SCIENCE HISTORY INSTITUTE

CHRISTIE G. ENKE

Transcript of an Interview
Conducted by

Sarah L. Hunter-Lascoskie and Hilary L. Domush

at

The home of Christie G. Enke
Placitas, New Mexico

on

16 and 17 April 2013

(With Subsequent Corrections and Additions)

ACKNOWLEDGMENT

This oral history is one in a series initiated by the Science History Institute 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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Science History Institute

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CHRISTIE G. ENKE

1933	Born in Minneapolis, Minnesota, on 8 July

Education

1955	BA, Chemistry, Principia College
1959	PhD, Chemistry, University of Illinois, Urbana-Champaign

Professional Experience

	Princeton University, Princeton, New Jersey
1959-1961	Instructor in Chemistry
1961-1966	Assistant Professor of Chemistry
	Michigan State University, East Lansing, Michigan
1966-1972	Associate Professor of Chemistry
1972-1994	Professor of Chemistry
1994-2013	Professor Emeritus
	University of New Mexico, Albuquerque, New Mexico
1994-2006	Professor of Chemistry
2006-present	Professor Emeritus
	Indiana University
2008-2014	Adjunct Professor of Chemistry

Honors

1957-1958	DuPont Teaching Assistant, University of Illinois
1964-1969	Fellow, Alfred P. Sloan Foundation
1974	ACS Award for Chemical Instrumentation, American Chemical Society
1981	Fellow, American Association for the Advancement of Science
1982	Senior Research Fellow, Sigma Xi, Michigan State University Chapter
1989	ACS Award for Computers in Chemistry, American Chemical Society
1992	Best Paper Award, Sigma XI, Midland Chapter
1992	Distinguished Faculty Award, Michigan State University
1993	Distinguished Contribution in Mass Spectrometry (with Richard A. Yost), American Society for Mass Spectrometry
2003	J. Calvin Giddings Award for Excellence in Education, American Chemical Society Division in Analytical Chemistry

2011	Fellow, American Chemical Society
2011	ACS Award in Analytical Chemistry, American Chemical Society

ABSTRACT

Christie G. Enke was born in Minneapolis, Minnesota, in 1933. While attending high school, he worked as a stage manager and took electronics classes at Dunwoody Industrial Institute (now Dunwoody College of Technology). He attended Principia College and there became interested in electrochemistry through an electroplating project he worked on between his junior and senior years. He selected the University of Illinois Urbana-Champaign for graduate school to work with Herbert A. Laitinen. There he assisted in developing Howard V. Malmstadt's electronics course, which he loved, and worked on his thesis with Laitinen about electrolytic formation and dissolution of oxide films on platinum. Upon graduation, Enke took a teaching job at Princeton University, but soon realized he would not be there for long since the chemistry department was phasing out analytical chemistry. However, he stayed busy at Princeton and also completed some consulting work with Standard Oil and American Cyanamid on the side. After six years at Princeton, he transitioned to Michigan State University.

At Michigan State, Enke got involved in automated instrumentation and later mass spectrometry when he had problems finding good academic jobs for his electrochemistry graduates. On the way home from a conference, Enke started talking with his student, Richard A. Yost, about a problem, and they came up with a plan to build a tandem quadrupole instrument. They started collaborating with James D. Morrison to achieve efficient ion fragmentation between the quadrupole mass analyzers. The triple quadrupole mass spectrometer was patented, and its success quickly spread through the mass spectrometry community. From there, due to curious results of a student's experiment using electrospray with a metal ion, crown ether mixture, Enke got interested in the electrospray process. He then developed the equilibrium partition model of ion evaporation. By that time, he had transitioned to University of New Mexico after retiring from Michigan State. At UNM, he continued his work on tandem time-of-flight mass spectrometry and invented the technique of distance-of-flight mass spectrometry. Near the end of the interview, Enke talks about his interest in natural philosophy stimulated by *Zen and the Art of Motorcycle Maintenance*, and his thoughts on facts and explanations, explanatory versus empirical lobes of the scientific method, re-envisioning the chemistry curriculum, and the field of mass spectrometry.

INTERVIEWER

Sarah L. Hunter-Lascoskie earned a BA in history at the University of Pennsylvania and an MA in public history at Temple University. Her research has focused on the ways in which historical narratives are created, shaped, and presented to diverse groups. Before Sarah joined CHF, she was the Peregrine Arts Samuel S. Fels research intern and Hidden City project coordinator. Sarah worked both in the Center for Oral History and the Institute for Research at CHF and led projects that connected oral history and public history, producing a number of online exhibits that used oral histories, archival collections, and other materials. She also contributed to CHF's Periodic Tabloid and Distillations.

Hilary L. Domush was a Program Associate in the Center for Oral History at CHF from 2007–2015. Previously, she earned a BS in chemistry from Bates College in Lewiston, Maine in 2003. She then completed an MS in chemistry and an MA in history of science both from the University of Wisconsin-Madison. Her graduate work in the history of science focused on early nineteenth-century chemistry in the city of Edinburgh, while her work in the chemistry was in a total synthesis laboratory. At CHF, she worked on projects such as the Pew Biomedical Scholars, Women in Chemistry, Atmospheric Science, and Catalysis.

ABOUT THIS TRANSCRIPT

The Center for Oral History, Science History Institute, is committed both to preserving the recording of each oral history interview in our collection and to enhancing research use of the interviews by preparing carefully edited transcripts of those recordings. The preparation of interview transcripts begins with the creation of a verbatim typescript of the recording and proceeds through review and editing by staff of the Center; interviewees also review the typescript and can request additions, deletions, or that sections be sealed for specified periods of time. We have established guidelines to help us maintain fidelity to the language and meaning of each recorded interview while making minor editorial adjustments for clarity and readability. Wherever possible, we supply the full names of people, organizations, or geographical locations mentioned during the interview. We add footnotes to the transcript to provide full citations for any publications that are discussed, to point to extant oral history interviews, and to clear up misstatements or provide context for ambiguous references in the transcript. We use brackets to indicate the addition of material that was not in the audio, and bracketed ellipses to indicate the deletion of recorded material. The transcript also includes time stamps at five-minute intervals. We omit without noting most instances of verbal crutches and all instances of nonlexical utterances. We also make small grammatical corrections where necessary to communicate interview participants' meaning. Finally, staff of the Center create the abstract, chronology, and table of contents. With the availability of online full-text searching of our transcripts, the Center for Oral History opted to discontinue the practice of preparing a back-of-the-book index for each oral history transcript in 2020.

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INTERVIEWER: **Sarah L. Hunter-Lascoskie**
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LOCATION: **The home of Christie G. Enke**
Placitas, New Mexico

DATE: **16 April 2013**

HUNTER-LASCOSKIE: Okay. Today is April 16, 2013. I am Sarah [L.] Hunter-Lascoskie, here with Hilary [L.] Domush. We're interviewing Chris [Christie G.] Enke here at his house in New Mexico. And as we mentioned, we usually start at the beginning of your life. And we know that you were born in Minneapolis, [Minnesota], in 1933, but other than that, what was it like growing up? What was your childhood like in Minneapolis?

ENKE: I was a Depression baby because the [Great] Depression was going on [in 1933]. I interrupted my parents' college ambitions by happening. And so my dad, who was going to be an electrical engineer, ended up repairing radios and then got a job at Dunwoody Industrial Institute [now Dunwoody College of Technology] in Minneapolis, a trade school. And I lived in various houses around Minneapolis. And I guess, you know, it was just a, sort of, middle-class family—not upper middle-class, definitely. And my parents hadn't finished college.

Growing up in Minneapolis was wonderful. I remember walking everywhere. I remember just going out on my own. I was an only child. I had a lot of friends, spent a lot of time at the library, loved reading, had tons of hobbies, and also got interested in electronics, I think through my dad. And while I was [in high school], I took electronics courses at Dunwoody because sometimes he'd be teaching night classes, and I would go there and take a class, and then we'd go home together. And so I got an early training in electronics and loved science. Shy, but I did well in science classes, not so well in non-science classes. I had plenty of interests and friends. I was on the stage crew, was in fact stage manager in high school, which was fun. That was a lot of fun.

DOMUSH: So did you build—as stage manager—did you have to help build sets and things like that?

ENKE: Yeah, build sets and set things up. [. . .] [They had regular sets in the basement, and this was done like a regular stage. You screwed the sets to the floor. That was all pretty amazing. I don't think high school students anymore have this kind of creative activity], and yeah. It was very interesting. I played the piano. And I think I've had really a very full life.

HUNTER-LASCOSKIE: It sounds really busy and fun.

ENKE: It was busy and fun. I'm still busy and [having] fun. There's always more to do than there is time for, and I enjoy that.

DOMUSH: What did your mom do? Did she work outside of the home?

ENKE: My mom was a clerk at Dayton's, which has become, sort of, branched off into Target on the one hand and Marshal Fields on the other. That was the big department store. [Then, of course, the Second World War came along, and then my dad got involved in technical developments during the Second World War, particularly bomb sights for bombers and things like that.] [. . .] He wasn't able to serve. [. . .] And then he went overseas a couple of times to set up electronic training institutes in Korea, which didn't work because then the Korean War started, and Indonesia. But by that time, I was off in college. **<T: 05 min>**

DOMUSH: So when it came time—when you were in high school—when it came time to figure out about going to college, was it, kind of, a foregone conclusion that you would go to college? As you said, you interrupted your parents' college ambition by showing up. So was it expected that you would go to college?

ENKE: Yes. It was expected that I would go to college. I think I've been driven by a desire to be a teacher as long as I can remember. And so, when I was in the high school environment, I realized you had to go to college in order to teach high school, which was, of course, what I wanted to do. And then I got to college, I realized you had to go to graduate school in order to teach college, which is, of course, what I wanted to do. So yeah, that just kept pushing the education along. Really was driven by my interest in teaching, and that's never changed.

DOMUSH: Were there any teachers in high school that you maybe wanted to model yourself after, or were particularly interesting or inspiring, or . . . ?

ENKE: Yeah. I went to Central High School. It was not in the best neighborhood in town at that time, and it didn't . . . the people there didn't have the highest ambitions, I think, for the students that they had. So, nevertheless, there were a few standout teachers. I don't remember my chemistry teacher for inspiring me as much as scaring me. [laughter] But I think that I remember a geometry teacher. And there were . . . I took a, sort of, special college-oriented literature and history series, and they were taught by a couple of women I still remember who were very encouraging, extremely encouraging, to me. [. . .]

The chemistry teacher was doing a section on oxygen—on hydrogen, sorry. And so she had a Kipp generator going up there, and she was making these little bottles of hydrogen, and she asked people what would happen if hydrogen burned. And I figured out that would be hydrogen oxide. Oh my gosh, that's water. So, you know, everybody laughed when I suggested that, because then the water'd put the fire out. So then I said, "Well, you could hold a glass plate over the flame and see if it got condensation."

So she said, "Well, okay why don't you come down and do that?" So she wants me to burn hydrogen at the lecture desk. [laughter] So we ended up with an explosion, which, fortunately, just drove the cork with its [glass tube], which I put in there to mount the flame on, up into the ceiling. [laughter] Anyway, interesting introduction to chemistry.

DOMUSH: A memorable one at least.

ENKE: I wasn't particularly attracted to chemistry in high school. I loved physics, math. And it really wasn't until my junior year in college that I decided where I was going to head.

HUNTER-LASCOSKIE: When it came time to pick a college, what were you looking for? You know, did your parents have any input in that decision?

ENKE: Yeah, they did, but, you know, I just was talking with a kid that I grew up with out in the country, and he went to Hopkins High School and Hopkins High School had a lot of bright people. And so he went to Yale [University]. Nobody ever, ever, ever in the world [of Central High School] thought of the possibility of going out of state, much less some prestigious place. I mean, it was just, sort of, pretty much out of the question. I was planning to go to the University of Minnesota **<T: 10 min>** just like all good Minneapolis kids do or did. And then I went to a presentation that Principia College gave and ended up just being dazzled by it. A delightful little campus, you know, with [beautiful buildings and grounds]—just absolutely picture perfect on the bluffs of the Mississippi River, you know, in southern Illinois.

And I was pretty idealistic. And Principia College is a school for Christian Scientists, and I was very deeply into Christian Science. So I thought this would be just perfect. But you see, they didn't have a program in electrical engineering, so what I could do is [take a sort of] pre-engineering program for a couple of years and then transfer, and that's what I was going to do. So off I went. I got a scholarship, which made it financially possible and got jobs while I was there, and that was the choice. It wasn't made on any solid academic background concerns or anything like that.

DOMUSH: Were your parents concerned about you going so far away? I mean, you had been planning to stay in Minneapolis and go to University of Minnesota.

ENKE: Yeah. I don't think so. I don't think so. I think they were happy for me.

DOMUSH: How far away was it for you to get Principia?

ENKE: Well, so it's in Illinois just across the river from St. Louis, [Missouri].

DOMUSH: Okay. So a little ways. A little ways away.

ENKE: It's a ways. It's definitely a ways. It's two train rides.

HUNTER-LASCOSKIE: So when you go there, you know, what was the program like? Was it what you were expecting?

ENKE: I had no idea what to expect. [. . .] It was, sort of, on what was called "the Dartmouth plan" at that time.[1] You took three courses solid. You don't do that anymore, right? We take two-credit courses and three-credit courses and load them up until they get to fifteen. And these were all five-credit courses, basically. And it was on the quarter system, so you had ten, twelve weeks of five days a week of each of your three courses. And then I had . . . because in order to do the pre-engineering thing, I had a half course on top of that of drafting or something that was related to engineering. And so chemistry, physics, and math. And yeah. Solid.

DOMUSH: Was it interesting once you got there? Were the classes—I mean were they . . . ?

ENKE: Small college, four hundred students total. I had more people in my graduating class in high school than there were in the whole college. Everybody knew everybody. It was a lovely environment, really. It really was.

DOMUSH: So how many people were in your chemistry classes, or in your drafting class?

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[1] The Dartmouth Plan, implemented at Dartmouth College in 1973, is a scheduling system that divides the calendar year into four academic terms. Under the Dartmouth Plan, students typically enroll in three courses each quarter.

ENKE: I was in the drafting class. And alone.

DOMUSH: Alone? Okay. [laughter]

ENKE: I *was* the drafting class, as I might have put it. Yes, no one else was doing that. And I think they were surprised they even had it in the catalog, actually. Classes were twenty people.

DOMUSH: You said it was a college that was a Christian Scientists' college.

ENKE: Yes.

DOMUSH: Did you have to take also classes on religion, or anything related to that?

ENKE: Actually no. They didn't teach Christian Science. It was just an environment which was totally Christian Scientists, even the faculty.

DOMUSH: Oh wow.

ENKE: Yeah, still is.

HUNTER-LASCOSKIE: Now, you said your intention had been to transfer.

ENKE: That didn't happen, did it?

HUNTER-LASCOSKIE: Yes. What happened that made you stay?

ENKE: Well, a couple things happened. One was that after my [sophomore] year, I was so saturated with chemistry, physics, and math—and I love science and knew that's what I wanted to do—but suddenly here I was in a liberal arts college getting [few] liberal arts. **<T: 15 min>** And, of course, I met somebody. So my [junior] year, I didn't take any science classes at all. None. Maybe math. Maybe math. I don't remember now exactly, but just said, "I've had it."

Somehow or another I had this little revolt I guess, and I took comparative literature and philosophy, and I just did what I should have done, actually, in that environment. Thank goodness. But that meant I didn't take organic chemistry, and so when I got back on the science track my junior year, well, then I was no longer fit to go the University of Minnesota and transfer in as a junior, and I'd, kind of, lost my interest in engineering by that time. I realized there was something really great in the solid sciences. And so I stayed at Principia and graduated.

DOMUSH: So then junior year, did you have to kind of double up to finish the sciences thing?

ENKE: Not really. I never did take organic chemistry—well, until I got to graduate school. So, yeah, they didn't know quite what to do with me about that. What was my degree exactly? Oh, I guess I got a bachelor of arts. But I got to graduate school.

DOMUSH: You said, though, that junior year in college was when you really decided that chemistry, as opposed to some of the other sciences, or as opposed to the engineering, was what you wanted.

ENKE: Yeah. Have you ever thought back about those points in your life when you've made decisions that made your life go this way or that way? Well, I've thought a lot about those, and I've thought a lot about the reasons that I had why I made those decisions. Hardly any of the reasons that I had at the time for making the decisions had anything to do with whether the decision was good or bad. Try that sometime. It's really, really interesting. And then you think, "Why did I decide to do that? Well, wasn't that irrelevant?" You know? [. . .] So at any rate, I knew that I wanted to teach. Again, that was the overriding thing. I was going to teach college. "Oh, if I could come to a nice little college like this and teach, wouldn't that be wonderful?" So, and I was going to teach physics, or chemistry, or math.

But by that time, I'd really, kind of, gotten a little bite of research and really thought that was fun. And not only was it fun, it was enlivening. It made the science come alive. And math by that time was beginning to get a little abstract. And physics—this was the time now when it seemed like the only thing that was really going on in physics was nuclear. The physics prof there was doing a little research, and what he was doing was he was analyzing plates that had been exposed to cosmic rays from a balloon, [looking for tracks that would indicate new particles], and that was all he could do. And he had done his graduate work where they had an accelerator and that was [where it was at that time]. And I thought, "I don't think I want to have to be where there's an accelerator in order to do my work."

So I thought, well, chemistry will be it. Then in my . . . towards the end of my—I think it was—yeah, actually, beginning of my junior year. No, it was the summer between my junior and senior year, my chemistry teacher prof hired me to work over the summer on an electroplating project that he had. And all of a sudden, here was chemistry and electronics. And it just came smack together, and I said, “Yes, this is it. I’m an electrochemist.” So that’s what I did. And then, yeah, that got me involved in electrochemistry, and I thought that’s just the perfect match.

DOMUSH: Were you able in the junior **<T: 20 min>** or senior year to continue to take any of those kind of liberal arts courses that you’d done sophomore year?

ENKE: Oh yes. Yes, I did continue. I took two years of Spanish, and, you know, I did continue to take liberal arts classes.

DOMUSH: What about some of the hobbies that you talked about in high school? Were you able to continue any of those?

ENKE: Not really. I tried to continue the piano, but there really wasn’t space and time for that. So that fell away. Photography, I kept an interest in. [. . .] But, no. [I had a small business upgrading the professor’s console radio/phonographs to play the new LP record that were just coming out.]

HUNTER-LASCOSKIE: Now obviously, you know, research is also something you want to do, so you’re getting started thinking about graduate school. And we know that you stayed in Illinois, but was . . . you know, were there particular professors or types of research or things you were looking at to, kind, of push you to [University of] Illinois [Urbana-Champaign]?

ENKE: No. Yes and no. I shouldn’t say no. There definitely was. I had an opportunity to, sort of, interview at MIT [Massachusetts Institute of Technology], and I understood [from] talking with the students there, they didn’t get enough money for their teaching assistantship to support them. And my family had already sacrificed quite enough to get me as far as I was. And [I] understood that there were places where you could go and be self-supporting. And meantime, my chemistry prof had, in fact, just completed his doctorate working with an inorganic chemist and an analytical chemist at the University of Illinois. That was Herb [Herbert A.] Laitinen, who was the analytical chemist, [and Therald Moeller, the inorganic chemist]. And he really thought that it would be good for me to go and work with Herb Laitinen. So I guess he primed Herb to [. . .] set me up for that.

And so I interviewed at Illinois as well. I applied a few other places. I applied at Harvard [University], and they referred me to Radcliffe [College] because of my name. [laughter] This happens all the time. And so, just for stupid little reasons, you know, I ended up eliminating one after the other. When I went to interview at Illinois, Herb Laitinen just took me in.

DOMUSH: Now did you—sorry—did you apply to University of Minnesota? Did you consider going back home?

ENKE: I don't remember. I don't remember now whether I did or not. Yeah. Could have done.

DOMUSH: But by the time you got to visit Illinois, you met Herb Laitinen and he…

ENKE: I met Herb Laitinen, and he offered me a summer job before starting graduate school. And that just made life easy. All of a sudden, it didn't matter what else happened. That's what I was going to do. Yeah, so—and that was really fun. That was really fun. I was assigned to one of his graduate students [Benjamin Mosier], who was trying to measure the double layer capacitance of a mercury drop. And the drop is growing and what's happening is organic chemicals in the solution are being absorbed on that drop, so that's changing the double layer capacitance because it's putting in more dielectric between the electrode and the solution. And so the question was how to catch this. I devised an impedance measurement device that you could tune up that would come to balance at various times in the drop growth. He could catch it at any time that he wanted to and plot the whole thing out. It made his thesis, really, and it was fun for me because it was electronics. And so that was . . . suddenly, I was just in my milieu. And I took German **<T: 25 min>** probably. In those days, you needed to be able to read technical German and French.

DOMUSH: Oh wow, and French.

ENKE: And French, right. And of course, I'd taken Spanish, so here I am. Yeah. So I took—they had courses particularly for people who just needed to know how to read technical German and French. So I started that. I was taking these language courses and preparing for exams. And now, of course, I don't have organic chemistry. My physical chemistry wasn't good enough. I was taking . . . when I started there, I was taking remedial physical chemistry, undergraduate organic chemistry, and I had to have a full load of graduate classes.

HUNTER-LASCOSKIE: In addition to…

ENKE: In addition to that, in order to avoid . . . in order to keep my deferment from the Korean War. And I needed to teach in order to support myself. I was busy. It was probably the hardest year, academically, I've ever had.

DOMUSH: Was the deferment from the Korean War something that was easy to acquire as long as you were taking the full load of courses? How did that work?

ENKE: There was an exam that you had to take that showed that you were fit for graduate work, that you were on an intellectual level that you could do graduate work. Yeah. But if you could do that, and you maintained a total full load of graduate work, then you could be deferred.

HUNTER-LASCOSKIE: So in the midst of this very busy and difficult first year, how were you finding the teaching part of it?

ENKE: Oh gosh. I loved teaching.

DOMUSH: Even right away?

ENKE: Oh, absolutely right away. Yeah, it was teaching the quantitative analysis lab. I did that for a year. Then Howard [V.] Malmstadt had tried to teach—Howard Malmstadt was a young faculty member at the University of Illinois at that time, and he had also an interest in electronics. He had been in the Navy. He'd gone to Navy radar school, and he'd come back, and got his PhD at [University of] Wisconsin doing conductivity, conductometric titrations, actually, which nobody does anymore, using high frequency signals instead of DC, or low frequency AC.[2]

And so he was electronically oriented, and he could see that electronics were becoming really important. Instrumentation was just beginning to come on, and he had this incredible vision about chemical instrumentation. He wanted to teach electronics to the graduate students. And he tried a lecture class, and it just didn't work. And so he realized that he [needed] to have a laboratory, and he needed a teaching assistant, and he needed experiments. So he recruited me to be the teaching assistant for this class and to help work with him to devise some experimental setup that would allow us to teach the electronics. That got me busy because every week we tried something, and it didn't work as well as it ought to, and the next week we had to try something else. It was . . . . I was there day and night, pretty much.

DOMUSH: Did you have any . . . you know, I have friends who are chemistry professors now, and when their department devises a new laboratory for the students, they take a semester or take the summer months to, kind of, plan it all and make sure that they have enough experiments

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[2] H. V. Malmstadt, "High Frequency Titrations" (Ph.D. dissertation, University of Wisconsin, 1950).

or have the prep work done. Did you guys have any lead-time like that where you could kind of . . . ?

ENKE: Not that I remember.

DOMUSH: You just dove right in?

ENKE: No, he was just a dive-right-in kind of person. [. . .] But **<T: 30 min>** we worked it out by the third or fourth week, we had something that was going to carry us through the rest of the term. The typical way to teach electronics those days was to have all the components that you were going to use—the vacuum tubes and the resistors and everything—screwed down to a piece of plywood. And then you would connect these components with wires. It's a little bit hokey in a way because you can only do one thing. And then if you wanted to do another thing, you get another whole board. And that . . . so it wasn't as efficient as what we needed to do. So we ended up creating something that you [did] not have to solder wire anything together in order to make whatever you wanted to make.

DOMUSH: So you worked with Howard Malmstadt, then, throughout your time at Illinois on this . . . in this laboratory.

ENKE: Yes, I did.

DOMUSH: Okay.

ENKE: Right.

DOMUSH: And did you have to do any other teaching? Did you ever go back to teaching the quantitative laboratory?

ENKE: I taught in that class. He was gone a lot. I'd take over the lectures. You know, it was great. Yeah. And I tutored other students. But no, I didn't go back to teaching the quantitative. I taught that electronics—well, I—wait a minute, because that was only taught one term a year. It wasn't taught every term and so, of course, in the terms in between, I was doing something else.

HUNTER-LASCOSKIE: What was the student interest like in this, kind of, new electronics lab and course?

ENKE: That's a nice question, Sarah. I think the students—well, they were graduate students. It was a graduate class. And the student interest was pretty high. It was pretty high. That was at a time, you know, when people wanted to hook recorders up to their experiments so they could get a record of what happened, and sensors were just coming on, oscilloscopes were just becoming something that was a general-purpose laboratory tool. But how do you use them and how do you get your experiment hooked up to these new devices? And I think that people really, really realized that this was where things were going, and this was the way to get there. And that turned out to be true because the course spread, and it was very popular in physics and chemistry departments all over the country.

DOMUSH: Now, when you guys started with this class, and figuring out the laboratory, were other colleges and universities . . . did they have similar classes, or was this really something, kind of, brand new?

ENKE: I think it was pretty new. I think it was, in fact, brand new. In order to really learn electronics at greater depth, I took [electronics classes in the physics department]. But they were just lectures, again. I learned an awful lot, now, about not just practical electronics, but really the fundamentals of electronics. And in order to do that . . . see, and this was the barrier. In order to take the electronics classes and physics, I had to take electricity and magnetism. In order to take electricity and magnetism, I had to take differential equations. And so I had to go through all these prerequisites in order to take two semesters of electronics in the physics department. Now, this is getting to be more than a minor, actually. You know?

And so that was a big barrier. [. . .] I chose the physics department to do this in because double-E [electrical engineering] was even worse, before you could get to anything practical, any useful level things. And so **<T: 35 min>** there really wasn't a mechanism for students who just wanted to use some electronics to get some practical electronics and to get it in a condensed enough form so that it didn't become a whole subtopic for it.

DOMUSH: So then for the laboratory course, did you guys have prerequisites that students needed to take? I mean, did they need differential equations? Did they need . . . ?

ENKE: No. They needed an undergraduate degree in science. Yeah.

DOMUSH: And the interest.

ENKE: And the interest, right. Yeah.

DOMUSH: Now, the whole time that you're devising these laboratory experiments on instrumentation, you're continuing your graduate research with Herb Laitinen, right?

ENKE: That's right.

DOMUSH: So how did the research go?

ENKE: I loved the research. He put me on a problem. He taught me something. I had so many embarrassing moments with Herb, but one of them was [when] I started getting in towards the end of my first year, the beginning of the second year, he called me into his office and he said, "Well, Chris, what do you want to do for your research? What kind of things are you interested in?"

Well, I had done some reading, and so I said, "I'd really like to do something along a pretty fundamental line, fundamental electrochemistry, like that fellow," [. . .] who had arranged electrodes with different barriers and so he could study the diffusion through different shapes on the way to the electrode. And so I said I thought that was really neat. I'd like to do something sort of like that.

And he said, "Well, that was my thesis.[3] Now what's yours going to be?" It was just one of those moments—well, I didn't really know that was his because at the time, I was in college. I wasn't paying any attention to who did what. [laughter] So, that was embarrassing. That was seriously embarrassing, but at any rate, got the idea.

[Herb suggested a project.] [. . .] Platinum electrodes were considered to be inert, but at the same time, something was going on [at] the platinum surface when you used it, into the anodic and cathodic range, over its full anodic and cathodic range. And the suspicion was that there was a layer of oxide, just a monolayer of oxide being formed on the positive end. And so that was what I was trying to do, is to unravel what was going on [at] the platinum surface. The amount of electricity that goes to forming just one oxide layer on top of one square centimeter of platinum is not very much. [laughter] Okay? It was electronically challenging in order to do this.[4]

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[3] H. A. Laitinen, "A quantitative study of diffusion processes by electrolysis with microelectrodes" (Ph.D. dissertation, University of Minnesota, 1940).

[4] H. A. Laitinen, and C. G. Enke. "The electrolytic formation and dissolution of oxide films on platinum," *Journal of the Electrochemical Society* 107(1960): 773-781.

And one of the exciting things was that in the aftermath of the Second World War, it turns out that one of the big electronic developments were operational amplifiers. And operational amplifiers are still huge, but we just don't talk about them anymore, because digital computing has taken over everything, but these were the building blocks of analog computers. Now they're the building blocks of all analog electrical circuitry, from the amplifiers to radios, you name it. It's all of everything you see in any electrical instrument has got operational amplifiers, and it's tremendous.

But they were just coming on, and I was fascinated by them. And it turns out that you could measure . . . they would allow you to measure tiny quantities of electricity, tiny currents, [and] tiny charges. And so I got Herb to get me some, and we built the power supply to run them, because that was too expensive. And so I was one of the first people to employ operational amplifiers in analytical instrumentation.

DOMUSH: When you told Herb that you wanted to do that, was he interested or excited? I mean obviously he said yes, ultimately, but . . .

ENKE: Yeah. It wasn't ultimately. He just—I don't even remember having an argument about it. It was, "Look, I think this would work and I think this would be—" He says **<T: 40 min>** fine. Yeah. Come to think about it, he trusted me. It was fun. Yeah.

DOMUSH: Was he an advisor that was easy to talk to and easy to work with?

ENKE: He was a reserved northern Minnesotan. A Finn. He was not easy to talk to. I would never say we had a personal relationship, even though I really think that we liked each other. It's kind of strange, isn't it? But he just had this . . . he had this reserve. He had an interesting sense of humor, but I can't remember a time when I was able to really have any kind of a deep conversation with him about anything. Even though there were a couple of times when I was . . . when I needed to and tried. That wasn't there. But an astute advisor. The person who gave me my run. Smart. Oh my gosh, [an incredible] intuitive sense.

He would get in our group meetings on Friday afternoons, and somebody would tell about an experiment that they had done. And he [might say], "No, do it again." He just knew what made sense to him. If it fit intuitively into what it is he imagined was going to be going on, then that was fine. He told me that once, too, [. . .] and he was absolutely right. I went back, and I realized I was trying to make a constant current source [. . .] [with a battery and a big resistor, but the electrochemical component took most of the voltage, making the current smaller than I thought it was. Based on my results, he sensed that and just said no.] So, you know, he was a model in a lot of ways.

But I was very, very lucky to have that relationship with Howard Malmstadt at the same time. I really had two mentors. I had a teaching mentor, and I had a research mentor. I don't know if many people have a chance to do that. And when I think about it today, I don't think I know of anybody, and I think I would have a hard time myself letting a graduate student go as much as Herb let me go in the work that I was doing with Howard. Because these days when you have to support a graduate student, and you have to get two or three papers a year out of that graduate student in order to have the grant that's supporting him continue, you can't afford to let students have that kind of [. . .] experience on the side. How incredibly enriching it was for me, and how totally impossible it would be today.

DOMUSH: Now did Howard have his own research group?

ENKE: Oh yeah. Oh, didn't he! Yes. A huge research group. He graduated people that went on to populate analytical departments, analytical faculty and chemistry departments all over the country. I'm working with one now [as adjunct professor in the lab of Gary M. Hieftje at Indiana University, Bloomington]. And they very graciously allowed me into the Malmstadt alumni group as well.

DOMUSH: You know, as you're moving on in your career at University of Illinois and you have these two parallel experiences, you have these two advisors. You have the research advisor and the research with Herb Laitinen. You have, kind of, the teaching and the research to make the laboratory experiments work with Howard Malmstadt. How do you know when you're done and when you're ready to leave, since you do have so much going on?

ENKE: That's a really good question, isn't it? And that's still a complicated question for graduate students, I think even more and more. **<T: 45 min>** And I think I left before most research advisors today would have or probably even should have let me go. I hadn't written the paper up on my thesis research when I left. And it was just one paper. But [I] did get done. It was good. So, yeah.

DOMUSH: Were other people at that time getting more papers than just the one?

ENKE: No. It was a different time.

DOMUSH: Okay.

ENKE: It was a different time. It wasn't that I was really letting the side down. But it wasn't good training for me, in a sense, for a future career. Because I needed to learn myself to become way more paper-oriented than I was at the time that I left graduate school.

DOMUSH: Did you publish anything with Howard? I mean I know ultimately you guys went on to write a book.[5]

ENKE: Yes.

DOMUSH: But I'm not exactly sure when you wrote that, if you wrote that when you were at Illinois or if you wrote it together later.

ENKE: We wrote it together later, and we did write a paper on the class itself. Let's see. *Analytical Chem* A-pages paper in 1961. "Electronics and Instrumentation in Chemical Research."[6]

Yes, but then we did go on to write a book. And more importantly I think—or as importantly—as part of that was that Howard realized that you really had to make the barrier for adoption of this as low as possible. And he also knew from his experience that if people tried to do it as just a lecture course, it wasn't going to work. And so there needed to be an inexpensive available source for the equipment, the method of connectors and the assembly of parts and power supply and all of that sort of thing. And that was an oscilloscope and what have you. [. . .] That needed to be available to people to put into their own labs with a minimum of fuss. And so, we went to the Heath Company in Benton Harbor, Michigan. They made Heathkits. And they'd never produced finished stuff, but they did have, in kit form, some of the test equipment that we needed, and we actually were using, and they had the assembly line to produce this, and they had a low-cost mentality along with it. And they were well-known. And so we got them to build the Malmstadt-Enke [Instrumentation] Lab [Laboratory] Station.

And so they did. And that's what made it possible. It was a box with doors that opened in front, and it sat on top of a double pedestal desk. And students sat there, and they had everything that they needed to do the full range of electronics and measurement experiments for a whole semester. So that was . . . and that was a really, really, really important part of this. Because then all of a sudden people could say, "Yeah, we can do this, make this investment in time and this investment in space. Take this book, the experiments are all there. The text is all there. Just do it." Physics departments were among the first to adopt, because they knew that they needed electronics. Their students needed electronics.

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[5] H. V. Malmstadt and C. G. Enke, *Electronics for Scientists* (New York: W. A. Benjamin, 1962).

[6] H. V. Malmstadt and C. G. Enke, "Electronics and instrumentation in chemical research" *Analytical Chemistry* 33 (1961): 23A-27A.

DOMUSH: Right. **<T: 50 min>** Chemistry departments were a little bit slower?

ENKE: Yes, they were, but not by much.

HUNTER-LASCOSKIE: That's interesting. I don't know if it's something maybe people today think about. You know, today electronics, and every single thing you have in your home, every device, is meant for convenience. And, you know, we typically think about instrumentation and everything from that era as being big, and bulky, and very expensive. And here's something that is, kind of, changing the game a little bit in terms of how we might think of this.

ENKE: It changed the game in terms of research because it gave the full power of electronic measurement devices coming out of the Second World War available to people doing laboratory experiments. It allowed them to devise their own, essentially, custom instrumentation, measurement systems. And then that was a big job for us over the next fifteen years, because along came transistors, and then integrated circuits, and so on. The operational amplifiers went from being a pair of vacuum tubes and a thing about this size [fist size] down to a little bitty circuit this size [pinky finger nail]. And we had to [. . .] keep the course up to date and the books up to date [with] the experiments, and the lab stations. So the Heath Company kept up with that, too, and we devised ways of making increasingly complex systems out of the increasingly powerful electronics.

DOMUSH: Now, the relationship at the very beginning with Heath Company. . . . I mean, they were making Heathkits and people knew of them, but did you or Howard have any relationship with them? Did you . . . ?

ENKE: Oh, I was an avid customer. [laughter] I built so many Heathkits. Yeah. But no, we hadn't. But this appealed to them. It was privately owned, and I think that what appealed to the owner was the possibility of having a line that [. . .] would bring some prestige to the company. And I think they liked that idea. So [. . .] it wasn't too hard a sell. We had several meetings up there, and they assigned us an engineer [Wayne J. Kooy] to put together the package of the devices that we needed.

HUNTER-LASCOSKIE: And what was that transition like? I mean as you're finishing up your PhD, you're ready to, kind of, move on to your own full-time position, and meanwhile, you're also developing the Heathkits. You know, what was that, kind of, balance and transition like, as you're moving on to your career as well?

ENKE: Well, the textbook and the Heathkit thing came while I was trying to make my way as an academic in my first academic job. So, I felt that it was something very important to do. But I also didn't neglect the teaching and the research that I had to set up, too, at that job. So, it seemed to dovetail in.

You know, I don't think that the senior faculty . . . my first teaching job was at Princeton [University]. And [the senior faculty there] were of two minds. One was telling me that this was really not good for me to be doing. It was not going to help my career. And another one was telling me that I really needed to teach electronics by having students construct their own vacuum tubes. I mean literally. [laughter]

So, I wasn't getting a lot of support from that environment. I'm glad that I persisted with it though, because I still got a lot of research done, and I had good students, some really good students. Keeping touch with them today, still, and I don't think that it really would have made any difference in the end at Princeton whether I had or hadn't done **<T: 55 min>** this book and course and things like that.

DOMUSH: How . . . sorry.

ENKE: No, go ahead.

DOMUSH: How was the teaching generally? I mean, you had said, you know, you always kind of . . . you wanted to teach. You had this desire to teach. And now at Princeton, you're in charge of your own classes. Did you feel excited and free to teach as you wanted, or did you feel hampered because you were getting these, kind of, two different attitudes about how to teach instrumentation, how to do electronics and all of that?

ENKE: Well, I wasn't teaching electronics at first. And so I was just teaching . . . when I got there, there were two other analytical—three other analytical chemists. One had been there for seven years and had just been let go, which should have taught me something right then. And the other was Clark [E.] Bricker. And N. Howell Furman. N. Howell Furman was just a luminary. He's in all the history of analytical chemistry things. You know, he was the mentor of Charlie [Charles N.] Reilley and "Buzz" [Ralph N.] Adams, and he was a great gentleman, just absolutely terrific, but he was getting very near retirement. Clark Bricker was one of his students and was really a tremendous teacher. So in terms of mentoring, you know, how to teach, how to engage students and that sort of thing, Clark Bricker was really great.

DOMUSH: Now, I'm sorry, did you say he was the one who had just been let go or was he . . . ?

ENKE: No. The fellow who had been let go [. . .] [was Wallace McCurdy who went on to a fine career at the University of Delaware.]

DOMUSH: Because Clark Bricker didn't stay at Princeton. He went—

ENKE: You're quite right about that. And so two years after I got there, Furman retired and Bricker left for [University of] Kansas. And suddenly, I was the analytical chemistry professor—teacher, actually, instructor. They decided they couldn't have an instructor running a division, so they made me an assistant professor. And I had two undergraduate classes and two graduate classes. We were trying to get some more help. Yeah. So it was very demanding. I had a very heavy teaching load there in an area that turned out really not . . . that was to be an area they didn't want.

DOMUSH: Right. Aside from the teaching, and the difficulties of Princeton, what was the move to the East Coast like? I mean, you'd been in the Midwest for your life and for your education, and you made this big move to the East Coast.

ENKE: Yes.

DOMUSH: So what was your impression of it? I mean, other than the trip to MIT where perhaps you learned that MIT was not going to be the right place for you for graduate school.

ENKE: Right. Well, Princeton was kind of a dazzle. You know, [I thought], "Gosh, [to be on] the faculty at Princeton, this is really something." [. . .] It soon became very clear to me that I wasn't going to really belong there for a long time, either in the town or on the faculty. I don't know. One of the organic chemists shortly after I'd arrived asked me how I liked it there. And I bubbled all over the place, and he said, "Well, *Enkah*," and pronouncing my name with the correct German pronunciation, he said, "Well, *Enkah*, don't get to liking it too much."

DOMUSH: They really rolled out the welcome wagon, huh!

ENKE: Really rolled it out. Yeah. Absolutely. **<T: 60 min>** Absolutely. And it was a place where, I, kind of, joke about it, I say I learned a whole new meaning of the word "critical faculty." It was a rough place. *Very* competitive. These were the days of one-upmanship and they had it down to a T. So it was one of the most stratifying environments I've ever been in.

And there was a physical chemist on the faculty there, and people asked him where he did his work, he'd say "Cambridge," hoping that they would think Cambridge, England. But, when they'd say, "Oh, Cambridge, England?" he'd say, "Oh, no, Cambridge, Mass [Massachusetts]," hoping they'd think it was Harvard. And when they'd say, "Oh, Harvard." And then he'd say, "Well, MIT."

So here was a person, and he just, sort of, hide that he had just only graduated from MIT with his degree, and here I am from Champaign-Urbana, right? [laughter] I mean, you can see how it was going.

DOMUSH: When you had been at University of Illinois, was the atmosphere that you could observe, both amongst the professors and with your fellow graduate students, was it more collegial than competitive? I mean, you said when you got to Princeton, it was the most competitive that you had seen.

ENKE: You know, I didn't experience any of this competitiveness. [. . .] It's probably due to an incredible level of naiveté that I managed to carry with me for a long time. And because subsequently I learned about some of the tensions and problems that Herb was having and Howard was having with the other faculty there at Illinois. But I was not aware of that at all. So I . . . this was quite a shock to suddenly come there.

And I got Herb to come and give a talk. And he came and gave his talk, and I'm driving him back to the Newark Airport from Princeton, and I tried to engage him on this topic, what is it about analytical chemistry? What's happening here? And he didn't want to talk about it. I was really hurting and trying to sort it out, but he wasn't going to engage. So that was one of those examples of where I really needed to have a heart-to-heart, but I couldn't get it.

DOMUSH: When you left University of Illinois for Princeton, were there other opportunities that you could have taken?

ENKE: Yeah. I had an offer open from Cornell [University]. And I had an NSF [National Science Foundation] postdoc to go to [the Max Planck Institute in] Stuttgart. Yeah.

DOMUSH: How did you decide to go to Princeton over Cornell or Stuttgart?

ENKE: I liked [the concept of Princeton]. Princeton seemed like an easier environment than Cornell. The possibility of going to either one of those straight out without the postdoc sounded good at the time. Again, one of those decision points you think, well, did you review the bidding? I think it didn't hurt that my wife's brother had graduated from Princeton, and this was, kind of, a dazzled environment for her too. So, yeah. That's not my current wife we're talking about, but the one that I had at the time.

DOMUSH: When did you get married?

ENKE: I got married in graduate school, **<T: 65 min>** second year. Yeah, 1956.

DOMUSH: A busy time.

ENKE: Yes. Right. I went into Herb, and I said, "Herb, I'm going to get married this summer." He just looked at me. So I didn't know what to say. He just looked at me like, "Why are you telling me?" [laughter] And so I said—well, I'm trying to think of something, right? And I said, "Well, I'm hoping this won't affect my work particularly," or whatever. And he said, "Never knew anyone it helped." [laughter]

HUNTER-LASCOSKIE: So again, not the real heart-to-heart . . .

ENKE: No. Not your warm fuzzy. Not your warm fuzzy. Yeah.

DOMUSH: I'm curious, while you were at Princeton, if it was difficult to maintain the relationship and the collaboration with Howard. I mean, now, maintaining a collaboration like that, in theory, is so much easier because you can jot off an email, someone can read it, you know, in a matter of minutes—or not, depending on what time zone they're in. And in some ways, that's so easy. How difficult was it at the time, or was it not difficult?

ENKE: Well, it sure wasn't as easy as it would have been today. I had—just a year ago—I had an intense collaboration with a fellow in Belgium that I'd never met before. And so we did it by Skype, you know, and email. It was amazing. So, of course we had to do all this by mail and phone, but I would make frequent trips out to Michigan.

DOMUSH: To meet him at Heath or . . .

ENKE: To meet him at Heath. Well, he had a house on Lake Michigan not very far from Benton Harbor. And so we'd go there and stay and we'd work and go to Heath and do stuff.

And so in a few days, we could get a lot done. And sometimes, I'd go to Champaign-Urbana [Illinois], to his house there.

HUNTER-LASCOSKIE: While you were at Princeton, you mentioned, it was this incredibly competitive, stratified environment, and maybe it was the first time you had noticed this kind of environment in an academic setting. How did that affect your ability to work and juggle these things? Was it something that you tried to not be a part of, or was it something that had an effect on the things you were doing?

ENKE: I think my response to that kind of thing was just to, kind of, pull in and do my thing myself and hope it turned out. I couldn't change my credentials. So there was no way to really try to get into that game.

HUNTER-LASCOSKIE: Did you find it was something other faculty members, kind of, wanted to make a big deal out of?

ENKE: It seemed to be part of the environment. There were a few notable people who were just not that way, that I was able to be friends with—Ted [Edward C.] Taylor [Jr.], an organic chemist, and a few others. Right, but it was lonely.

DOMUSH: How were the students? I mean, you said there were some of them that you're still in touch with and that your students were very good. Did they also feel this competitiveness, or did they seem to, as students at least, kind of, work well with each other and have that more collegial atmosphere?

ENKE: I don't really know whether they were part of that competitive life or not. [. . .] I really don't have a feeling for that.

DOMUSH: That's okay. That's okay. You mentioned that . . . **<T: 70 min>**

ENKE: It was an all-boys school at that time.

DOMUSH: Yes.

ENKE: Yeah. I sat in a faculty meeting where they were trying to decide whether it would be okay to have women graduate students or not over in Nassau Hall. I couldn't believe it. I couldn't believe what these guys were saying. I mean, I had gone to co-ed schools. I'm like, "What's the problem?"

DOMUSH: Was Principia co-ed then?

ENKE: Yeah.

DOMUSH: Huh.

ENKE: Yeah, absolutely and so . . .

HUNTER-LASCOSKIE: But they weren't there yet at Princeton, necessarily.

ENKE: Oh, no. These things . . . why it wouldn't work and, you know, and…

DOMUSH: I don't think Princeton allowed female undergrads until about 1969.

ENKE: I think that's right.

HUNTER-LASCOSKIE: They were really late.

DOMUSH: Yeah, because the . . . if I remember correctly, they were the last Ivy League to allow women—undergraduates, at least.[7]

ENKE: I would totally believe that.

DOMUSH: I have to double check, but that's my recollection.

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[7] In 1972, Dartmouth College became the last Ivy League School to admit women.

ENKE: Yeah. [It was thought that] women graduate students weren't going to get jobs. They weren't going to hold jobs. They were going to leave partway through their degree. They were going . . . you know, it was just . . . [laughter]

DOMUSH: Had there been any women in your graduate program at the University of Illinois?

ENKE: Oh gosh, yes. Oh, sure.

DOMUSH: How about in the . . . either in the Malmstadt group or in the Laitinen group?

ENKE: Yes. There were. Not so many as came later, but yeah. Yeah, there were, and there were quite a few in the organic and inorganic areas.

DOMUSH: You mentioned that after a couple years at Princeton, you started to realize that Princeton, aside from being intensely competitive, didn't necessarily care that much about analytical chemistry, if I was understanding you correctly. And I'm curious if you were noticing anything, or if there was any sort of larger issue in the chemistry world at this time that was going on about whether or not analytical chemistry was important, useful, something that should be taught at the undergrad or graduate level, or if this was just Princeton not particularly caring about analytical chemistry?

ENKE: No, I didn't . . . . I wasn't aware of it so much outside of that, because the analytical community is so vibrant. I mean, there's Pittcon [Pittsburgh Conference on Analytical Chemistry and Applied Spectroscopy]. And as an analytical faculty member, I can say I have never had any trouble placing students. The demand for them is always very, very high. Most of the places where I've taught, there were more analytical applicants for graduate school than there were any of the other divisions. I've never really felt that it was unsuccessful or dwindling or, you know, in any way in the world, not highly successful. It just had a "Rodney Dangerfield problem" in the department. [laughter]

DOMUSH: Were you going to things like Pittcon and some of the other national meetings?

ENKE: Oh yeah. Right. Yes.

DOMUSH: Okay.

ENKE: I'd like to pause this for a minute.

DOMUSH: Sure.

[END OF AUDIO, FILE 1.1]

DOMUSH: Just before we took a break for a second, we were talking about how you had felt that the analytical community was always vibrant and always strong, that you've never had a problem placing your students. And I asked you if you had gone to things like Pittcon or some national meetings, and you said yes. And I was just wondering when you started going to some of those meetings.

ENKE: Oh, right away. I went to Electrochemical Society meetings when I was still a graduate student, and I started going to Pittcon right away as a new faculty member. Back to your previous question though, had I noticed anything else? I did notice that L. B. [Lockhart "Buck"] Rogers left MIT, and J. J. Lingane wasn't replaced at Harvard. So, there were some signs that at least in the Ivy League area that analytical was suffering some problems. And I was aware that it wasn't just Princeton. It did include some of the other [schools], but certainly not the Midwest.

DOMUSH: I remember, when I was an undergraduate chemistry student, someone came through to give a lecture about the importance of analytical chemistry. I mean, they were giving a chemistry seminar as well, but as part of it, they were talking about the importance of analytical chemistry, but that they saw the future of analytical chemistry not as its own division, but they saw it as something that would be important enough to defy its own division, in a way, and just be split amongst all the other traditional divisions of chemistry, but that he foresaw the future of chemistry departments not having analytical divisions.

And for some reason, hearing him talk about that has really stuck with me all these years later, because I just haven't seen that come to fruition either when I was in graduate school or in talking with chemists now over the years, both those who are analytical chemists and those who are not. And I don't know. I have no way of knowing if it was just that's what he thought the future was or if it was based on some feeling that he had from his fellow chemists that, you know, analytical is really important. Everyone needs to understand it and to know how to use it and it just doesn't necessarily work as its own division. And I don't necessarily have a question in that story I'm telling you, but it . . .

ENKE: Well, let me make two, hopefully fairly quick, comments about that. One is that analytical chemistry alone now, among the four or five divisions of chemistry as we put them together, is the only one that has not developed a science. And that could very well be the basis of this person's comment. Analytical chemistry is taught as a compilation of techniques. And from the point of view of the person that you just mentioned, anybody can learn a technique. I feel as though teaching just techniques is really an unfortunate waste of time, and that the most important thing that I could impart to my students—graduate or undergraduate—is to give them a sense of how I think. What are the problems that attract me? What's the bag of solutions that I have to bring to those, apply to those problems, and what is the thought process that goes on in reaching some solutions?

I think that that's different for analytical chemists than it is for inorganic chemists, and it is for organic chemists or even for physical chemists. Probably closest to physical, but it's different. It is different. And the fact that we haven't formulated that, nobody has really, but particularly analytical hasn't, is part of what I think is its problem.

I tried to pull that together in an undergraduate text that I wrote, but it wasn't adopted, so it couldn't go up against the mainstream.[8] But my thoughts about what analytical chemistry is and how you come to an analytical solution, **<T: 05 min>** I wanted to be able to teach somebody that if one day they ran across an enzyme that was exquisitely specific for a particular substrate that they would know how to make an analytical method out of that enzyme for that substrate. And you see, people don't think that. They don't think of that . . . they think about, well, is it chromatography or is it mass spectrometry? But I was trying to broaden it out. Analytical chemistry is a very interesting—

BEA REED: Hi. Excuse me. I just want to say I saw you found it.

DOMUSH: Yes.

ENKE: Would you like some more water?

DOMUSH: Oh, I'm okay. I'm fine. Thank you.

ENKE: All right.

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[8] C. G. Enke, *The Art and Science of Chemical Analysis* (New York: John Wiley and Sons, 2000).

DOMUSH: And you were just finishing up saying about how you wanted to teach analytical chemistry as, kind of, a problem solving and thinking mentality as opposed to just kind of picking a mass spec or a chromatography technique type of . . .

ENKE: People think of measurements as measurements, but the thing that's unique about analytical chemistry is that the thing you're measuring is mixed up with everything else. So it's not just a matter of putting a ruler on it or measuring its mass. You have to [. . .] learn what are its distinguishing characteristics—the thing that you're trying to analyze. You have to discover its distinguishing characteristics, and then you have to devise a means to get those to respond while everything else stays quiet, right? Now, that's a real challenge, and that is really the fundamental part of what analytical chemists do, but it just hasn't been worked out in a rigorous way.

Inorganic was suffering the same problem [that] analytical was in the fifties and sixties because it was a series of recipes for making inorganic compounds. But then along came the fundamental part of inorganic chemistry, when it, sort of, met with physical chemistry and figured out bond angles and so on, and how many complexes would fit around a nickel [ion] and so on. And that changed it. That put them on the map. And analytical chemistry's never done that.

DOMUSH: Why do you think that the Midwestern universities perhaps would be more interested in analytical chemistry than some of the Ivies? Do you think, perhaps, it has anything to do with the application? That they were more interested in some of the applications that could come from analytical chemistry?

ENKE: I think they're not as prestige-oriented, and so they're not so worried about hiring people exclusively who have a chance to get into the National Academy or win the Nobel Prize. And I think they know which side the bread is buttered on. They can afford to be practical. This is what students want. This is what our local industry needs. Why don't we just supply it, and not worry about whether it's dragging us down or not?

HUNTER-LASCOSKIE: Now, while you were at Princeton, I noticed you were also doing some consulting work and . . .

ENKE: Yeah, I did a little.

HUNTER-LASCOSKIE: So, can you talk about how those opportunities came about?

ENKE: I was just invited in each case. There wasn't a lot of that. [. . .]

HUNTER-LASCOSKIE: And what kind of work were you doing? I know one was Standard Oil [Company, Inc.], and the other was American Cyanamid [Company].

ENKE: Standard Oil was interesting. And American Cyanamid. Yeah. American Cyanamid, they would just put me in a room and people would come in every hour or every forty-five minutes, you know, and present me with a problem I'd never seen before and ask me if I had any ideas, you know.

DOMUSH: That sounds exhausting.

ENKE: It was exhausting. It was interesting. It was challenging. They had just developed an LCD [liquid crystal display]. This was long, long, long ago and they wanted to patent every possible application, and so could I think of any ways that people could use an LCD? **<T: 10 min>** In hindsight, this is so stupid. The only thing I could think of is, well, it might be handy to . . . if you had a telephone that could reveal the number of the person that was calling you. That was the only thing I could come up with. Imagine all the ways we use LCDs these days, but that was . . .

Standard Oil was more interesting. I was in their electrochemical group, so we would talk about electrochemistry and electrochemical measurements and problems and things like that. An interesting thing came up there. There was a fellow who'd just . . . there was a form of carbon that was . . . . I'm trying to remember the name of it now, but it's very porous. I think that's what you call it, porous carbon. And so it had huge surface area for the amount of carbon that was involved, and the weight. And he [Robert A. Rightmire] was measuring the double layer capacitance of this, and it was huge, absolutely enormous. And so suddenly, he realized he could have a big capacitor in a very small space. And this was a way to store energy; it was a way to make a new electrical device. And this was going to be big, and I thought this was absolutely stupendous.

For twenty-five years, it went nowhere. And now we have these supercapacitors. Finally, we have circuits for which one volt is a [useful] voltage. We have circuits that can operate on these very low voltages that these capacitors can sustain, and we need this kind of thing. And they're called supercapacitors. But for years, and years, and years, it never happened.

DOMUSH: Can you talk a little bit about the transition from Princeton to Michigan State [University]?

ENKE: Yeah. Moving back to the Midwest was getting back into my comfort zone. That's for sure. And Michigan State was a wonderful environment at the time. It was a department that had just gotten a grant to . . . . Centers of Excellence they were called. And what the government was trying to do was to break up this pattern where only a few schools on the two coasts got all of the research money that was coming out. So they were trying to create these Centers of Excellence around, and I came into Michigan State on the Centers of Excellence program that they had there. They were expanding the department. The department was working together. I worked with physical chemists to create new courses and new curricula. It was just the whole . . . it was a whole new life. It really felt wonderful. I remember driving home from work one day after I'd been there a few weeks, and I was whistling and realized that this was it. This was where I needed to be.

DOMUSH: Were you able to start teaching the instrumentation course right away?

ENKE: Yes.

DOMUSH: Was that something you were able to . . . ?

ENKE: Yes. They wanted that right away and so yes. Definitely. And I was still an electrochemist at that time. But that changed shortly. Not completely. I still consider myself an electrochemist to a considerable extent, but I had an exceptionally good graduate student, and in fact, he moved with me from Princeton to Michigan State, which I thought was really tremendous. Pete [Peter H.] Daum. [. . .]

Very, very, very bright fellow. And, time for him to graduate, there weren't any departments hiring electrochemists. Electroanalytical had filled up. Everybody had one. And I just . . . . I felt, "I can't do this. I can't take in graduate students that deserve really good jobs when I know there aren't really good jobs for them." And so I started to switch. And about that time, **<T: 15 min>** Digital Equipment [Corporation] had come out with a PDP-8 computer, lab computer, and I managed to get a hold of one of those, and I started transitioning into automated instrumentation in general, including optical and electrochemical, and then eventually mass spectrometry.

HUNTER-LASCOSKIE: Where did that student end up?

ENKE: I think I'd rather not say for fear of hurting somebody's feelings at this point, but he [now has] a high management position at Brookhaven [National Laboratory].

HUNTER-LASCOSKIE: Oh.

DOMUSH: Oh.

ENKE: So he's done very well.

DOMUSH: Yeah.

HUNTER-LASCOSKIE: But that was a definite trend you saw where there was just the demand wasn't going to be there.

ENKE: But the school that, you know, the best academic job that I . . . that we could find at the time did not provide him with the research support that he deserved.

DOMUSH: Was it difficult to start to make the transition into, kind of, automated instrumentation, or was that a logical next step based on some of the instrumentation work you were already doing?

ENKE: Both. Yeah, I mean it required me to learn new things, but at the same time, I was attracted, definitely, into it, and I saw some opportunities to do [novel things] from the instrumentation standpoint. Even though I wasn't going to do anything novel in optics or spectroscopy at that point, but I might be able to do some things that were interesting . . . for example, in emission spectroscopy, you know about emission spectroscopy, there's huge empty spaces, right? And if you want to collect data—if you're scanning, if you want to collect data, and you want to get each of these lines that you've got, you want with precision, you've got to be—you've got to stay there to get enough counts to get a good precision. Well, that means you have to be scanning slowly through that space. And at the time, that meant you were also scanning slowly through all the empty space as well. So we [Edward J. Darland and I] devised a computer operated arrangement where we'd look and see was this an empty space or not? You can tell that soon enough. And if it was, then we move on to the next one. Is this an empty space or not? And then if it starts . . . and then if we start to get counts, well, you stay there until you've got enough counts to meet the precision you need and move on. So it improved the efficiency of collecting emission spectra. So [Darland] wasn't doing anything with the optics or with the spectroscopy, but we improved the rate of data collection by a couple orders of magnitude.

DOMUSH: So in order to do that kind of work, are you, kind of, writing computer code or are you modifying an instrument in some way? How . . . ?

ENKE: Yes. We had to work out a means to first of all to interface the spectrometer with a computer, not just for the collection of data, but for the control of the instrument. And so that was the part that excited me. Because now the instrument control is not just by knobs and the delay time of the operator, but it's going to be done by data interpretation in the software.

HUNTER-LASCOSKIE: What was that transition like for your students, you know, as you're moving to a different area, you know, were there students who were, kind of, in that overlap period?

ENKE: Oh yeah. There were, and I don't know. It seemed to me it, sort of, enlivened things. It broadened things out. And the computer part I think was exciting everybody to do that. So we were computerizing electrochemical experiments as well.

DOMUSH: And this transition started to take place shortly after you got to Michigan State because if . . . ?

ENKE: Yeah, that's right.

DOMUSH: Okay.

ENKE: Yeah. Well, I got to Michigan State in '66, and **<T: 20 min>** this was . . . this started to happen in the . . . oh yeah, early seventies—late sixties, early seventies. It was very shortly after I got there. Right.

DOMUSH: How did the teaching go when you got to Michigan State? Were you still as excited about teaching as you had been previously, or was the . . . ?

ENKE: Oh yes. The students were different. The classes seemed to me more vibrant. There were more people in my office hours. All that changed a little bit over time. And the transition here was also very interesting in terms of the students and the nature of the student-teacher interaction. But yeah, it was really fun, and it was a pretty open department. People were looking for new ideas, and we devised a whole new curriculum that integrated analytical and physical chemistry. So we were able to teach the equilibrium with a p-chem [physical chemistry] perspective, which you really should have.

In a way, the analytical curriculum is still upside down. You teach these instrumental techniques to the seniors, and you could do that to freshman just as well, almost, and we teach the analytical chemistry, which is really largely equilibrium studies and so on, before people have even learned about Debye–Hückel equation. So, it really isn't too sensible, but we haven't changed the chemistry curriculum for over fifty years.

And it isn't just whether you have an analytical division or not, or whether the analytical division is going to be absorbed or not. In my opinion, the biggest problem that chemistry has to [solve in in order for the discipline to] survive is [. . .] the divisions, the fact that there were any and the fact they got so totally compartmentalized. Chemistry is seriously hurting as a field right now, academic field. There are departments in England that have dropped it.

DOMUSH: Really?

ENKE: Yeah, I mean not departments—universities. They can get along without perfectly well.

DOMUSH: Wow. I hadn't realized that.

ENKE: And physics is taking over. You can do courses in medicinal physics, and we're just going to get bridged. Think about it. Physics got cryophysics, nuclear physics, cosmology and so on. They're still in the physics department, right? Where is medicinal chemistry? Where is material science? Right? Where is mineral science, mineral chemistry and so on? They're not in the chemistry departments.

DOMUSH: Right.

ENKE: Why? Because they didn't fit any of the divisions. And so they wouldn't hire because they had to hire true-blue organic, true-blue physical, true-blue analytical, and they gave away all the boundaries, all the areas where the excitement is happening.

HUNTER-LASCOSKIE: So all of that, kind of, interdisciplinary, you know, wiggle room work is being done elsewhere?

ENKE: Yes. Yeah. Don't you think?

DOMUSH: That sounds so sad.

ENKE: It is. It is, but we've done it to ourselves. We have done it to ourselves by being so internally competitive.

DOMUSH: Right, and so rigid.

ENKE: And so rigid, exactly.

DOMUSH: So rigid in the divisional—the division requirements and…interesting.

ENKE: Yeah. It's starting to loosen up a little bit, but I think it's a little bit too late. [. . .] [Departments are] accepting biology, now and then, biological chemistry. But if you ask people if they'd like to bring in the department of medicinal chemistry **<T: 25 min>** or invite the geological chemists over or whatever, that's still, "Not interested," pretty much anywhere. Yeah. Well, anyway, that's my little soapbox moment. Sorry.

DOMUSH: That's okay. It's okay. If you can't stand on a soapbox during your oral history, when can you? In some ways, I think, I don't have any more questions for today. I think tomorrow, we can really get into, kind of, the transition into mass spec. So unless there's anything else that you want to add for today, I think we're all set for the . . . until tomorrow.

ENKE: I can't think of anything.

DOMUSH: Okay. Well, if you do, we can certainly make sure we discuss it tomorrow.

ENKE: Okay. What are the kinds of things . . . what do you want . . . what have you got left on your list to do?

DOMUSH: Well, we certainly want to talk more . . . since we haven't talked at all about mass spec, we certainly want to talk about mass spec, both about the transition to how you got there and about some of the, kind of, big accomplishments in your career in mass spec. When we talked with Mike [Michael A.] Grayson before we came out here, he wanted to hear a lot about the triple quadrupole, of course, but he also suggested that we should be talking quite a bit about the distance-of-flight mass spectrometry.

ENKE: Okay.

DOMUSH: But of course, if there's anything that, you know, we miss along the way . . . . I know that there was a good period of time where you were working on electrospray ionization. You know, certainly anything about that, we could talk about as well. And of course, we want to ask some . . . we've talked about some big picture things already today, but I think we want to talk a little bit more about some big picture things tomorrow as well. Perhaps about this new interest in history and philosophy of science and about—I'm going to get the title of your paper wrong if I don't look at it. And it's somewhere in my folder. The paper you wrote about reuniting natural philosophy and scientific research.[9]

ENKE: Oh, right. My first, sort of, beginnings into feeling like we should know about—more about—what we're doing.

DOMUSH: Yeah. Well, it's something that we're very interested in at CHF [Chemical Heritage Foundation]. So we're hoping to talk a little bit more about that tomorrow.

ENKE: Yeah, right. I think it was [Richard P.] Feynman that said that scientists have about as much use for scientific philosophy as birds have for ornithology. And then somebody said that they thought if birds really could understand ornithology, they might get quite a bit out of it.[10] [laughter] [. . .] I've really been struck by this. We don't really even have good nomenclature for the philosophical terms that we use. And I think we get—and science journalists—get very, very confused about what it is we know for sure and what it is we don't, and what's the difference between fact and what are facts, you know. What do we know for sure? And then when you try to defend something, that, [indeed], everybody knows is going to change in ten years as fact, you run into some credibility problems.

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[9] C. G. Enke, "Reuniting natural philosophy and scientific research," *Journal of Analytical Atomic Spectrometry* 27 (2012): 1177-1180.

[10] This analogy is widely attributed to Richard Feynman, although the source is uncertain. Several philosophers have refuted his opinion on the utility of philosophy of science; particularly, Craig Callender, in conversation with Jonathan Schaffer, remarked, "I do think that birds, if they did know ornithology, would be in a superior position." See "Craig Callender and Jonathan Schaffer," *Philosophy TV*, 8 September 2010, http://www.philostv.com/craig-callender-and-jonathan-schaffer/.

DOMUSH: It's interesting that you phrase it that way. I conducted an oral history with someone who does aerosol mass spec in relation to emissions and air pollution recently and we were talking about public understanding of science and public understanding of climate change. And he said that one of the things he's most frustrated with is people will have discussions about science and about climate change and they'll use the word "belief": "I don't believe that to be true, or I don't believe in those facts or in that result." And he said it doesn't matter if you believe it, whether that be a fact or just the result of a study, that is the results. **<T: 30 min>** [Those] were the conclusions that we came to, and it's not for you to believe in. It's for you to try and understand. It was more the word that he took issue with I think in response to, kind of, a belief to him was something perhaps that didn't need . . . perhaps like coming from a religious point of view where you don't . . . you can just believe in something. You don't necessarily need a scientific experiment to prove it to you.[11] But it's interesting. It just made me think of it when you were saying that.

ENKE: Well and see, but . . . he's exactly right. But the point is that we don't distinguish between the experimental results and observations, which are in fact factual, they are real, and the interpretation we make of them. The interpretation may change. We're learning that. I mean, what are people supposed to believe? We're now finding that the oceans are absorbing more CO_2 than we thought they were going to. The ocean temperature change may not be exactly what it's predicted to be. But at the same time, the arctic ice is sure as hell melting. So how do you hold these uncertainties with these things that we're going to learn more and better and revise and adjust and so on and how do you hold those things up against somebody else who says, "Well, my theory is . . . this is what I believe"? You know? "I think everybody made it up. I think it's all just a scam to get us to use more solar power or something." I don't know.

You know, but whatever the people's arguments are, as long as you're just pitting one person's interpretation against another person's interpretation, then everybody gets to play the game, and everybody can believe what they want to. So if we don't separate, if we don't have a way to distinguish between the empirical knowledge that we have from the interpretation that we make of that empirical knowledge, and if we don't have a way to show how the empirical knowledge and the explanations work together to reinforce each other to create new science, if we don't understand that, then we're always going to be open to questions of credibility. People know theories change. They expect them to change. We expect them to change. We do experiments to change them, to test them. So then how do you believe a theory? Is that appropriate to believe a theory?

DOMUSH: Right.

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[11] Peter F. DeCarlo, interview by Hilary Domush and Jody A. Roberts at Drexel University, Philadelphia, Pennsylvania, 10 and 11 April 2013 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0716).

ENKE: Okay. But it is certainly appropriate to have confidence in experimental results that just support and re-support and re-support over and over again something that's going on. And if you put it that way, it's different. But we're not putting it that way. We're having an argument over theory, belief systems. And so then we get down to, "Do you believe in science, or do you not believe in science?" Well, I don't know. I don't believe in science, but I go to my doctor, and I listen to the weather report.

HUNTER-LASCOSKIE: Yeah, there's a definitely language issue.

ENKE: Really serious.

DOMUSH: Well, I think after discussing the problems with chemistry departments and the pitfalls of the way we discuss science, I think it's time to call it a day.

ENKE: Okay, sounds good. Yeah. You've got other things.

[END OF AUDIO, FILE 1.2]

[END OF INTERVIEW]

INTERVIEWEE: **Christie G. Enke**

INTERVIEWER: **Hilary Domush**
Sarah Hunter-Lascoskie

LOCATION: **The home of Christie G. Enke**
Placitas, New Mexico

DATE: **17 April 2013**

DOMUSH: Okay, today is Wednesday, April 17. I am Hilary Domush. With me, again, is Sarah Hunter-Lascoskie, and we are in Placitas, New Mexico, for day two of Chris Enke's oral history. And yesterday, we got to a point where you were talking about the transition from being an electrochemist to looking more at automated instrumentation. And we wanted to hear about how the transition was from electroanalytical chemist to automated instrumentation to mass spec and start getting a little bit more into how you got involved in the mass spec research.

ENKE: Right. So as I mentioned, Digital Equipment had come out with the PDP-8. A colleague of mine—Dick [Richard S.] Nicholson at Michigan State—had gotten one of these. This was a laboratory computer, but it was still in a big electrical rack, you know, six feet high, two feet wide and so on. Two feet deep. But it was on rollers, and you could move it around. [Dick then took a leave from MSU to be a rotator at NSF and never came back. He ended up being the executive director of AAAS. He, too, became a mass spectrometrist working with Hank (Henry M.) Fales at the National Institute of Health on chemical ionization, but I didn't know that until much later. Dick donated his computer to me, which] was exciting for me because it really brought the capability of data collection and data interpretation right into the laboratory. Up until that time, people would collect their data, enter them manually onto punch cards and carry them over to the campus computer, you know. And then write programs in FORTRAN or something to do the data analysis.

Also, because I understood the electronics of what was going on inside the computer, I realized there was a bus in there that was just passing data from one place to another. And if I could get access to that bus and make the appropriate interface, [the laboratory computer could] get the information directly. So that could eliminate that whole read out step. Then I realized, too, that if in fact the data could be read directly into the computer . . . not only that. If I could get the information that was on the computer bus out to the instrument, I could write programs in which the computer would be telling the instrument what to do.

So this was an exciting time. [. . .] One of the things that we did in the second generation of Digital Equipment's PDP-8, which now had come down to almost what you might call desktop, it was, sort of, the transition between individual transistors and integrated circuits for Digital Equipment . . . again, we were still working with Heath Company. We were still upgrading into the modern era, and we built an interface that plugged directly into the PDP-8 bus and gave external access to that bus, so it was really the forerunner of now what you buy from National Instruments [Corporation] for interfacing equipment to experiments to the computer and controlling them. Of course, we didn't have LabVIEW [Laboratory Virtual Instrument Engineering Workbench] program, so we had to develop that ourselves.

But it was pretty exciting. We did electrochemical experiments under interface control, and then [we] started to do optical. I realized that one of the things that the computer was very good at was handling large amounts of data, larger amounts than you could get onto a strip chart recorder, so I got interested in the Vidicon camera, which is the precursor now of all the CCDs [charge-coupled devices] and things that are in cameras. And we did [real-time polarization measurements] and other kinds of optical things.

One of the things that intrigued me about computer control of instruments is that when an experimenter does an **<T: 05 min>** experiment, they'll set up the experiment, run it, review the data, decide what to do next and go, then, do another experiment. Well, I realized, too, that it was possible for the computer to do some of that interpretation and decide what to do next based on the information that it had just gotten from the experiment. Now this could happen in sub-millisecond time as opposed to the next day time, which is the usual for experimental operations. So that really intrigued me.

And I gave you an example yesterday of—a simple example of how that worked—in the case of emission spectroscopy. Not spending any longer than you needed to collect the data that you want. I conceived the idea of a universal analytical instrument, and I also thought—and this is something we still don't have—but my vision was that you wouldn't tell the computer what [the instrument] was supposed to do, what [question] the experiment was supposed to [answer]. You would tell the computer what it is you wanted to know.

And so now I want the computer to be involved in looking up what the specific characteristics of [the target analyte are], looking up the characteristics of the matrix in which it's found, devising an experiment, devising an analysis, and carrying it out. So in order to carry out that analysis, then, there will be two principal functions. One is separation, and the other is identification. I mentioned yesterday that analytical chemistry measurements, the principal problem is the fact that what you're looking for is mixed up with everything else. So you have to first find the distinguishing characteristic and use it to, sort of, bring that analyte up to the fore. And so I needed a method of separation, and then I needed a method of identification.

Now, the method of separation as you both very well know, that everybody uses and was already in full swing at that time is chromatography. But chromatography was not going to be useful to me because you can't just go look at any peak whenever you want to. You have to wait until it happens. And so this is not a real-time method, and I wanted real time. So I wanted to be able to take a mixture and separate it into parts and pick out one of those parts at will and identify it.

[. . . .] So I had this pretty well in mind, and at that time, a new graduate student came into the group, Rick [Richard A.] Yost. And he decided he'd like that project. So we went to a FAACS [FACSS, Federation of Analytical Chemistry and Spectroscopy Societies] meeting. I think it was in Cincinnati [Ohio, October 1975].

DOMUSH: I'm sorry. At what meeting?

ENKE: It's now called SciX [FACSS's "The Great Scientific Exchange"]. But anyway, it was an analytical meeting of the more local kinds. And we were driving back to East Lansing, [Michigan]. It was evening, and some of the students were already asleep. And we were trying to keep each other awake and so we started to talk about how we were going to accomplish the separation. And Rick was there, and Rick had worked with [M.] Bonner Denton at University of Arizona as an undergraduate **<T: 10 min>** and had some experience with Bonner with a quadrupole mass analyzer. And he said, "Well, maybe we could use a quadrupole mass analyzer to do the separation," because you could pick any given mass out of the [mixture] of masses at the beginning, and transfer it on to the next thing, and you just set it for that mass, and it would do that. And that sounded good, but then we needed a method of identification.

Well, you can't just put another mass spectrometer there to identify it, because all you're going to get is the mass of what you've just separated out, and there's going to be no new information, right? So, we realized that what we needed to do in order to identify, then, the particle that we . . . the mass . . . the ion that we had separated, was [that] we were going to have to fragment it so it would give us a characteristic fragmentation pattern, which is [similar to] the usual [electron impact ionization] mass spectrum. So we needed a way to fragment ions between two mass analyzers.

So we're looking into the literature, and there is . . . people had been fragmenting ions for a little while between the magnetic and electric sectors of double focusing mass spectrometers. And they were not using this for analytical purposes at all. They were using it to study the fragmentation process, what it took to fragment the ions, and so on. [They were] more fundamental physical chemistry kinds of investigations. [John H.] Beynon was involved in this, and [R.] Graham Cooks. So we recognized Graham Cooks' name and Graham came to Michigan State [. . .] right while we were trying to sort all this out. He was an outside advisor, consultant or reviewer for the mass spec center that was in the biochemistry department at Michigan State. And he was coming to campus, and so Rick and I had breakfast with him one morning when he was there, and we presented this problem to him.

What was known at the time was that the efficiency of this fragmentation [was very low]. What they did was they would just have a little gas leak between the two sectors. If they put in too much gas, the fragment [intensity], the signal, just disappeared completely. If they put in just enough, of course, most of the ions missed getting collided, and so the efficiency was pretty poor. And not only that, the efficiency dropped off dramatically as the ion energy decreased. So someone, in fact, and I've been trying to locate this paper—Rick and I both have—but I remember it distinctly, someone even plotted the efficiency of fragmentation as a function of voltage, and it just tanked when you got below . . . it wasn't very good at 3,000 volts. It was pretty bad at 1,000 volts, and below that, it was like never mind.[12] So Graham suggested one possibility was to [accelerate them up to the high voltage so we could get fragmentation and then decelerate them before they went into the next quad.] You know, in a quadrupole, the ions are going at maybe 10 to 20 electron volts. Not thousands. [. . .]

And so we wrote a proposal to [NSF] to build this tandem quadrupole instrument. We admitted in the proposal that we needed a means of fragmenting the ions in between these two analyzers and that this was going to be a novel part of the research. And we presented the possibility of this acceleration-deceleration **<T: 15 min>** plan that Graham had suggested. That [suggestion was a little disappointing] because obviously, that had occurred to us too, and it didn't seem like it was probably going to be very efficient. We were going to lose a lot of ions in the acceleration and decelerations, and it already wasn't very efficient. So the other possibility was to use energetic electrons to fragment the ions, and we presented both of those possibilities [in our proposal].

The proposal got horrible reviews. [. . .] One of the main criticisms was that we were total unknowns in mass spectrometry, and this was a complicated problem, and if anyone was going to solve it, people who knew what they were doing would do it. The other was that there was really no recognition [of] the analytical need for this. It was already going on in sector instruments. So, why? And so the proposal was badly received. [Then later or maybe around that same time we sent a copy] to the Office of Naval Research.

Now, part of the proposal was that . . . . I realized that now the National Instruments 4004 microcomputer had first come on. [It was tiny, with] very low power processing, but you could do [something with] it. And I thought, now, really, the way to run this instrument control is to have [. . .] a separate little microprocessor in charge of each part of the operation. We'll put one in charge of this quadrupole, one in charge of this quadrupole, one in charge of the voltages and so on, and we'll arrange a communication scheme among them. And [we'll have] the main processor still the PDP-8, [. . .] running all of this. And so we had this, which is now called distributed intelligence concept, for running this complicated instrument. Now the Office of Naval Research didn't really care at all about the mass spectrometer, but they were really intrigued by this distributed intelligence kind of thing. And so they funded us, twenty-five thousand dollars a year.

And now [to build] a mass spectrometer from scratch, I mean, we didn't have a mass spectrometry lab. It was the first vacuum thing I'd ever done, so there were vacuum pumps and vacuum gauges and interlocks and all of this kind of stuff—these expensive pieces. So we were scrounging and working to try to get this together, at the same time working on the electronics part of it. So [Rick and I] went to an ASMS [American Society for Mass Spectrometry] meeting and that was in [1977]. [. . .] I for the first time, Rick for the second. Now we had money and we

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[12] See A. Giardini-Guidoni, R. Platania, and F. Zocchi, "Collision induced dissociation of $C_2H_4O^+$ ions of different origins with helium," *International Journal of Mass Spectrometry and Ion Physics* 13 (1974): 453-456.

had [a lot to learn]. I didn’t know anybody particularly in the mass spectrometry community, and the meeting was still fairly small. There were just a few hundred people who were attending ASMS at that time.

And I noticed among the attendees Jim [James D.] Morrison. Now, Jim Morrison as you know from your research, is an Australian, but I had met him at Princeton. He came to Princeton as a visiting prof, it turns out, and ended up doing some work with a friend and colleague of mine, George [E.] Leroi. So I met Jim and his wife, and we hit it off, and we had several nice interactions there. So when I saw Jim Morrison’s name on the program **<T: 20 min>** at ASMS, I thought, “Great, there’s somebody at least I know and I can talk to.”

So Rick and I, and Jim Morrison and Austin [L.] Wahrhaftig. It turns out that Jim Morrison had an adjunct appointment at this point with—or a visiting professor appointment—at the University of Utah, and he would come and spend [his] winters here, our summers. Anyway, he would come and spend some time just about every year at the University of Utah. He and Austin were sharing a room. Rick and I were sharing a room. After dinner, we went back to Morrison and Wahrhaftig’s room and we’re sitting around and talking. And so Jim says, “Chris, you’re not a mass spectroscopist, what are you doing here?” We explained to him what it is we’re trying to accomplish and that we had this problem of trying to get ions fragmented between two quadrupole mass analyzers. And he said, “Wow.” You know, “I don’t know.” Well, “Jim, what are you doing?”

“Well,” he says, “As a matter of fact, I have two quadrupole mass analyzers hooked together, and I’m studying photodissociation. And so what I’m trying to do is use photons to dissociate the ions between these two analyzers.” [. . .] He’s [principally] a physical chemist, and he wants to [study the process of ion photodissociation]. He’s not thinking of this as an analytical technique at all. [. . .] He says, “I’ve got my laser [beam collinear through my ion selection quadrupole and into the RF (radio frequency) quadrupole I use to contain the fragmentation products]. I have a separate quadrupole to [mass select] the fragmented products.” Well, that was interesting. And then he said, “It’s really not efficient.” He said, “Despite my pulsed laser and all the rest of it,” he says, “I have to use synchronous demodulation.” Do you know what that is?

DOMUSH: I don’t.

ENKE: Okay, it’s that you pulse the laser, and then you time your detection in synchronicity with the pulse of the laser, so that you can see the little extra bump in synchronous time that you get from the pulsed laser. And he said, “That’s what I have to do in order to sort the ions that my photons are making from all the background.” And I said, “What background?” [laughter] “What do you have for background?” And he says, “I have fragment ions when the light’s off.”

I said, “I want those.” [laughter] We realized what his . . . something was going on in his instrument that was producing fragment ions from the ions he was selecting in his first quadrupole, and it was doing it at a rate that was troubling to him, that was huge, in fact, compared to his intended method.

Well, we spent the rest of the evening trying to figure out why that was happening, because he believed right along with everyone else, you needed thousands of volts to get any kind of efficiency in fragmentation. So we were thinking is the RF [radio frequency] in that center quadrupole somehow or another giving him enough energy, or what is it that's going on?

And so I couldn't sleep that night. It was really an incredible time. I said, "My God, that's it." I woke up the next morning. I said, "Rick, you're going to Australia." [laughter] And so we got Jim and we said, "Jim, we'd just really love it if Rick could come to Australia, and we could run some experiments on this machine and study this fragmentation process that's going on without the light." And he said, "Gosh, that'd be terrific." So, we arranged it . . .

DOMUSH: Sorry, can I interrupt?

ENKE: Sure, please.

DOMUSH: Before Rick went to Australia, that first night at ASMS just talking about this, did Austin Wahrhaftig have any contribution to this? I mean because he was also involved in mass spec **<T: 25 min>** and obviously he was at ASMS, but the work he was doing at Utah?

ENKE: Yeah. Right. You know, I don't remember him having any substantive input. I know he was enjoying the conversation and chiming in from time to time, but I don't remember that.

So, Jim agreed for Rick to come to Australia to do experiments. Of course, we didn't have any money for him to make such a trip, so I got a hold of the program director at ONR who had funded our [project], and I said, "Can you get Rick on some Navy plane flying out there or something?"

He said, "Wow, that's a really unusual request. I don't know." I told him what was going on and what I thought was at stake here and he said, "I'll get back to you." And so he called back the next day and supplemented our grant with enough money for Rick to make that trip out there.

DOMUSH: Oh, wow.

ENKE: Yeah. So that was a really nice thing. And Rick went out there. One of Morrison's many contributions to mass spectrometry was the initial development of the program called

SIMION, which is an ion trajectory simulation program everybody uses to this day. And they did SIMION [simulations] to see whether the RF was giving enough kick to these ions to bring them up to fragmentation voltage levels, and it wasn't. Nowhere near it. Ten, 20, 30 volts maybe, possibly, max. It was all they could figure out that it was getting. So what was happening?

Nevertheless, what they did was, they . . . now, we want to do collisional dissociation in this middle quadrupole that's in Jim's machine. And in order to do that, you've got to introduce collision gas. And that puts pressure—gas pressure—inside the mass spectrometer, which is not a good thing. [. . .] If you get a collision while you're in the process of doing a mass analysis, that hurts that part, but also at some point, your detector is going to get [damaged]. So they brought the pressure up about as far as they could manage and got really nice fragmentation efficiency out of that.

So Rick came back with data and then set out to write a paper. We wrote a paper and submitted it to *JACS* [*Journal of the American Chemical Society*]. I sent a copy off to Graham, asking for his comments before I submitted it, and I'm still trying to figure out why I thought to do that, whether it was insurance or because he was such a nice guy and we'd had such a great conversation and so on. I'm not sure. But at any rate, he sent me back some comments and we revised the paper before we . . . so when we sent it in, it was already with Graham's blessing. And then the reviews came back, and one of them was clearly Graham's because it was exactly the same comment that we had already seen. And the other one was *scathing*, just absolutely . . . and, "Why do you know this? This is impossible. These data are wrong. This can't be happening. This is . . . and besides, you need to do these other experiments." This was just a letter, [not a full paper]. I wrote back a rebuttal that was longer than the letter that we had submitted, and the editor, playing Graham off against the other person, accepted the paper.[13]

DOMUSH: Do you recall who was editor of *JACS* at the time, who was . . . ?

ENKE: You know, I looked that up the other day, and I did discover it wasn't who I thought it was. I don't remember the name.

DOMUSH: Okay. Well, we can look that up.[14]

ENKE: I think I may be able to check on that later, if you'd like. Yeah. **<T: 30 min>**. So he accepted the paper and we published, and then we published another paper with Morrison and

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[13] R. A. Yost and C. G. Enke, "Selected ion fragmentation with a tandem quadrupole mass spectrometer," *Journal of the American Chemical Society* 100 (1978): 2274-2275.

[14] Enke adds: "The associate editor handling our paper was Robert L. Vold."

filed a patent disclosure.[15] And the patent disclosure, Michigan State didn't know what to do with it, so they sent it to the Research Corporation, and Research Corporation looked at it and then they called me up. It's quite a lot of work to file a patent disclosure, it turns out. And it was quite a lot of extra work to do it through Research Corporation because they had a much more extensive form to fill out in order to do all of this. And so they called me up one day and said, "Professor Enke, we've decided not to pursue this."

And I said, "Okay." You know, because I didn't really care about the patent. I've never gotten involved in patents before on things. No, I don't think so. Anyway, but, I said, "This took quite a lot of time. So before I do this again, please tell me what criteria you use in determining whether something's patentable or not or whether you think it's going to be worthwhile to patent it."

And they said, "Well, we don't like things that are just minor improvements over something that's already there because then we don't have the core patent."

I said, "Well, I understand that."

He said, "What we're looking for is something that's a really breakthrough idea."

And I said, "Oh, okay. Well, how do you test that?"

And he said, "Well, we go out to people who are using similar technology and ask them if they would find this worthwhile."

And I said, "Well . . ." I said, "But if it's really, really new, how are they going to know? If it's going to do something they've never done before, how will they know how valuable it is?"

And he said, "You know, I'll get back to you." So he called back a week later and he said, "We've decided to go ahead." So it very nearly didn't get patented.

Now the thing is—and this is really, really important—the patent is not for the triple quadrupole. Jim Morrison already had a triple quadrupole and the people who were writing the patent already know this.[16] What was patented was the fact that you could achieve efficient fragmentation of ions at low collision energies when you do it in a quadrupole containment. Okay. So that really was the patent. The patent described the application of this [collision process] between a quadrupole primary analyzer and a quadrupole secondary analyzer. So the configuration that was patented was the triple quadrupole, but the innovation, the novelty—was not the triple quadrupole—it was the low energy ion fragmentation process intentionally done

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[15] R.A. Yost, C. G. Enke, D. C. McGilvery, D. Smith, and J. D. Morrison, "High efficiency collision-induced dissociation in an RF-only quadrupole" *International Journal of Mass Spectrometry and Ion Processes* 30 (1979): 127-136.

[16] D. C. McGilvery and J.D. Morrison, "A mass spectrometer for the study of laser-induced photodissociation of ions," *International Journal of Mass Spectrometry and Ion Physics* 28(1978): 81-92.

for the purpose of chemical analysis. Now that was what was new. And that's what a lot of people don't really understand and why there's quite a lot of controversy. They think the patent was on the triple quadrupole, and it shouldn't have been because there already was one.[17]

This low energy collision process is the cornerstone of all tandem mass spectrometry instruments today, still. All MS/MS [tandem mass spectrometry] instruments use low energy fragmentation. It doesn't matter whether they're ion traps that do it sequentially in the same trap, or whether it's serially in space along with quadrupole, collision cell, time-of-flight, or time-of-flight, collision cell, time-of-flight. It's all low energy collision fragmentation because it's efficient, because it's effective and that was what the breakthrough was. So the entire **<T: 30 min>** MS/MS industry depended on this one patent, [even though it only covered the 3-quadrupole configuration].

DOMUSH: What was the response like from the rest of the mass spec community? I mean, you said that—you know, I mean the review from Graham, obviously you worked with him a little bit to kind of make sure that some of those questions in the paper were answered, but the other reviewer was scathing and said that this couldn't be. And certainly, the people that the Research Corporation went to say, "Would you use this? Does it make sense?" They didn't really get it or believe it to be true either, necessarily. So, you know, how did you deal with the response from the mass spec community and how did they slowly . . . or did they slowly come about to . . . ?

ENKE: It wasn't so slow. What happened was—and this is really pretty interesting—we published the paper and went to the next ASMS meeting in St. Louis, [Missouri]. And we were presenting. We were given a terrific spot on the program, which was the middle of the last morning of the meeting. Most people had gone by then. But nevertheless, we got on the program and gave our talk on this, and we were still now in the process of building the instrument. [Rick] came back, and now we know exactly what we want to do. We need baffles here, and another quadrupole and so on, and we're in the process of trying to get this instrument built.

So we're reporting on the data that Rick had gotten in Australia. And we had some fun. We were sitting around the pool outside and overhearing a conversation that was taking place a couple of chairs away say, "Do you know what some guys have done?" [laughter] "They've stuck two quadrupoles and they're—" You know, and it was really fun listening to that conversation. That was neat. But then we gave the paper. Didn't really know much about the response. There were only a few people in the audience and so on. But then, about two weeks later, I think it was—and Rick still has this letter—about two weeks later, we got a letter from Don [Donald F.] Hunt, University of Virginia. Don Hunt. Opened it up, and here's a collision spectrum [with the words "It works."] Boy, did that light a fire. He had a really, really bright person working in his lab, Jeff [Jeffrey] Shabanowitz, and Jeff destroyed three operating

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[17] R.A. Yost, J.D. Morrison and C.G. Enke. Tandem quadrupole mass spectrometer for selected ion fragmentation studies and low energy collision induced dissociation therefore. U.S. Patent 4,234,791, filed 13 November 1978, and issued 18 November 1980

quadruples and stuck them together to make this work and got it done in just a couple of weeks.[18] And he was off and running and Don was starting to publish analytical papers, you know, identifying this particular part of a mixture of crude oil, for heaven's sakes. I mean, it was really, really nice. And it also turns out that Graham went immediately back to the lab, and he didn't have three quadrupoles, but he wasn't sure he really needed that middle one. And so he had stuck two quadrupoles together and was trying to work this. I mean, it just went viral.

And I realized that I really needed to get going. This was going to be [a race and] I could lose it quickly. And so we did. There were a lot of interesting little things. It seemed so obvious in hindsight, but how do you get a normal spectrum when you have a tandem mass spectrometer [with an operating collision cell without removing the collision gas]? How would you get just a normal mass spectrum? And the way that they did it in the sector instruments was that they had a little deflector that they could turn on to a second detector that they put right after the first analyzer. So if you wanted just an ordinary spectrum, you sent the beam off that way and if you wanted your tandem spectrum, you send it through. **<T: 40 min>** And we were trying to figure out how to put in this second detector and what to do and so on. And then one morning, I realized we didn't have to do that. All we had to do was run the third analyzer in RF-only mode, so all the [ion masses] would get through, and it didn't matter whether they were fragments or not. They started out as the mass you selected, and you could get a mass spectrum without any problem.

Well, now it's like, "Duh." But at the time, you know, there was just those little things that we kept . . . because when you're in an area where something is totally new, a lot of the things that you learned are in the way. And so anyway, so then it went hot. It went hot, and Graham and Don and I were on the stump at just about every meeting, which was good, and we were able to keep up. And we wrote some definitive papers on doing ion structure, organic structural analysis using the tandem quadruple because you could do hard ionization in the first, and then get all the normal mass spec fragments, and then you could fragment those. So then you could see which sub-fragments the fragments came from, and it shortened the whole process of organic structural analysis just collapsed, you know, six months' worth of work into an experiment.[19]

So that was nice. And then the mixture analysis work was good too. Other people were way ahead of us. They had operating quadrupole instruments. They had instruments that had been [commercially engineered]. They had ion sources that were commercial. We were still hobbling along with homemade stuff. It was hard for us to keep up at that time because other people had . . . who were already in the mass spec area [had instruments that] were more versatile than ours. So, but at any rate, we [kept at it].

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[18] D. F. Hunt. S. K. Sethi, J. Shabanowitz, "Studies of negative ions by collision-induced decomposition and hydrogen-deuterium exchange techniques," *Environmental Health Perspectives* 36 (1980): 33-8.

[19] R.A. Yost and C. G. Enke, "Triple quadrupole mass spectrometry for direct mixture analysis and structure elucidation," *Analytical Chemistry* 51 (1979): 1251A-1256A; R.A. Yost, and C. G. Enke, "The fragmentation of the ions of nonan-4-one: a study by triple quadrupole mass spectrometry" *Organic Mass Spectrometry* 16 (1981): 171-175; R.A. Yost and C. G. Enke, "An added dimension for structure elucidation through triple quadrupole mass spectrometry," *American Laboratory* (June 1981): 88-95.

Now, it turns out, interestingly, that Morrison's was not the only triple quadrupole that existed at that time. When Morrison [took] one of his first trips to Utah, he was talking to Jean Futrell. Jean had been studying ion molecule reactions and was interested in ion fragmentation process, collisional fragmentation. He'd been doing this with sectors. When Jim met Jean, he suggested . . . and at this point, Jim's triple quadrupole instrument was a conception in his mind. He hadn't actually built it yet, but he was planning this, he had it in mind to do this. So he suggested to Jean, "Maybe you'd like to use this approach for your ion fragmentation studies." And Jean apparently thought that was a good idea, so he built a triple quadrupole, but, in fact, he didn't use it for ion fragmentation study—collisional fragmentation studies. He used it for what Morrison was trying to use it for. He [. . .] got interested in photodissociation and thought he would do that. He basically built a competing instrument with Jim Morrison's and published first.[20]

DOMUSH: Oh wow.

ENKE: But Morrison didn't **<T: 45 min>** know about that paper. He was still working it out and he published . . . the instruments are somewhat different and both of them were present in that history that I . . . that historical chapter that I sent you.[21] And so then he knew . . . he had the same background problem that Morrison had. I've talked to him since. He said it was awful. He knew it was from collisional dissociation, but [for him, it was just a problem. He gave it to] Marv [Marvin L.] Vestal. And then Vestal went off to [University of] Houston, I think, and took the instrument with him. Vestal—somebody told me the other day or maybe I read it—Vestal said that he knew that collisional dissociation was causing all those background ions and he thought they were a royal pain in the ass. [laughter]

So, I think this . . . we were talking yesterday about how different kinds of chemists think about things in a different way. Here is a really prime example. I mean, if . . . because Marvin and Futrell, Morrison, all physical chemists. They're studying this physical process. They want it neat and clean. They don't want anything else going on. They want to be able to study just that one interaction that they want to characterize it. So the fact that there was something else going on there that would be of tremendous benefit in the analytical application of tandem mass spectrometry never clicked. See? And I think there is a way that people think differently. So, I came into this from one side needing a process that was already there, in a way, but hadn't been exploited and hadn't been developed to be optimized.

[. . .] Fred [W.] McLafferty grabbed me at an ASMS meeting and said, "Chris, what's going on here? This can't be. Where is this fragmentation coming from?" and so on. He was quite upset because he was building, at that time, [a tandem double-focusing sector instrument].

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[20] M. L. Vestal and J.H. Futrell, "Photodissociation of methyl chloride ion and methyl bromide ion in a tandem quadrupole mass spectrometer," *Chemical Physics Letters* 28(1974): 559-561.

[21] C.G. Enke, "Development of Special Techniques: Tandem Mass Spectrometry," in *Encyclopedia of Mass Spectrometry,* Vol. 9, ed. Keith Nier and A. Yergey (Oxford: Elsevier, 2011).

One of the problems with tandem mass spectrometry in [a two] sector instrument was that the magnetic sector had resolutions of a thousand or two [Daltons]. The electric sector had resolutions of [only] twenty or thirty. Not good. Together, they made a high-resolution mass spectrometer, but separately, they were not superb. So [in one of the sectors] you had pretty good resolution, and the other one not. And you could arrange it, you could put the electric sector first and have poor resolution of your precursor and good resolution of the product, or the other way around, but you couldn't get good resolution of both. And that's the thing that really got people excited about the triple quadrupole, because you got good resolution of both, all of a sudden, in a simple instrument.

Recognizing the resolution problem of where would you rather have your poor resolution, you know, for the precursor or the product, recognizing that this was a problem, McLafferty was designing a tandem double sector instrument. So he was going to have electric/magnetic, and then the collision cell, and then the electric/magnetic [for four sectors all together. Fred] had actually gone to Australia and spent time with Morrison right next to his triple quadrupole using SIMION to design his tandem instrument. [It is ironic] that what he was trying to accomplish was already going on right next to him, [albeit in the background noise]. **<T: 50 min>** It was waiting for us, I guess.

HUNTER-LASCOSKIE: So it seems like you went from a . . . you're talking about when you're initially thinking about this, you and Yost are unknowns in mass spec. And you're going from unknown to, kind of, at the middle of people just really taking hold of this technology and trying to do whatever they can with it. So it, kind of, skyrockets in a very short time.

ENKE: Yes. Exactly. I remember when we were trying to tell Jack [F.] Holland, who was in the mass spec facility in the biochemistry department at Michigan State, what we were trying to accomplish. He said, "You're nuts. You've got to crawl before you can walk. You can't just do this, you know, complicated thing in a field you don't know anything about." So you're right. [. . .] And curiously, here's another little sort of psychological twist. Rick was accepted as a mass spectroscopist right away because he did his thesis in mass spectrometry.[22] I didn't. So I wasn't. [. . .] I had to battle my way into the area. Now mass spectroscopists are a wonderful bunch. They really are absolutely fantastic, warm, accepting, sharing, really, really terrific, just nurturing to a man—person. And so it wasn't that much of a struggle. But certainly getting funding was. Getting our first paper published was. But after that, things ironed out.

DOMUSH: Now did you know any of the people from your experiences at Pittcon? Had you interacted with any of the people from ASMS just, kind of, casually at Pittcon? Because I know there is great overlap between people who attend ASMS and people who attend Pittcon.

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[22] R.A. Yost, "Selected ion fragmentation with a triple quadrupole mass spectrometer" (Ph.D. dissertation, Michigan State University, 1979).

ENKE: Yeah. No, because I wasn't interested in mass spectrometry before we got into it. In fact, mass spectrometry wasn't really a central part of analytical chemistry [. . .] at that time. There was electrochemistry, chromatography, and that was basically it. Electrochemistry and chromatography. Spectroscopy, sorry. I knew there was another one.

Those were the three areas that were covered in analytical chemistry. Mass spectrometry now, of course, is central, but at the time, the analytical application of mass spectrometry wasn't very big. Petroleum industry was using it for, kind of, empirical identification of different types of crudes. It really wasn't a strong analytical tool. Organic chemists use it for structural analysis, basically, because you get the fragmentation pattern of the molecules or ions that you made. Wasn't very sensitive. Wasn't very efficient. [Finnigan, now Thermo, was making a good go at environmental applications with a single quad. People were working on using mass spectrometry for chromatographic detection, so the analytical applications were developing. But MS/MS gave mass spectrometry a good boost] on the analytical scene, and I've been a happy mass spectroscopist ever since.

DOMUSH: So, I mean it sounds like the mass spec community was really excited about this pretty quickly. After, kind of, that first attempt at getting funding, after that first paper, was it easier to get funding? Were the other papers more easily accepted by the journals?

ENKE: It was easier to get funding, but it was still very difficult to get an instrument. And so I mean, it was hard to get somebody to cough up several hundred thousand dollars for an instrument. And I have to say instrumentation building, building new instruments, has been a very, very difficult funding game all along. It's much easier to get funding to solve a particular problem with an existing technique than it is to be developing new techniques. One of the problems is you don't have an immediate payout goal in sight. **<T: 55 min>** The other is, it's somewhat risky. [. . .] And there's just a perception among some people of tinkering, and so it's a hard game. And it's also a hard game because you can spend tons and tons of student months just battling trivial problems, problems that aren't going to get you a paper because they're just practical engineering kinds of problems.

DOMUSH: Now in building the instruments, how much of it—or any of it—were you doing versus your students? I mean, it sounded like Rick was doing a lot of the building. Were you actively participating in any of the kind of physical building as opposed to just kind of the theory and discussions about the building?

ENKE: The hands on? No, I was involved in design, but Rick and the machine shop were involved in sticking it together. So I was following it all very closely. But I had probably ten people in my group at that time. I was trying to work on funding and everything, so most profs at that point in their career are not too hands-on, but they can be "minds-on," and I certainly was with this project. I knew what was important.

DOMUSH: Now did it seem like your lab and your—the research of your lab—kind of, very quickly shifted over into doing mass-spec oriented things? Or did it continue . . . did it have multiple research paths that it was on?

ENKE: The students who had started off on other projects, of course, kept doing them until they were done, but all of the new students that came in were doing . . . starting to do mass spectrometry. Yeah.

DOMUSH: Were you getting different students? I mean, obviously new students, but were you finding that you were getting students who were maybe physical chemists as opposed to analytical chemists now?

ENKE: No.

DOMUSH: Okay.

ENKE: Not particularly, but no. As far as I know, it was the same sort.

DOMUSH: Well, we've been talking for almost an hour, and perhaps it's a good time to take a break for a couple of minutes and get some water and things like that.

ENKE: Okay.

[END OF AUDIO, FILE 2.1]

ENKE: I wanted to say another thing about the history that I've just given you about the origin of the triple quadrupole. Having the responsibility for writing this up, the history of MS/MS, tandem MS/MS, for the *Encyclopedia of Mass Spectrometry* I took very seriously. And I knew there were [several] versions of the story out there, and there were a lot of players involved, and I wanted to make sure that I wasn't writing something that they would find controversial. And so when I wrote the draft of this, I sent it to Rick, I sent it to Jim Morrison, I sent it to Jean Futrell, to Graham Cooks, all for their input on it. And I got some input back, and I made some changes and so on, and what I'm telling you is exactly what I got from them. The major players who were part of this have all signed on to this. This is the story. It was just last year somebody published an erroneous history. [It came out in *Laboratory Equipment Magazine*]. And I wrote the editor, Tim Studt with a correction, and he promised to publish it and didn't.

So it's still there. I think a lot of people probably think that we did very well with the patent on the triple quadrupole. And Research Corporation did do very well. The arrangement that we had Michigan State gave 15 percent of the patent proceeds to the inventors. It's now much higher at most universities, but that's what it was there at the time. But Research Corporation took half. So this is now 15 percent of the half that goes back to Michigan State, right? And there were three inventors because, of course, we included Jim Morrison as an inventor on that patent. And so it ended up that each of us got fifty dollars for each instrument that was sold [in the U.S.], which is nice. I'm not saying that I feel at all slighted by this. It's just that the perception that anybody got rich over this, any of the inventors got rich over this, is wrong.

And at that time, Finnigan [Instruments Corporation] adopted it right away, and [AB] Sciex. Those were the two companies that came out with it right away, first. There was a European company that began to do it also, but they decided they didn't really need to have the patent. They just did it. And there wasn't enough money in the instrument business those days [for litigation]. If a company sold a hundred mass spectrometers of a particular model in a year, that was good. That was really good. So, Hewlett-Packard [Company] looked into it, but they don't like using other people's patents. And so they didn't come out with a triple quadrupole until the patent expired.

DOMUSH: Oh wow.

ENKE: Now fifty dollars an instrument would be wonderful because they're selling like hotcakes. It is still the version of [tandem] mass spectrometer most widely sold. It has the highest volume still. And the reason is even though it's limited in terms of the data production rate, it is the most sensitive and the most selective and the best for quantitative analysis of all of the tandem versions that are out there. And when electrospray ionization came along, it just went crazy.

HUNTER-LASCOSKIE: Can you talk about that transition, you know, obviously, happening from triple quadrupole to moving into electrospray? Obviously, that was big deal starting in the eighties in the mass spec community. You know, what was that transition like for you moving toward that path of research?

ENKE: Well, I thought electrospray ionization was interesting. **<T: 05 min>** [But] I wasn't particularly interested in [doing] it, and other people were developing it. I wasn't interested in big molecules. I'm still, kind of, pretty much a [fundamental] chemist. I took on a minor in physical chemistry and physics in graduate school and, as I told you, when I told Herb what I wanted to work on, it was something fundamental. And so I wasn't all that interested in [doing] it, but I was watching it just because I thought it was an interesting process.

I was at an ASMS workshop in Florida, and electrospray was one of the main topics coming up there and they were . . . some people were talking about what was going on, and how everything was all just [roiling about in the drop as it evaporated]. It was because [it was in the atmosphere] only ten microseconds or a hundred microseconds [. . .] and the solution was evaporating so rapidly and so on. And I raised my hand and I said, "But surely all the charge is on the surface of the drop." "Oh, no. That's absolutely impossible. It's way too dynamic a situation," and so on.

And I went home, and I said, "Okay, there's something needs to be done here." Okay, because now this goes back to electrochemistry. And this is part of my philosophy: nothing is wasted. Because here I am, I've abandoned electrochemistry in a way, but it's still here. And I knew about mercury drops and charge. [. . .] I had done work in rapid electrode kinetics. I knew that the double layer formed in fractions of a microsecond. I mean, ten microseconds was an acre of time to me in terms of where the charge was going to go. This was no problem.

Then what happened was [an interesting story here], unusual. A student of mine, Calin [G.] Znamirovschi—and you'll have to look in my vitae to get the spelling on that—was really interested in inorganic chemistry and he decided that he wanted to use electrospray to study the ratio of complexed to uncomplexed metal ions in a complex mixture. Interesting. And I like equilibrium stuff, so that appealed to me, and we were looking at it. Two things were going on. One was, in order to get any spectrum, he had to use [millimolar] concentrations, which are huge for electrospray, just out of sight. And the other is, the fraction of complexed mass versus uncomplexed mass, and he was using silver ions and crown complexes, was totally wacky. I mean, completely upside down, orders of magnitude just completely wrong.

So, I thought that there was some huge selectivity process going on in the creation of the electrosprayed ions and that that was why we were getting what we [saw]. I think that interpretation of the data is wrong now. I think that the reason we were seeing so many silver ions is that the complexes were getting fragmented back into silver ions in the collision process. But nevertheless, that's what I thought, and that's what got me into it, and I started to think about what would make . . . what [selection] process would be going on? Well, if all the charge is at the surface of the drop—excess charge. Okay, there's lots of charge in there, but the excess charge is at the surface of the droplet. And that has to be . . . this is going back now to taking electricity and magnetism in graduate school. [. . .] You got a conductor. The excess charge is at the surface, period. [That was rule number one]. And I knew from polarography, and studies with the double layer and so on, exactly how that was going to get set up.

So, I thought, well, if all the excess charge is at the surface, then . . . and the ions that are formed are formed out of the excess charge, **<T: 10 min>** they're going to be coming from the surface. So probably the things that are at the surface, preferentially, are going to be preferentially ionized. So let's think about surface activity. So if we've got a combination of surface activity and the capability of accepting the excess charge—in other words, proton affinity—then those are the things that are going to be preferentially ionized.

And now at this point, I didn't have any data to work with. We didn't have [a working] electrospray ionization setup. I talked to Gary [L.] Glish, [now at the University of North Carolina], who did some early work on electrospray in Graham's lab [. . .] about building one, and he said it's pretty simple. But meantime, I got a hold of [a paper of Paul Kebarle's], and thought, well, I can work this up. He's already done the experiment that I need to do. And so I took Paul's [plots] and tried to get a hold of the raw data, but I couldn't. I had to read it off his graphs. [I] processed it and came up with the idea that the surface activity was like a partition process. Molecules would partition to the surface according to their surface activity. And so I could treat that as an equilibrium, so I had a charge balance equation, and I had a mass balance equation for the analyte and also for just the charged solvent, and worked that out and fit it to his data. And then I fit it. [It was] not too bad because it was just quadratic. But then when you put in an extra analyte, it becomes cubic. Well, okay, so I solved the cubic equation and fit his [two-analyte] data. And I did it with just one equilibrium constant for the partition coefficient for each of the analytes. [. . .] He had done this with tetra-alkyl ammonium salts and the one with the longer chain had the higher surface activity and had the higher response and it just started to fit beautifully. And so I wrote that up, and I did that by myself. I didn't have any graduate student on it at all. Hours and hours and hours of [algebra, developing this theory].[23]

Again, I got one nice review, sort of, "Well, this is interesting." One absolutely scathing, "You don't know what you're talking about. You should start at the beginning and work up from fundamentals." I presented the paper at ASMS, and the entire discussion period was taken up by somebody in the audience ranting that I hadn't taken into account all that was going on there and I really didn't know what I was talking about and I should just stick to my guns and leave well enough alone. I [presented] this even earlier at Graham Cooks's award symposium at ACS [American Chemical Society] when he got the analytical award. And Fred McLafferty was there [and] said, "Chris, I don't know where you're coming from. That's just not the way it works." And so again . . . but people saw the practical application of it and so it started to get quoted. And then Paul Kebarle decided that Albuquerque [New Mexico] might be a better place to spend a winter than Edmonton, [Canada], so . . . Calgary, [Canada]? Where is Paul? Anyway, someplace very cold.

DOMUSH: I think Calgary. I think Mike Grayson told me he's headed out to Calgary, if I remember.

ENKE: I think that's right. I think it's Calgary. Yeah. So he came and did a winter sabbatical in my lab, and he's sitting in his office and he's studying my paper. And he said, "Chris, I still

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[23] C.G. Enke, "A Predictive Model for Matrix and Analyte Effects in Electrospray Ionization of Singly-Charged Ionic Analytes," *Analytical Chemistry* 69 (1997): 4885-4893.

don't see the difference between what I've proposed and what you've proposed for this." And he had a two-solution proposal. [. . .] In the linear range **<T: 15 min>**, you were having this process and when you got into the competitive range, you were having [a different] process. And so he had two separate things going on, and I said, "Paul, it's a one parameter fit."

"Oh." Now, this is . . . I take this very seriously because it's easy to say, "Paul, what were you thinking?" I mean, I put it in the paper, but I look back on that and I say, "I didn't put it in the paper correctly." I didn't make enough of it in the paper. [. . .] [If] somebody who really is super smart knows exactly what they're talking about, is fascinated by the process itself, is studying the paper and misses it, whose fault is that?

So, that's . . . I think I keep getting reinforced that when you're breaking new ground, you really have an incredible teaching obligation going on if your message is going to get through. Because people are stuck in thinking what they've been thinking. And they're going to keep thinking about what you're presenting in light of what they already know. Can't blame them. [Unlearning is sometimes harder than learning].

DOMUSH: Now when you have something that's an innovative idea or an innovative new theory—like what you were presenting where people like Kebarle or McLafferty are stuck in kind of a different mindset. How do you deal with that in the sense that . . . you know, a couple minutes ago you said you know, sometimes the things that you learned get in the way of, kind of, accepting something new?

ENKE: Exactly, yeah.

DOMUSH: But when it came to electrospray, you were going back to something that you had learned in electricity and magnetism in graduate school, so you know, there's kind of this divide between sometimes you have to set aside things you know and sometimes you have to really go back to some of the fundamentals of maybe what you know.

ENKE: Yes. Yes. That's absolutely true. I don't think you can be innovative without any—very often, unless it's quite accidental—without knowing what you're doing, without having some background. Although I have to say that I feel like I backed into mass spectrometry. I did not really understand mass spectrometry. I understood the analytical part of the process and we understood what we needed to get from these processes. I learned and studied enough about quadrupoles to figure out how they were going to help solve the problem, but in terms of having a nice solid background in mass spectrometry, I really didn't. And I think that was hampering to some extent to not have that. I've had to develop that since. I've never taught a course in mass spectrometry. Or written a book.

On the other hand, I feel like when there's something very fundamental and it's real—and we can talk about this later when we're talking about what's real and what's theoretical. Facts are facts. Explanations are explanations. And something like, in a conductor, the surfaces on the charge—or the charge is on the surface, that's a fact. It's proven over and over and over again in all kinds of experiments. And so you have to accept that that's . . . and if you get a result that doesn't take that into account, you have to think it over. Well, then Kebarle became a believer and started to publish, himself, along that line and ran into trouble as well.

An interesting byproduct of this, and I think it's something that's still to be done, I got to thinking about, then, what are the characteristics? Surface activity has to do with, sort of, having one foot in the electrolyte, and some arms waving around in the air. And so it's a partition. I got to thinking about chromatography as also being a partitioning, kind of, an operation.

And when we tried to use partition coefficients as measurements of surface activity, it did not work. **<T: 20 min>** And [I] realized that the difference between surface activity and partition coefficient is that in the partition coefficient, the molecule has to go completely from one solvent to the other, not just hang there in the middle with one foot in the polar end and the other part in the nonpolar part. So then I thought that perhaps the sensitivity, the response factor that different analytes gave in electrospray ionization might be related to their retention time in reverse phase chromatography. And it is. And Nadja Cech tried that out, and it worked, and we published a paper on that.[24]

Okay, well, the other [shoe] has not yet dropped. I'm not a chromatographer, but what I'm suggesting is that a lot of the partitioning that goes on in chromatography may not be complete absorption into the stationary phase. And I really believe that that's very likely to be the case, that you have the nonpolar stationary phase, the polar mobile phase, and that the nonpolar part of a molecule can stick up here. The polar part is in the polar phase. That part of the polar phase isn't moving because it's right . . . from mechanics and flow that flow's mostly in the middle and [at the surface], not at all. So it's like it's stuck. And it would be the same as if it had been totally absorbed, but I don't think that it needs to be, and I think that partition retention time and partition coefficient may not be as perfectly related as chromatographers think.

DOMUSH: Interesting.

HUNTER-LASCOSKIE: That would be another, probably, hard-to-swallow paper for many chromatographers.

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[24] N. Cech, J. R. Krone, and C. G. Enke, "Predicting electrospray response from chromatographic retention time," *Anal. Chem*. 73 (2001): 208-213.

ENKE: Well, I'll tell you another one that's very, very simple and it's absolutely true, but yet you will not get chromatographers to agree to. In fact, there's two more. One is that in isocratic elution, line broadening is due to longitudinal diffusion. Okay. Because it's been on the column longer, the peak can spread out. It's not true. Very, very little of it is due to longitudinal diffusion. What it is due to is . . . it's due to the fact that if you've got an analyte that's not well retained, what's its velocity through the column? [makes whishing sound] What's its velocity past the detector? [makes whishing sound] If you've got something that's fairly highly retained, what's its velocity through the column? What's its velocity through the detector? Same equation. Exactly the same equation as you get if you postulate longitudinal diffusion.

See, [many scientists believe that if] they get an equation that matches the data, then that must be what it is. The fact that there might be other equations [that would produce that same result, or other processes that could produce the same equation, that isn't often considered.] So we still have Van Deemter plots and all of this stuff going on based on longitudinal diffusion. Vicki McGuffin at . . . I was convincing Vicki McGuffin of this at Michigan State, and she was looking at liquid chromatography and she was using a spectroscopic probe to probe the band broadening on column before it came to the detector and demonstrated that what I said was right. But it's still not accepted, and you still won't find it in the textbooks.

And another thing that people [still don't accept is the] Poisson distribution of peak overlap. Very few people accept that—that you've got that much overlap that you've got hidden compounds and all of that, sort of, stuff, because they'll run the chromatogram and they'll see a peak. Okay? In the total ion current [plot], **<T: 25 min>** they'll look there and look at the mass spectrum, and that's a nice clean spectrum. So, what's the problem? Rarely [does it appear to be] a mixture spectrum. But the reason, of course, is that the components in the chromatograph example have a dynamic range of ten to the fifth or sixth [power]. So you can [easily have one] prominent compound and a bunch of other things that wouldn't show up. So I've argued with chromatographers over that for a while.

But anyway—but we do get stuck. We get seriously stuck, and it takes, you know, something like a bomb to get people to change their minds. We see that in all fields of science, and it's painful. And I think it's unfortunate. You were asking about that paper that I wrote on reuniting natural philosophy and scientific research. And if we understood, if we were a little more open minded about our explanations and realized that not only were they not unique, but they're probably wrong, we'd be looking for new ways to interpret our data rather than resisting them.

HUNTER-LASCOSKIE: Now while the electrospray research was going on, that's when you were . . . or had you already transitioned to New Mexico?

ENKE: That all happened here. Yep.

HUNTER-LASCOSKIE: Can you talk about coming? Obviously, you had been at Michigan State for quite a long time and then you, you know, you came out here.

ENKE: That's right. Well, the chair sent me a formal letter saying, "You've been here twenty-five years. You're eligible to retire." And I had little model on this. When Herb Laitinen got a letter like that, he said, fine, and went to Gainesville, [Florida]. Right? And he was a happy adjunct and so on there for the rest of his active years.

DOMUSH: With a much nicer winter.

ENKE: With a much nicer winter, right, than Champaign-Urbana. And so . . . and before that, it was . . . . I had his name on the tip of my tongue—organic chemist, invented rubber, synthetic rubber. It was a prof at University of Illinois, and when he got that letter, he went to Arizona. [Carl] Marvel. What was his first name? Speed. Speed Marvel.

So, also I think they'd heard all my jokes and all my ideas on how things could be better. And so, and by that time, families had dispersed, I'd remarried. And there really wasn't anything very strong holding Bea and me in East Lansing. [. . .] Except our jobs. And we realized that when we weren't working anymore, we're probably going to be someplace else. So whenever we traveled, we'd be thinking about where it might be nice to be. And then [we] weren't making very much progress on that, and Bea said, "How about your student who's out there in Albuquerque? You said you could get [an invitation to] give a talk out there sometime. Why don't we go look?" And so I thought, well, yeah, **<T: 30 min>** and maybe I could investigate having a sabbatical, and we could, sort of, check it out.

So Tom [Thomas] Niemczyk was the name of my student, and he was a joint student with me and George Leroi at Michigan State, and he came out here directly and was a [member] of the faculty here. And so I invited myself to come and give a talk and explore sabbatical possibilities. And we stayed at a nice little B&B [bed and breakfast] in North Valley for a week, and looked around and explored the area, and wandered around through old town and up on the mountain and up the Turquoise Trail.

And the last morning that we were going to be there, I remember it was a Friday, woke up and I said, "You know, it's really stupid to take a sabbatical here. [. . .] [Bea], as a psychoanalyst in private practice, [would] lose her practice. Then you have an obligation to go back to your home institution for a year after that at least. And so then [Bea] would have to try to reconstitute a practice and then . . . and I had been on a sabbatical before, and I did not like what happened being at a distance from my graduate group. There were a number of things that went on that really did not work out well for me or for the students. And so I thought, "This was really silly. I've got grants. Why don't we just move here?"

So, no, it was actually—it was the night before. Thursday. So, Tom Niemczyk and [Fritz S. Allen] and I went out to dinner with our families, and I said, “I’ve changed my mind. I don’t want a sabbatical. I just want a job.” [. . .] It was really nervy, but it turns out they had just had a failed senior hire in analytical chemistry. And Tom desperately wanted to take his sabbatical the next year and wasn’t going to be able to do that if they didn’t have enough people to cover the courses and so—timing. Timing was everything. So anyway, it all worked out. So we just decided to move. And it was a really, really nice thing. It really worked out well. I love the students here. I had a better time teaching undergraduate here than I’d ever, ever had before.

DOMUSH: Were you teaching graduate courses here as well?

ENKE: Oh yeah. Yeah. Graduate courses and undergraduate. Yeah.

DOMUSH: What made teaching the undergraduates here more exciting? What was it?

ENKE: They had this naïve idea that they were going to college to learn something. Not just to get a degree or pass a course, but to actually learn something. We figured it out. Bea understands this, too, from her psychoanalysis. There’s a huge difference in terms of what people are willing to put into it, that depends on who’s paying. So if the insurance company’s paying, it’s just something you go do. You know? “Fix me, doc.” And if it’s coming out of their pocket, it becomes—they take more personal responsibility. The students here are largely older. A lot of them have families and jobs. They’re taking classes part-time. They’re really . . . this is biting into their discretionary time and money, big time. [. . .] Just one evidence of it is [. . .] I had so many students in my office hours, they wouldn’t fit in my office. I had to get a separate room for it.

DOMUSH: Oh, wow.

ENKE: Now, that’s more people than I would see in a year at Michigan State. Each time. It was really wonderful. I enjoyed it. And the ethnic mix here is just incredible. The old Spanish families that have been here a hundred years before Jamestown, [Virginia], you know, we just don’t quit appreciate how . . . what history **<T: 35 min>** existed here before it became part of the United States. And that’s still here and it’s still very evident. And then there’s the Native American groups as well. And it’s just . . . it was rich. Just a really rich experience. Yeah. Made good friends, had a lot of wonderful students to work with. It really didn’t matter whether I was teaching quant, or instrumental, or what.

DOMUSH: Now, did you have graduate students that came with you?

ENKE: I did have some. I had some that had already passed their prelims [preliminary qualifying exams], so they graduated from Michigan State and went back and had their finals there—their orals. And then I had some that came with me, and their credits transferred, and they did their prelims and finals here. And then I got more students. And the interesting thing was that some of my very best students came here to work with me after I moved, even though, clearly, University of New Mexico [. . .] doesn't have the stature that Michigan State did, but really, some of my very, very best people that I've ever had worked with me here. So I've got quite a lot of nice work down here.

DOMUSH: And I would imagine slightly nicer winters.

ENKE: Well, you know, I grew up in Minnesota. It didn't bother me. The winters never bothered me, but the grayness did. You had three hundred thirty grey days in Michigan and three hundred thirty sunny days here.

DOMUSH: It's a nice switch, I think.

ENKE: Yeah. It was a nice switch. Well, the electrospray work did come to be accepted. A number of people did some other fundamental work using spectroscopy and suspended drops and a number of other things that have verified the equilibrium partition theory concept. That's been adopted by a number of people, and I think that it was a helpful move forward for the area. It's one my most widely cited papers. And then Nadja Cech was one of the principal students that I had working with me on that project. And so we ended up writing a chapter for the encyclopedia and also we have a chapter in Richard Cole's new book on . . . his second edition on electrospray and MALDI.[25] It, kind of, put us front and center into that field.

DOMUSH: Now did the electrospray ionization work . . . did it continue to be, kind of, thought experiments and theory, or was there experimentation or instrumentation . . . ?

ENKE: Oh yeah, we had a lot of experimentation coming out. Right. Exactly. Right.

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[25] N. Cech and C. G. Enke, "Electrospray Ionization: How and When it Works," in *Encyclopedia of Mass Spectrometry*, Vol. 8, eds. Richard Caprioli and Michael Gross (Amsterdam: Elsevier Science, 2006); N. Cech and C. G. Enke, "Selectivity in Electrospray Ionization Mass Spectrometry," in *Electrospray and MALDI Mass Spectrometry*, ed. Richard B. Cole (Hoboken, New Jersey: John Wiley and Sons, 2010).

HUNTER-LASCOSKIE: And you mentioned that paper is one of your most widely cited papers. Do you have any thoughts as to why that paper as to any other number of things you might have done?

ENKE: I think it opened up a new way of thinking about things and so when people are writing a paper that uses that concept—surface activity as being a driving factor in electrospray response—they're likely to cite that paper.

DOMUSH: Did you have any expectation at the time that it was going to be your most cited paper?

ENKE: I didn't mean it was the most cited. Probably the MS/MS paper is the most cited, but yeah, it's certainly one of the most. No, but it did occur to me afterwards that the most cited papers that I've had were the ones that I had the most difficulty getting published.

HUNTER-LASCOSKIE: So really the stuff that was challenging people's existing thought process?

ENKE: Yes.

DOMUSH: So how else did the research develop while you were here at New Mexico? I mean, in addition to the electrospray ionization work, what else were you guys working **<T: 40 min>** on?

ENKE: I was working on trying to get MS/MS information more efficiently. When your first mass analyzer and your second mass analyzer can each only pass one mass at a time, like in the triple quadrupole, then when you think about the entire data set that's available to you, you have intensity here, and you have first mass axis on here, and your second mass axis here. So you've got a three dimensional—two dimensions of [mass] parameter and a third dimension of intensity that's available to you. And in a tandem quadrupole instrument, you can get one point at a time. The nice thing about the tandem quadrupole instrument, however, is that you can scan in any direction or any combination of directions you want to when you scan. All that entire data set is available to you, scan by scan, but on the other hand, the time that would be required to collect the entire set would be prohibitive, generally, in terms of time and sample, and so people don't do that [very often].

Now if you combine the quadruple with time-of-flight, time-of-flight's a batch analyzer and so it gives you a complete spectrum every time it runs. So you can select your precursor mass and then run. With time of flight, you can get the whole product mass spectrum at one time. So now you can do a whole slice through this at once and you can scan the precursor ions and get the entire data set. People don't even do that very much. But it certainly is much more efficient.

More efficient still than that would be is if you had a way to use batch analysis for both. So we got interested in time-of-flight/time-of-flight, tandem time-of-flight. And we got interested in doing that about exactly the same time as Bob [Robert J.] Cotter did. And we designed almost identical instruments—instruments in which the flight path came here, went through a mirror, angled off through an interaction region, went into another mirror, and came to the detector, sort of a z-configuration, but we wanted to do the fragmentation with photodissociation.

It just keeps coming around, doesn't it? But we thought that if we could focus the ions at the interaction point and focus the laser right there, that we could really get some pretty high efficiency for the fragmentation. In fact, we were able to do quite a lot. So we worked on that and built an instrument and published some results.[26] But the collisional fragmentation was still more practical, and so then . . . so we weren't really competitive with Cotter's results. And also then Marv Vestal started [a company] to do this and so the tandem time-of-flight took off from there. But that was one of the things that we were working on.

DOMUSH: When did you start working on distance-of-flight mass spec?

ENKE: Okay, so Graham invited me to come to Purdue [University] and give a talk on ion mirrors. I don't even know why. So I was thinking about ion mirrors, and I was thinking about the possibility of ions retaining—as they got deflected into the mirror—of maintaining their position as they came to the detector, sort of, just like this. And so I imagined a time-of-flight instrument that had analysis of the ion mass **<T: 45 min>** in this mirror this way, but which in fact would retain the spatial information that the ions had coming in, so we could get mass information and spatial information.

And the other thing that I was thinking about was that in . . . if you fragmented ions, so here I am, I'm trying to solve this MS/MS business still to get all that information at a time and even TOF/TOF wasn't really doing it. And I thought, "I need to get all of this on a two-D detector." And so here's the problem. You have to create all the fragments right away, but then you have to have a means of telling which precursor each fragment came from. And that's the problem, right? I mean, you can make all the fragments right away, but then [sorting] them out according to their precursor is the thing that you end up having a problem with. So, what

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[26] D. J. Beussman, P. R. Vlasak, R. D. McLane, M. A. Seeterlin and C. G. Enke. "Tandem reflectron time-of-flight mass spectrometer utilizing photodissociation," *Analytical Chemistry* 67(1995): 3952-3957; M. A. Seeterlin, P. R. Vlasak, D. J. Beussman, R. D. McLane and C. G. Enke. "High efficiency photo-induced dissociation of precursor ions in a tandem time-of-flight mass spectrometer," *Journal of the American Society for Mass Spectrometry* 4 (1993): 751-754.

happens when you fragment an ion? Well, the interesting thing is the fragments continue to have the same velocity that the precursor did if you keep them out of fields afterwards.

So, if you fragment the ions, the fragments are going to have the velocity [of their precursor]. So now that's a clue. The fragment velocity tells you what the precursor mass was. Now, so then I realized that if we fragmented the ions, all of them, they would sort out according to their precursor's mass, so then if we put them in this time-of-flight [section], the precursor mass information would be in the position. The product mass would be in the time-of-flight. And we'd get it all. So, I can see . . . so I thought of this as a simultaneous MS/MS machine, and I wrote a patent disclosure and presented the concept at an ASMS meeting in Montreal, [Canada].

It was in the process of writing the patent that I realized that hidden in that concept was a new kind of mass spectrometer called distance-of-flight, that in fact [. . .] if you just took those ions and accelerated them directly to a detector, you'd get mass separation by distance. Not just a detector, but an array of detectors, right? And suddenly I realized this was a whole new kind of mass spectrometry, because the mass information is in which detector it hits, not in when it hits it. And you have a whole array of detectors to work with instead of trying to get all your information, and this is a huge amount of information we're talking about now, through one detector at the end of your flight path.

[. . .] [So] there's distance-of-flight mass spectrometry that's part of the patent.[27] But then I realized that in order for this to work, I had to have a means of focusing ions at their respective flight distances. And this is totally different from focusing ions at the detector. Now, time-of-flight is a very simple thing. Ions start in the source, and you accelerate them, and if you give them all the same energy, they'll all have the same velocity, so they'll all arrive at the detector at the same time if they're the same mass, right? But, in fact, they don't all start from the same place. And they don't have zero energy when you accelerate them. Some are going this way, some are going that way, [and] with different velocities. So that's what leads to defocusing.

You need a means of getting focusing. Time-of-flight was one of the very first **<T: 50 min>** methods of mass analysis that was ever invented, but it has lousy resolution unless you do these focusing things. So, now, accelerating the ions to a constant energy and focusing them at a single detector is something that's now accomplished universally by using an ion mirror to refocus the ions.

Getting all the isomass ions to hit the detector at the same time, having flown the same distance, is not the same as getting them all focused at the same time, having flown different distances. And so I realized that in order to really solve this problem correctly, I was going to have to learn how to focus ions in a distance-of-flight mass spectrometer. [. . .] I had a postdoc, [Gareth S. Dobson]. He was working on an instrument that . . . now, we had a grant from Sciex

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[27] C. G. Enke. Distance-of-flight spectrometer for MS and simultaneous scanless MS/MS. U.S. Patent 7,041,968, filed 18 March 2004, and issued 9 May 2006.

and they [gave] us an old instrument, an old Q-TOF [quadrupole time-of-flight mass spectrometer], which would have been perfect to do this in. And he was mostly working on the hardware by which we could accomplish the separation, and the detection, and so on. And I was mostly working on the means of focusing, how we were going to get ions to focus. I worked with SIMION. I wrote [programs in Microsoft Excel and Basic], Visual Basic to try to figure out how we could possibly do this. And I probably spent a year, maybe more, trying different things. Just sitting at the computer, and does this work, does that work, how about this shape of acceleration pulse, and that, and so on. And finally, things started to click. I began to . . . and all this time, I'm un-teaching myself things that I knew about time-of-flight.

I was conscious of doing this. I thought, "Well, this doesn't apply anymore. We've got to forget this." And then another thing would come up that I realized I was assuming incorrectly. And then finally it clicked. And when it clicked, it was simple, and it was exactly the direct complement of the way that you do it for time-of-flight. Uses an ion mirror. But it uses a different means of acceleration. That's a means called constant momentum acceleration instead of constant energy acceleration. And time-of-flight, it turns out, focuses the spatial dispersion of the ions as they start, but doesn't focus their energies. It achieves the energy focus by just giving it one hell of a jolt to begin with so that it tries to get [all the ions] lined up at the same time.

But the one thing that it doesn't do is that an ion that was moving first this way [backwards] now gets reaccelerated this way [forwards]. By the time it comes back to where it started, it has the correct velocity. But it's a little late. So that turnaround time is the principal source of defocus in time-of-flight mass spectrometers. When you use constant momentum acceleration—and I published this paper, also, in *Anal. Chem.*—when you use constant momentum acceleration, all the ions get the same momentum.[28] Turns out that now there is a time at which they will be spatially focused, energy focused, actually, at their respective flight time, flight spaces, and at that time, you can drive them to a detector array, and you've got a spectrum.

And it's the exact complement. It focuses the energy dispersion, but not the spatial dispersion, whatever spatial dispersion they had to start with, you end up with. Well, I think this has great potential for surface ionization techniques, because you don't have any ion depth to start with, but you have a lot of energy dispersion in MALDI [matrix-assisted laser desorption/ionization] and other processes like that **<T: 55 min>**. So I think that it has some good potential value in that direction.

I was excited about that, to find that, because it was . . . intellectually, it was probably the most challenging thing I've ever done, because [I had to break] all kinds of thought barriers in the process of getting there. It's something no one else has done and other people have looked at constant momentum acceleration before. It has the potential, I think, for really making array detection a good option in mass spectrometry. Now, array detection has several advantages. One

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[28] C. G. Enke and Gareth S. Dobson, "Achievement of energy focus for distance-of-flight mass spectrometry with constant momentum acceleration and an ion mirror," *Analytical Chemistry* 79 (2007): 8650-8661.

is the one I've already mentioned. You've got a whole bunch of detectors going. Each one of these is there to just detect one [ion mass]. So it can independently be accessed. It can [be] independently auto-ranging, so the potential for dynamic range in this is absolutely huge. There is no limit, basically, to the dynamic range one could accomplish with array detector mass spectrometry. So is that a problem? Do we need dynamic range?

So we sent off a proposal. By now I'm collaborating with Gary Hieftje at Indiana University, because Gary and Bonner Denton developed the array detector. That was what I needed was an array detector. By now I don't have a research group. I've retired, and my postdoc is gone, and Sciex has stopped funding. And so I start this collaboration with Gary, and he's got the detector. That's why this seemed like a nice match, and it has been. It's been a very nice match. But he's [principally] an elemental mass spectroscopist, and so his ion sources are glow discharge and ion . . . and ICP [inductively coupled plasma-mass spectroscopy] and things like that. That's what we've been using.

So it's actually . . . we're doing some really neat things. We've now done energy focus at a time-of-flight detector. We call that Zoom-TOF because it turns out that you can spatially focus a beam, accelerate it, and then even though you've introduced some energy components in the process of the focusing, those get canceled out by the constant momentum acceleration and you can treat the time-of-flight detector as though it were just one element in your spatial detector and get about a five or ten times improvement in resolution at the time of flight detector, but just for an narrow mass of ions. [. . .] [It turns out] for elemental mass spectrometry, this might be very good because there are argon oxides and things like that that interfere with some of the metals and are only, you know, a few thousandths of a mass unit separate from the ones that you want, but you can zoom in on that and separate out those interferences. So that's what they're working on right now, and I think it's pretty exciting.

DOMUSH: Do you have to go to Indiana to . . . ?

ENKE: I Skype with the students and postdoc there—and research associate that's working on this—every week.

DOMUSH: Oh, wow.

ENKE: I missed it this morning for you guys.

DOMUSH: Oh. Thank you. You said yesterday, you mentioned very briefly that you had a Skype collaboration with someone in Belgium just recently. Was that as part of the distance-of-flight work?

ENKE: Indirectly.

DOMUSH: Okay.

ENKE: When we proposed to build this instrument, and Gary is seeking funds for this, we went to NIH for an instrumentation proposal, arguing to them that this dynamic range advantage was going to be really, really important for complex mixture determination. You've got your abundant components, your ubiquitin and things like that, coming out here, but now how much . . . at what point is your detection, when you get down to your detection limit, what are you missing? And can you get [more] by increased **<T: 60 min>** dynamic range? Well, one argument is that if the detection limit is, of course, the signal that comes up over the background noise, what's the background noise? Well, the background noise is ions that are below the detection limits. So if you could move the dynamic range, this would really help.

And the proposal got very bad reviews. Some of them were from mass spectroscopists who said, "We already have ten to the sixth dynamic range." Which is true, as long as you can change the integration time. And you can't only do that for one mass—you know, particular masses. But anyway, if you change the integration time, you can definitely get ten to the sixth dynamic range. On the other hand, if you're using it as a chromatographic detector, you don't get to change the integration time, because you're taking fractions of the chromatogram that are coming along like that [snaps fingers]. So, that was not correct. And the chromatographers said that [. . .] dynamic range wasn't the problem. It was interferences, which is another way of saying we need more dynamic range, but they don't see it that way. So in any case, it didn't sell. And I realized it didn't sell because I didn't have a compelling argument. I couldn't say what was going on.

So I started to look into complex mixtures. How complex are they? How many complements are there in a complex mixture? What is their concentration distribution? If I knew what their concentration distribution is, then I'd know what was missing. So I started to look into the literature, and I read bunches and bunches and bunches of papers on . . . there are only a small set of chromatographers that are working on this, but Joe Davis and some Europeans that work on this sort of theory of chromatographic separation. So they kept referring to it as exponential. And I had heard this before, the lower you go in concentration, the more compounds there are going to be in this lower level, and it just goes up exponentially and they kept referring to this one paper by Luc [J.] Nagels.[29]

And I go to the paper, of course, and read it, and he's done this for lipids, plant extract lipids and painstakingly tried to decompile all the overlap peaks and stuff with a computer algorithm and so on and came up with some data. And he says he tried to fit it to an exponential

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[29] L. J. Nagels, W. L. Creten, P. M. Vanpeperstraete, "Determination limits and distribution function of ultraviolet absorbing substances in liquid chromatographic analysis of plant extracts," *Analytical Chemistry* 55 (1983): 216-220.

curve and wasn't very successful, but everybody quotes that paper when they say that it's exponential. This is a really interesting twist, isn't it? [. . .]

[END OF AUDIO, FILE 2.2]

ENKE: [. . .] Well, so I thought, well, I could start with Luc Nagels's data and see what it could fit to, but I was having a little trouble figuring out exactly what his columns meant for his raw data figures. So I found him by [internet, and] as he was beginning to explain more and more of his data, I thought this would be a nice collaboration. And so I asked him if he wanted to collaborate on this, and he said that he did. So we started a Skype collaboration and were working with his data. And it's clearly not exponential, so what is it?

And I had already come to the conclusion that it probably wasn't exponential because there is a way to make a new exponential curve that has a finite area underneath it, but it just didn't . . . . I guess in a Laitinen sense, it just didn't make sense. It made some sort of sense, but in another way, it didn't. I even put it in my textbook that it went up exponentially, that every time it went down in order of magnitude and detection limit the number of things to detect went up an order of magnitude. And I believed that. Sid [Sidney] Siggia had told me that, thirty years ago. This was one of those facts, right?

But I began to doubt it more and more, and so I started to try to figure out what it could fit to, and when I plotted it on a log-log plot, it looked like it was part of a parabola. And it did start to fit quite well to it, to a second order equation. Then I discovered that there's a whole literature about things in nature turning out to be log-normal. Log-normal is the ordinary Gaussian distribution curve where the x-axis is the log of the variable rather than the linear. It's called a long-tailed curve because if you plot it on a linear axis, it looks like this [traces the shape of log-normal distribution curve in the air]. And I thought that that was really interesting.

Then I started to try to fit it to a log-normal curve using statistical distribution fitting programs, but there is a problem. The statistical fitting programs will work with missing data which is, of course, all the things below the detection limit, but it needs to know how many data points it's missing. So it would have had to know how many compounds we weren't seeing, which, of course, we don't [know]. That's what we want to find out. On the other hand . . . and this is a thing that I've discovered, which I think is really useful, but it's going nowhere at the moment, and that is that I derived a set of equations that equated the sigma and central value of the log-normal distribution with the three parameters in the parabolic equation when you plotted it log- log. Now it turns out, did you know this, that if you plotted the normal Gaussian distribution on [semi-log] plot, you've got a parabola?

DOMUSH: I did not know that.

ENKE: I didn't either, but it does. Exactly. Perfectly. And so there's a correlation, you see, we can make between the part of the curve that we can get plotted on a parabolic equation. Take those parameters off, fit them into the **<T: 05 min>** log-normal distribution, and we already have an estimate for the number of points that we're missing, which we can get from analyzing the parabolic curve, and we can refine that. And we were able to fit his data down to three nines [99.9 percent] on [the log-normal distribution] and that's looking kind of good.

And so then I went and found a paper in *Anal. Chem.* that was metabolites that they had [analyzed]. This group had done [this] using two different instruments and getting different data. I thought, "Well, there's a big, rich data set. I could work with that."[30] And I also knew that Al [Alan G.] Marshall was doing all of these components in crude petroleum, and so I got a data set from him. And we fit the mold. Now, these are data sets not with just a hundred or so compounds in them, but thousands. And so all three of those data sets fit the log normal really, really nicely, and I published this paper. And with Luc.[31]

Not only that based on the fraction of the components that were below the detection limit in Al Marshall's experiment, I was able to tell him what his background noise was. I said you've got a background noise of 5 percent. He said, "How did you know that?" So this explains that [chemical] noise is just undetected compounds that are distributed over the spectrum. And I think this is . . . may turn out to be one of the most important things I've ever done. Because nobody has . . . it's a whole new field. It doesn't even exist [yet], theory of complex mixtures. I think it's real. I think that there's a huge amount to be gained from it.

I'm looking now at . . . and I'm probably within a month or two of having a paper out on what is the dynamic range and what are the detection limits that are needed if you've got a thousand, ten thousand, a hundred thousand, ten million component mixture. And you have fifty micrograms of sample or a hundred micrograms of sample or two micrograms. I started to think this gets quantized because these are individual molecules. The statistical programs are perfectly happy to give you fractions of molecules. But life doesn't do that, so I'm now correlating the facts, the quantization facts of actual analysis and the fact that you had to detect discreet events with what the statistics are telling me about what the component distribution is going to be.

And it's starting to turn out that dynamic range is way more important than sensitivity. And that is totally unintuitive to me. It's unintuitive to me that the component, the last components, the component with the least concentration in a fifty-microgram sample and a million components still has ten to the fourth copies. It is . . . . I would not have guessed that. I would have guessed that you've got to be down to looking at one copy or one copy every ten runs or something like that, but that's not how it is. Avogadro's number is huge. It's seriously huge. So even if you've got ten million compounds and they're distributed amongst even a

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[30] H. G. Gika, G. A. Theodoridis, M. Earil, R. W. Snyder, S. J. Sumner, I. D. Wilson, "Does the Mass Spectrometer Define the Marker? A Comparison of Global Metabolite Profiling Data Generated Simultaneously via UPLC-MS on Two Different Mass Spectrometers," *Analytical Chemistry* 82 (2010): 8226–8234.

[31] C. G. Enke and L. J. Nagels, "Undetected Components in Natural Mixtures: How Many? What Concentrations? Do They Account for Chemical Noise? What Is Needed to Detect Them?" *Analytical Chemistry* 83 (2011): 2539–2546.

millionth of an Avogadro's number of molecules, [the least abundant component still has lots of copies]. So it's a really, really interesting intellectual activity for me now and I can do it without a group and without a lab and I think that it's going to be very important.

And [this is] related to the distance-of-flight. The distance-of-flight got me in there. I said I may have to prove that dynamic range is an important thing or find out that it isn't and so that's the other science thing, besides working **<T: 10 min>** with Gary's group on the distance-of-flight instrument. We have an NSF grant—Gary's group does—for doing soft landing with distance-of-flight. One of the advantages of distance-of-flight, besides the fact that you have an array of detectors, is that you now have isotopic separation, or mass separation, of your components. And with electrospray ionization, we can be talking about mass-separating viruses. They're intact. Right? They can be collected using Graham Cook's soft landing approach.

DOMUSH: Well, unless there's more to say on that topic, Sarah and I were thinking that this might be a good time to pause for a minute and set up a video camera and talk about some of the other, kind of, big picture things that we wanted to talk about. In particular, kind of, the interest you've developed in history and philosophy of science.

ENKE: Yes, okay.

DOMUSH: And do some of that on the video camera if that's okay with you.

ENKE: Okay. Where would you like to do it?

[END OF AUDIO, FILE 2.3]

HUNTER-LASCOSKIE: Okay, so back after a quick break. And as Hilary mentioned, we wanted to spend some time talking about these bigger picture issues that have some up during the course of the interview. I'm curious what prompted your interest in the history and philosophy of science, and when that kind of started being something you were really interested in.

ENKE: Right. It actually came about in a sort of trivial way, almost. But I . . . it crystallized with my reading of *Zen and the Art of Motorcycle Maintenance*.[32] I know that's, kind of, a hokey book in a lot of ways. Nevertheless, he's got some really interesting points that he makes

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[32] Robert M. Pirsig, *Zen and the Art of Motorcycle Maintenance: An Inquiry into Values* (New York: William Morrow & Company, 1974).

in there about how science is done and there were a couple of things that really caught my attention in that book. And one was his discussion of [Jules Henri] Poincaré's assertion that if there's one explanation for a given set of events, there is an infinite set of explanations for that same set of events. And that really got me thinking because this gets right down to the whole idea of scientific theory. [. . .] Theories are based on empirical observations and making sense of them. That's what we do.

And I'd already run across a situation where, with a pH electrode, the first interpretation of the results that people got empirically from the pH electrode was that of selective diffusion, of protons through the glass. And it fits perfectly. That's what people believed. And so for a long [time], for several years, they tried to improve on pH electrode by making glass that the protons would diffuse better through and studying that process. And then finally somebody did it with deuterium isotope and demonstrated that the response was perfectly fine for the deuterium and there was no deuterium diffusing. So that sent people for another set of explanations. And another explanation for it is that it's selective adsorption and that's the one that we mostly go along with today, though that still isn't perfect; pH ends up being defined as that which a pH meter reads. That's our IUPAC [International Union of Pure and Applied Chemistry] definition of pH these days.[33] That's how much we understand.

But that is an example. It turns out that the Nernst equation, which the pH meter follows, is just the relationship between chemical potential and electrical potential. And so any means there is of converting chemical potential difference to an electrical potential difference is going to give you a Nernstian response. There could be others. There are others.

So, another thing that came up in *Zen and the Art of Motorcycle Maintenance* was the discussion of manuals, of motorcycle manuals. It got me thinking about writing and communicating because he said you don't start out a motorcycle manual describing the threads on the head bolt. You start with what the motorcycle does—what's its function is—and work down. And it introduced me to the whole concept of top-down learning and teaching as opposed to bottom-up, which chemistry is largely based on.

The third thing that was in *Zen and the Art of Motorcycle Maintenance* was the concept of gumption traps, and how you can sort of get your energy drained by getting stuck in places, and how to move your way out of them. And I saw that was of immediate value in operating and managing a research group. You know, I could see several gumption traps at work right away, in real time.

My research group [came] over to the house. Once a week [. . .] I dragged them through chapter by chapter reading *Zen and the Art of Motorcycle Maintenance*. We had wine and crackers and *Zen*. And so it was good. Rick Yost was in the group at that time. It was a time of high activity for the group, but it got me thinking not just about doing science, but how science is done.

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[33] *IUPAC Compendium of Chemical Terminology- the Gold Book*, Version 2.3.2, s.v. "pH."

HUNTER-LASCOSKIE: Do you think it's something **<T: 05 min>** that other scientists are paying attention to, or maybe they should be paying attention to a bit more, these ideas of multiple theories and how science is done and really thinking critically about their process?

ENKE: I think it would be very helpful. And there was a time when philosophy and science were not separated. Science wasn't called science then. It was called natural philosophy and people studying it were studying the arena of ideas and truthfulness and expression and things of that sort along with trying to sort out natural phenomena. It became separate with the rise of empiricism and the development of the term "science" and scientific research. So scientific research became this combination of empirical investigation and theoretical interpretation and became increasingly divorced from natural philosophy over the years [. . .]. The people who study how science is done and the people who do the science are quite different sets.

DOMUSH: You mentioned a couple of times today and yesterday that scientists and the public would be better served if there was a increased understanding of the difference between facts and explanation and real and theoretical understandings of things. And I was wondering if you could talk a little bit more about that.

ENKE: Well, I came to be thinking about what it is we do know for sure, and even if you take Poincaré's assertion that there may be multiple explanations; infinite, of course, is way too many. For us, it's finite, and it's finite because we have limited vocabulary, and we have a limited set of mathematical tools and so on. But if you imagine a universe, you know, and you imagined populations which grew up in a different context using different languages, different mathematical approaches and so on, you'd find that they were probably interpreting the same observations that we make, but they'd end up with different equations. Some of the things we [put in our equations], like entropy is something we constructed and potential energy. These are mental constructs that we have [invented], even though they work exclusively accurately in empirical equations that we've developed. I'm sorry. I lost track of the question. Oh, no.

DOMUSH: I was just hoping that you could talk a little bit more . . . it was kind of a vague question, but just to talk a little bit more about the understanding of facts, explanations, and real and theoretical.

ENKE: Oh, right. So back to Poincaré. If there are multiple explanations for a given set of observations, the thing that isn't changing when we think about that is the set of observations. Now, it may be that these are faulty, but it may be that they're accurate, assuming that we've made a set of accurate observations, or that they've been confirmed, or they're . . . in fact, I ran across a quote the other day that said, "No experimental observation should be believed unless it's confirmed by theory," which is a nice inversion of the way I've been thinking about it all.[34] But what it means is [that] when we're able to make an explanation of something, when we're able to develop a theory, what it means is that the facts that we used in coming to that explanation are consistent. That's really all that it means. But it does mean that. They're consistent. They're consistent with each other. They collectively make sense.

And so if you can take a set of observations, empirical facts, and make sense of them, that, sort of, confirms that this . . . that these observations are valid or real. And now is that the only explanation? It would be very nice if people understood absolutely not. And we expect . . . it seems to me a disconnect to argue that a particular theory is true, when at the same time, back here someplace, and in the back of the public's mind as well, we know that it's going to be revised or replaced. It will be.

I believe that my theory of equilibrium partition and electrospray ionization is useful. Do I believe it's true? Do I believe that it's *the* fact of how things work? No, I don't. I mean, **<T: 10 min>** the electrospray drop does what it does. We make an analogy of it; we make a metaphor or a picture best we can. Now, the fact that I've been able to [derive] some empirical equations that actually work, those things I think are valid. They work and they're going to have to be taken into account in any future version of somebody's explanation. You see?

And so if we're constantly backed into a corner of defending theories that we know are going to change, instead of demonstrating how it is that we believe what we believe, then I think that we're arguing on the wrong side of the argument. I think that . . . and the irony of it all is that [. . .] theories get all the attention, "Oh so and so has developed a new theory," scientists say. You know, we hear this all the time, and it's all about explanations and theories. That's the only thing that's satisfying, really, in the wrong sense, but when you think about what it is you learned when you were taking science classes, very, very little of it was theory. How many theories were parts of problem sets? Really? It isn't there. It's . . . so we study, and study, and study all of these empirical relationships that we've developed.

Now, the gravitational equation doesn't explain gravity. But it does [predict] the motion of planets and satellites and you name it throughout the universe. Well, not throughout the universe, throughout as far as we know. Philosophers in the past have not—by and large—have not made a clean distinction between what we don't know about explanations and what we don't know about empirical observations. They put them more or less in the same category because one cannot assert that they are universally true, either of them.

In terms of empirical observations, they are consistently true within the boundaries over which we've tested them and which we've verified that they work. Ohm's Law works just absolutely great with any conductor. With [junctions between different kinds of conductors], forget it. So, we know where these boundaries are for the empirical laws that we use all the time. And part of the refinement of working with empiricism is to define [with increasing

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[34] Commonly attributed to Arthur S. Eddington. Source unknown.

accuracy] or extend those boundaries. But that's . . . but within those boundaries, by gosh, it's true. It works.

And it works consistently, and we count on it or we couldn't cure disease or build bridges. And so this is the fundamental part of science that really works. It's the part that we spend 95 percent of our time teaching, and it is universally ignored in terms of this argument about what's true and what's not true. Is science valid? Is it not? And so that's the argument that I'm taking.

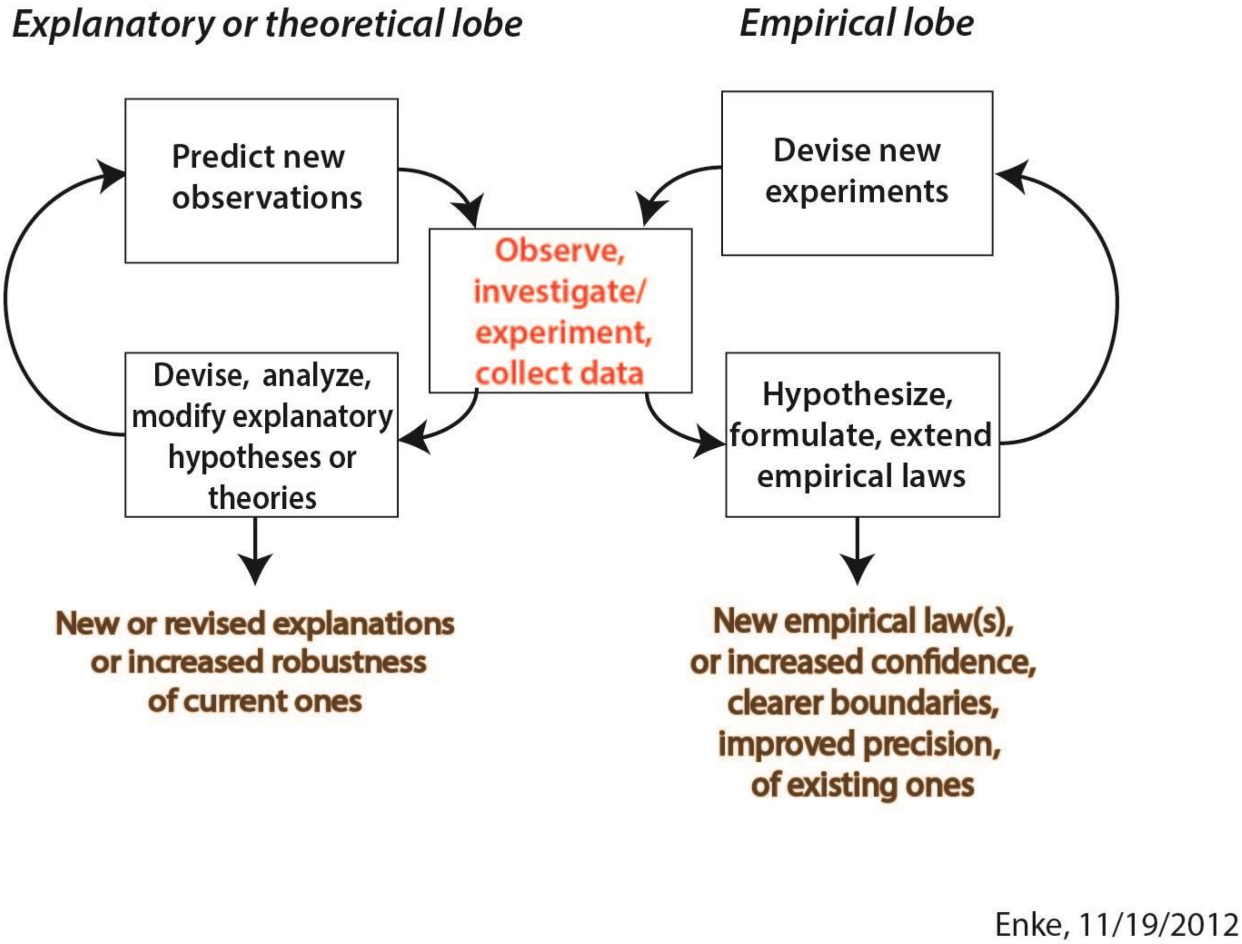
[. . .] I don't know if this is appropriate or not within this context, but I have been thinking about the diagram of the scientific method, and it's a circle, right? And you . . . what comes out? In virtually every diagram of the scientific method, what comes out is a theory. And so the empirical facts that went into helping develop that theory are just lost up here someplace. They're not really illustrated as a product. I think scientific knowledge comes in two parts.

I think it comes in the "what" and the "why." And I think these are very different from each other. In terms of the what, we have our empirical relationships that work, that we use all the time and [which] have boundaries. And these boundaries are what get tested in that form of knowledge. In the explanation part of it, in the why part, <**T: 15 min**> we have these models and metaphors that we produce that help us to understand why this is the way it is, and they build pictures that are useful for us and they're useful for predicting new tests of the theory and so on. But [. . .] the theory part of it, itself, is likely to be revised and may even be totally changed. That doesn't mean it wasn't useful. And theories can be . . . . I think theories should be tested, [and] should be measured on their degree of robustness, not on their degree of truthfulness, because that's not a test they can pass.

But robustness comes from, like, [Albert] Einstein's general theory of relativity. You make another test and by gosh, you know, there is [. . .] mass lensing and things like that. And so there is time dilation and we've tested those things. We've found them to exist and so that makes that theory increasingly robust. Theories that have no way to be tested, have no way to become more robust. And so trying to compare a theory that has no way to be tested and say that this theory deserves to be taught in science classrooms along with every other theory because all theories are equal, you see, isn't really a very good argument for a science class. You know, however useful that theory might be in other contexts, in terms of science, if it can't be tested, if its robustness can't be improved, [it isn't scientific].

So we were talking about beliefs. You know, if you don't believe in [global warming], then fine, you don't believe in it, but the thing is that the conjecture that it's occurring and that it's significantly caused or enhanced by man's activity, [which] is an explanation of the empirical observation that the warming is happening and it's testable. Its robustness can be improved, [. . .] and is.

I've made this diagram of the . . . what I would now use as a diagram of the scientific method [see below]. It has two outputs. We're producing two different kinds of things and it has two lobes. We have the explanatory lobe over here. We have the empirical lobe over here. What comes out down here is what we call laws or rules. What comes out over here are the explanations of why. And the reason that scientists say that they don't ever follow the scientific method is that it's just too simplistic. This has the complications in it. You can wander around in this figure eight. You can work over here. You can work over here. You know, [the idea] that you have to start someplace and end up someplace else, is, of course, all scientists know that's nuts. That isn't how it works. That isn't how we think. That isn't at all what happens.



DOMUSH: So you mentioned yesterday that there's a problem in the teaching of chemistry, in the structure of chemistry departments, in part because we haven't revised the chemistry curriculum in over fifty years. And I'm curious if you would like to see more than just the chemistry curriculum revised, but perhaps science education revised so that more of the general public understands the way that scientists actually think as opposed to perhaps a scientific method that doesn't really portray how science works at all?

ENKE: Right. There's several problems. One is we worry too much about cramming a certain number of facts into students in order to satisfy some curriculum requirements, in my opinion, and there was a study that showed up in [an email] **<T: 20 min>** blog that I get out of Stanford [University] a [number] of years back that showed that in terms of retention tested several years after graduation, there was no difference between BA and BS degree graduates in science. Which was really astounding when you think about it because the B.S. people have had some twenty more or more credit hours of extra intense stuff. Where did it go?

And so I really came to the conclusion that towards the end of my teaching career, though I didn't really know exactly how to do it, but more and more I tried, that it was important to impart to students how it was that I, as an academic or as a practicing analytical chemist, thought about problems. What it was that intrigued me, what my tool set was for going at a solution, and how it came to a solution, and then how I disseminated that solution out to the world and what difference I thought that it might make. I thought that was as important as any underlying set of facts.

[. . .] I thought, too, "Gee, just think, if a student went through college and they learned that same thing, [how they think], from an historian, and from a person teaching literature, and from an economics professor, and from an organic professor, how rich they would be." [. . .] Imagine the set of tools that they would [be] able to bring to any kind of problem that they [would face] in the future, not just a set of equations that might work, but actual ways of thinking about something. That would be rich. Wouldn't that be rich, indeed? And [. . .] we don't do that.

I'm thinking right now about an essay, collaborating with a grade school chum [Henry Heikkenin] that I've just reconnected with, on the problem set. I was helping a student work through a problem they came in with in an office hour. And I was at the board [and] turned around to ask her a question about what she would be doing next on this, and she's busy flipping through the textbook. Page after page, and I [said], "What are you doing?"

And she said, "I'm looking for the example problem."

And I said, "How is that going to help you?"

And she said, "Well, it's going to show me how to set this up."

I'm trying to get her to think through how to set it up. She's looking for the example problem that's going to show her how to set it up. My friend, Henry, calls this learning by pattern recognition. And so when does life present us problems in the same pattern that they occur in a problem set?

So in a way, I think that what we find that we end up doing is—what the students end up with—is something I call "vaporware." It really isn't likely to stick. So this problem set mentality, this method of teaching science, got set up from junior high on. And the students get into a pattern of how to pass a course by, in a sense, gaming the problem set. Do you do this for the problem set? Then you can do it in the same problems. They see patterns of problems are going to show up on the exams and the final and then you're through. You've got it made, right?

And now 10 or 15 percent of the students in the class are [not] going to do it that way. They're going to learn the problems. They're going to learn how to solve the problems by thinking them through. You could give them extraneous data in the problem, and it wouldn't throw them off. But for the most part, I think that a lot of our teaching is wasted. And I think that's very unfortunate.

HUNTER-LASCOSKIE: I wanted to switch gears a little bit and ask kind of a big question about . . . you know, you've been able to see the field of mass spectrometry **<T: 25 min>** evolve, and you said you, kind of, backed into it, but you backed into it during a time when lots of things were changing. And I was curious if you could reflect on that a little bit and also talk about maybe where you see mass spec going in the future.

ENKE: Oh, it was so wonderful to get into mass spectrometry. Not only do I love the people as I mentioned before, it just feels like home going to a mass spec meeting. It is a really dynamic, a tremendously dynamic field. And I really feel as though ASMS—American Society for Mass Spectrometry—has been key in this. They have gotten people who are interested in mass spectrometry together for decades now, and it doesn't matter where they're coming from, whether they're in environmental testing, or whether they're in a pharmaceutical company, or whether they're academics, whether they're interested in instrumentation or fundamentals or whatever, we all go. And learn. And then the setting up of the journal [*Journal of the American Society for Mass Spectrometry*] and so on. Their main goal has been to share information. That's been the primary thing. They keep it inexpensive. We all get copies of the journal. It's something you can [get] out of pocket without even thinking about it, which is almost unique in organizations these days.

And it's a selfless group, by and large, of people willing to share their ideas and I think that's what causes things to happen so quickly, as I mentioned what happened with the triple quadrupole, for example. I mean, it was just like that [snaps fingers], but it was around ASMS that that acceleration occurred, the same thing occurred with MALDI, and the same thing occurred with electrospray, and every time some new breakthrough happens, the users are there, the manufacturers are there. The academics are there. The researchers are there. Everybody. And they can all take their part in it right away.

And the field has continued to grow. The society has continued to grow. [. . .] I can't predict a plateau in this right now because there's still so much more to [do]. [With] the first triple quadrupole, we lost 99 percent of the ions going between each quadrupole. What was left, almost, you might think, my gosh [might not be useful]. But we got such a huge improvement in selectivity that the loss in sensitivity hardly mattered. And now I was hearing from one of my colleagues as Sciex the other day. I think that with electrospray ionization in the triple quadrupole setup in [non-selective] mode, that they're looking at something like ten percent . . .

they're getting detectable ions for several percent of the molecules that are going in from the sample. When you think about Avogadro's number, that's incredible. It is really marvelous. And I think we're still moving. It's still moving. The work that I've been doing on the theory of complex mixtures—think about it. [. . .] Now, today, we talk about "comprehensive analysis." That's a word that's just come into the vocabulary in the last decade or so—comprehensive analysis. Nobody thought that was possible before. Comprehensive analysis, every metabolite. You know, that was unthinkable.

And it's brought about as a result of the combination of chromatography and mass spectrometry, incredible improvements in resolution in the mass spec particularly, the implementation of MS/MS and MS/MS/MS and so on in the solution of these problems. Data interpretation techniques that are used to sort out the metabolites from the things that aren't, and that are used to identify the proteins based on their peptides. It's incredible, and I don't see the end of it. I think it's just the beginning and mass spectrometry is now having an absolutely major impact **<T: 30 min>** on virtually every area of research, chemical and biomedical. Yeah. I'm a fan.

HUNTER-LASCOSKIE: Do you have any other questions?

DOMUSH: I don't.

HUNTER-LASCOSKIE: Well, I think that's all for our questions. Obviously at the end we always make sure there's nothing more you'd like to talk about, or anything we missed that you want to make sure gets in.

ENKE: No, I'm fine. Thank you very much. It's been very pleasant.

HUNTER-LASCOSKIE: Well, thank you so much.

DOMUSH: Thank you.

ENKE: It's been a very nice experience.

[END OF AUDIO, FILE 2.4]

[END OF INTERVIEW]