to see they had good taste when they should have turned you down." [laughter]

GRAYSON: It's nice to have friends, Fred. So something occurred to me when I was getting ready for this interview. There's this kind of situation that occurred at this time. Sy Meyerson, Ron Grigsby, you, O.P. Tanner, chemical companies would bring someone in for a job interview and more or less put them in front of a mass spectrometer and say, "You're going to do this," and so they're turning over a multi tens-of-thousand-dollar equipment to somebody that walks, essentially kind of walks in off the street?

McLAFFERTY: Yeah. That's right.

GRAYSON: And, I thought, "That's kind of crazy." You wouldn't do that today. But then on the other hand, if you think about it no one was qualified. I mean, they didn't have mass spectroscopists like today. So it kind of, in my mind, I don't know if maybe I'm missing something here, kind of suggests how corporate keeping-up-with-the-Jones' was that everybody had to have this new instrument, and even though there was no one that they could find qualified to run it they would figure, "Well, we'll just pick this guy. He looks like a possibility." Put him in front of the machine and say, "Here. do it. Make it happen." If you think about those days and the fact that people were essentially assigned this job without a heck of a lot of experience.

McLAFFERTY: Yeah, I suspect the reason I got the job offer is because when Vic Caldecourt showed me this instrument I was genuinely interested. I was fascinated with it, and I asked him questions. "Why does this do that?" And, Vic only had a bachelors degree but he was a very bright guy. He loved electronics and vacuum systems and things like that, so he was perfectly capable of doing these other things. And so I assumed that, as you say, there wasn't anybody they could hire that was familiar with it. Well, they could have, if they'd known about Sy Meyerson they could have stolen him away from Standard Oil of Indiana. He would have been perfect for them. But to give Dow credit for this spectroscopy lab -- I should get that in; they were great. Norman Wright ran it--.

GRAYSON: Is that W?

McLAFFERTY: W-R-I-G-H-T. Norman Wright. There's a Pittsburgh Conference prize that has his name on it, and he was an early infrared spectroscopist that really used infrared for solving problems. And his lab there invented the double-beam infrared spectrophotometer. Well, two places invented it at the same time, but his lab was one of them. And so, his infrared lab was the best in the world. It had thousands of reference samples. And that was part of the reason it was way fun for me there. I had access to all these samples. They got the first IBM collator card sorters, and things like that. And, I could use any of this stuff that they had. And besides that, they invented the direct-reading emission spectrograph. And somebody else, so Alcoa also invented it. During the war they were big on magnesium. They had this big three-meter spectrograph with photocells set for the wavelengths of the different elements, and they could run a sample every thirty seconds. And, those guys were there and they were—well, in fact, my direct boss, Jason Saunderson, who was a really, really bright guy—

GRAYSON: Is Saunderson S-A-U?

McLAFFERTY: S-A-U, Saunderson, who is still alive up in New Hampshire. We got a Christmas card from him. Jason was terrific and he was part of the team that did these direct-reading spectrographs. They had some of the best powder-diffraction x-ray equipment, in the world. Ludo Frevel, a terrific scientist was there — and they did some x-ray crystallography.

GRAYSON: Let's get that name again. The—

McLAFFERTY: Ludo, L-U-D-O.

GRAYSON: L-U-D-O.

McLAFFERTY: Frevel, F-R-E-V-E-L.

GRAYSON: That's one word?

McLAFFERTY: That's one word.

GRAYSON: Ludofrevel?

McLAFFERTY: Oh, no. Ludo's the first name.

GRAYSON: Oh, Ludo Frevel?

McLAFFERTY: Ludo is the first name and Frevel is the last name. And, he had one of the best x-ray facilities in the world, both for powder diffraction and for crystallography. They had one of the first NMR people—Ned--well, I'll think of his name—and I got to know Jim Shoolery of Varian through Ned.

GRAYSON: How do you do Shoolery?

McLAFFERTY: S-H-O-O-L-E-R-Y.

GRAYSON: E-R-Y?

McLAFFERTY: Yeah. He was at Varian Associates, the nuclear spin NMR guy, through Ned. Well, Dow also had Raman. They had a Raman spectroscopist who never succeeded. All of us developed things that would do things for the Dow Chemical Company, as well as have fun with it besides. In those days the Raman source was the Toronto Arc, that cylindrical thing and its sensitivity was horrible. But anyway they tried all these spectroscopy methods just because they were so successful with them. And so, of course, they would try mass spectrometry. And, of course they were supporting me. Then when I said, "Well, I need more people and I need more mass spectrometers." It was *laissez faire*, entirely different than industry now.

GRAYSON: Oh yeah.

McLAFFERTY: You do something good for them and things go very well.

GRAYSON: So they had a Westinghouse instrument when you got there. So, what was that instrument like, I mean, in terms of the kind of spectra it produced, how long it took to take a spectrum? (McLafferty: Oh, I'm glad you asked.) How do you get a sample into it? [laughter]

McLAFFERTY: Well, it as a ninety-degree magnetic sector instrument with a Nier source. And a scan took thirty minutes.

GRAYSON: Were you scanning the voltage or the magnet?

McLAFFERTY: We were scanning the magnet.

GRAYSON: Because the early CEC machines scanned voltage.

McLAFFERTY: The early CEC machine also used photographic paper.

GRAYSON: Oh.

McLAFFERTY: And this one had a pen and ink recorder. And Vic Caldecourt had made this into machine that really was analytical. For example, for the pen and ink recorder he had a switching system that if it started up the peak fast enough it quickly put in a factor of ten attenuation. If it was still going too fast it put in another factor of ten. And so, with a thirty-minute scan the time it takes you to go over peak, of course, we were scanning past mass twelve to a hundred, a hundred and some. Usually we didn't scan past a mass of hundred and twenty. And so, when you could got to the peak top it would switch back. He also put a magnetometer into the magnet, that vibrated, and so he got a direct DC feed of the magnetic field. He fed this into a little machine that took exposed movie film and made it run by a window in the front of the mass spectrometer control with a light behind it. And, you could use a pencil to make marks on this film. And so, that was our mass scale. And so, the operator, while the thing is running, he sees the water peak go by. He sees his mass scale is calibrated. Here goes the mass twenty-eight of nitrogen and the thirty-two of oxygen and he gets to know these peaks well enough. If he's running xylenes he marks the 106 and makes sure that it's on, and then we got a big strip chart, a usual paper chart of the mass spectrum. It would have been notated by the operator

what the masses were along the bottom and of course you could read this mass scale. And so, you just take a ruler, a millimeter ruler, and measured peak heights and then you know what your abundances are. And then for liquid samples, to do quantitative analysis, we had a big expansion volume. We had a five-meter flask inside a heated reservoir. In the top he put, the glass blower put a thing with a, oh I'd say about a quarter-inch hole in it and you had a device you put on top of the hole. The hole was plugged with a Teflon slug. I think I still have the Teflon slug just sitting in my drawer, because I love to admire it. [Opening/closing drawers] There it is.

GRAYSON: So, this is from 1950?

McLAFFERTY: Here is a—this is a 1952 Teflon slug.

GRAYSON: Oh wow.

McLAFFERTY: I don't know if that has a hole in it or not. Anyway—

GRAYSON: They had Teflon in 1952?

McLAFFERTY: Yup.

GRAYSON: Oh yeah, it's got a little hole on the side.

McLAFFERTY: Yeah. And so, here in this hole sits a blank Teflon plug. Then this little holder he designed held this and another blank plug on top of it. That holder you put on there, you squeezed the handle and it pushed two plugs in and let, so this one, with the sample, went in and left one to plug the hole, and we weighed this on a balance ahead of time. And, you could weigh it with the other plug on top of it as a seal. And, he invented all those things. There's a 1954 *Analytical Chemistry* article by Victor Caldecourt about these things. And so, we ran a real analytical lab. And thanks to all of Vic's— [Drawer opened] inventiveness.

GRAYSON: Let me get a picture of that. I'm sorry. I've got my camera here.

McLAFFERTY: You want a picture of that?

GRAYSON: Yeah. I want a picture of that.

McLAFFERTY: Very few things I am able to keep very long, but I, every once in a while I—

GRAYSON: So—

McLAFFERTY: You want, maybe you want it on this black bag, that bag? There you go.

GRAYSON: That'll be a nice background there. So, you guys were really into this analytical thing, and you were actually performing quantitative determinations of the amount, of the various amounts in the mixture?

McLAFFERTY: It's according to [Phone rings]—Hold on, that might be— [Recording paused]

GRAYSON: Okay. So Dow really was investing a very large effort in this physical analytical chemistry?

McLAFFERTY: Yeah, they were really ahead of most places. And, well the thing about the Pittsburgh Conference is that I could meet the people from other places. But Dow had a big presence at the Pittsburgh Conference. And so, when I went there I was always very proud to be from Dow because in those days that was an important part of the modern analytical scene. Of course there qualitative and quantitative analysis was still a big thing, and the Pittsburgh Conference was much more an industrial conference, and places like Alcoa, and steel companies, and so forth had industrial analytical problems that sort of drove the conference. And of course, the mass spectrometry was driven by the petroleum industry, as you well know. [laughter]

GRAYSON: Oh yes.

McLAFFERTY: And so, there were not too many of us that—

GRAYSON: So Dow was not really into petrochemicals, were they, at the time.

McLAFFERTY: No. They had things like chlorobenzene and phenol, and so forth, that came basically from petrochemicals. Originally, their benzene came from coal tar. But, they then became petrochemically based for those things. Well styrene was a very big thing for them. They had a big petrochemical base but, at least in those days, there was a much bigger markup between raw materials and products than what you think of when you say "petrochemical industry" these days.

GRAYSON: So, getting back to the Westinghouse mass spectrometer, that was considered to be a fairly good piece of equipment for mass spec at the time?

McLAFFERTY: Well, the petroleum industry had CEC (Consolidated Engineering Corporation). And, to CEC's credit they had standardized things so that you got these wonderfully reproducible spectra. You ran butane every morning to see if the mass fifty-eight forty-three ratio was the same. You changed the temperature of the ion source if it wasn't. And, of course the API (American Petroleum Institute) reference spectra weren't qualitative spectra, they were quantitative spectra. And I tremendously enjoyed those petroleum people. One thing we could do, if you want, before we're done is go through those early programs and just point out the people. Because the people were wonderful people. Jack O'Neal and Jack Sharkey and Gus Friedel, and all of these people were wonderful. But, the number that were doing non- petroleum things was small, and yet as far as I was concerned there was plenty to do with new things that pleased my boss. Of course Dow had mass spectrometers at their Texas division too. And, they had General Electric mass spectrometers that were ninety-degree sector. But almost all the petroleum companies just had CEC.

GRAYSON: Yes, my first experience was on the 103C that O.P. Tanner had at Monsanto. And so, we actually did the oscillographic recording with the paper you had to develop in the darkroom, where you put the sleeve on your arm and that kind of stuff. [laughter] So, it was an interesting machine.

McLAFFERTY: I got a 103 at Dow, oh, in 1953 I think, with all of that. By then, of course, with the other two instruments we had, we could use the right instrument for the right problem. But yeah, it was a fine machine and the photographic thing was a nuisance but we could do it.

GRAYSON: Yeah. Yeah.

McLAFFERTY: And we ran two shifts a day on, eight in the morning until twelve at night on three instruments. Well, thirty minutes a spectrum is a lot different than modern instruments recording a spectrum every second, or that kind of thing.

GRAYSON: So, the work that you did was, was it supporting a process stream someplace?

McLAFFERTY: All sorts. That was the interesting thing, that again there was this very openness at Dow. When I first joined them they didn't even have a Director of Research. The son of the founder, Willard Dow, was not only president but he was Director of Research, but he was killed in a plane crash in 1948, I think, and I joined in 1950. And, they just had a Committee for Research and then they got a Director of Research in a couple of years. But, they'd had so much success with new products like polystyrene and latex paint that it just came out of these guys doing things, and they were terrific money makers that they didn't worry about it. And, I solved problems with the mass spectrometer. Midland, Michigan had I think 12,000 inhabitants and Dow employed, at Midland, 15,000 people. You knew what went on in lots and lots of places. We were right next to the physical research lab, and this was a very famous lab that had invented polystyrene and latex paint, and I've forgotten what other things they had. So, I would just go listen and if they were having some problem I thought mass spec would solve I would speak up. And they were nice guys but often, we had a lot of fun, I got ridiculed, [laughter] and sometimes I wouldn't even dare speak up. I'd just go around and get a sample anyway. You had to teach them not to send it in quart bottles.

GRAYSON: Oh yeah. [laughter]

McLAFFERTY: You had to bring your own little sample vial. "Can I have a little of that?" And sure, if we couldn't do anything I never said a word, but once in a while we could do something. And, I think I've told my specs in polystyrene story enough?

GRAYSON: Yes

McLAFFERTY: You've heard that one?

GRAYSON: Yes.

McLAFFERTY: But there the culprit was carbon tetrachloride. And, when the plant manager said, "Infrared couldn't see it." I'd said, "You're right. Infrared can't see it." [laughter]

GRAYSON: So, how many of these rescues, firefights I guess you'd call them—I mean, when I was at McDonald Douglas we'd have these problems pop up and we'd always call them

"firefighting problems" where they would pull out all the stops because something really important to them was giving them problems. So, you were called in on these "firefighting problems" regularly?

McLAFFERTY: Yes. Well, you remember early on Dow was getting one, was getting some butadiene process know-how from Germany, from Badische I think. Anyway, this Ludo Frevel, who was born in Germany, had gone over to do it and he says, "I'm going to send you back samples." So, he airmailed me back little glass vials of gas samples that he made them take from the different places in the process. And, I would get them and we would, of course, as you say it's a firefight. We'd run them, finish the work at three in the morning, and in those days you cabled the results back. And he said my analyses got there before the Germans got their analyses out [laughter] and mine were much more detailed. Of course, I had to guess what the isomers were, but nobody knew any better than I did. [laughter] And so, I gave them chapter and verse on other hydrocarbons that they didn't even look for. And I think they were doing mostly Podbielniak distillation analysis. And so it was so early on that even the petroleum people weren't sure that there were labs that could do this kind of thing really well. Not every place could.

GRAYSON: Yes. So, you were at Dow for a while and then they decided to maybe set up a lab in the East?

McLAFFERTY: Oh right. Right. Nineteen fifty-six.

GRAYSON: Then it was about five or six year that you were in their mass spec?

McLAFFERTY: Yeah. I went there in 1950 and this was 1956.

GRAYSON: So, this was a period when you were really in a hands-on laboratory environment? You were day-to-day dealing with spectra and working with instruments?

McLAFFERTY: And actually, I put in gas chromatography too in that period, with Roland Gohlke.

GRAYSON: So, GC [Gas Chromatography]—

McLAFFERTY: G-O-H-L-K-E, Gohlke. [laughter]

GRAYSON: Yeah. Gohlke. Roland. GC was just getting the attention of people.

McLAFFERTY: Well, I had never heard of it until I went to a Gordon Conference.

GRAYSON: Do you remember which year that was?

McLAFFERTY: Fifty-four, I think. I always had trouble remembering. I think I've got the original Gordon Conference notes and I look them up every time. But, Steve dal Nogare—

GRAYSON: D-O-L?

McLAFFERTY: Small D-A-L, N-O-G-A-R-E. Was at DuPont, and he did wonderful things with it. And, there was a guy from ICI in Britain, Wilson, Dr. Wilson. I think it was Harold Wilson. I'm not sure. Anyway, they were both at the Gordon Conference and described GC. And me, I take them aside and, ---

GRAYSON: Ply them with beer? [laughter]

McLAFFERTY: I plied them with beer and got a whole notebook full of notes. In fact, I rode the train back to Boston with Steve. He became a very good friend, just a terrific guy. And, when I got back to Midland I said to Roland, "Here's something we've got to do." And, Roland's such an inventive guy. Well, he had to do it his way. But, our first gas chromatograph was just copper tubing rolled up. For a thermostat he used a two-quart dewar. He had GOW-MAC detectors, only he got in touch with the GOW-MAC salesman and they got him some experimental model that was ten times as sensitive, or something like that. And, he tried different column packings. One of his favorites was Tide laundry soap. And, we did the same thing with GC that we had done with mass spec; running samples for people from all from different parts of the company. Only there, we didn't have to run the samples because Roland would build them a GC and he would go over and install it in the plant and teach them how to run samples. And he installed more than two hundred of these before Dow started to buy them commercially. [laughter] Yeah he, Roland was just amazing.

GRAYSON: Of course, those were the days when you could build one. You can still build your own GC today. It's a very simple concept. But, I would say it's much more refined than when you were first doing it?

McLAFFERTY: Well, in those days you didn't care—a gram of sample or a microgram of sample, or a femtogram of sample [laughter] you had a sample. Sensitivity was no problem. These things were reproducible. You had temperature control. And—

GRAYSON: So, was it during that period—let's see, you left the Dow Midland lab in 1956, you said?

McLAFFERTY: Oh yeah. That's when they, they sent me out to, it was in that period that the other chemical companies got into basic research and most of them put labs in Europe because that was much more basic. And, we always—well, you're from the Midwest. We always joke about, for Midland, Michigan. Boston was just as foreign as Switzerland was to DuPont. [laughter] And so, they'd sent me out and they had a little EDTA plant that they had bought. The Bersworth—

GRAYSON: EDTA is ethylene—

McLAFFERTY: Diamine tetraacetic acid, a chelating agent.

GRAYSON: This was, so this was kind of like a stock chemical that they used in the—

McLAFFERTY: Well, it's a specialty chemical that has all sorts of uses for chelating metals, even for medical uses. You can control calcium and also get rid of iron. Well, it's even used to treat lead poisoning and things like that. It's a very tight complex; ethylene diamine has two amino groups on each end, and tetra-acetic acid is out like that. So you've got aminos and carboxyls and, well you know. And so, I started the lab there and one of the big researchers in the field, Art Martell, was a consultant to the Bersworth Chemical Company. So, I had inherited him and two of his PhDs.

GRAYSON: That's M-A-R-T-E-L-L?

McLAFFERTY: M-A-R-T-E-L-L. Correct.

GRAYSON: And what company was he with?

McLAFFERTY: He was at, at—

GRAYSON: Bers?

McLAFFERTY: No, he was at—the Bersworth Chemical Company; the one Dow bought. Art Martell was at University of—oh, a town just west of Boston. (Added later: he was at Clark University in Worcester MA.)

GRAYSON: Cambridge? No.

McLAFFERTY: Well, it doesn't matter. He then moved from there to IIT and then to Texas A&M, and he's spent the last twenty or thirty years of his career as, he was department head at Texas A&M. Art Martell.

GRAYSON: Okay. So, and what was this company that they bought? Bersworth?

McLAFFERTY: Bersworth, B-E-R-S-W-O-R-T-H. And, but Martell—

GRAYSON: Now, Dow bought this company?

McLAFFERTY: Martell was just a consultant to them.

GRAYSON: Okay. A consultant?

McLAFFERTY: But they're the ones that made EDTA. And they made it in an old garnetting plant.

GRAYSON: What's a garnetting plant?

McLAFFERTY: It's a plant that takes old wool garments and makes them back into wool fiber that is then remade into new ones.

GRAYSON: So, it's a recycling operation for wool?

McLAFFERTY: That's right.

GRAYSON: Weird.

McLAFFERTY: And the reason there's the virgin wool sign is because—

GRAYSON: Of the garnetting? Okay.

McLAFFERTY: Is because garnetting is the other way to make it.

GRAYSON: I see. [laughter]

McLAFFERTY: Well, it was a new word for me, but this building was built in 1840 or something, and when the tanks were too full the timbers would creak. But it was a wonderful place for a lab because you could knock a hole in the wall if you wanted to run a line or anything like that. And they had a full complement of people, so I immediately had a secretary, and we immediately had people that if you needed a hole in the wall they could do it, and they could weld, and they could—

GRAYSON: So these personnel came from the company that was once—

McLAFFERTY: Well they're, Dow owned it by then.

GRAYSON: Dow owned it?

McLAFFERTY: Yeah. Dow had bought it two or three years before that.

GRAYSON: So, why did they select you to go back east and set up this foreign plant? You were doing mass spectrometry in an analytical chemist mode. So, why did they pick you out of the, ---

McLAFFERTY: People. Not anymore, but people used to remind me what I told them once. "Well, this is a great opportunity at Dow and I think mass spectrometry has reached its peak." [laughter] I told them that. I have several people that have reminded me that I told them that.

GRAYSON: So, this is, you were essentially kind of getting antsy? You thought you'd done—

McLAFFERTY: Well—

GRAYSON: You'd done what you could in the mass spec business and it was interesting for a while and you were ready to move on.

McLAFFERTY: You, if you look around—well, you were, you were in industry. Look around at people that have been there for five and six years and they get antsy, and, the company keeps them distracted by giving them something different to do, and this is what the company gave me to do. And man, I thought it, "Wow! Oh, is this great stuff." And when I got there and got people in the lab and got to doing things all of a sudden I had lots of time on my hands. So, I went back to doing mass spec.

GRAYSON: So, when you went there though were, you were in charge of setting up a research laboratory right?

McLAFFERTY: That's right.

GRAYSON: So you then had to hire people?

McLAFFERTY: Yeah.

GRAYSON: I mean, there wasn't anybody of a research caliber there when you appeared on the scene?

McLAFFERTY: Well, there were these two, already these two PhDs that did chelation type of research. It turned out that both of them left before I left. Gee, the people I hired were terrific. One of them, George Olah got the Nobel Prize.

GRAYSON: Uhm-hmm. How do you spell that? O?

McLAFFERTY: O-L-A-H.

GRAYSON: Yeah. So, how many people did you end up hiring for this research lab?

McLAFFERTY: Oh, when I left there I think we had fourteen PhDs, maybe thirty-five all together in the lab.

GRAYSON: And did you get any direction on what kind of research they were supposed to be doing?

McLAFFERTY: Well, we were supposed to be doing fundamental research, and supposed to be doing research in different areas that would get the company into different fields.

GRAYSON: Okay. But did they specify what they thought of as fundamental? (McLafferty: No.) So, it was up to you to determine? (McLafferty: Yes.)

McLAFFERTY: The director of Research, Ray Boundy, was a chemical engineer. He—

GRAYSON: Boundy, how do you spell that?

McLAFFERTY: B-O-U-N-D-Y.

GRAYSON: Okay.

McLAFFERTY: Great guy. Very good with people. Had a broad knowledge of Dow's processes and chemicals, and where they ought to go, but was not a PhD chemist. And so, when we would do something different he never once complained. You know that furan is a five-membered ring with one oxygen and two double bonds? And thiophene has a sulfur in place of the oxygen. Well, we came up with compounds that had very different metal atoms there. I remember presenting this when I'd go back to Midland every couple of months to tell them how

wonderful we were, presenting this to whatever scientists would come. And Ray Boundy was there. Well, somebody always said, "Well, what in the world good are these?" I said, "I don't know, but they're sure different. And we sure got a great patent position." [laughter] Ray Boundy just smiled. Well, what was the other thing about these? Oh, for example, the tin one the official name for it is stannol, and in the slide I would have these different things and pronounce them. The last picture on the slide was the one with arsenic in it, and I'd say, "You know, I'm not sure what we should call this and then they'd all go through arshol you'd hear in the back of the room. [laughter] And so, at least I'd get away with them.

GRAYSON: Yeah.

McLAFFERTY: So, we invented arshol [laughter]

GRAYSON: Was it Boundy or Boundry, this guy Ray's last name?

McLAFFERTY: Boundy.

GRAYSON: Boundy? Like in D-Y?

McLAFFERTY: Like Mutiny on the Bounty.

GRAYSON: Boundy.

McLAFFERTY: Oh sorry. Not Bounty. It's Boundy.

GRAYSON: D-Y? Okay.

McLAFFERTY: Yeah, like "bound in the air." Yeah.

GRAYSON: Okay.

McLAFFERTY: B-O-U-N-D-Y.

GRAYSON: So, you would get a little bit of flak going back to Midland?

McLAFFERTY: I wouldn't get any flak out of him. No. And he had a right-hand man, Jack Chamberlain, who was sort of sort of assistant director of Research. Ray was the guy that sat there and smiled. Jack was the guy that was the communicator; he would bounce around and get to all the people and talk to them. He's the guy that I could go to afterwards and find out what the lay of the land was. Ray Boundy wouldn't tell me but Jack would "Fred, it's brand new chemistry. Ray is very pleased. I'm very pleased. In fact," he says, "We really get a kick out of which people in the audience are the ones that are saying they don't see what good these are going to be."

GRAYSON: I see.

McLAFFERTY: And so, for example, one of my scientists had worked with a guy in Indiana that came up with stannous fluoride toothpaste for Proctor & Gamble, that made them, and it still makes them, money. And, he did research in these areas that came up with other compounds that were outside this and had bacteriacidal and other kinds of specific activities. Well, it was a niche market but it not only made the company money but it was one of the first things that Dow then got into the pharmaceutical business with. This was one of the first things they had for this pharmaceutical business, was this stuff that Horst Langer came up with.

GRAYSON: How do you spell that?

McLAFFERTY: L-A-N-G-E-R. He was from Germany.

GRAYSON: Horst, H-O-R-S-T?

McLAFFERTY: H-O-R-S-T.

GRAYSON: Okay.

McLAFFERTY: And, in fact, they then bought an Italian pharmaceutical company, Lepetite. I think that's L-E-P-E-T-I-T-E, in Milan. Another researcher I hired was Fred Leavitt, L-E-A-V-I-T-T, who was a polymer chemist and he had a nice polymer research program there. When

I left the lab Fred Leavitt became the director. And then, Dow actually sent Fred Leavitt, when they bought Lepetite in Milan, over to run that research lab over there.

And so, well the other part of the story is that I also recruited for Dow in the East Coast universities. I had plenty of time, so I recruited at Harvard, and Yale, and MIT, and Columbia, and Princeton, and all these places. And, they would always say, "Well, I don't want to work in Midland, Michigan" [laughter] And, I said, "Well, we have a plant in Texas." "Well, I don't want to work in Freeport, Texas." [laughter] And I said, "Well, but I'm hiring you for my lab in Boston." "Oh, well I like Boston, yes. But, but then you'll just transfer me to Midland." I said, "It's a free country. Look at the Constitution." And so, I hate to say it but lots of these bright guys that I hired then transferred to Midland. I don't know if any of them transferred to Freeport, Texas. [laughter] So the lab was a good place to hire people that Dow couldn't hire because who wants to live in Midland, Michigan? [laughter] I mean being from Nebraska, I thought it was great.

GRAYSON: Sure. So, you mentioned patent positions. The work that was done, you guys had a patent attorney on the staff there and you actually worked, I mean made sure you patented the developments?

McLAFFERTY: I was the patent liaison, and Dow had a Boston, no, New York City patent attorney, that they had come do our patents. And, I've forgotten, maybe just as well I've forgotten his name. [laughter] He was great and, at tax time I can remember, I labored over my income taxes. He says, "Fred, good night, expenses, put them down." He says, "You know what I do?" He says, "I put down every expense I can think of. I keep track of all of the meetings I ought to go to in New York City, talks I ought to hear. I put them all down in my tax form, and then sometimes I don't always go to them but, you know, it's really ones that would have been expensive if I went there." [laughter] And he says, "Yeah, usually they cut my expenses down by so much when they get it, but then I just add that amount to next year because, of course, I really deserved it. [laughter] Well, anyway he instructed me on how to do taxes.

GRAYSON: So did, do you have any idea the number of patents that your operation developed in that period? Is it like dozens, hundreds, tens?

McLAFFERTY: Oh we got up to where we put in a dozen a year, maybe.

GRAYSON: Oh, okay.

McLAFFERTY: Yeah.

GRAYSON: These are all chemical, chemical compounds? Synthesis routes?

McLAFFERTY: We really did have a wide variety of—well, George Olah, did superacid research. And as I say, after I hired him and within, I think it was only a year or maybe a year and a half after that, I left and went to Purdue. And, of course he kidded me about that. "The only reason I came here Fred is because I can put up with you." [laughter] And so, he went to Case Western within a year after that, and he's been at the University of Southern California for many years. But, he is *the* person in this superacid field and it has many great applications. And, well, it's especially good for hydrocarbons and for derivatizing hydrocarbons, and for all sorts of things. And so, we got lots of patents while he was there.

GRAYSON: So, during this period you really were functioning not so much as an analytical chemist but as a more, really more—

McLAFFERTY: Oh, I was supposed to be running the place.

GRAYSON: And so, you're research director, and—

McLAFFERTY: Oh, I didn't do analysis. Well, in my spare time I did paper mass spectrometry research [laughter] and well Roland and I, the infrareders had set up an infrared search system in which they put all their reference infrared spectra on Hollerith punch cards. And so, of course, we did that with our reference file of mass spectra. And we actually published this in *Analytical Chemistry* in 1959 or so. Anyway, the infrareders deserved credit. They had a big switchboard with little toggle switches in it, and they, well I think there are eighty holes in a punch card? Well, there ere eighty toggle switches, so each one corresponded to a wave length range, and ours corresponded to masses, of course. So you'd go there and toggle the switches and then take the reference file cards and shoot them through the sorter and out would come the ones that might match your unknown spectrum.

GRAYSON: McBee card sorting?

McLAFFERTY: Not McBee card sorting—IBM cards that are easier than that. And then we made it more sophisticated. But anyway, this meant that I could have all the reference spectra with all the masses and so forth on these cards. And, I can remember bringing the cards back in my suitcase one time, back to, where we lived in Wellesley. So I would use them to write these papers correlating spectra. And, also papers for rearrangements too.

GRAYSON: So, you were actually doing and developing rules for mass spec interpretation in your "spare time." [laughter]

McLAFFERTY: Yeah.

GRAYSON: And the research director had this kind of opportunity?

McLAFFERTY: Yes, if you ask Tibby, after we'd been there awhile and the fifth child arrived in Wellesley we bought a house with nine bedrooms, [laughter] six fireplaces, a four-car garage, and a heating bill that would knock your socks off, from this old wealthy couple. And they sold it to us at what we could afford, I think, because they loved having five children in their house. But, it was a marvelous house. And so, I had one of these little bedrooms, it was actually in the maid's quarters in the house, that I used for an office. And, I used to get up a couple hours before the rest of the family every morning, get out my cards and correlate my spectra.

GRAYSON: And play with your cards? [laughter]

McLAFFERTY: I did that little book on mass spectra correlations. You know it?

GRAYSON: Hmm-uhm. I'm not sure I've seen that book.

McLAFFERTY: Well, that's one thing about doing it in the office. Mass Spectra Correlations. I must not be looking at it every day. There, Mass Spectra Correlations.

GRAYSON: Oh, I've seen this. Yes. Okay.

McLAFFERTY: Well, it was an ACS publication.

GRAYSON: Yes. I might even have a copy of this. So, this was the precursor to *Interpretation of Mass Spectra*?

McLAFFERTY: Yes. This is the result of going through every one of those four thousand cards and, and recording what I thought was the reason for what the mechanism of where the peak came from.

GRAYSON: Uhm-hmm. So, it's kind of like your groundwork?

McLAFFERTY: That's, the basis for my view of mechanisms, is going through what then was a library that nobody else had. [laughter]

GRAYSON: That was published by ACS?

McLAFFERTY: By the ACS, that's right. And then, when I got to Cornell I had this great Babu Venkataraghavan. We brought out a second edition only, this one he did everything on the computer; or most everything on the computer. We had a much larger database then. But, actually, things came out about the same time. Yeah, Babu was great.

GRAYSON: So, tell me a little bit, if you would, about your wife's name.

McLAFFERTY: Elizabeth.

GRAYSON: Elizabeth? Okay.

McLAFFERTY: And, ever since her two-year-old sister couldn't really say "Elizabeth" and called her "Tibby" [laughter] she's been T-I-B-B-Y, Tibby.

GRAYSON: I see.

McLAFFERTY: Yeah. So.

GRAYSON: Okay. Well, it's an interesting, you know, I've heard of some interesting—

McLAFFERTY: Elizabeth has all sorts of shortened names. But, hers is a little different than most.

GRAYSON: Okay. So, then you were kind of antsy about moving on?

McLAFFERTY: Your talk starts in eighteen minutes.

GRAYSON: Okay. Well, why don't we stop here?

McLAFFERTY: Oh, I forgot to tell those guys to go get the laptop.

[END OF AUDIO, FILE 1.1]

GRAYSON: So, we're back from lunch and we talked about a number of topics. But, two that I want to follow up on, one is Sybil Rock. Sybil worked for CEC and did a lot of their data analysis stuff and probably one of the earliest women to work in the field. Tell me about Sybil. What was she like and what kind of things was she doing?

McLAFFERTY: She was very professional. I only remember her coming to the meetings at the Pittsburgh Conference. There was a subsection on mass spectrometry that then turned into E14. But, my first conference was 1952, I think.

GRAYSON: Pittsburgh?

McLAFFERTY: Pittsburgh. And yes, I think that's where I met her. She might have come to the 1953 conference in Pittsburgh. It seems like by 1954 E14 went to New Orleans, 1955 in San Francisco. Anyway, I only remember her from two or three conferences. I remember corresponding with her about the keysort cards and about using reference spectra that they collected at Dow, and then with people like Charlie Judson, we got together the Uncertified Spectra Committee and we got that going in the mid 1950s.

GRAYSON: So, there was this issue of trying to compile a collection of spectra? The differentiation was that CEC and the petroleum companies were interested in getting these "certified" spectra which they could use in their quantitative (McLafferty: Right.) and analytical work, and the uncertified spectra were just spectra of compounds that weren't necessarily needed in a quantitative sense but to (McLafferty: That's right.) just get a record of the spectra of the compound?

McLAFFERTY: And the early Pittsburgh Conference meetings were dominated by the petroleum chemists, but really nice guys. And, they used to tease the few of us that were organic mass spec people that did other kinds of compounds, and yet they never gave us any trouble about giving papers, and they came to our papers, and yet that wasn't their bread and butter. It wasn't really that you shouldn't send your reference mass spectra to API, but they had some bureaucrats there that would send them back, "You don't have the proper fifty-eight to forty-three ratio." Or, "What's this instrument you're running it on called the Westinghouse?" And the real problem was that people had shoeboxes full of spectra that the rest of us would like to look at and we would like to contribute ours but nobody had the time to put them into careful format and everything. And so, the thing that we usually all agreed on—I think I was the first chair of the Uncertified Spectra Committee.

GRAYSON: And that would have been in the 1950s?

McLAFFERTY: That was around 1955 or something like that. And we just sort of all agreed—well, Jack Sharkey and —, from the Bureau of Mines in Pittsburgh, I said his name this morning.

GRAYSON: Friedel? No. Friedel.

McLAFFERTY: Gus Friedel. Jack Sharkey and Gus Friedel were a backbone of this and, other people were interested, and they all, we all said, "Well, come on. Everybody's got to send in, let's all try to send in fifty spectra this year." And some of them did but I didn't, and yet I was collecting them, and I was putting them in the collection and everything. And, of course, to make the long story even shorter, you remember that I finally had Roland Gohlke put our collection together, and that's that yellow thing with the Dow diamond on the back. But that had 2,000 and API, plus all the other contributions at that time were maybe 2,500, and so we nearly doubled the collection when we finally got it out. [laughter]

GRAYSON: So, these were uncertified spectra?

McLAFFERTY: These were all uncertified. Yes. That's what it says on, Dow's Collection of Uncertified Spectra. It actually was a very nice thing and the people worked together on it. I can even remember at one of those meeting I had already started working on a paper for *Analytical Chemistry*; Correlating the Mass Spectra of Alcohols, and to my surprise Friedel and Sharkey had a paper on The Correlated Mass Spectral of Alcohols at the conference. And so, we got together and we said, "Well, what else are you doing?" And, I said, "Well, we're

already started on aldehydes and esters," and I've forgotten who else was doing what. And so, we decided that, "Well, why don't you go ahead and publish alcohols and we'll do aldehydes next." And I think we had promises from several other people who all came through. But, this way most of the people didn't write papers, but at least they sent spectra in.

GRAYSON: So, I mean, people were interested in looking at compounds other than just the hydrocarbons and seeing how they behave, the kind of spectra they would get, and the kind of fragmentation patterns that you would have from different compounds?

McLAFFERTY: For this history talk I gave at the 50th ASMS meeting, I looked up those things and the names that were attached, and that helps my poor old brain. [laughter] But, it was 90/10, or 85/15, or something like that, in favor of petroleum papers. But that was fine.

GRAYSON: So, now I'd like to pick up on this, on the Boston laboratory. What happened to it after you left?

McLAFFERTY: Well of course it went downhill very rapidly. [laughter] No. It actually stayed around and was an important thing for Dow. I think that they folded up in 1988. I left in 1964 and, I bought thirty acres for the laboratory for $100,000 overlooking the Masspike, in Wayland, Massachusetts, not far from Route 128. And so, if they didn't make any money on the research, my real estate investment was a good one. [laughter]

GRAYSON: I can imagine so. [laughter] So let's see, I guess it's kind of interesting. There is this little period when industrial operations wanted to have these research laboratories and they all went out and did this, and then in the late 1980s and the early 1990s they decided, "We don't want to do that," and they all started canning researchers and divesting themselves of these activities.

McLAFFERTY: I didn't really find out—in fact, I guess I, to admit it, I wasn't too interested in finding out why they shut it down. But I used to get told about the patents and the successful new products that came along. And, I still think probably the most important thing they got out of it was the people. Yeah. We certainly could hire excellent people—and the people were perfectly happy after this five, or six, or seven years we were talking about, suddenly to go home and tell the wife "I can be a group leader in the XYZ lab in Michigan and I get a twenty percent raise." And by that time (Grayson: It's a lot cheaper to live.) they've got two kids and another one on the way. In the book (by Carsten Reinhardt) there's Nobel Laureate Richard Ernst, NMR, and he tells about leaving Switzerland to go work at Varian where he invented 2-D NMR.

GRAYSON: This is the book by Carsten Reinhardt?[1]

McLAFFERTY: Yes. He, interviewed me before he went there and did the—and some years later I introduced him, I was chair of the Chemistry Section of the AAAS, at the national meeting and I had a thing with people from all over. And, when he got up he said, "Yes, I've known Fred a long time. My career was really stunted when I turned down his offer to work at the Dow Chemical Company." [laughter]

GRAYSON: But, I think you made a very telling comment just a minute ago. You said, "The thing that Dow got out of the lab was the people." I think that today that doesn't carry any weight, people. I mean, industrial operations today; people don't mean much at all. And so, it's a sad commentary. Maybe I'm coloring the situation but having worked in an industrial laboratory (McLafferty: You know that—) I've seen it disappear. (McLafferty: Well, yeah.) Yeah, it goes. And, I mean, we had very good people at McDonnell Douglas Research Labs who were doing very good work and it was work that was useful to the company, but for some reason or other the management style became totally focused on the bottom line at all costs and whether you had a quality person working for you, doing serious good science was irrelevant. That didn't matter. I think that it's a very telling comment that what Dow got out of the (McLafferty: Yes.) Boston operation was good people.

McLAFFERTY: Oh, for example, I started a Dow Annual Basic Research Symposium. By that time I'd gotten to know people. Dow had a Walnut Creek, California plant and one in Texas, and some operations in Switzerland, and so forth. I and a few of my cronies chose the forty people we thought were doing the most basic cutting-edge research and got them all out to our lab. We had this house with nine bedrooms. Tibby put on a big party for all of these guys, and we had two or three days of papers by all of them. Oh, they thought this was the greatest thing, that Dow is really scientific and it was great for all of us to learn what the rest of them were doing, and we could collaborate. And, you know, it was a real great thing that these people stayed at Dow. They had made contacts inside the company. The people you know in the company make all the difference. You can call up Joe Schmoe and say "I need this," or "I just heard this." It really makes things work. But, these money managers, all they want is to show the year's profits are better. They can take your salary and put it in the profits by firing you.

GRAYSON: Yes. As I say it's a sad situation that's evolved, but things change and I guess that's the way it was. We have a friend that retired from McDonnell Douglas and when he retired about fifteen years ago, his commentary was that he recognized, even at that time, that he

[1] Carsten Reinhardt, Shifting and Rearranging: Physical Methods and the Transformation of Modern Chemistry (Sagamore Beach: Science History Publications, 2006).

had lived through a golden age in the industrial research laboratory. When he started working your capabilities were respected as a scientist, the work you did was respected. Whether or not it made the company money in the bottom line was not an issue. It was the fact that you were doing cutting-edge research. And then towards the end of his career he saw that that no longer held value for management. And so, that was his kind of swan song -- that he was at least glad to have worked during what he would call a 'golden age' of working in an industrial laboratory and being appreciated for his science.

McLAFFERTY: Well, the other thing I saw was that having good research supervision, management is very difficult to get; because people that are good at that often go into academic careers. But there's no kind of training in companies usually for this kind of a thing. Often laboratory directors kind of totally went to seed and then immediately the whole lab went to seed. They couldn't be interested in everything that was going on and enthusiastic, and if they couldn't be enthusiastic they had to do something about it. Melvin Calvin was a Dow consultant. Do you remember Melvin Calvin, Nobel Prize? In fact, he ended up on their Board of Directors. He came to Midland as a consultant and he got to my lab one time, he was terrific. He just started asking questions, "What are you doing? Well, why are you doing that? Oh, well, is this a—why do you do it that way? Well, doesn't that mean you could do it this way?" And he didn't scold you or anything else he just caught onto it. And, I used to have him come out to our little tiny lab. I've forgotten whether he charged $200 a day or $2 million a day, they paid for it back in Midland so I didn't care, but oh, he just lighted a fire under these guys. He was interested in anything they were doing and he could ask questions about anything. And, if he asked questions and they were really doing everything almost right, well how totally flattered they were. And almost always when he asked about my research I wasn't doing everything totally right. [laughter] Yeah, he was great.

GRAYSON: That's an interesting motivation is to have someone with that kind of reputation, , (McLafferty: Yeah.) interact with a bench chemist and reinforce the fact that he's doing well; which really gives him a big moral boost.

McLAFFERTY: Oh, I certainly tried to copy his techniques as a lab director, but if we'd only had him instead teach a course to all the lab directors and have some way of making them take it in the company would have been better off.

GRAYSON: So, during lunch we were chatting a little bit about this gentleman, Beynon, John Beynon. So—

McLAFFERTY: Oh, John what a great guy.

GRAYSON: Yeah. So you said that there was an ASTM meeting on the West Coast and then he stopped by?

McLAFFERTY: In San Francisco, and he stopped by at Midland to see my lab. That was 1955, I'm pretty sure. At least for this talk I got out to the San Francisco E14 meeting based on the notes that I have. I even made a slide out of notes he had on his first high-resolution data. Even before he came to my lab, he had mailed me and tried to get me to go in with him on the purchase of a double-focusing instrument. I can't remember just why, but by that time things were really set, but he still wanted me to do it because that would help reduce the cost of his instrument. And, of course, I should have done it except that the instrument would then have arrived in Midland, Michigan after I had gone to Framingham. That was the only reason I was glad I didn't.

GRAYSON: So he, he was trying to politic you into buying one of these early MS-9 type instruments?

McLAFFERTY: Exactly the same instrument he'd gotten.

GRAYSON: Okay. So the company would then cut his price down and you'd help him (McLafferty: Yeah.) get a better deal then?

McLAFFERTY: But note the kind of guy he is. It wasn't that he wanted to hog the whole field. John was exactly the opposite kind of guy. He thought that high resolution MS was such a great thing. He thought we all ought to do it. And, he still did all the early work and gets all the early credit, even though he gave dummies like me a chance to share in the credit. [laughter]

GRAYSON: So then you were, like you say, if you'd gone ahead and bought the instrument you would have left Midland labs by that time and gone east?

McLAFFERTY: It would have been close. Yeah.

GRAYSON: So, I'll just maybe do a segue for a minute into your private life. You had a pretty good sized family by that time? You had like four or five kids, is it?

McLAFFERTY: Yeah. The three of them arrived in Midland and the other two arrived in Boston.

GRAYSON: So, that must have been a pretty exciting time back at home? Did you have this huge home with all these bedrooms? Did you get a chance to use all those bedrooms?

McLAFFERTY: Well, actually we lived four years in another part of Wellesley in a much smaller house. And it was the fifth one arriving that really made us look for a larger house. Somehow we got this place and it had three acres. Recently, my friend that I haven't seen for some time told me how many million dollars those three acres are worth now—

GRAYSON: Oh wow. I can imagine.

McLAFFERTY: But, it's right in the heart of Wellesley Hills, and had these marvelous huge trees and the ash tree that some famous politician gave his first political talk under.

GRAYSON: Oh really?

McLAFFERTY: And things like that.

GRAYSON: So your wife had her hands full with a young family?

McLAFFERTY: Tibby is absolutely terrific. She took care of everything. She still does.

GRAYSON: Well, she takes care of (McLafferty: She does everything.) you?

McLAFFERTY: Takes care of me. I mean, that's a full-time job. [laughter] And five kids? Oh yeah.

GRAYSON: So, finally you decided to leave even the Dow research lab that you'd set up in Boston in the 1955-1956, 1956?

McLAFFERTY: Fifty-six to 1964, I left. So, I was there eight years.

GRAYSON: Okay. So, you left in sixty, up there in 1964?

McLAFFERTY: I went to Purdue in 1964.

GRAYSON: Purdue in 1964? So, finally you were able to get into academia. Did they come to you for the opportunity or (McLafferty: Yeah.) how did that work? So, they wanted you.

McLAFFERTY: Well, the first thing is I was—part of the reason for putting the lab there was to keep in contact with all the very fine chemistry departments locally and on the East Coast. And I worked hard at that; I became chairman of the Northeast ACS Section, and of course I interviewed at all these places, and I'd gotten to know many of the faculty. And, one of the guys I got to know was "Buck" Rogers, Lockhart B. Rogers.

GRAYSON: Is that R-O-G?

McLAFFERTY: L.B., his initials are L.B. but everyone called him Buck because of the obvious connotation to Rogers.

GRAYSON: This is R-O-G-E-R-S?

McLAFFERTY: Yes.

GRAYSON: Okay.

McLAFFERTY: It was like the astronaut. Or the —

GRAYSON: Right. Buck Rogers. [laughter]

McLAFFERTY: Yeah. And he was an analytical chemist at MIT. This was the time when all of the best university chemistry departments were getting rid of analytical chemistry. He had tenure and I don't know all the circumstances; but anyway Purdue made him an offer he couldn't resist to build a great analytical department and once he got there, he wanted to attract me there. I interviewed and I turned down their first offer. But then, after things got boring and I still loved to do research, and was getting tired of the administration part—I had a stack of

*JACS*s (*Journal of the American Chemical Society*) unread on my desk. I said, "It's here whether I go left or I go right," and so that's when we moved to Purdue.

GRAYSON: So, you decided to finally get into academia and they wanted you now? You didn't have to go around and [laughter] get rejected or not hear from anybody?

McLAFFERTY: Yeah. In fact, the, the head of the Chemistry Department at MIT was Art Cope and he had come in to make the MIT department a great one, and he did a great job. He was head of the department and doing things, and he's the one that decided they would do mass spectrometry and got an instrument for Klaus Biemann. I talked to him about it, but I must admit it's the fact that Tibby was in an organization with his wife that probably had more to do with my knowing Art Cope. But anyway, you take what you get. I knew Klaus early through Art Cope, and that was great fun. I could drop into Klaus' lab when I was ever down at MIT and talk.

GRAYSON: So, you actually interacted with him in that period? You could visit him whenever you were in the neighborhood and see what he was up to?

McLAFFERTY: Yeah. But you know Klaus; Klaus doesn't need help from anybody.

GRAYSON: Oh no. [laughter]

McLAFFERTY: But, I'm not making fun of Klaus. He did a great job. And, well Al Burlingame was one of his grad students, and of course Al was easy to talk to. And, oh he had a whole bunch of people. The Spitellers were great friends of mine. Who else was in there? Walt—

GRAYSON: How do you spell Spiteller?

McLAFFERTY: S-P-I-T, Spit, (Grayson: Uh huh.) E-L-L-E-R. Spiteller. And both, they were both postdocs. They were both, I think, from Austria. He ended up at -- well I'll think of it – a University in Germany. And, both of them ended up there as professors.

GRAYSON: And Walt, was it Walt?

McLAFFERTY: Walt McMurray.

GRAYSON: Walt McMurray is—

McLAFFERTY: Walt McMurray at Yale. And, oh gee, well I essentially knew all of the students.

GRAYSON: Early Biemann graduate students?

McLAFFERTY: Yeah. And Klaus was totally nice to me. He never acted like I was spying on him or something when I came into his lab. And oh, now don't tell Klaus this, [laughter] because, but I think he asked me -- I don't think it was Burlingame -- but I was in there one day and they hadn't had their instrument going very long and they said, "Oh, we've got this funny peak that shows up in our spectrum." And I said, "What mass is it?" They said, "One forty-nine." (149) "I know what it is." [laughter] *Added note: The 149 peak in a mass spectrum is almost always due to the presence of a phthalate compound. These are common plasticizers and ubiquitous in any laboratory with plasticware. MAG*

GRAYSON: Yes. We all know what it is.

McLAFFERTY: And you'd have known. If you'd visited his lab you'd know what it was. Well, of course, he knew, from that day on he knew what it was. It wasn't that I'd made any great discovery for him, but those are the kinds of things I talked to him about. And his peptide stuff was absolutely terrific. Absolutely terrific. And his indole alkaloids; as a Dow person I certainly wasn't going to compete with him with that. For heaven's sakes. So, we weren't really in competition about things. So I had to have people to talk to about mass spec and I didn't have anybody. And so, he was a life raft. He was wonderful.

GRAYSON: So, when you set up the Boston lab you didn't have any mass spec capability there?

McLAFFERTY: Well we finally got Roland Gohlke to move out from Midland. He was, unhappy there and I said, "Well, come on out to our place." And so, he got a Bendix time-of-flight and we had great fun with that. Well, one of the, one of the pictures, I think the picture's in—

GRAYSON: Yeah. There's some picture's in Carsten's book about it.

McLAFFERTY: I think, with Roland up against the time-of-flight. Speaking of direct probes I think it's visible, because he built a direct probe for the instrument that we had. Well, in fact, we got all the details of it from Jack O'Neal at Shell.

GRAYSON: Yeah. And why don't we talk about that now? Because, I mean that's a method of introducing samples in the solid state that obviously has been extremely useful. But, in my own "research" into the history of it it's a little bit foggy as to exactly who's the main person who actually got that going. So, you're saying—

McLAFFERTY: Want my recollection?

GRAYSON: Yeah, what is your recollection of it?

McLAFFERTY: Well, we could look it up in the ASTM E14 things, but my recollection it was the 1959 ASM E14 and Jack O'Neal had put this on his 103, (Consolidated Engineering Corporation) 103C mass spectrometer) and told us all about it. How he'd put very large hydrocarbons in there and gotten spectra from them. And hell, we were all excited and Roland built it for our mass spectrometers. I mean, the implementation wasn't totally terrible, but I went, along in there about that time, maybe 1960—and we could probably get the dates out of—do you remember Ivor Roland Reed, R, I, Roland Ivor Reed at Scotland, at, not Edinburgh but Glasgow?

GRAYSON: Yeah. In Glasgow.

McLAFFERTY: University of Glasgow. I visited him there and he had made a direct probe that -- I'm not sure -- he might have done it independently of Jack O'Neal. But, it was a copper rod that he'd pushed through a set of O-rings. And, he showed me running samples, and he had a spot on the sample on the end of the copper rod and they put a Bunsen burner on the other end and he got spectra. [laughter] I said, "Why don't you heat it electrically?" "Well, what's the matter with this?"

GRAYSON: So, that was a pretty expeditious way of warming it up?

McLAFFERTY: So, I think, that's written up somewhere in the literature and Ivor Reed has a textbook along in there that I think it's mentioned in.

GRAYSON: Do you know what year that would have been in?

McLAFFERTY: That was around 1960 or something like that.

GRAYSON: Nineteen sixty?

McLAFFERTY: I probably can—

GRAYSON: So, people were coming to this concept in different directions, but I mean it's also implemented in the time-of-flight?

McLAFFERTY: Oh, another story. Along in there, maybe it was 1962 or something, I visited Vlada Hanus, H-A-N-U-S, in Prague at the—well, anyway.

GRAYSON: What was his first name?

McLAFFERTY: V-L-A-D-A, Vlada.

GRAYSON: Vlada?

McLAFFERTY: Hanus. And, he was a great pioneer in running large organic molecules. Even alkaloids. Only the way he ran it—and he had a Russian, of course Czechoslovakia had nothing -- Russian mass spectrometer, and I witnessed this in his lab. He broke vacuum, unbolted the whole ion source, put the sample inside of the little capillary that he had pulled out then put it back in the ion source, shut the whole thing together, bolted it down, pumped it down, and then of course the ion source heated itself when you turned on the filament, and he was getting beautiful spectra. "Well," I said, "How many spectra a day do you run?" "Well, we can run three or four." And they were beautiful spectra and you'll find them in the literature back in the early 1960s. And, he didn't have a direct probe or anything.

GRAYSON: Yeah. Well, that was one way of getting it done I guess. [laughter]

McLAFFERTY: So, so this direct probe was the cutting edge. It sounds totally foolish after electrospray and MALDI and all those things. But—

GRAYSON: So, what about, I thought Bendix was involved in this whole thing too?

McLAFFERTY: Who?

GRAYSON: Bendix time-of-flight? They (McLafferty: Yeah.) were? So—

McLAFFERTY: Yeah. Bendix put one on very early.

GRAYSON: So now, their design was derived, did they do that, derive their own or was that taken from other work that they were aware of, do you know? Or—

McLAFFERTY: Oh, I think they got it from Jack O'Neal.

GRAYSON: From O'Neal.

McLAFFERTY: Oh yeah. (Grayson: Okay.) Yeah.

GRAYSON: So, okay.

McLAFFERTY: Yeah, I'm, I'm pretty sure the Bendix thing came out in 1960 or 1961 and Roland, we had very close relations with Bendix, (Grayson: Sure.) because they were in North Detroit and they were only an hour and a half drive from Midland. And, that's when we did our first GCMS. We took Roland's GC and put it in the backseat of the car and we drove down to Bendix -- and I think that was February 1956. And, and so that made it very easy because they had the instrument. No. Yeah. February 1956? Yeah, that's right. But we got the first laboratory time-of-flight, except that it arrived just about the time I had left.

GRAYSON: To go east?

McLAFFERTY: DuPont got the first two, but they got them for the process control. But anyway, so Bill Wiley and—

GRAYSON: Did you know Bill Wiley?

McLAFFERTY: Oh yeah. Yeah. He was the guy who called and arranged our thing. And, the guy I saw the most after that was—

GRAYSON: Younginger? There was a guy by the name of Ed Younginger and Burt Luchte.

McLAFFERTY: Oh, all those guys, yeah.

GRAYSON: Don Damoth

McLAFFERTY: Oh gee, I can't remember [Drawer opens] Dan Harrington.

GRAYSON: Oh, Harrington. Yeah. Sure. Yeah.

McLAFFERTY: He sent me a letter that described our first experiment. Another of my good stories is how Roland pulled the wool over my eyes. Well, it took me years to figure out, but here's Roland sitting in front of the oscilloscope and they had a slave scope that I'm watching, and Roland beat me every time interpreting the spectra that came up. And it was years later that I woke up in the night and realized that Roland was the one that made up the samples and Roland's the one that had tried them out on the GC before we left, and so, he knew which ones were going to come out first. [Clap] Darn I got suckered. [laughter] Pardon, oh this—

GRAYSON: He [laughter] wasn't looking at the mass spectra so much as he knew that each one came out in a specific order, and that—

McLAFFERTY: Well—he was, he was way ahead of Fred. [laughter] Fred thought he was coming close to having them identified and Roland beat me every time.

GRAYSON: Now, we've got Roland on the grill here. You told me a story at lunch that was a little unusual, about how he was able to make a demonstration using his fingers on the time-of-flight. So, I need to get that again.

McLAFFERTY: That—oh, you want me to put that on the tape?

GRAYSON: Oh yeah. We need to get that story.

McLAFFERTY: Okay. This was, I would guess, 1958 or 1959 and I was at the eastern research lab and I went back every month or two to report to the management. At the time there happened to be an open house at the spectroscopy lab for families, and Roland had the Bendix time-of-flight running and he had a demo for families to show what a wonderful thing it was. And so, here's the oscilloscope showing the spectrum, and the spectrum had an eighteen peak for water and 28, 32 for air, and Roland explains all this, and then he put his finger on the pump-down valve, near the ion source, on the housing of the mass spectrometer as I remember. He might have had a petcock or something, and he opened that. So, he pulled a vacuum on the finger of his left hand and then with everyone watching the oscilloscope he put the finger of his right hand into carbon tetrachloride. And my memory says twenty or thirty seconds later up on the oscilloscope came the characteristic isotope peaks of carbon tetrachloride. And every time I tell the story to mass spectrometrists or to anybody that understands what's going on, of course, it means the carbon tet gets into your bloodstream and back out the other finger and into the mass spectrometer in twenty-five seconds.

GRAYSON: Uhm-hmm. And then he tried—

McLAFFERTY: And then he tried DMSO, dimethyl sulfoxide. And, it came through so powerfully that it shut down the pumping system, which has an automatic shut off. When the pressure gets above a certain place it shuts down, and so DMSO came through him so fast it shut down the pumping system. But, try it some time.

GRAYSON: That's okay. I don't think I want to do that experiment. [laughter] I'm sorry.

McLAFFERTY: Well, you can try it with ethanol.

GRAYSON: Yeah. Well, ethanol would be fine. [laughter]

McLAFFERTY: I mean, at least we could do it, try it with scotch.

GRAYSON: So, yeah there you go. So the, what you're saying is the skin is pretty permeable and the—

McLAFFERTY: The blood stream gets around pretty fast.

GRAYSON: Mixes, right, yeah. It gets picks up the compound and delivers it to the other end of it.

McLAFFERTY: Yes.

GRAYSON: That's pretty scary, actually.

McLAFFERTY: I've been telling that story for years and I have yet to hear of anybody that has ever actually tried it themselves.

GRAYSON: Well, I might try it with ethanol. [laughter]

McLAFFERTY: Okay. Try it with ethanol. Try it with ethanol.

GRAYSON: So, I think we can move, I think we can move into academia now. You finally had the—

McLAFFERTY: Finally got to academia.

GRAYSON: So, do you consider yourself being an analytical chemist or a physical chemist?

McLAFFERTY: I went there as an analytical chemist, yes.

GRAYSON: Okay. And so, you're at Purdue and you're working for this guy Buck Rogers?

McLAFFERTY: Yeah.

GRAYSON: And so your job's—

McLAFFERTY: They had divisions in chemistry and I was a member of the Analytical Division and Buck was its chair.

GRAYSON: Okay. And so, while other schools were divesting themselves of the analytical chemists, Purdue was deciding that they were not going to go that route?

McLAFFERTY: Yeah, there were a few. I mean, University of North Carolina at Chapel Hill is famous for their analytical chemistry. And, well I said Charlie Reilley was elected to the Academy in the 1970s and they have, so the, so that there are other places that, oh, the University of Illinois and Wisconsin always had good analytical departments. The state schools were quite well known for having good analytical chemistry. But almost all of the better chemistry departments, especially the private schools, essentially shutdown analytical chemistry or something that was called that. And of course there are many people, there have been people that tell me that I'm not an analytical chemist, I'm really an organic chemist, or a biochemist, or a this, or a that. Well, for me, it's easier to be an analytical chemist.

GRAYSON: So, when you went there, did you go there as an assistant professor or did you go with tenure?

McLAFFERTY: Oh, I went as a professor.

GRAYSON: So, you went there as a tenured position?

McLAFFERTY: I went there in 1964. I was forty-one years old.

GRAYSON: Okay. And so, you had established yourself so they gave you, essentially gave you a tenure, a tenured (McLafferty: Yeah.) position.

McLAFFERTY: And, when people talk to me about academe I tell them it's much easier to go as a professor than as an assistant professor. I recommend that.

GRAYSON: Uhm-hmm. I have a son who's having problems with that. [laughter] So, your tenure there, though, is not terribly long? You were there for what, about four years?

McLAFFERTY: Four years. (Grayson: Okay.) Right.

GRAYSON: So, in this business that's just barely enough time to get your shop up to speed?

McLAFFERTY: Yeah. I was very lucky on grad students. I got a crop of five or six grad students the first year. They were great and got everything going and we got a new 21-110 double focusing instrument, and a RMU-6 single focusing, RMU-7 double focusing.

GRAYSON: These were Japanese instruments the six, the RMU?

McLAFFERTY: Hitachi. Yeah. These were Hitachi instruments. I was consulting for Hitachi. Well actually, I first consulted for Varian and so I had a Varian ICR. But, I think Purdue actually bought me the 110, and of course that made it much easier to get NIH grants. And, oh, I had grants. I had Army grants, and having my Army experience made it easier for me to write grants that would go to the Army. NSF grants for all—

GRAYSON: You had to get into the grant acquisition business pretty quickly?

McLAFFERTY: Yeah. But actually being in industry, and especially running the lab and doing patents and things like that, and having a secretary and having this kind of stuff sort of under control helped me get started. The poor assistant professor who has to start out and has to do all of the trivial stuff himself too, has a very big extra burden. Oh, and Dow kept me on as a consultant and so that was a big help. And, as I say I really did a lot of consulting for Varian, and then I started to consult for Hitachi and that was very helpful. I went to Japan a number of times. And, that's when they were planning a super double-focusing high-resolution instrument that turned out to be the RMH-2. I remember going to Japan and seeing it and being so struck by it. And I said, "Well, to sell this you've got to demonstrate what its resolving power is; and your competition is demonstrating 40,000 resolving power." They said " It won't do that." "Well," I said, "You've got to have something to put in your ad." It must have been midnight when I was insisting they had to do this. So I came up with something like a xenon and dichloromethane had a doublet at 131 and to split it required 135,000 resolving power. "Oh yes, we can do this." Of course, they set the slits on the tool bench ahead of time to make sure they were just the right size, and everything else -- put it in there and they got this doublet. So,

all the years that the RMH-2 was advertised they used this [laughter] one doublet that I came up with at midnight in Japan. [laughter] Anyway, that's where scientific talent really comes in.

GRAYSON: Right.

McLAFFERTY: Yeah.

GRAYSON: So they, they wanted to get into the accurate mass high resolving power instruments?

McLAFFERTY: They wanted to beat out CEC.

GRAYSON: CEC? AEI or Kratos or whatever they were at the time? They keep changing. So, there was a pretty good market at that time for accurate mass, high-resolving power instruments.

McLAFFERTY: And, they were selling a lot of these RMU-7s, the small one. It didn't have the resolving power.

GRAYSON: What kind of resolving power did that have, the seven?

McLAFFERTY: Oh, 10,000, maybe 15,000. We had an RMU-7 there and we got an RMU-7 when I came to Cornell. And it seems like on a good day with the wind behind our backs, we could get 20,000.

GRAYSON: Were these Japanese instruments reverse or forward-geometry?

McLAFFERTY: They were all forward geometry. Well, that's a—can I tell more stories?

GRAYSON: Oh yeah. That's what we're here for.

McLAFFERTY: Well, the reverse geometry came out and was fascinating.

GRAYSON: Who did that? Who was the first to do the reverse geometry instrument?

McLAFFERTY: The guys at AEI, Bordoli, Sedgwick, and (Grayson: Oh really?) I've forgotten which ones. Maybe Mickey Barber had something to do with it too. I've forgotten. But, it was, like in 1970 or one of the meetings in 1970. Anyway, I came back here, and we talked about this -- I've always had fun arguing with my students. That's what I love to do and I usually lose the arguments. But anyway, two students Paul Bente, B-E-N-T-E, he went to DuPont and worked on DuPont mass spectrometers first, and Tim Wachs, W-A-C-H-S, they came in one day and said, "Well, we figured out how to reverse the geometry on the RMU-7." And, I was very skeptical that these two grad students could do this. And, "Oh yes." "Well, you know, we're doing it for this project." "Well, nobody's using the instrument this weekend. We've looked at the sign-up sheet. If we can't do it this weekend, Monday morning we'll put it back in shape and they can use it." " Well, okay." "Well, I came in Monday morning and they were sweating and it didn't work. "We got everything right. It's got to work." "Well, you know," I said, I didn't say, "I told you so." I walked in my office and an hour later and they come running in yelling and screaming. They had the thing going. "Well, what was wrong?" "We forgot to reverse the magnetic field." [laughter] And so, we used the reverse geometry. For some years it was our primary MSMS instrument. From 1968 to 1978, ninety percent of the papers on MSMS for structure were ours and most of them were on that instrument.

GRAYSON: So, why, who was the first person then to do reverse geometry, to come up with the reverse geometry instrument?

McLAFFERTY: I think it was the guys at AEI. (Associated Electrical Industries)

GRAYSON: At AEI?

McLAFFERTY: But, and there was a paper at one of the ASMS meetings along that time that, at least I think it's the first one, and I've forgotten what their actual publication was. But, that's when I learned of it and we went ahead and used it. Because the first MSMS, the first collisional activation to do structures that we did in 1967 was on the time-of-flight.

GRAYSON: Uhm-hmm. So, you did that in 1967? And that was with retarding potential?

McLAFFERTY: That was a retarding potential in the flight path. And we did that work. The retarding potential was much later. The boys at the Lincoln Lab outside of Boston did some very nice work,—what were their names? Anyway, they were tremendously helpful to us.

GRAYSON: Bendix had these time-of-flight symposia in Cincinnati. Did you go to any of those meetings?

McLAFFERTY: Yeah. I think we went to one.

GRAYSON: Yeah. They were kind of fun because it was a small group and they all had the same instrument and everybody had the same problems. [laughter]

McLAFFERTY: If I went, it wasn't more than one. In fact, I remember one in Detroit but I'm not sure I remember any in Cincinnati.

GRAYSON: Okay. Yeah, they were, years later I was, that would probably have been in the, oh, late 1960s, early 1970s, right around there.

McLAFFERTY: I probably didn't go then.

GRAYSON: Yeah. So—

McLAFFERTY: Don—what was it? Don—

GRAYSON: There was Don Damoth.

McLAFFERTY: Damoth. Don Damoth, yeah.

GRAYSON: Was with those guys. So, you were only at Purdue for four years. That seemed like kind of a short period of time. You came up here to Cornell?

McLAFFERTY: Yeah. We had a time-of-flight there. We had all sorts of instruments. And I got Babu Venkataraghavan to come with me, Rengachari Venkataraghavan.

GRAYSON: So, you've got to do some spelling here for me, or you just want to show me?

McLAFFERTY: I'll get the book back for you. [laughter] I can spell it but it's easier to—

GRAYSON: So, he had a nickname, Babu?

McLAFFERTY: Babu.

GRAYSON: So, he was with you at Purdue?

McLAFFERTY: He came in 1965 and he as with me until 1977, twelve years.

GRAYSON: So, he came up here with you?

McLAFFERTY: Cornell, also.

GRAYSON: To Cornell?

McLAFFERTY: He was here 1965 to 1977. And he had gotten his PhD in India with Ram Rao, C.N.R. His initials are C-N-R. Rao, R-A-O, but everybody calls him Ram, R-A-M. And, he ended up being the presidential science advisor for the president of India, and he's a foreign member of the U.S. National Academy of Sciences, and so forth. And, of course, I've known him almost ever since I got Babu. Babu had done a postdoctorate with Norman Jones, a very famous infrared-er in Ottawa at the National Research Council. He had done computerization of infrared spectra with Norman Jones. And so he worked on the computerization of things for us. He was absolutely terrific. And, the PDP-8s we got there, then we got a Sigma V that they kept at Purdue, then we got PDP-11s here, and on and on until, well he left in 1977. Well, the first thing we interfaced was a Grant densitometer." And, he did it. Klaus Biemann had done this kind of thing also, his Grant densitometer was fancier, but Babu's programs spit out the data much faster than previously. But, certainly I don't want to take anything away from Klaus. He really pioneered all of that. Babu really pioneered getting away from the photo plates and getting the data out directly. And, I think he's the first one to do that. The tragic part of that was that I was consulting for Hitachi and PerkinElmer sold Hitachi instruments in the United States. They had an exclusive license. I told my PerkinElmer friends, "Well, come and see it." They came and they saw that the RMU-7, and binga-de-bing out comes the high-resolution elemental composition data from the other end in real time with this PDP-8. What did PerkinElmer do without any contract with me or anything? They bought twenty-five PDP-8s,

took Babu's software and started selling this. And, the first one they sold was to our good friends at the EPA lab in Georgia.

GRAYSON: Yeah. I know the name but I can't think of it either.

McLAFFERTY: Oh, now I'll be in trouble. Well, he'll forgive me. (added later: "he" is John McBride.) (Grayson: Uh huh.) Anyway, but of course by the time that they implemented it and got it there it didn't work. Well, the higher-ups at PerkinElmer said that that was my fault. "My fault?" [laughter]

GRAYSON: What did you do?

McLAFFERTY: Well, "I don't have a contract with you. You're not sending me any royalties." And I said, "I want it to work. Heavens. Of course I want it to work." And so, we sent Babu down and Babu got it going. And I said, "But for heavens sakes, send your own software engineers down there so you learn how to do this so Babu doesn't have to do the next one." The guy never showed up. And, the next time, and—well, they finally had to bail out of the business and they lost enough money on the venture. Then they decided at about the same time to bring out their own tiny double-focusing instrument designed by Al Nier. It was for GCMS but it was double-focusing. It was slow as hell scanning. It had a terrible pumping system on it. The background was horrible and it was a total disaster, and besides it was the same time that the computerized quadrupoles came out. They had to scan spectra. So, the president of PerkinElmer, Horace McDonald, I still remember Horace McDonald.

GRAYSON: How do you spell that one?

McLAFFERTY: Horace, H-O-R-A-C-E. McDonald.

GRAYSON: N-O, Donald as in—

McLAFFERTY: McDonald. M-C-D-O-N-A-L-D. Oh, you know McDonnells. Yes.

GRAYSON: Well, that's a (McLafferty: That's different.) different spelling.

McLAFFERTY: Is it?

GRAYSON: McDonnell. (McLafferty: That's right. It's not—) as Bill McFadden said, "That's supposed to be McDonnell." [*pron*. Mick-da-nell] [laughter]

McLAFFERTY: McDonnell? [*pron.* Mick-da-nell] [laughter]

GRAYSON: Yeah.

McLAFFERTY: I haven't heard that one.

GRAYSON: Yeah. So, Horace McDonald was the president of Perkin Elmer?

McLAFFERTY: He was the president of Perkin Elmer. Well my side of the story is it's not my fault that it didn't work and they never paid me a nickel or promised to pay me a nickel. Anyway, about that time I'm moving to Ithaca and, of course, Purdue kept all my stuff, including my new $2 million NIH facility. When I told my friends at Hitachi they said, "Oh Fred, we'll give you another RMH-2. No problem." I said, "Well where is it?" I called my friends at PerkinElmer, the lower down ones. They said, "It's in a warehouse now in New Jersey but Horace McDonald has stopped the shipment to Cornell." So, I went down to New Haven and spent the better part of a day talking to Horace McDonald and whoever he had come in. McDonald said "You can't do this to me." And, "Yes, I can." "It wasn't my fault." McDonald countered "Well, somebody's got to pay for this." At the end we worked out something where Cornell—oh, Carl Sagan, the astrophysicist was here, of course. You remember Carl Sagan?

GRAYSON: Yeah.

McLAFFERTY: Well, he had a program going in which he needed a GC mass spec and so Carl, so—no that's, that's ahead of the story, sorry. We needed a GC mass spec the service lab and so I got Cornell to buy one of their GC mass specs that he couldn't sell, and he then shipped the free $150,000 instrument, that Hitachi had given me, up here. And, every time I'd meet somebody from Perkin Elmer years later, I'd say, "Well, how's my friend Horace McDonald?" [laughter] "Oh, he talks about you." [laughter] I said, "Well, maybe I better not hear what he said."

GRAYSON: So they were a little bit naïve, I think, to (McLafferty: Oh, they were totally naïve.) think that they could just take a computer and a box of software and turn it into a data system?

McLAFFERTY: That's exactly right. You put your finger on it entirely.

GRAYSON: Except that was, (McLafferty: That—) your fault, not their fault? [laughter]

McLAFFERTY: That was my fault. And, of course, I couldn't do it either, but Babu can do it. And he had programmed it in machine language. He could do all these things. And we did, we did probability-based matching. We first did the Self-Training Interpretive and Retrieval System (STIRS) and he was terrific for that. And then we did the Probability Based Matching (PBM) system, which is used in more mass spec laboratories than any other matching system in the world, at last count 25,000 or whatever.

GRAYSON: So, there's a couple of things I wanted to talk about before we got too far afield here. It seems like a rather cursory stay at Purdue before you came to Cornell. What's going on there?

McLAFFERTY: Purdue was great. The first person I should mention is John Amy and Bill Baitinger. Amy, A-M-Y, and his right-hand man was Bill Baitinger, B-A-I-T-I-N-G-E-R. You'll see their names on some of my papers at that time. John ran an instrument lab that was—John had a PhD—entirely created by him. So, for doing things with the mass spectrometer and revising it or fixing it or making it do better, he was tremendous. And, he was tremendous also in training my students so that they were hands-on mass spectrometrists. And, he was very interested in this project and very enthusiastic about it, and just gave us tremendous help.

GRAYSON: And this project, this project, what is that?

McLAFFERTY: Oh, well the project of getting me a whole new research program going. (Grayson: Okay.) For the first six years at Dow I had a bunch of projects like improving the instrument, getting new instruments, putting in gas chromatography, finding correlations of spectra. For the eight years I was there all I could do was correlate spectra because we used the equipment to help the laboratory. We used it for some reference spectra and for spectral correlations, but I didn't really get into many new things along that line. But anyway, we got into all sorts of new things at Purdue in terms of, well all the stuff with Babu and the computer was competitive. We got an ICR (Ion Cyclotron Resonance mass spectrometer). Hardly

anybody else had an ICR. Mike Gross was my first ICR man. So, you know about that? We had a time-of-flight and we did collisional activation on the time-of-flight. We had the first collisional activation for determining structures of ions.

GRAYSON: But now, this is up at Cornell?

McLAFFERTY: This is—no, no. This is all at Purdue.

GRAYSON: At Purdue? Was Gross at Purdue? No. Gross was, I think Gross came up here. Mike Gross.

McLAFFERTY: No, he was at Purdue.

GRAYSON: He was at Purdue? Okay.

McLAFFERTY: He went from Purdue to Nebraska.

GRAYSON: Okay.

McLAFFERTY: We did the first MSMS for structure determination with metastable ions in 1965 there. People don't know about it, but we did the first collisional activation for MSMS there in 1967. And, the collisional activation, or CID as they like to call it, that, my good friend at Warwick—uh, well today is not my day. [laughter] Oh. (Added later: Keith Jennings.)

GRAYSON: I know who you're talking about.

McLAFFERTY: You know, Peter Derrick's, uh—Alan McCall. Alan.

GRAYSON: Last name is?

McLAFFERTY: Is that right? Alan. No, that's not right.

GRAYSON: McCollum?

McLAFFERTY: No, that's not the one. Just a minute. This is terrible. [laughter]

GRAYSON: Well, it happens to me a lot Fred, so don't worry about it. [laughter]

McLAFFERTY: There are some things you sort of have to get your head back on your shoulders. [laughter] If I say the wrong name—Keith Jennings.

GRAYSON: Jennings. Yes.

McLAFFERTY: Keith Jennings. Sorry about that.

GRAYSON: No problem.

McLAFFERTY: Anyway. Keith did this beautiful collisional activation work in which he took benzene ions and bashed them up in an MSMS instrument and came up with fragment ions that were very instructive about the benzene ion. And of course you've got the same fragment ions if you use, any C_6H_6 isomer. I think our publication came several months after his. But, we took isomers that are common and differentiated the isomers by their collisional activation spectra.

GRAYSON: Tandem MS?

McLAFFERTY: Common, common—oh, well, you take ethyl alcohol and knock out a hydrogen and you have the $CH_3CH = OH^+$ ion. And, if you did the same thing to dimethyl ether that has lost a hydrogen you'd have $CH_3O^+ = CH_2$. Well, of course, they're identical composition but under collisional activation they fall apart to give highly characteristic spectra; and that's what we did. We did that when I was at Purdue. Well, actually the first metastable work on it was 1965. And before anybody did much of anything on it, it was the late 1970s. We were a voice crying in the wilderness. And, Keith Jennings used it for fundamental things but never for separating isomers. Of course, that's the basis of our MSMS. We had the 1970 publication in which we did this for peptides. We put a mixture of peptides in, isolated the molecular ion of the peptide in the first mass separation and broke it up and sequenced it in the second one. Well, that's now proteomics.

GRAYSON: Yes. Yes, that's—

McLAFFERTY: That's proteomics.

GRAYSON: That's the beginning.

McLAFFERTY: And, that was 1970, and in the thing we gave full credit to Klaus Biemann for showing you can do the sequencing part. But we did, we put the two things together.

GRAYSON: So, the time at Purdue was pretty productive? You had a lot of stuff happening?

McLAFFERTY: I started a whole new research program.

GRAYSON: You had a big grant from NIH to set up a research facility (McLafferty: Right.) and research resource?

McLAFFERTY: Right.

GRAYSON: So, I still haven't gotten to the reason why you packed up and went to Cornell? I mean, you're at the beginning, at the threshold of a great career here in Purdue, which is a respectable institution.

McLAFFERTY: Yeah, why did I come?

GRAYSON: And so, all of a sudden you packed everything up and come up here?

McLAFFERTY: Purdue was a great place. I kid you not. Purdue was a great place.

GRAYSON: I mean, it seems just a little weird that you would decide to just head east [laughter] all of a sudden and come up here. Cornell is a great place too, but it just seems like a sudden wrench in your career; just taking a left turn right when you're about ready to get off the ground.

McLAFFERTY: Well, Cornell does attract better grad students. Cornell's ranking isn't number one, but it's—

GRAYSON: Better than Purdue?

McLAFFERTY: Quite a lot higher than Purdue's. Purdue people were really nice, and of course the other thing is Cornell was my alma mater. And, I've kept close track of Cornell over the years and it always has this aura of being a friendly place.

GRAYSON: Now, did you—

McLAFFERTY: And, it was totally a friendly department. And, when we had something important to decide like "Who's going to get tenure?" we'd meet, and we'd meet, and we'd meet. If we can't decide we'd just have another meeting, and then if we can't decide we'd have another meeting. Finally we're exhausted enough that we have a unanimous decision and then we all got to stick with it. If the person gets a Nobel Prize we're all brilliant. If the person turns out to run off with the dean's wife well then we're all at fault. [laughter]

GRAYSON: So, so I mean part of it, I mean Cornell—

McLAFFERTY: I loved Cornell.

GRAYSON: Okay. But, Cornell came to you? Did they come to you (McLafferty: Yeah.) when (McLafferty: Yeah.) you were at Purdue (McLafferty: Yeah.) and say, "Hey dude, why don't you come up here"?

McLAFFERTY: Well (Grayson: Okay.) it was out of the blue. I still had close ties with Cornell, but—

GRAYSON: So is this, I mean in the final analysis it was a place you'd rather spend the rest of your career (McLafferty: Yeah.) anyway?

McLAFFERTY: Yeah. It was.

GRAYSON: And even though, it was a bit of a dislocation because you had to move and the grant didn't.

McLAFFERTY: It turned out to be a huge dislocation. I had arranged with NIH to transfer the grant to Cornell and then Purdue decided otherwise. Purdue got their Senator, actually, to decide otherwise. And, well that's fine. That's fine. It turned out that Hitachi gave me a mass spectrometer to replace the one that John Beynon got. It turned out that John Beynon found that his had a second-order error in the ion optics. Of course only John would find that. There's where it was really hard on Hitachi because Fred would have never found the error in the second-order term. John's a physicist. He would find it.

GRAYSON: Yeah.

McLAFFERTY: Certainly it never affected my relationship with John. In fact, I was pleased that they got the thing. That if I couldn't have it, well it was great that John got it.

GRAYSON: Beynon is pretty well established in the UK. How was he convinced to go to Purdue to pick up the ball when you left Purdue?

McLAFFERTY: Well, I think—

GRAYSON: Did he come immediately after you left or—

McLAFFERTY: Oh, it took him a year, I think. But, it was pretty fast, considering. Well he was still with ICI when he came. I think he was still with ICI when he came here.

GRAYSON: This would have been like 1969?

McLAFFERTY: He went in 1969.

GRAYSON: Okay.

McLAFFERTY: Well, you can check. But he had connections with the University of Manchester but I think he was still on the payroll and very well situated at ICI. Of course, being a professor in Britain has much more stature than being a professor here. [laughter] With all due respect. [laughter] You saw where we ate lunch?

GRAYSON: Yeah. Well.

McLAFFERTY: We professors, where we go to eat lunch. Well, you know about that, professors in the United States;—but I'm sure his reasons for going, well when I talked to him—yeah, I talked to him about it before he made the switch and he had the same reasons I did for going from industry back in the UK. He's a little older than I am, but, of course he wasn't there long before he recruited Graham. I was delighted that John went there. It did not affect our relationship whatsoever and he certainly made things hop there and did things to fix the second-order aberration, to correct that.

GRAYSON: So, let's see, Caprioli was—

McLAFFERTY: Caprioli went there.

GRAYSON: Well, he spent some time with, was it Beynon? He spent time with you at Purdue or was it John Beynon, or Graham? When did Caprioli come through Purdue?

McLAFFERTY: Oh, I think Graham. Beynon got there in 1969. He then got Graham there. I think he had a, part of his deal was that he wasn't even there full-time, that maybe he was already at Swansea.

GRAYSON: Oh, Okay.

McLAFFERTY: He kept his appointment at Swansea. So, maybe he had already gone to Swansea, but not very long before that. I remember talking to him about going to academe, and we both did it essentially the same time. Anyway, he got Graham to come and Caprioli was a year or two after Graham. If you want to know we can look it up in the Directory of Graduate Research.

GRAYSON: So, you basically kind of got things started at Purdue and then Cornell just was too good an opportunity to pass up?

McLAFFERTY: Yeah, but things really started up in a hurry there. I suppose another thing you can look at is that I had been saving up ideas that I couldn't work on all the time I was an administrator at Dow. Well, part of what triggered it is they offered me a job running all the analytical in Dow and the main analytical lab was 250 people, or something like that, and the salary was much greater, and the honor was terrific, but we'd have to move back to Midland and I'd have no time for personal research. And so, "Well, what do you want to do?"

GRAYSON: Uhm-hmm. So basically, if you were going to stay in an industrial environment you would have to forsake research and go up the management tree?

McLAFFERTY: Oh sure.

GRAYSON: And so, you didn't want to really bail out of the research side of the thing?

McLAFFERTY: And, there were other people that are better managers than I am. There are. I'm volatile. I get all excited. [laughter]

GRAYSON: Okay. Well, let's see. It's 4:30. Do you want to maybe take a break, or, and—

McLAFFERTY: Well, I would suggest that anytime you want to quit you go back to the hotel and we'd pick you up at six or six-thirty and go to dinner.

GRAYSON: Okay. Well, I'd like to come back in the morning and spend maybe an hour or two (McLafferty: Okay.) just going over a few things. We've kind of gotten up to an area where there's a reasonable amount of information from the Cornell video interview and from Carsten Reinhardt's interview. So, I don't think we need to go over a lot of that, but I did want to go over some names of people and also maybe do a little bit of a discussion of Reinhardt's interpretation of how things came about.

McLAFFERTY: Oh, that'd be interesting.

GRAYSON: Because, I assume you've read the book?

McLAFFERTY: Yeah, I have. I thought he understood the science much better than I expected he would.

GRAYSON: Okay. Well, I think we've kind of worn ourselves out for the day. (McLafferty: Okay.) Why don't we call a halt to the (McLafferty: Okay.) proceedings at this point and then we can stop by in the morning.

McLAFFERTY: You can leave this here if you want to.

[END OF AUDIO, FILE 1.2]

[END OF INTERVIEW]

NTERVIEWEE: **Fred W. McLafferty**

INTERVIEWER: **Michael A. Grayson**

LOCATION: **Ithaca, New York**

DATE: **23 January 2007**

GRAYSON: Ready to start recording our proceedings. This is day two. [Recording Paused] January 23rd, and I'm still talking to Fred. And, did you remember the name of that high school? [laughter]

McLAFFERTY: The high school, where I went to high school? Oh, Omaha North.

GRAYSON: Omaha North?

McLAFFERTY: I was forgetting the name of the chemistry instructor, Mr. Dorsey.

GRAYSON: Okay. Well, if you think of it we'll—we can even correspond by E-mail.

McLAFFERTY: I should have asked—that's right.

GRAYSON: If you want. So, you've got some of the proceedings or the little program booklets from the early ASTM conferences?

McLAFFERTY: I thought I was going to look up Jack O'Neal, but no. Let's talk about other things. (Grayson: Okay.) About his being the first direct probe.

GRAYSON: Yeah. Do you know if he published that in the literature anywhere?

McLAFFERTY: I think it's in *Analytical Chemistry*.

GRAYSON: Okay. I can go back and search for that.

McLAFFERTY: In 1959, is what I remember.

GRAYSON: Yeah. So much of it is available electronically. Last night, during supper we talked about a few topics. I just want to revisit them for a second. You said that Babu Venkataraghavan had gone to Washington University in St. Louis to get some pointers, or a little bit of background on the use of the, of the DEC computer

McLAFFERTY: The PDP-8. And the one before that was the—well, I don't remember. Go ahead.

GRAYSON: Yeah. So, that would have been at the mass spec resource, at the med school?

McLAFFERTY: Right.

GRAYSON: Bill Sherman was involved?

McLAFFERTY: Bill Sherman. But, and that was, 1965.

GRAYSON: Okay. There were two people that were involved in that. One of them is a fellow by the name of Don Rempel.

McLAFFERTY: Don?

GRAYSON: Rempel, R-E-M-P-E-L.

McLAFFERTY: But he came with Mike (Gross) from Nebraska.

GRAYSON: Okay. But he actually started out at Washington University. He was doing graduate work at Washington University about that time and was associated with some of that activity in the med school. Then there was also this fellow that started Technivent.

McLAFFERTY: Yeah. That's the guy.

GRAYSON: Rich Berger. I can't remember if it was Rich or Phil. I know a couple of Bergers.

McLAFFERTY: Phil Berger.

GRAYSON: So Phil, was Phil on the—

McLAFFERTY: No. But if I get my magnifying glass, this book goes back forty years. [Cabinet drawer closes]

GRAYSON: So, you've got this little black book where you keep the names? [laughter]

McLAFFERTY: That I used to be able to read.

GRAYSON: I need a magnifying glass to read that Fred. [laughter]

McLAFFERTY: Well, you're not the only one.

GRAYSON: Does it just keep a log of all these various people that you—

McLAFFERTY: Well, I've just written peoples names down because I'm terrible at remembering names. And, well, sometimes I can find them right away and sometimes they just never show up. Oh, here's Wash. U. Well, Oliver Lowry, but that, Dave—

GRAYSON: There was a guy by the name of Denny Bier that was there for a while. (McLafferty: Uhm-hmm.) I don't know if that rings any bells?

McLAFFERTY: I don't think so. No, these are all guys that—no, these aren't the names I couldn't remember. Peter Gaspar. They're not the names I couldn't remember.

GRAYSON: Okay.

McLAFFERTY: Sorry.

GRAYSON: No problem. And so, this was pretty common in those days that you could go to a "competitor" in business and pick his brain and he'd help you out?

McLAFFERTY: Oh yeah.

GRAYSON: You know, and people were pretty cooperative that way?

McLAFFERTY: Science is always—well, my view of science has always been that way. But, some places are better than others. But Wash.U. Facility and their PDP-8 expertise, they developed what then became the PDP-8 is the way I remember it. They had a great deal to do with the original development of the PDP-8, and there's some name like PDP-7I, or something like that.

GRAYSON: Yeah. They had a whole bunch of different models.

McLAFFERTY: But these guys were totally wonderful to me. I had a connection there, Oliver Lowry.

GRAYSON: L-A-U-R-Y?

McLAFFERTY: L-O-W-R-Y. (Grayson: Okay.) Oliver Lowry, (Grayson: L-O—) who's a cousin of my mother's and who's famous at having more citations for one of his papers than anybody else.

GRAYSON: Ah. One of those guys? [laughter]

McLAFFERTY: Well, you ask people there. They'll know Oliver Lowry in the med school. But anyway, these guys were wonderful and I think they came over to see us too, but Babu went over there and they gave him everything. But, if somebody comes to our lab, well maybe not my books,— [laughter]

GRAYSON: Well, you know. [laughter]

McLAFFERTY: If somebody comes to our lab and wants to learn about ECD, of course we show them, [laughter]. We did the first electrospray with FTMS, and it got published in 1989 or thereabouts. Well, the first thing was that John Fenn himself was tremendously helpful; he provided so many details. I didn't have the connections to get the ions into the mass spec but Don Hunt did. And so, my guys put our electrospray stuff into the back of the car and they drove down to Virginia. And, Jeff Shabanowitz helped them hook it up there, and they were going to stay for three or four days. And, the weekend came and Sunday night they got spectra. And, we published that, of course. We published it with Don and Jeff. But, you know, that's what makes science fun.

GRAYSON: Good. So, your experience has been one that's fairly collegial and cooperative.

McLAFFERTY: Oh heavens yes. I'm sure that, that we didn't invite people in to show them Electron Capture Dissociation before we got the manuscript off to *JACS* , but did we talk about it? Yeah. Well, we talk about things at meetings.

GRAYSON: So, you came to Cornell and there was a kind of price to be paid in terms of the fact that a lot of what you thought you were going to bring with you wasn't going to happen. How long was it before you felt like you had gotten back to where the point you would have been if you'd stayed at Purdue?

McLAFFERTY: Oh that's a good question.

GRAYSON: I think grant money started to get a little tighter and NIH and NSF were not as free-wheeling with their money at that time. I think, you had to wait a while? I mean, you got grants from the Army, but—

McLAFFERTY: Well, I had all sorts of different things to handle. It was like going to Purdue in 1964. We started all sorts of things in research but I wasn't doing it all. When I came here we actually changed and started other things in the research program, and some of them had to do with the fact that we didn't have the same instrumentation. But, for example, this wonderful NIH grant, to set up a big facility there, because we were sure that the computers were the next thing in mass spectrometry. We had gotten them to buy us a Sigma V computer because Babu really thought that was the most wonderful thing in the world. And, this was in 1966 or seven that we ordered the computer and it hadn't arrived when I left. In 1968 it hadn't arrived yet.

GRAYSON: So, at Purdue?

McLAFFERTY: At Purdue. But by the time we got here I had a much closer connection with Digital Equipment Corporation and Babu was saying, "Oh, the Sigma V is outmoded." [laughter] Well, I mean, three years is a lot for a computer. "PDP-11 is the real thing," and I got my DEC friends to loan us a PDP-11. Well, the Sigma V took up a huge part of the room, and the PDP-11 was a few relay racks. Things like that really did make a big difference and it wasn't Purdue's fault, and it wasn't that we made a bad selection. When John Beynon got there he wasn't interested in the computer at all, and so it just stayed boxed up, which is no criticism of John. If somebody had left me something but it doesn't fit into not my original program I wouldn't feel obligated to use it.

GRAYSON: Sure. So, I would guess, maybe another three or four years, four or five years, at Cornell before you kind of felt like your program was back at the—

McLAFFERTY: No. No. We had just gotten collisional activation going at Purdue on the Bendix time-of-flight. And so, one of the first things we did—well, I told you yesterday about reversing the geometry -- was that we set up to do MSMS on the magnetic sector instruments. Well, for one reason I didn't have a time-of-flight. (Grayson: Right.) [laughter] I was trying to think of—it's just like the computer. I didn't have a time-of-flight so we had to do it with what we had. And yet, this looked like a tremendously exciting area. And so the publication I told you about in which we did MSMS of peptides, by putting a mixture of peptides in and getting sequences on them individually. That's a 1970 publication. And so, we really had things going. Oh, one thing that was great was that all of my research group came with me, so I had good hands here at Cornell and I got some good people when I first got here. Cornell already had an AEI MS-9 double-focusing instrument for service work. So, there was high-resolution going and the RMH-2 took a while to get here. The story I told you yesterday. But, I would say in a year we were really going again.

GRAYSON: Okay. So, it didn't take too long but it was a, it was a little bit of a setback.

McLAFFERTY: Yeah, but the thing that's hard to explain is how getting into a new situation with all new relationships can start you into brand new areas.

GRAYSON: Uhm-hmm. Sure.

McLAFFERTY: It's just something I've talked to people about who are considering moving and I have a hard time explaining. But, if we go down my list of publications for that time—well, we have kind of landmark papers in several fields within the next few years after coming

to Cornell. What is Modern MSMS? At least from my selfish point of view, traces back to that work we did in those first few years, because nobody else was doing much of that kind of thing. And that was our first paper on this self-training interpretive and retrieval system, which is an example of one of the first artificial intelligence programs. That was a whole new area for research. We had started a little of that at Purdue, but we did it in a big way here and, though we did some of it on the PDP-11 we had a very strong relationship with the Cornell Computer Center, and so we did most of it on the big IBM. And that turned out to be something I'm very proud of. Also, our file of reference mass spectra, our first collection published by John Wiley, I did in collaboration with Einar Stenhagen. He's a person I think is one of the real pioneers of organic mass spectrometry; and GC/MS, the jet separator was his, and Ragnar Ryhage. Anyway, in 1969 we brought out the first edition of that with 8,600 mass spectra. And in 1974, we had 19,000. And, the latest edition we've done has 600,000. Oh, I didn't show you, we have a couple of offices in there where my two venerable ladies measure mass spectra and put them in the computer.

GRAYSON: So, this is a continuing process?

McLAFFERTY: Stenhagen and his colleague Abrahamsson have both died long ago. So, it's been my project. But, National Institutes of Standards and Technology is up to 193,000 and we're over 600,000. Let's put it that way.

GRAYSON: So, this is an area of competition, you feel, between you and NIST?

McLAFFERTY: Oh sure.

GRAYSON: You and the other guys?

McLAFFERTY: I would say since we have 600,000—oh, the 600,000 actually includes all of theirs, of which less than 100,000 come from them. And so, I don't feel that they're really competitors. [laughter]

GRAYSON: Okay.

McLAFFERTY: Except they have the U.S. government paying all their expenses.

GRAYSON: Right.

McLAFFERTY: And I have my royalties paying all my expenses.

GRAYSON: Royalties? Okay. They must be sufficient to do that without too much trouble?

McLAFFERTY: Oh, and we use the Cornell library and our physical sciences library is one of the finest in the world. And the library is on the same floor. [laughter] I'll go show you the library.

GRAYSON: Speaking of royalties, when did you decide you wanted to do the *Interpretation of Mass Spectra* book?

McLAFFERTY: Oh, that came out of the short courses of the American Chemical Society in 1965, I think it was, started short courses. And, I think that mine was one of the first three short courses and it just happened to be that the guy that was running the short courses for the ACS went to graduate school with me. So my, it was my real talent that got me this job, of course. [laughter]

GRAYSON: Oh yes. Definitely!

McLAFFERTY: It's not what you know but whom you know.

GRAYSON: So, that was the impetus for trying to take all this information that you were providing in the short course and put it into a book form?

McLAFFERTY: The easiest way to write a book is to teach courses. And so, we taught the short course many times. I think I've taught it a hundred times now, if you count the courses and so forth. And so, it just naturally turned into a book.

GRAYSON: And then, so the first, the publication came out in what, the late 1960s then?

McLAFFERTY: Sixty-six.

McLAFFERTY: Sixty-six, 1973, 1980, and 1993.

GRAYSON: And you're guesstimating probably a hundred thousand copies had sold?

McLAFFERTY: A hundred thousand copies. Yeah.

GRAYSON: Now, that's kind of interesting because that basically just discusses EI, electron ionization, as the method of getting gas phase ions, but it's still a popular item.

McLAFFERTY: Well, the, since you're a mass spectrometrist I can tell you. The great advantage of electron ionization is you make an odd electron molecular ion. And so you get both even electron ion reactions and odd electron ion reactions. Odd electron ions are radical side reactions and so they're more specific and they're of a different nature. So, you get not only twice as many types of reactions to use to characterize the structure but you get much more specific reactions. The even electron ions are more stable so you've got to put more energy into them to make them fall apart and that means they have more chance to isomerize to something else. So, it's as simple as that. And, the nice thing about Electron Capture Dissociation, to have the wheel go all the way around and come back to where it started, is that we finally have another method that puts in an odd electron. And so, Electron Capture Dissociation can dissociate by odd electron ion reactions. We haven't exploited this at all, except that it brings you back to the same chemistry.

GRAYSON: Yes. So, in the traditional early ionization method by electron ionization you get your odd electron ion by removing electrons from the molecule?

McLAFFERTY: Got it!

GRAYSON: But, in electron capture you get your odd electron ion by stuffing in electrons?

McLAFFERTY: Hey, man. [laughter] you got it! People don't realize this, but isn't it neat?

GRAYSON: Yes.

McLAFFERTY: I mean, this is the kind of thing that you get from teaching courses is that you try to condense things down to the simplest sort of thing. So, why should we ever give up [laughter] good old electron ionization, which as you say knocks an electron out of there, except that we could do it the other way and put an electron in? [laughter]

GRAYSON: So, when did you get into the Electron Capture Dissociation business?

McLAFFERTY: Well, our first publication was 1998. So, it's pretty recent. The story is kind of interesting too. For proteins and, well for large molecules and large biomolecules obviously has, since NIH has always supported my work, large biomolecule, it's because of this even electron ion problem. All of these wonderful methods, starting out with chemical ionization, which is a wonderful method, and going through FAB as you pointed out yesterday, and electrospray and MALDI all give even electron ions. We had been doing larger and larger peptides on the FTMS. The first FTMS was 1984. And so, we did FAB on the FTMS and we're very proud of the fact we could get up to 2,000 molecular weight peptides, and we don't take any credit for that. We were following Mickey Barber and those people. But then we wanted to dissociate these to find out the sequence. And we tried collisional activation; and we tried infrared multi-photon dissociation, the CO_2 laser you saw in there. And we tried surface-induced dissociation, various ways of slamming the ions into surfaces. Oh, we invented neutralization reionization mass spectrometry along about that time. Our first publication on that was actually 1980, but that was in our big four-sector instrument. Well, that was the way you could actually get odd electrons into it. But a four-sector instrument isn't what you want in everybody's lab.

GRAYSON: Yes. Right.

McLAFFERTY: And, what other methods did we try? Oh, we tried excimer laser, 193-nanometer photons. I said, "Gee, we're going to put 6.4 eV of energy into that ion. When it absorbs that photon it's going to blow apart [clap] in a different way." Rats! They came apart. The darn peptides would come apart in exactly the same way.

GRAYSON: Oh really?

McLAFFERTY: Oh, of course. From a physical organic chemist point of view, a physical chemist's point of view, if you raise this ion to a certain energy level and let the energy equilibrate randomly over the thing it's going to fall apart statistically. By hindsight, this is wonderful. Well, it turned out that in this excimer laser thing with these very hot photons, once in a while our people would see other ions and it wasn't always the very best people that saw it. And when, when Roman Zubarev arrived—

GRAYSON: How do you spell that?

McLAFFERTY: Z-U-B-A-R-E-V.

GRAYSON: Okay.

McLAFFERTY: He's now a professor at Uppsala, the University of Uppsala in Sweden. I said, "Roman, we've tried a variety of ways to figure out what's going on here. In fact, we are even having trouble making reproducible spectra." And, I won't go through all the things that, moonlight shining through the window and things like that [laughter] that were making the difference here, that we'd gone through. But one of the things that we considered was it could be electrons that were, and we couldn't figure out why this was. But, Roman said, "Oh, we can test that by just putting on an extra pair of trapping electrodes. You know, in ICR we trap the ions in the center with a magnetic field with positive electrodes which keep the positive ions in there." He said, "But, of course, then if you have electrons in there they see this positive charge and they go roaring out of there." (Grayson: Right.) And, Roman says, "I'll put another set of trapping electrodes and I'll make them negative, and then I can keep any electrons we get in there." And when he did that he found he could make these peaks get higher and higher. And, it turns out that what was happening is this $30,000 laser wasn't aimed perfectly and it was hitting the inside of the metal surface of the ion cell making electrons. And, then Roman put the old-fashioned filament electron gun back on, which actually electrons cost less than $30,000. [laughter] In fact, it was the old gun that we'd had on the instrument when it first came and it was sitting on a shelf. In fact, even the—a story—Kent Henry was a grad student—

GRAYSON: Is it Kent?

McLAFFERTY: Kent. K-E-N-T, Henry, as in Henry, was a grad student who had already gotten his PhD. And I said, "The last guy I think worked with that gun was Kent Henry." "Why don't you call him up?" So, Roman called up Kent Henry. Kent says, "Oh, you'll find on the right back side of the third shelf in the junk room behind the thus and so." And there it was. And, Roman put it on there and we've had ECD spectra every since.

GRAYSON: So you inject electrons into the cell?

McLAFFERTY: You have to trap the electrons. And there's several things about it that the electron—here you have not just an ion but a protein ion that has ten or even twenty positive charges in there. And, gee, an electron comes near it, well of course it's going to go zinging into it. Why don't we ever see it? Well, the electron has to go slowly enough to turn around and be captured. And so, Roman is very good at physics and he measured energies in electrons and he found that one volt electrons, that is going one volt kinetic energy, had three orders of

magnitude less chance of being captured than the electrons he could make go as slow as he could. And so, this is another reason people hadn't observed it because, if you try to trap electrons it's entirely the opposite of trapping. And so, there have been several ways to overcome this problem, but now it's pretty straightforward and everybody uses Electron Capture Dissociation. And, the real problem was that you could only do it in the FT, we could only do it in the FTMS. Don Hunt's come up with a way to do it in a quadrupole instrument. And now just at this last meeting the guys at, at what used to be VG—maybe, I guess it's part of Waters' development.

GRAYSON: Yeah. That's a—

McLAFFERTY: They've come up with a way to do it in an ion trap that's really a very clever method. In Don Hunt's method he has to use a great big anion. But, the first thing he used was anthracene minus, and he could keep this from being knocked out of there. Of course, when an RF field is changing it changes and throws the electrons out long before it throws ions out. What the ion trap people have done is instead of having an RF sine wave they put in a square wave. And so the voltage has totally changed in a nanosecond or something like that, so the electron and the ion are both seeing this change and they can get the electrons trapped next to the ions long enough for the capture to take place. Well of course we didn't do any of that, but the more the merrier -- the more people that use it. At least I say to the students, "Don't worry if you haven't done the experiment perfectly because we'd never got ECD if these guys hadn't aimed the laser low enough." [laughter]

GRAYSON: Well, serendipity always plays a part in research and being open to a serendipitous result is part of progress, I think.

McLAFFERTY: I've forgotten now but I bet it was over a year after we noticed these peaks, and it might have been two years before we figured out what they were. And, it was always a nagging thing. It was never kind of on the top of the burner. Because, you know, you could say, "Well, somebody got a messy solution."

GRAYSON: Yeah. It could be an impurity in the sample.

McLAFFERTY: Or, they didn't, measure this right or there could be something. So anyway. That was ECD.

GRAYSON: So this kind of leads to an interesting point and that is that—and I don't know if this is so true in other areas of the physical analytical sciences—but it just seems to me that

every time mass spectrometry looks like its got a dead end or it's limited in some way or another, somebody discovers something and all of a sudden a whole new area of science, a whole new class of compounds, a whole new way of looking at the problem develops. And, it just seems like there's this continual, every three, four, five years some new idea comes along and captures the imagination. People take off with it, and then in another couple of years something else comes along. It just keeps growing. I mean, you, I think, yourself one time figured that electron ionization mass spec was, the end of the road?

McLAFFERTY: As I told you, 1956. I was out of it. [laughter] It was done. [laughter]

GRAYSON: And it does. I mean, it just keeps reinventing itself. I don't know of any other technique that (McLafferty: Well—)

McLAFFERTY: NMR is certainly a parallel to that and I think that that's the point that Reinhardt makes in his book. But, infrared is interesting in that when I first got into mass spec, at least locally, infrared was totally dominant. I mean, they had twenty people in the group and they were known across the Dow Chemical Company as the court of first and last resort for solving molecular problems. And then, infrared really went into a total slump and Fourier transform infrared gave them another three orders of magnitude sensitivity and really kind of put them back on the map. But, they don't compete. Their biggest competition is NMR not mass spec. But, NMR, 2-D and multi-D NMR just creams infrared. And so, infrared was way up there. And, and they didn't get enough new things, exactly what you said, but NMR has –

GRAYSON: Well, I still think mass spec—

McLAFFERTY: Mass spec keeps getting new things.

GRAYSON: -- outdoes NMR. But, it is an interesting phenomenon because people kind of just kind of put it, well, it's exhausted it's utility in that it's just a run-of-the-mill technique and all of a sudden something happens. And, this whole business of being able to get biological molecules of interest ionized has just been revolutionary. The ASMS meeting has gone biological. There were 6,000 people, I think, at the last ASMS meeting.

McLAFFERTY: And it all, and so many of them were interested in, and the big argument was top-down versus bottom-up, and we totally lost that argument not too long ago, but now we're beginning to get converts.

GRAYSON: Well, let's talk about that a little bit. When you say "top-down versus bottom-up," we're talking about being able to sequence proteins?

McLAFFERTY: Yeah. Characterize proteins.

GRAYSON: Characterize. What's the top-down method of doing it?

McLAFFERTY: Well, the big difference historically is that when mass spec first got into the protein business the standard way for separating proteins was 2-D gel electrophoresis. And so, that gave you a spot, and the biochemist wants to know what's in that spot. And so, the only way they could get that spot into the mass spectrometer was to digest it and make it into peptides and then put the peptides in the mass spectrometer. And so, that then became the standard way to examine proteins.

GRAYSON: So, by obtaining mass spectra of the digested proteins, digested as peptides, as small fragments of the protein, that would have provided sufficient information for them to determine the nature of the protein?

McLAFFERTY: Yes, well, there are two problems in proteomics. The first is just to identify the protein. Now here, you have the advantage that the DNA sequence has predicted what proteins will be generated, and so you have a few thousand to pick from. And so, if you find a peptide—let's see if I can do the statistics—if you find a hexapeptide that has six amino acids and you actually can get the sequence of those six amino acids there's only one chance in twenty for each of them, because there are twenty different peptides. And so, you have twenty to the sixth power uniqueness of this peptide. And if you then look at your thousands of proteins that are identified and see if you can find this sequence of six amino acids and you can find it in one of those, there's a pretty darn good chance that, just by identifying this hexapeptide that you've identified the protein. But the trouble with that is that the genome only tells you the sequence. It doesn't tell you about post-translational modifications. And so, to find that out, finding this hexapeptide and identifying it -- well, what they do with bottom-up is they sequence all the peptides they can but they don't worry about the rest of them. And, in fact, in shotgun sequencing, they digest a big mixture of proteins and they just look for individual peptides and they know that there are going to be lots of peptides in there they can't identify, that they can't do anything about, and they don't even worry about them. And so, if the peptide happens to have the part of the protein that has the post-translational modification they're not going to find it. And so, top-down, first you've got to get the whole protein into the mass spectrometer, and the reason that gel electrophoresis doesn't stop you now is that you separate proteins by liquid chromatography, and especially if you want to do top-down. And, and now you can get the molecular weight of the protein.

GRAYSON: So, you say "molecular weight," we're talking about 10,000 daltons, 20,000 daltons, a hundred thousand daltons?

McLAFFERTY: Fred's spiel. Fred has just started into his spiel.

GRAYSON: Uh oh. [laughter] I'm in trouble.

McLAFFERTY: Well, I think if you—Top-Down Visualization of a Protein Mixture.

GRAYSON: Okay.

McLAFFERTY: Here, here are fourteen different molecular ions from a mixture and you can see we're doing the molecular weights of these to a dalton. So, here's 14,712.2 and that should be, it's not 14,714 or 14,710. And, we can worry about accuracy. But, this is our old six tesla instrument. And so now, these are all inside the Fourier transform instrument. Now you can do Alan Marshall's SWIFT [stored-waveform inverse Fourier transform] and knock out all of the ions except 14,712. And then, we can break it up. We can break it up by shining the infrared laser on it, or we can break it up by collisions, or we can do Electron Capture Dissociation, and we'll get pieces of the molecule. So here's one. Ah, this one is 20,211. It turns out, if you look at the different proteins that are possible there is one, this is 20,211.3. There's one 20,211.9. So that's hopeful, but it could be a coincidence. And so, you do the SWIFT on it. Here's the molecular ion that you get with all the others gone, and you break that up and you get these things. And, for example, between this peak and this peak there are fifty-seven daltons, between this one and this one there is a difference of fifty-seven daltons. That means there is a glycine there. Here's one that's 146, so that's phenylalanine. And so, you can get the sequence from this spectrum just by getting these pieces of the molecule. You look at the sequence that is in this 20,211 guy and it's the same sequence; so, you know you've pinned it down. Because, if you had gotten this peptide by digesting it, that would be your evidence that this was originally this protein. But, this protein has exactly the same molecular weight and so it doesn't have any post-translational modifications. And so, now you've pinned the identification down much more.

GRAYSON: But this is a really itsy-bitsy, teentsy-weentsy peak here?

McLAFFERTY: Yeah. That's right. [laughter] Well, signal-to-noise. And—

GRAYSON: I mean, there is something there but it's pretty small? So, I need to get some copies of these to go with the transcript so people have some idea what it is we're talking about.

McLAFFERTY: Okay. You, may have copies of anything. Here's Electron Capture Dissociation, carbonic anhydrase, 29,000. In one mass spectrum there are 512 ion mass values. When you look at the tiny peaks you have to remember that we have this tremendous signal-to-noise ratio. NMR and infrared guys have no concept of such a large S/N ratio. The record as far as I know is a mass spectrum with over 5,000 masses in it. For the 259 amino acids in carbonic anhydrase, the 512 mass values represent 183 cleavages. As far as finding out about the structure of this molecule; well the reason we show it is because it's a joke on us. Carbonic anhydrase is well known. Its sequence has been in the protein database, one of the first one that was ever done, and we've always used it as a model for our top-down things. Our first publication on this was 1993. And, and all of our pieces fit the sequence. The postdoc Newman Sze comes into me and he says, "Well, of these 512 fragment ions, 45 of them were off by one dalton." And I said, "Well, maybe you didn't calibrate your instrument." I mean, this is 29,000. Twenty-nine thousand, of course, the fragments aren't 29,000.

GRAYSON: No, but still.

McLAFFERTY: And he said, "But they're all off by minus one dalton, not plus or minus."

GRAYSON: It's a systematic error.

McLAFFERTY: And then when I tried to explain it he said, "And all the assignments are between the tenth and the thirtieth amino acid out of 259 amino acids" What happened is that the sequence in the protein database had always been wrong. Aspartic acid and asparagine are different by only one dalton. And so, to make this fit all you do is switch the aspartic acid and the asparagine, and then they all fit. So, we corrected the sequence in the protein database. But we had been using the wrong sequence, and in fact we had a paper in 1959 that we had "top-down" in the title. I mean in 1959, whatever the—

GRAYSON: You mean 1999 or 1997?

McLAFFERTY: To—1999, yes. And, you know Neil Kelleher?

GRAYSON: Yes, I know Neil.

McLAFFERTY: Yeah, well Neil's the first author of the paper. And, in it we were showing how top-down works and we showed how it worked on carbonic anhydrase, and he said, well, he missed one of the fragments by one dalton. And of course, he hadn't missed it but at least he reported it honestly. So, I said, "Well, this is all Neil's fault. It's not my fault. [laughter] Because he's the first author."

GRAYSON: Yeah, Neil talked to our local discussion group a few years back. He's really a lot of fun.

McLAFFERTY: So top-down is far better to characterize the protein where bottom-up is cheap and fast to identify proteins. And, it turns out that, of course, you can identify them by top-down. The figure that people usually get is that the identifications by bottom-up are ninety-seven percent accurate or something like that. Neil's program for calculating the probability of his top-down identification being wrong it comes out to things like one part in ten to the thirteenth. And, you can see the difference. I mean, if you've got the molecular weight right, sure, there's some chance of being wrong. But since our molecular weights are so large, there are 20,000 possibilities for the elemental composition, but then you get pieces, of course we'd get many pieces. And so, here's a case where we had forty-five masses that told us this was wrong. And though Fred would have ignored one or two, [laughter] or obviously Fred had ignored one or two, when there are forty-five he doesn't ignore them.

GRAYSON: No, you can't ignore that. So, could it be that the bottom-up is driven by people who have come out of the biological community who are used to working with gels and they're trying to adapt mass spectrometry from that perspective, whereas the top-down people are mass spec oriented and think more in terms of creating molecular ions?

McLAFFERTY: Sure.

GRAYSON: I mean, that is almost always a fundamental desire, I think, of any mass spec person is to break a molecular ion? I mean that's why people liked FAB; the way it would give you a nice protonated ion, chemical ionization provided a means of creating molecular ions for a lot of compounds that would just fall apart by electron ionization. But, here with chemical ionization you could say, "Yes, well there is a molecular ion. It's alcohol. It's a C-9 and not a C-10 or whatever." So, it's that mentality that the mass spec people bring to the biological problem of creating molecular ions, whereas the biologists are coming from these, pieces.

McLAFFERTY: Mike, I couldn't have said it better. [laughter] That's absolutely right. It was the way we had to do this. And so the real thing we contributed was -- I named it "top-down,"

and I named theirs "bottom-up," which has a some connotation of being [clears throat] lesser, of lesser quality. And of course as soon as we did this it caught on. But these really good people that developed the bottom-up approach, I think they were all at genomics type companies. And so of course that's what they were doing. And, if they found a method that worked, even though they had to do digestion to get it out of the spot; if they had a pure protein that came off the column they still did it that way. Even though you and I, especially if we had electrospray and FTMS. That's the other thing they didn't have. Oh, they didn't have a chance, of course, to do this. They all had quadrupoles and ion traps and they had no chance to do this without FTMS.

GRAYSON: So, the main thing that we're going to get out of FTMS is enough resolving power to nail down the mass accurately? There's no ambivalence about it? I mean, one mass unit difference as you show here means something that has to be looked into?

McLAFFERTY: Yeah.

GRAYSON: Whereas in a quadrupole or ion trap one mass unit difference would be considered to be quite adequate or, pretty damn good? [laughter]

McLAFFERTY: The other thing is that MALDI can't do it. We need to have all of these charges on the molecule. In order to do our dissociations we have to have those charges and MALDI only puts on one or two charges. Pardon me. I've got to get you one more reprint. This is in *Science*. It's a little harder to get into *Science*, and that was this fall. And, just to complete the story, the problem we've had with top-down is that as these proteins get bigger and bigger and we have them stored inside the FTMS, they get harder and harder to break up. And, for years we have been studying what happens to the conformation of the protein when you go from solution to gas phase. Our first paper was 1993 on HD exchange. And, there had been a lot of controversy about this and it's not settled at all. But we have found a way to break up these larger proteins. Before it was very difficult to get any information out of a fifty-kilodalton protein, or a sixty, or a something like that. And, and now we're, we're getting lots of information on—

GRAYSON: Two hundred kilodaltons?

McLAFFERTY: A hundred and fifty, and 229 is one of the largest we've gone to. And it's mostly been through understanding this folding process and the fact that the conformations are different in the gas phase than they are in solution. And so, that's been a nice fundamental study. The biggest point of controversy is that many drugs act by associating with the enzyme of the protein. In developing new drugs they have screening methods -- "How do you find out

which of these small molecules associate with the protein the best?" A way they have come up with is to take a whole mixture of these small molecules in solution with the protein, electrospray the whole mixture, and then look at the molecular ions plus the drug molecules. They are 200, 250, 300, Daltons and look for those molecular weights, and the ones that are of the largest abundance are the ones that have bound the best. Sounds great? Well, they bind because of the conformation of protein. That's how they bind in solution.

GRAYSON: But that's conformation in the solution?

McLAFFERTY: And the conformation changes in the gas phase. And I have been on the side, "Hey fellas, watch out. They change in the gas phase." And, they keep selling FTMSs to the pharmaceutical companies. And, oh I had a plane ride with Nathan Yates who's at Merck; and he's a very nice guy.

GRAYSON: He's at Bruker did you say?

McLAFFERTY: Merck. And he's a very nice guy and I said, "Nathan, you can't do that. You know that." And he said, "Fred, I'm not arguing with your science or anything." He says, "But, is it true that the conformation changes more in some cases than it does in others?" And I said, "Well, of course." He says, "Well, what we find is that we use this as our primary screening method and we want to screen zillions of molecules. This can eliminate ninety or ninety-nine percent," I've forgotten what number he used, "of these possibilities. And then we can concentrate on those that are left. And sure, we find that many of them are absolutely no good at all, but we find some of them are good." And so, he said, "You're absolutely right, but we had no screening method at all, and so this method is better." (Grayson: Than nothing?) He had four FTMSs in his lab. And, I said, "I've got this one old FTMS. You've got four." Oh, I think he had four of the new Finnigans. You know, Finnigan?

GRAYSON: Mass spectrometers are like handbags to them? [laughter]

McLAFFERTY: And the pharmaceutical guys when I talked to them they all laughed and said, "Fred you're absolutely right." Before, there were scientists who got very angry with me because they were sure, well there are many papers in the literature that show how the conformation stays the same in the gas phase. But it turns out that ECD, Electron Capture Dissociation, is a great way to look at the conformation in the gas phase because it's so crazy it breaks the backbone of the protein but it doesn't break the noncovalent binding. It's so specific for the backbone. Proteins like this, it'll break the backbone here, but in the gas phase it's still stuck together. And so, then if you bring in, say, collisional activation and break it apart like

this then that piece will fall off. So, we can study what the conformation is by looking at what doesn't break up with Electron Capture Dissociation. That's another way we got that.

GRAYSON: So, that's kind of weird? In other words the, the soft bonds, so to speak, survive?

McLAFFERTY: Yeah, I tell my physical chemist friends, they don't know anything about it. I say, "A noncovalent binding is much stronger than covalent binding." [laughter] "Wha!" [laughter]. It's also great because this specificity for the backbone makes it so that it doesn't knock off the side chains. If you do collisional activation with a glycosylated protein those side chains fall off. If they are phosphorylated they fall off. But, if you do it with Electron Capture Dissociation they stay on. And so ECD is, has become a great method that way. And we hadn't predicted that. No.

GRAYSON: Well that seems to me that a whole lot of mass spectrometry is almost a serendipitous experience. I mean, the thing about the field, in my mind, is that it just flies in the face of these scientific management concepts that come out of the business schools where everything has been managed and you're going to get this result from doing that, and so on. There's no room in any of that type of management for the serendipitous outcome, the unexpected result, the ability to follow down a path that's different from the one that you had planned in front of you. And it's such a, such a dull way of trying to conduct science and research. But, unfortunately the business management schools have taken over and everything has to be devoted to a certain plan. And if you deviate from the plan then you fail, and you're out the window. This whole technique just flies in the face of that type of management concept. It says, "You know, none of this would ever have been developed if we'd been related to that type of approach."

McLAFFERTY: Another breakthrough we had is that, I said to the troops, I said, "Why don't you try adding things to the electrospray solution and maybe we'll get more charges on it, and so forth." And, Evan Williams has shown that things like glycerol get more charges. And so, I told them these things to try and they tried them and came back and said, "No, we get the same or worse results." "But we're trying other things," they said. And then they came back and said, "Oh, we've been trying ammonium tartrate, ammonium succinate, and ammonium—"—oh, what's the other? Well, anyway, and "We get fifty percent more cleavages." And, my only contribution is that they got so much joy out of the things I told them to try that didn't work [laughter] that they really wanted to find something that did work. And so, I said, "Well, why does ammonium tartrate work?" "Well, that's your job." [laughter] And so, this is wonderful.

GRAYSON: You have to do something Fred.

McLAFFERTY: I joke with them. What's going on? Here's this electrospray solution. Here's these great big proteins that are going to fold up and be too tight. And how is ammonium tartrate doing this? Come on, Mike, I need the help. [laughter]

GRAYSON: Well, don't ask me. You're looking at the wrong fellow, let me tell you. [laughter]

McLAFFERTY: And one of the, one of the reviewers wanted the paper turned down because I hadn't explained how ammonium tartrate worked. "Well, ammonium tartrate, that doesn't make any sense. He has to explain this." And, of course, I think you and I believe that a good result is really important, and if you can explain it well that's good.

GRAYSON: Yeah. Really amazing.

McLAFFERTY: Fifty percent more cleavage.

GRAYSON: Wow.

McLAFFERTY: And so the current troops are supposedly trying all sorts of other things that we theorized what the mechanism is. And so far, ammonium tartrate and succinate, and whatever it is are the best.

GRAYSON: Well, amazing. So, this all just came out in this *Science* paper last fall?

McLAFFERTY: October. Yeah.

GRAYSON: Uhm-hmm. Well, I'd like to do one or two things before we wrap up here. We've got a little bit of time.

McLAFFERTY: Shoot.

GRAYSON: One of which is, I'd like to discuss Carsten Reinhardt's book a little bit.

McLAFFERTY: Oh sure. I'd love to.

GRAYSON: This came out last fall and in the book he does a very good job, I think, of tracing the history of the analytical techniques involved..

McLAFFERTY: Yeah, I think he did a great job.

GRAYSON: Primarily Mass Spec and Nuclear Magnetic Resonance. And, he does this by looking at the careers of some pretty influential people, you being one of them, and Klaus and Carl Djerassi being the others in mass spectrometry. And I guess that's okay. It's certainly a way to approach the topic, but I was a little bit disappointed on the mass spec side because he neglected to discuss anything about the impetus or the input of the mass spec meetings, the ASTM (American Society for Testing and Materials, Committee E-14) meetings and the ASMS (American Society for Mass Spectrometry) meetings. It seems to me that while certainly these developments have been led by these individuals; the meetings also bring people together and even in the earliest ASTM meetings, which were really run under more or less a kind of engineering aspect, there was a group of people that were interested in pursuing fundamental aspects of what's happening in the ion source when you do electron ionization on a molecule. He does allude to some work that Sy Meyerson did, which was really important work. But, there's a lot of, like for instance, Frank Field and Joe Franklin, and Burnaby Munson, and he mentioned something about Henry Rosenstock, but only in one reference. There's a whole cadre of people that were working in this area doing various other aspects of it and getting mass spec away from this analogy where you described it as dry ice, banging a piece of dry ice in a bag and just breaking it up into random fragments and then actually developing an understanding of how electron ionization fragments these molecules. So, I guess felt that he kind of maybe missed something that he should have had in the book. I don't know if my criticism makes any sense to you or not?

McLAFFERTY: Well I agree with you that I think he was perceptive in seeing this development of instrumentation as a totally interesting scientific phenomenon that hadn't really been recognized sufficiently, at least in print, with a book that summarized it. By bringing more than one method in together NMR, especially NMR and mass spec, complement each other so beautifully and their developments were competitive but really much more complementary, and the fact that NMR got better helped us to sell our better things. He has quite a bit in there about how much more trouble we had selling mass spec to chemists and biochemists than NMR people did. Though, they had some trouble; but we had a terrible, terrible time. And so I think that he never set out to give proper credit to all of the people—well, he couldn't have given proper credit to all the people in mass spec without making the whole book on mass spec. I think that as in most cases we need to give him credit for what he accomplished (Grayson: Okay.) and for not what he missed. But, as far as what drove this whole thing and what made it possible—I think, my contributions are really very different than both Klaus' and Carl's.

That's because I was interested in very different things than they were. Or, you can put it another way. I'm a much worse organic chemist than either one of them. [laughter] That's another way to put it. But, as far as the meetings are concerned, I totally agree with you. The meetings were the most important thing of all to me. I've only missed one meeting and I've forgotten which one it was, 1959 or something like that. And, the reason for that is that as far as these things that I've done, these come out of ideas that I've collected by knowing all these people, by talking to all these people, and sure I haven't given them enough credit for the ideas I stole from them. [laughter] But, my only defense is that we almost never hid anything. We always talked about it at these meetings also, and I conversed with all of these people. And, well you brought up the LCMS interface thing?

GRAYSON: Uhm-hmm.

McLAFFERTY: I remember meeting after the conference where we would get together with Marvin Vestal and Don Hunt, and lots of these good guys, and we would discuss, "How are we going to do this LCMS coupling?" Well, my method was the first, the first real commercial method that worked. Hewlett Packard did my method. The one with Mike Baldwin that we published in 1973. And that worked. As you say there were all—it worked better than, that's right, its competition was the moving ribbon. And it worked better than the moving ribbon. It was where we did chemical ionization with the LC solvent as the chemical ionization gas. But, of course, when electrospray came along who would ever use our method? At these meetings we had all sorts of fun just kicking the thing around. And, I don't know who had gotten the patents or anything else out of these things. And, we really didn't care. Cornell patented our LCMS interface and got money from Hewlett Packard out of it, which was nice. But mostly—well Electron Capture Dissociation we did not patent. And, I've had people in industry say, "Gee, why didn't you tell us about this?" And, I'd say, "I did. I went to a meeting and I talked about it. You never came up to me and said you wanted me to patent it." I said, "It's better this way." It's really better this way that, as far as my reputation is concerned everybody can use ECD and nobody is trying to stop anybody else." I got away from the question. All sorts of people didn't get credit, in there.

GRAYSON: Yeah. But, I mean the meetings did play an important part in the evolution of mass spectrometry from the beginnings to where it is the analytical technique it is today?

McLAFFERTY: For me personally, there was nothing more important. I went to every single—well, I missed one because I had to do something at Dow, and I've forgotten what it was; it was around 1960. But, I went to every one of these. I went to every single one of the international meetings.

GRAYSON: Those are triennial?

McLAFFERTY: The triennial mass spec meetings. The first one was in London in 1958. I think I told you that's when we went up to see John Beynon, and Al Nier and I were the official U.S. representatives. [laughter] And the British, there was a formal banquet and I think we had to wear tuxes.

GRAYSON: Oh really? [laughter]

McLAFFERTY: Yeah. I had never been to a British formal banquet, and what they do. But anyway, after the toast to the queen there are responses to the toast, and Al Nier was the first and he said some very nice things about being there, and so forth. Well, it turned to the second guy, who, well it doesn't matter. The second guy, what you're really supposed to do is tell stories, tell jokes. John Beynon's the best storyteller. So, he had these marvelous stories. And so, I'm next. And hell, my platitudes that I was supposed to say -- I came up with one joke that got a good response, and I've forgotten what else I did. But I was just totally overwhelmed. I was just a green kid from Nebraska. Anyway, when John Beynon had the meeting in Swansea in 1985, or whatever it was, he said, "Fred, we never get many people from the U.S." And, that was true. And he says, "You have to do something about this." And so, I know Fred Findeis at NSF and I got a grant to support young people to go to that meeting. I think it would support fifty. It was a large number. So I got out applications and everything else, and I had a committee to pick who these would be. And, I think the figures were that the previous time there had been thirty U.S., or we had included Canada, North American people, at the meeting. And this time, although we only said fifty on this thing, there were 180. Because all of these guys that applied, and gals, that applied to us—and we had, I think we had 150 applications—almost all of them went someplace else and got the money. And John Beynon, well he's terrific. You know, he comes up with these great ideas.

GRAYSON: So, the international meeting is important and I suppose it should be, right?

McLAFFERTY: That was, for me it was always an entirely different view of mass spectrometry. They have their perspectives, in those days. I mean, the world's much smaller now and there aren't as big of differences. But in those days, their perspectives were very different than ours. Victor Talroze, I got acquainted with him long, long before that.

GRAYSON: How do you spell Talroze?

McLAFFERTY: T-A-L-R-O-Z-E.

GRAYSON: He's from Russia, right?

McLAFFERTY: He's from the Institute of Chemical Physics in Moscow. And, Victor is the guy who invented CH_5^+, in 1952 and I was at Dow and this publication came out. And since I was doing mass spectrometry my organic friends all over said, "You mass spectrometrists, you're the only ones that can make carbon greater than tetravalent. [laughter] Haven't you heard Fred that carbon's tetravalent?" [laughter] Oh, I got all sorts of garbage. And Victor, of course the Russians had reasons for Americans not to like Russians anyway, and I got acquainted—I've forgotten when I first met Victor—but I always thought it was great stuff and tried, though, I didn't do the theoretical calculations. And so, the kinds of things that he was doing and the kinds of instrumentation he built, you know, that influenced my work tremendously. I felt I could, well I don't know all 6,000 people, but I used to know everybody at the ASMS meetings.

GRAYSON: Well I've given up on that. (McLafferty: Yeah.) It's hard.

McLAFFERTY: But, it was great fun and they had—well I like to tell the story that at one of these international meetings a young man came up to me and asked me about the database, you know, of reference mass spectra, and I said, "Boy, if you have any we'd love to get them," and things like that. I don't know if I wrote him. Anyway, the next meeting three years later this young man comes up to me and he looked vaguely familiar. He says, "Do you remember what I promised you three years ago?" [laughter] And I said, "No, I don't." "Well, here's a floppy disk with 5,000 reference mass spectra." And, you know, I mean that's a nice gift. That's a nice gift, especially when NIST has only collected 193,000 in the last forty years.

GRAYSON: Yeah. So, the conferences then, both of them, the American and the international conferences are—

McLAFFERTY: And Klaus (Biemann) used to go to all of them, but Carl (Djerassi) hardly ever did.

GRAYSON: Well, I don't know. Carl, I don't really consider him to be a mass spec person per se, in the same sense that you and Klaus are. He used the technique and did a lot of good stuff with it, and was very adventuresome, but it's, he's more of an entrepreneur person, as I see him.

McLAFFERTY: He has more money than I do.

GRAYSON: Well, he has more money than a lot of us have

McLAFFERTY: I like Carl.

GRAYSON: Oh yeah. Sure.

McLAFFERTY: I mean, I've always had a wonderful relationship with Carl and he contributed—well, he as much as anybody changed the organic chemists' opinion of mass spectrometry.

GRAYSON: Yeah. That's true. I mean, the stuff he published with Budzikiewicz was, they worked together, didn't they? Budzikiewicz and Djerassi?

McLAFFERTY: Oh, Budzikiewicz. Yeah, those books of theirs were great.

GRAYSON: How do you spell that? I—Budzikiewicz? B—

McLAFFERTY: B-U-D-Z—

GRAYSON: B-U-D-Z-I-K-I-E-W-I-C-Z? Budzikiewicz? Okay. Budzikiewicz.

McLAFFERTY: And, and I'll tell you where that came. Carl, of course, had his own mass spectrometer. He wouldn't let any of the students touch the instrument. My students had to take care of the instrument. And, Budzikiewicz came to me and he says, "You know, I've been trying to find a job. And Carl really wants me to stay here and I would suspect he's not recommending me." And so, I found him a job.

GRAYSON: There you go.

McLAFFERTY: I helped him.

GRAYSON: So, if he was operating the instrument did he have a degree or was he just—

McLAFFERTY: Oh, he had a PhD back in Europe.

GRAYSON: Yeah. Okay.

McLAFFERTY: And was on a postdoc. And so was Dudley Williams.

GRAYSON: Okay.

McLAFFERTY: Oh, Dudley is terrific. And Dudley went to Cambridge, of course, and was a big help in popularizing mass spectrometry. And, he's never gotten the credit he deserves.

GRAYSON: Well, it's hard. I mean, there are so many people that have done so many things it is kind of hard to acknowledge them all. We can't really criticize Reinhardt for not writing the book that we thought he should have written, or writing about what he thought he should have. But, I think the meetings were definitely a big fertile ground where people, ideas came together and people were motivated to try different things and go off and do things that moved the field forward.

McLAFFERTY: Why did I go to every meeting? Because, for very selfish reasons. Well, I enjoyed the people, and I enjoyed stealing their ideas.

GRAYSON: Yeah. A friend of mine once mentioned that he'd gone to work for a pharmaceutical company in England. At that time the meeting rotated, East Coast, Midwest, West Coast. And so, his strategy was, since he had gone to England he would come to the East Coast meeting which was every third year. I saw him, after that arrangement, he said, "This isn't working because too much happens in three years for me to miss this meeting." So, he had to change his strategy.

So, what I'd like to do then is I'd like to just run down the list of names of people that have been in the field and get your impressions or thoughts about them and what you think they contributed. So, one of the first is Sy Meyerson. You've known Sy, I'm sure, for many, many years.

McLAFFERTY: Well, as I told you I knew Sy not only through the Pittsburgh Conference and ASTM but we had this Midwest Spectroscopy meeting when I was at Dow in Midland, from

1950 to 1956, so, that's more than fifty years ago. And Sy was always interested in more than just getting the analytical samples run. He worked with Paul Rylander; a very good organic chemist. I think Paul very soon after they published the paper on the tropylium ion, took a different job in the company and got out of the mass spec lab. Well they had follow-up papers too. I didn't know Paul well, but Sy's the guy I usually talked to. But, I think Paul made a real contribution too. Sy was a fixture at meetings and his contributions in this are significant. I have an early book with chapters in it. This is it, so the people that I thought were doing really good work, I had them write for it. And that's 1963, I think. Here's a chapter by Archie Hood and he worked with, with Jack—

GRAYSON: Sharkey?

McLAFFERTY: No, Jack—

GRAYSON: O'Neal?

McLAFFERTY: O'Neal. And so, this chapter will tell about the direct probe.

GRAYSON: Oh, okay. Yeah, that's the book; *Mass Spectrometry of Organic Ions*.

McLAFFERTY: I tried to get O'Neal to write it and he turned it over to Archie.

GRAYSON: Well, you know, that's a bit of a chore.

McLAFFERTY: Jack, by then, was up rather high in the company. M.J. O'Neal is junior.

GRAYSON: So, the fact that you included Sy in when you put that book together is a testament to how well you thought of his science?

McLAFFERTY: His science was great. And, the tropylium ion, for them to be organic chemists enough to recognize the argument in the fundamental science of organic chemistry, the tropylium ion was a brand new concept, and very important to physical organic chemists. And yet, somebody running samples for a petroleum company, how could that affect them? They picked up on it and knew that their instrumentation could look into that. They showed that the deuteriums were scrambled and that fit the mechanism, tropylium ion mechanism, and I think it

was wonderful. That, as you had said before, in industry it's very hard to find time to do such things, except that back then it was easier. It became harder as the years went by.

GRAYSON: So, what about this fellow Mynard Hamming? Do you remember Mynard Hamming?

McLAFFERTY: He worked for me.

GRAYSON: Oh, okay. Was it at Dow? Or—

McLAFFERTY: He was, I think he was the first guy I hired at Dow.

GRAYSON: Oh, okay.

McLAFFERTY: Yeah. And so, maybe it was 1952 or 1951 that he came to work for me.

GRAYSON: Oh my. Okay.

McLAFFERTY: And after I left Midland to go run this lab he then went to work for somebody in the Pittsburgh area. Not Alcoa, but—and then he moved to Oklahoma.

GRAYSON: Oklahoma.

McLAFFERTY: Yeah. And, I kept track of him all those years. He was; well, I hate to discuss personality. He was a wonderful person and a tremendously hard worker; tremendously committed to things. He didn't seem like an academic type at all, except that he was just so pleased to get to work in mass spectrometry and so appreciative all of these things, and he always came to meetings. And, he then ended up writing books. It was unbelievable to me that he was so dedicated to all these things. He was a real fixture in mass spectrometry. One of my earliest papers on the McLafferty Rearrangement with Isotopic Labeling was due to Mynard. We always called him "Maynard." He had found that Herb Brown at Purdue University, later a Nobel Laureate, had made these esters with deuterium labels on it, and he had gotten the samples from Herb and had run them on his CEC 103. And, I think he was still in Pittsburgh then wherever his company was, and he wrote me about it and he couldn't interpret the spectra. Or, I shouldn't say he couldn't, what he couldn't do. But anyway, he wrote to me for help and

then I was very excited because it fit into the McLafferty Rearrangement beautifully, and we published it in *Applied Spectroscopy*. But anyway as far as the record is concerned it's probably, well it's not the first report of the rearrangement. Happ and Stewart actually did one in 1952 in which they labeled butyric acid. So, I'll take it back. Only, they didn't call it the McLafferty Rearrangement. [laughter] And, do you know why they didn't? Because McLafferty hadn't done anything on it!

GRAYSON: It always helps to have something named after yourself. [laughter] So, what about guys like Frank Field and Joe Franklin. They were gas phase ion chemistry.

McLAFFERTY: Well, they worked together at Humble (Oil and Refining) and were tremendous in the physical chemistry background of things. Joe Franklin always was that way when he went to Rice University and his academic research program there was based on that. And, of course, Humble was wonderful in giving them freedom to do those kind of things. Frank Field had much more to do with chemical ionization than Joe, though they both came out of the same sort of work together. When Frank went to Rockefeller and had an NIH facility there, he really did a great deal for establishing relationships with their scientists—Rockefeller was one of the finest biological research institutions in the world. And, of course, he got Brian Chait to come there and Brian has made it into one of the finest of biological mass spectrometry labs in the world. Even though he's a physicist also. But, Frank Field is a towering figure. He's as tall as Mike Grayson.

GRAYSON: No, he's taller than Mike Grayson. [laughter] If he was standing out in the hall, talking we'd be able to record him in here. [laughter]

McLAFFERTY: And, Frank, well, what you say about meeting Frank, he was always a real presence at meetings and he was always contributing to the discussions in a very positive way. Frank could say things like they were really true. And oh, he made it a lot of fun.

GRAYSON: Yeah. That's not the kind of guy you think you'd want to get into an argument with unless you were on really, really, really firm ground. [laughter]

McLAFFERTY: Well I always loved Frank, and I was on his advisory board there. And, I'm on Brian's too. I guess I've been on the advisory boards of most of these. Not, never Klaus'.

GRAYSON: So, these are the NIH Research Resources.

McLAFFERTY: Cathy Costello's NIH Research Resource.

GRAYSON: Were you very well acquainted with Brian Green, the fellow at AEI?

McLAFFERTY: Yeah, of course I didn't see as much of him. I thought very highly of Brian. I was giving a talk in England and I got a note from Brian saying, "Fred, I want you to stop over to our factory. Our last four-sector instrument is on the line, it's number 110, and I think it would be appropriate if you would be here to see it come off the line." I was giving a talk nearby. Our four-sector that we built here was the first one and then VG got into it and built a very fine instrument. And Klaus I think, had the first of theirs, or almost the first of theirs. They were, I think, a million and a half dollars apiece.

GRAYSON: These things were getting pretty pricey.

McLAFFERTY: And I was totally floored. Maybe he called me. Maybe that was it. He called me. Because, I said, "A hundred and ten at a million and a half apiece?" "Well," he said, "We've sold some of them for less than that." [laughter] And I said, "You sold some of them for more than that too?" "Well, if you put on the automatic martini maker." And so I was, Brian and that group and what you had, the way it evolved through all sorts of companies. But they were very, very clever.

GRAYSON: Brian was behind a lot of that?

McLAFFERTY: And Brian was a big part of that. He sure was. No, a wonderful guy.

GRAYSON: Did you actually have an opportunity to interact with, or meet, or talk to Harold Wiley of CEC?

McLAFFERTY: Yeah. I didn't know him personally as well as others at CEC, but those very first meetings Harold Wiley was always there. He was always a dominant figure. But, there would be, forty people in the mass spec group at the first couple of Pittsburgh Conferences I went to. And, Harold Wiley, he was very nice to me. Of course, I was a potential customer and he had to be nice to me.

GRAYSON: Yeah. Well, my recollections of the earliest ones I went to is that they were obviously a lot smaller, but also the hospitality was flowing kind of freely [laughter] in those

early ASMS meetings. And so, I suppose that people got together and interacted in a really loose way? [laughter]

McLAFFERTY: Yeah. And, oh, their earlier experiences to me were always fascinating, the way CEC did mass spectrometry and so forth. But, I didn't have a real relationship with Harold Wiley.

GRAYSON: Okay. But, you did know, meet him, and interacted with him?

McLAFFERTY: Yeah. I did.

GRAYSON: What about Matsuda in Japan?

McLAFFERTY: The first time I visited his laboratory his instrument had just gotten up to—what was it? It's so hard with FTMS and resolving powers to keep things straight now. This was in the early 1960s and if you want a date on it, it was, I would guess it's—now I'm trying to guess it. I think I was still at Dow. I think, so let's say 1963 or something like that, at Osaka. And, the year before—so you can date it that way—they'd had a typhoon. The University of Osaka was in downtown Osaka, which is near the sea, near the harbor. And, the year before they'd had a typhoon that had put all of his mass spectrometers under six feet of salt water. And to say the least, and he was still mad as all get out.

GRAYSON: I can imagine.

McLAFFERTY: You sort of touch a nerve if you brought it up [Phone ringing] He was a gentleman, absolute gentleman in every way, and he'd moved to this new lab. Anyway he had just gotten his new instrument going, whether it was a year and a half later or something like that, and it had electric sectors, and magnetic sectors, and great spaces in between. It had marvelous resolving power. And, of course, he was running (analyzing) elements. I've forgotten which isotopes and what elements he was looking at. But I got to look at this display where you could see the lines go across the exit slit and I could see this marvelous resolving power. And, I went back to his lab several times. He was just a total gentleman. He designed all on the same sort of magnet—oh no, he had a time-of-flight—but he loved to design instruments. Oh, we had trouble with our RMH-2, and put on a new magnet so that we could get much better performance. Henk Boerboom, from the FOM Institute—B-O-E-R-B-O-O-M, Henk, H-E-N-K, Boerboom, terrific guy—who was great on sector instruments, came over and spent three weeks helping us. Matsuda then did the calculations for us, bless his heart, to see

what's wrong with our magnet and how we can compensate for it. And so all of this was gratis; well he was just a great friend.

GRAYSON: What about Eyring, Henry Eyring?

McLAFFERTY: I didn't know Henry. I'd met him. I knew all his students.

GRAYSON: He made some contributions to the fundamentals in the fragmentation?

McLAFFERTY: Rosenstock, Wahrhaftig, Wallenstein, and Eyring. And, I didn't know Wallenstein well, but Henry Rosenstock and Austin Wahrhaftig I knew extremely well.

GRAYSON: Well, I know Rosenstock went to, and both Wahrhaftig went to ASMS meetings. I know Rosenstock did. He was an officer, I think, at one time.

McLAFFERTY: And Wahrhaftig stayed at the University of Utah, and was crippled but a great mind. But Henry Eyring, his contribution of the Quasi-Equilibrium Theory was a great contribution to making mass spectrometry into real chemistry. You've heard of the Canvas Bag Theory? It's like when you break up dry ice you put it in a canvas bag and hit it with a mallet? Well, that's what most people believed happened in mass spectral fragmentation and the Quasi-Equilibrium Theory helped destroy that simplistic idea.

GRAYSON: When did he do that? Do you know?

McLAFFERTY: In 1955. Something like 1955, I think, was his first publication, or 1954 maybe.

GRAYSON: Burnaby Munson.

McLAFFERTY: Burnaby? Burnaby was at the last meeting. Oh, I knew him. He started coming to the meetings. I met him through Frank Field and Joe Franklin, and I remember when he went to Delaware, and I see him at every meeting and had a nice chat with him at the Seattle meeting. And, he's been a great figure in mass spectrometry and just sort of a stabilizing force, of course. Contribution of chemical ionization was terrific. And, as he switched to academe he helped academics love mass spectrometry.

GRAYSON: Keith Jennings, we mentioned him yesterday.

McLAFFERTY: Oh, Keith's an old friend, a great friend.

GRAYSON: Now, is he, what is he doing? Where is he? Is he in the UK?

McLAFFERTY: He's at Warwick. The university. He's been there a long time. He was head of the department; chair for many years. He did the first collisional activation; what we call "collisional activation," what many people call "collision-induced dissociation." And as I say, I think his publication on it beat ours by several months. But, he was always much more of a physical chemist. His contributions to the understanding of this are tremendous. And, he was always a presence at meetings and an important person for committees. And, he hired Peter Derrick. I knew Peter Derrick very well, because he was a starting assistant professor at LaTrobe University in Melbourne when I had a sabbatical there in 1976. Jim Morrison was chair of the department and Jim was my old friend from way, way back. And Jim was very busy with administrative things, so I'd eat lunch with Peter everyday and we were building this four-sector, or planning this four-sector instrument that was really the first one. And they were building a monster double-focusing instrument. Jim Morrison contributed to the design of it. They were really helpful. But anyway, Peter Derrick then went to Warwick, which has been a big name in mass spectrometry because of Keith, and later Peter Derrick.

GRAYSON: Jean Futrell?

McLAFFERTY: Jean? I was so glad you had Jean Futrell's tandem instrument (in Measuring Mass). I forget about that. I visited Wright Field and saw his instrument, and he actually worked there with—you have to give credit to somebody else that was there, Tom Tiernan. Tom Tiernan was there and got Jean to go there. And so, I think it was really Tom Tiernan and Jean Futrell that did it. But, it certainly was the start of Jean Futrell's career on doing these things. And he went to Delaware and he was much more interested in the fundamentals of the collision process and finding and using tandem instruments to elucidate them. And then, to our surprise, long after many of us moved, he moved to Pacific Northwest Labs, a very important position there but since has stepped down from that. But, he's always contributed to fundamentals in an excellent way, and a great friend, and of course he was president of ASMS.

GRAYSON: How about John Fenn?

McLAFFERTY: Oh, John. He's great.

GRAYSON: Did you know John until recently?

McLAFFERTY: Oh, I knew him; well, I can't say that I really knew him before he talked about electrospray at ASMS. But, I immediately forced myself to know him. [laughter] But, as I say he, he contributed greatly to our first use of electrospray on the FTMS, and he was totally helpful. Craig Whitehouse, his student that set up a company for electrospray, Craig Whitehouse. He let Craig tell me everything, and John told me everything. I think one reason that I kind of hit it off with John is that I knew Malcolm Dole from way back. I gave a seminar when I was first at Purdue, oh let's say 1966 or something like that, at Northwestern. Malcolm Dole had this crazy idea and was having trouble getting funding from NIH. And, I wrote to NIH. I said, "This is the greatest idea since sliced bread." And, of course, you said, "Why did he have trouble?" Well, the reason he had trouble on his Bendix time-of-flight -- and he electrosprayed 100,000 molecular weight polystyrene -- is how do you detect the ions? They're going so slowly when they're of that mass that they don't knock out any secondary electrons. And so he had to do this all with a Faraday cup. He got data that showed he had things but the signal-to-noise was poor. I kept track of Malcolm and used to see him in meetings too. And, of course, John Fenn gives Malcolm Dole lots of credit also.

GRAYSON: Dole didn't go to ASMS meetings did he?

McLAFFERTY: Oh, I think he was at one or two of the meetings. Anyway, I used to see him at meetings. I don't know if he was at ASMS. He could have been at other places.

GRAYSON: Graham Cooks.

McLAFFERTY: Oh, Graham Cooks. Oh, Graham I loved. He was a student of Dudley Williams. I met him through Dudley. And, he went to Kansas State. I knew him there. I think I had something to do with him even at Kansas State. He was very good at talking to people and a very fun guy to talk to; full of ideas. And, of course, when he went to Purdue I had to know him. [laughter]

GRAYSON: Oh yeah. Yes.

McLAFFERTY: I had to know him. And he's always been very nice to me. I tried very hard to get my stuff away from Purdue. I really didn't think I'd left any bad feelings but Graham

Cooks always worked very hard to make sure that I knew that there weren't any bad feelings back at Purdue. I've tried very hard on getting honors for Graham and things like that. I certainly think he's—

GRAYSON: He's a very productive scientist?

McLAFFERTY: Very. One of our proudest products.

GRAYSON: Marvin Vestal?

McLAFFERTY: Oh, Marvin. Oh, he went to Purdue.

GRAYSON: Oh, did he?

McLAFFERTY: Yeah. He got a masters degree from Purdue. Marvin actually worked on the Quasi-Equilibrium Theory.

GRAYSON: Oh, okay.

McLAFFERTY: Early on. With Henry Rosenstock. And, I think that's what he did his masters degree at Purdue on. Though I can't remember just who he did it under. This was before I was at Purdue, because he didn't work for me or anything like that. But, then when he went to Houston—oh, he was at Utah. . He worked on the Quasi-Equilibrium Theory, I believe with Austin Wahrhaftig, for his PhD at the University of Utah. He was always doing something different with instruments. And in fact, what was it when one of the early competitions of how we were going to do LCMS he had instruments in Texas? He was doing it one way on the time-of-flight and another way on the quadrupole. And then of course he started his own company down there and he had instruments galore. Of course when he finally sold his company he moved to Framingham, Massachusetts. Well, that's where I had my Dow lab. [laughter]

GRAYSON: Oh my. That's crazy.

McLAFFERTY: And, he hired one of my good students, Paul Danis.

GRAYSON: What was the name of the student?

McLAFFERTY: Paul Danis, D-A-N-I-S. Paul was the guy that did neutralization reionization with me. That's a method we didn't talk about. But, I love methods and that's one that's still used for fundamental studies by people like Helmut Schwartz, and Frank Turecek and Chrys Wesdemiotis, who both worked on it with me.

GRAYSON: Frank?

McLAFFERTY: Turecek, T-U-R-E-C-E-K.

GRAYSON: E-C-K?

McLAFFERTY: E-K. Actually, it was Frantisek, F-R-A-N-T-I-S-E-K.

GRAYSON: Frantisek.

McLAFFERTY: Frantisek. (Grayson: Okay.) Everybody called him Frank. He was from Czechoslovakia.

GRAYSON: And the other was?

McLAFFERTY: Chrys Wesdemiotis?

GRAYSON: Yeah. Wes, how do you?

McLAFFERTY: W-E-S-D-E-M-I-O-T-I-S.

GRAYSON: Chrys? Chrys Wesdemiotis?

McLAFFERTY: Yeah. He's at the University of Akron, and Frank's at Washington, the University, the other University of Washington.

GRAYSON: Uhm-hmm. Okay. There's Washington State and (McLafferty: No.) well actually there's a lot of schools named Washington.

McLAFFERTY: You're Wash U and—

GRAYSON: We're Washington University. They're University of Washington.

McLAFFERTY: He's University of Washington.

GRAYSON: Okay.

McLAFFERTY: In Seattle.

GRAYSON: Yeah. This Washington State, or Washington University gets confused with a lot of schools. (McLafferty: Yeah.) Al Nier?

McLAFFERTY: Al Nier. Well, I already told you about Al. I knew Al early on; I certainly knew him in 1958, that time we went to the meeting together. I went to his seventieth, I helped organize his seventieth birthday party, his seventy-fifth, and his eightieth. [laughter]

GRAYSON: Okay.

McLAFFERTY: And, oh let's see, I remember Mike Gross having a symposium in Lincoln, which he got Al Nier down to. Of course, he was from Minnesota. Mike's from Minnesota, and I was there. And, oh we crossed paths for many years.

GRAYSON: Let's see. I've got another name here. Oh, Jack Sharkey?

McLAFFERTY: Oh, Jack, yeah. Well I met Jack at the first Pittsburgh Conference I went to in 1952. He and Gus Friedel were at the U.S. Bureau of Mines. A.J. Sharkey, Jr. And, Gus was the group leader there, and Jack worked for him. They were different people, but they were a wonderful team. Jack then went on to the University of Pittsburgh where he was for many years after, I think he retired from the Bureau of Mines.

GRAYSON: Yeah, I think so.

McLAFFERTY: And, but you can do that in the government pretty early. He was very valuable at the University of Pittsburgh. I think I told you yesterday they were some of the biggest early supporters of doing research on mass spectral fragmentations. They did some of the best early work on that of anybody. Gus died rather early compared to Jack. And so, we know more about Jack. But, yeah, great guy.

GRAYSON: So, a lot of that fundamental work came out in those early ASTM meetings that Jack and Gus had done.

McLAFFERTY: That's where it got started. And, it was a wonderful inspiration on me to talk to these guys.

GRAYSON: So, I have an interesting question to propose to you. If you had to pick out five, the names of five people for oral histories to be conducted in mass spectrometry, name five people that you think would be good to have their perspective on the field.

McLAFFERTY: Well, I think you should pick the old ones, of course. [laughter] Get their perspective on the field.

GRAYSON: I mean I know, Sy Meyerson we have. I've got an oral history on Sy.

McLAFFERTY: Yeah, you told me that. I'm trying to think of some people that—I think you should think internationally and so, of the people, you only have U.S. people. John Beynon absolutely would be at the top of the list. Jim Morrison knows an awful lot about the earlier history and he's still alive, and we had a Christmas card from him, though interviewing him will be tougher since he's in Australia.

GRAYSON: Australia? Yeah. Well.

McLAFFERTY: But I, but you asked, you didn't ask for limitations.

GRAYSON: Right.

McLAFFERTY: He is a very clever guy who was involved in many of the developments, and is still alive. Vlada Hanus would give you interesting things except he's a very modest person. Well, put him down. He hasn't nearly as broad experience as the other two, because he's always loved Czechoslovakia and doesn't leave and go to meetings. I have to always go to see him. But—

GRAYSON: How do you spell his last name?

McLAFFERTY: H-A-N-U-S.

GRAYSON: H-A-N-U-S? Vlada? (McLafferty: Yes.) Okay.

McLAFFERTY: But, I've tried to think of people that are still alive that were around for these early—Alex Harrison.

GRAYSON: He's in Canada? Or is he—

McLAFFERTY: He's in Toronto. The University of Toronto all these years. So many of these people are gone.

GRAYSON: Yeah.

McLAFFERTY: Dudley Williams is a guy that wouldn't be at the top of the list but certainly is highly intelligent and has a perspective on things that is not as prejudiced as people as me.

GRAYSON: Uhm-hmm. Oh, okay. [laughter]

McLAFFERTY: Who are some of the better old people still around?

GRAYSON: What about Frank Field?

McLAFFERTY: Oh, Frank's fine. Yeah. Frank would be fine.

GRAYSON: He's kind of interesting. A while back, maybe five years ago or so, we had a local mass spec discussion group that I chair, and I thought, it would be kind of interesting to have Frank come over and talk about mass spec to the discussion group.

McLAFFERTY: Yeah, he's not so far away.

GRAYSON: No. But, he more or less divorced himself from all of the stuff and he doesn't even want to; he wasn't interested. So, I don't know if he'd be interested in an oral history interview or not, but it would be interesting to try. I mean, if he doesn't want to do it that's fine.

McLAFFERTY: I would put him ahead of Burnaby Munson.

GRAYSON: Well, that's a good list there, mostly international. Maybe we could get someone to do those interviews and it would be really a good idea. So, I guess this is your time to wax poetic. What words of wisdom would you want to wish upon posterity? [laughter] We're talking about sixty-some-odd years of experience in science and mass spectrometry research?

McLAFFERTY: Well, I guess it's just that research is totally fun. [laughter] It's a wonderful hobby. And the people part of it was a huge bonus; interacting with most people I found to be open. I have a guilty conscience that I haven't given nearly enough credit to all the people I talked to, in keeping track of who deserves credit for ideas. But my only defense is I hope I haven't held back in discussing things. Oh, when Mike Bowers got the ASMS award a couple of years ago he mentioned the ASMS meeting in Honolulu and something about his most memorable moment for ASMS was that we had a huge argument. Keith Jennings was the chair. And Keith, bless his heart, kept everything under control [laughter] with his British wisdom. We were just at it, hammer and tongs, about these things. Mike I consider to be a great friend. He's never turned down my *JACS* manuscripts because I was such a nasty guy arguing with him. [laughter] Well, he deserves to do that. People came up to me afterwards and thought that we hated each other. "We don't hate each other." [laughter] Yet, the whole conference was livened up and Keith Jennings used to talk about it afterwards and tease us about this. As far as I'm concerned, that's what makes science really work, is that we both thought we were right.

GRAYSON: Stuck to your guns?

McLAFFERTY: Yeah. And the work we were arguing about was with Myung Kim, who's, who's a professor of chemistry at Seoul National University in Korea. A postdoc I had a few years ago from Seoul National University said, "Oh, Myung Kim did a postdoc with you. He's the smartest person at the university." He actually came to the Texas ASMS meeting and he's still doing nice work. But I said to him, "You know, I still think your work was right with Mike." And, and of course when Mike talks to me about it he says, "Well, I'm glad that you finally admit I'm right, Fred." [laughter] And, I have a little trouble remembering what we argued about. So, that's what I think is great, as long as you can have fun in research and, make it a hobby with the other people that you work with.

GRAYSON: So, that would be your recommendation to generations to come is to—

McLAFFERTY: Yeah. Argue with Mike Bowers. [laughter]

GRAYSON: Argue with Mike Bowers? To have fun with the work? Okay.

McLAFFERTY: Yeah.

GRAYSON: Well, I think we've covered just about everything that I want to cover. And, maybe a little bit more.

McLAFFERTY: That's great.

GRAYSON: And, I hope that we've covered everything you want to cover.

[END OF AUDIO, FILE 2.1]

[END OF INTERVIEW]

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