CHEMICAL HERITAGE FOUNDATION

BURNABY MUNSON

Transcript of an Interview
Conducted by

Michael A. Grayson

at

University of Delaware
Newark, Delaware

on

9 April 2010

(With Subsequent Corrections and Additions)

ACKNOWLEDGMENT

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Burnaby Munson
_______ _______________________
Burnaby Munson

____10/6/2012___________________________________

Revised 07/21/2009

> Burnaby Munson, interview by Michael A. Grayson at the University of Delaware, Newark, Delaware, 9 April 2010 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0688).

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

BURNABY MUNSON

1933	Born in Wharton, Texas, on 20 March

Education

1952	A.A, Tarleton State College
1954	B.A., Chemistry with Highest Honors, University of Texas, Austin
1956	M.A., Chemistry, University of Texas, Austin
1959	Ph.D., Physical Chemistry, University of Texas, Austin

Professional Experience

	Esso Research and Engineering Company
1959-1962	Research Chemist
1962-1966	Senior Research Chemist
1966-1967	Research Specialist
	University of Delaware
1967-1972	Associate Professor, Chemistry Department
1972-present	Professor, Chemistry Department
1973-1975	Acting Chair, Chemistry Department
1976-present	Professor, University Honors Program
1986-1989	Director, University Honors Program
2005-present	C. Eugene Bennett Chair, Chemistry Department

Honors

1973	Excellence in Teaching Award, University of Delaware
1979	Spectroscopist of the Year, Delaware Chapter, Society for Applied Spectroscopy
1984	Mortar Board Award for Excellence in Teaching
1986	Mortar Board Award for Excellence in Teaching
1988	Mortar Board Award for Excellence in Teaching
1992	Frank H. Field and Joe L. Franklin Award for Outstanding Achievement in Mass Spectrometry
1992	Award for Scientific Achievement and Contributions in Chemistry, Delaware Section, American Chemical Society
1992	Francis Alison Award, University of Delaware
1994	Outstanding Faculty Mentor, University of Delaware, College of Arts and Sciences

1996	Distinguished Contribution in Mass Spectrometry, American Society for Mass Spectrometry
1997	Madison Marshall Award, North Alabama Section, American Chemical Society
1998	Service Award, Delaware Section, American Chemical Society
2000	Excellence in Teaching Award, Alpha Lambda Delta, University of Delaware Chapter
2002	Medal of Distinction, University of Delaware
2004	Special Issue: *European Journal of Mass Spectrometry*, dedicated to Jean Futrell and Burnaby Munson
2007	Outstanding Achievement in the Fields of Analytical Chemistry, Eastern Analytical Symposium
2008	Chemical Pioneer Award, American Institute of Chemists
2008	Outstanding Older Worker, Experience Works

ABSTRACT

Burnaby Munson hails from Wharton, Texas, a small town on the Texas Gulf coast and near the largest Frasch sulfur mine in the country. His father, paternal grandfather, and paternal great-grandfather all were lawyers; his mother was the librarian at the high school. Both parents and his paternal grandfather were graduates of the University of Texas, and it was assumed that Burnaby would also go to college. He entered Tarleton State College in central Texas and transferred to the University of Texas in Austin, Texas. The origin of his love of chemistry was unknown, but physical chemistry was his favorite subject. He studied the reactions of acetylene while in Robbin Anderson's lab. Munson continued at Texas for a master's degree, still in Anderson's lab. He spent a very cold year at the University of Wisconsin, working in John Margrave's lab, researching (ironically) high-temperature inorganic chemistry. He retreated to warm Texas, to Anderson's lab, to finish his PhD.

Munson's first job was with Humble Oil in Baytown, Texas, where he worked on solution thermodynamics, extracting paraffins from aromatics. Humble was collegial, and training continued with a lecture series organized by Joe Franklin, who was a good friend and mentor to Munson. Franklin's small group of high-profile scientists developed the field of ion chemistry in mass spectrometry (MS). At that time, MS was a field limited by the large size and great expense of the instruments, but it was crammed with ground-breaking scientists, many of whom Munson discusses in the interview. Munson published what he regards as his most significant paper while at Humble. He also obtained his one patent, which was later sold to Scientific Research Incorporated. Joe Franklin left Humble for Rice University, and the ion chemistry group began to break up. Frank Field took his high-pressure instrument to New Jersey; Frederick Lampe went to Pennsylvania State University; and Munson took a position at the University of Delaware, where he has remained ever since, advancing to full professor with a named chair and many honors.

Munson was recruited to use Delaware's two instruments, an old time of flight (TOF) and a new CEC 21-110. He had always wanted to teach, especially undergraduates; he has taught freshman chemistry every year, and he helped establish and served as Director of the University Honors Program at Delaware. He comments that teaching has become more difficult over the years because of safety regulations, legal concerns, and paperwork. Furthermore, grants are more difficult to get. He says the universities support overhead, not research; government supports development, not research.

As a replacement for Joe Franklin and Frank Field, Munson attended his first American Society of Testing and Materials (ASTM) meeting, which he says was "a plum." He has since attended most of the American Society of Mass Spectrometry (ASMS) meetings, which subsumed ASTM, and he was president of the Society. Munson answers the interviewer's questions about other important mass spectrometrists he has worked with; about how MS and ASMS have changed over the years; and about the growing influence of biological applications in MS. Munson concludes the interview with his regret over being easily tired and therefore not able to travel to ASMS meetings or to visit friends. He mentions his internal debate about retirement locations: Delaware, where his life has been for forty-five years, or Texas, where he would not have to "shovel heat."

INTERVIEWER

Michael A. Grayson retired from the Mass Spectrometry Research Resource at Washington University in St Louis in 2006. He received his B.S. degree in physics from St. Louis University in 1963 and his M.S. in physics from the University of Missouri at Rolla in 1965. He is the author of over forty-five papers in the scientific literature dealing with mass spectrometry. Before joining the Research Resource, he was a staff scientist at McDonnell Douglas Research Laboratory. While completing his undergraduate and graduate education, he worked at Monsanto Company in St. Louis, where he learned the art and science of mass spectrometry under O. P. Tanner. Grayson is a member of the American Society for Mass Spectrometry (ASMS), and currently is the Archivist for that Society. He has served many different positions within ASMS. He has served on the Board of Trustees of CHF and is currently a member of CHF's Heritage Council. He continues to pursue his interest in the history of mass spectrometry by recording oral histories, assisting in the collection of papers, researching the early history of the field, and preparing posters recounting historic developments in the field.

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Humble Oil in Baytown, Texas. Worked on solution thermodynamics, extracting paraffins from aromatics. Important publication. Funding from U.S. Army. Humble connected to Esso; hence advantages of both small and large companies. Joe Franklin and ion chemistry (IC). Frederick Lampe left, Munson took his place. Seminar series organized by Franklin; included Eugene Rochow and Ilya Prigogine. Esso's main labs in Linden, New Jersey. First layoffs at Humble about the time Munson left. Size of mass spectrometry (MS) field limited because instruments expensive and large. Frank Field's high-pressure MS. Victor Talrose, D.P. Stevenson, D.O. Schissler, Catherine Fenselau. Thomas Aczel and first MS9 in country. Publications in several journals. Rare gas atoms. Hornbeck-Molnar process. Reactions of hydrocarbons important to process. Field took instrument to New Jersey, then Rockefeller University. Wilburn Geiger. CH_5^+ and $C_2H_5^+$. Michael Gross and ion cyclotron resonance. Possible uses for methane. Ratios of product ions the beginning of chemical ionization. Publication in *Journal of the American Chemical Society* because of Field's reputation. Henry Fales and biological molecules. Fast atom bombardment (FAB). Important technique, but took some time to catch on. Field's sabbatical and year at University of Leeds with Michael Henchman. Esso's ion chemistry fundamentalists moved to New Jersey; Aczel and Lumpkin stayed in Baytown. Munson did radiation chemistry for a change. Franklin went to Rice University; Lampe to Pennsylvania State University.

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Recruited by William Mosher. Old time-of-flight (TOF) and new 110 instruments. Munson had always wanted to teach, especially undergraduates. Has taught freshman chemistry every year. Several different chemistry sequences. Established honors sequence. Using TOF for class research, the modified 110 for high pressure. Got another instrument from Du Pont. Size of school, region of origin of students. Intercollegiate Student Chemistry Convention. Exotic chemistry and thermochemistry. “Polish” ammonia. Chemical ionization continues; negative chemical ionization. Donald Hunt. Ion cyclotron resonance with Douglas Ridge. Munson’s patent went to Scientific Research Incorporated, William Johnston’s company. Teaching more difficult now because of safety regulations, legal concerns, paperwork. Universities support overhead, not research. Grants more difficult to get. National Institutes of Health support development, not research. Pharmaceutical companies not hiring so many now.

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INTERVIEWEE:	**Burnaby Munson**
INTERVIEWER:	**Michael A. Grayson**
LOCATION:	**University of Delaware** **Newark, Delaware**
DATE:	**9 April 2010**

GRAYSON: Okay. Well, what I'd like to do is begin by stating that this is 9 April 2010 and I am in the office of Professor Burnaby Munson at the University of Delaware, and we're going to do a little oral history recording of his career in chemistry and mass spectrometry more specifically. So, I think with that little detail out of the way, we can go ahead and get started. Really the first thing that we like to start is the beginning and the beginning is your birth date and what part of the country you were born and a little bit about your parents.

MUNSON: I was born on 20 March 1933. I was born in Wharton, Texas, W-H-A-R-T-O-N. And with reasonable certainty I was born in a hospital there. I grew up there. I went to high school there. I graduated in 1950. Wharton was a small town then and is a small town now. There were 50 people in my high school graduating class. Huge.

GRAYSON: And where is Wharton with respect to someplace that we could identify with Texas?

MUNSON: Wharton is about 60 miles southeast of Houston, [Texas]. It's on the Texas Gulf coast.

GRAYSON: I was going to say, you're getting close to the coast.

MUNSON: Yes, it is. Well, the other thing which doesn't work now but did for a while was that we were…oh, roughly 15 miles from the Texas Gulf Sulfur Company, which was the largest Frasch sulfur mine in the world…

GRAYSON: Frasch?

MUNSON: Taking the […] sulfur ore and melting it.

GRAYSON: Frasch is…?

MUNSON: F-R-A-S-C-H.

GRAYSON: F-R-A-S-C-H.

MUNSON: That's the ore and this is the sulfur block that the Texas Gulf Sulfur Company used to give at county fairs.

GRAYSON: So, it looks like it's pretty well intermingled with the hard rock.

MUNSON: Yes. It's rock. But that's pure sulfur that's in the rock. That's a low grade ore that's there.

GRAYSON: So, you had an experience with the sulfur thing in your youth?

MUNSON: Not really. I went over there and looked at it, but that was about it.

GRAYSON: What about your parents and their educational level? That's an important piece of information that helps us…

MUNSON: Both my father and my mother graduated from the University of Texas in Austin, and that was back in the days when it was simply the University of Texas. My father may have gotten his undergraduate degree someplace else, but he got his law degree at the University of Texas in Austin. And my mother was in school there at the time. My grandfather got his law degree from the University of Texas. And I think my great-grandfather did. But I'm the black sheep in the family. My father was a lawyer. My grandfather was a lawyer. My great-grandfather was a lawyer. And I'm a chemist.

GRAYSON: When your mother graduated, that was not common, was it, for women to go through bachelor's degree program?

MUNSON: I don't know how common it was at the time. She did and her sister did. My grandmother went to college. Going to college wasn't uncommon, but subsequently going to college in chemistry was, because when I graduated from University of Texas in 1954, I remember only two or three girls, two or three women […] in the class. There may have been more, but I only…I remember very few.

GRAYSON: So, it would have been in the 1920s that your mother graduated, probably.

MUNSON: Yes.

GRAYSON: And what was the degree that she took? Do you know?

MUNSON: It was, I think, just a general bachelor's degree. She was the librarian in the high school in Wharton.

GRAYSON: And what were your parents' names?

MUNSON: Milam Stephen Munson Junior, and my mother's name was Emily Elizabeth Burnaby, and so I'm Milam Stephen Burnaby Munson, to make the distinction between me and all the other Milam Stephen Munsons that are around.

GRAYSON: So, you elected to go by Burnaby?

MUNSON: I don't know that I elected to. You don't really have **<T: 5 min>** that much choice as to what you're called when you're growing up. But that was to make the distinction in there. But when I was growing up, I was Little Stephen, Burnaby, Bub, Bubba, you name it. I tell you, even these days, I answer to almost anything, probably including Hey You.

GRAYSON: Basically there was quite a bit of education in your family's background.

MUNSON: Yes.

GRAYSON: So, when it came time for you to move on from high school, there was pretty much an assumption that you would go to college.

MUNSON: Oh, yes. It was absolutely established that I would go to college. What I was going to do in college, I hadn't the vaguest idea. And my way of comment along that line is when I went to…I started going to college in 1950, it was not a federal case. We did not spend the summer looking at a dozen schools. In fact, we didn't look at any. I got information from a whole bunch. I went to a junior college in Texas [Tarleton State College]—central Texas—John Tarleton…I can't remember whether it was John Tarleton Agricultural College then, or whether it was Tarleton State College, but it was a junior college. It was two years. I wasn't taken and dumped there, but we really didn't consider…we weren't concerned about where I went. I had a cousin who was teaching there in the agronomy department, which is not what I had any interest in.

GRAYSON: Yeah.

MUNSON: And my sister had gone there for two years, so I went. And I enjoyed it. It was great fun.

GRAYSON: And where was this located?

MUNSON: It's in Stephenville, [Texas].

GRAYSON: Stephenville.

MUNSON: Stephenville, which is southwest of Fort Worth, [Texas], about maybe an hour southwest of Fort Worth.

GRAYSON: Okay. You're pretty much in the middle of the state.

MUNSON: Yep. Back then, it was a long way. It's a long way there. And when I went to school, I had no idea what I was going to major in, but nobody cared. I took a general liberal arts course the first year. I took chemistry because everybody took chemistry. I took algebra rather than calculus because only engineers took calculus. If I had stayed…if Tarleton had been a four-year school, I probably would have gotten a degree in political science or history, and become a lawyer. But it was a two-year school, and I transferred to the University of Texas in 1952. And I obviously had some interest in chemistry then, because the first semester that I was there I was taking courses, half in chemistry, and half in history, or something like that. Why I did, I don't know. Why I developed an interest in chemistry is absolutely beyond me.

GRAYSON: So, you didn't do the classic childhood chemistry set thing?

MUNSON: I had a chemistry set when I was growing up, but I didn't do anything significant with it. It was in play…

GRAYSON: You didn't hurt yourself with it?

MUNSON: I didn't kill myself. Right. And that was at the time that a reasonable number of mostly boys, I suspect, did get hurt making rockets. I didn't make rockets. I was, sort of, a coward, and didn't have anybody helping me. I had a good high school chemistry course. Unfortunately, I can't remember the teacher's name, [whom] I liked. But there was nothing…I didn't have anything special.

And in 1952, when I started at Austin, I was taking an analytic geometry course in the pure math department, which was the worst course I have ever taken in my entire life. I had no interest in this thing. I worked hard in that, but I couldn't understand any reason for doing it. Subsequent courses were in the applied math department, which was fine. But I took quant quantitative chemistry, and quant had two lectures a week, meeting at 8:00 in the morning. It had six hours of lab a week for each semester. My recollection is that we had a quiz once a week, beginning at 8:00, short quiz. If you were late, you had a few minutes less to take the quiz. Gilbert [H.] Ayres was the instructor.

GRAYSON: How do you spell the last name?

MUNSON: A-Y-R-E-S, I think. Gilbert Haven Ayres. Classical analytical chemistry, inorganic chemistry of rock. First semester was quant. Second semester, I'm sorry. The first semester was gravimetric analysis. The second semester was volumetric analysis. The number of **<T: 10 min>** people in the class was limited by the number of balances, because these double-pan balances, and you measured the weight, took the average of five swings or whatever it was. And weighing anything was a nearly federal case. And it was your balance, and you were the only one who used that balance during that lab period. I don't know whether it was because of or in spite of that course that I decided to become a chemist. But then I continued on and I got a B.A. rather than a B.S., not that it really made much difference, but the main reason was that the B.S. required German, and I had taken Spanish, and I didn't propose to take German.

GRAYSON: So, with the B.A. from Austin…and that's in chemistry now?

MUNSON: Yes.

GRAYSON: You have to decide on what's next in your career.

MUNSON: I've never planned well for anything.

GRAYSON: Okay.

MUNSON: In my senior year, I was taking…I think I took organic and physical at the same time in my senior year in order to graduate. I graduated in four years. And I was doing well in P-chem.

GRAYSON: That was kind of challenging, wasn't it? Both of those courses?

MUNSON: Yes.

GRAYSON: I mean, in one semester. I mean, organic and P-chem would be…

MUNSON: It was. Organic synthesis was not anything I was particularly good at. P-chem I did reasonably well in, and the professor who was teaching the course asked if I wanted to do some work in the lab, and I did. And he was studying—Robbin Anderson, R-O-B-B-I-N, Anderson with an O. And he was studying reactions of acetylene. And I was not quite, but close to, a pyromaniac, and so this sounded like fun, and I did it. And if you take acetylene and put it in a pressure atmosphere or something like that, you can get a flame in pure acetylene. It gives a tremendous amount of carbon, and awful mist, but the flame very slowly goes down the cylinder. And we were playing around with the effect of different additives—parameters—on the flame velocity and things like that. And so, that sounded like fun.

And then after I got my undergraduate degree, well…why not go to graduate school? It seemed like a reasonable thing to do. I didn't have anything I particularly wanted to do. So, I then started working with Anderson on that. And the policy then as it is in most places is you really shouldn't get all your degrees at the same place. So, I was working to a master's degree, and got a master's degree on thermal reactions of acetylene. And then I went and spent a year at the University of Wisconsin-Madison. And my way of describing that was I've never been so cold before nor since in my life. The school was fine. I was working with John [L.] Margrave, high temperature inorganic chemistry. But there was no way I was going to live in that part of the country. That was it. I was there for one academic year, and I left, and I came back to Austin. [laughter]

GRAYSON: So, this acetylene experiment, what was the motivation for that? I mean, there was some underlying reason for doing these…

MUNSON: The [U.S] Army was supporting that—back in the days when they were supporting […] basic research, which had some relevance to it. It was the idea that was acetylene was one of the intermediates in a lot of combustion reactions. And so this was then studying the reactions of acetylene. And that was what I wound up doing to get my master's and Ph.D. on thermal reactions of acetylene.

GRAYSON: So, you went back to work for the same fellow when you got back to Austin?

MUNSON: Yes.

GRAYSON: And eventually that led to…oh, let's see 1959?

MUNSON: 1959…I got a Ph.D. in 1959. I stretched it out as long as I could, but after a while, when people began to ask, "Are you still here?" you get the idea that perhaps you should finish.

GRAYSON: Yeah.

MUNSON: And again, then when I finished, I went to work for Humble [Oil and Refining Company]. 1959 was a good time to get a Ph.D., because there were lots of job offers. I don't remember how many I had, but I had more than one. And it didn't really occur to me that I should go into teaching. You went to work for a living. And that was what I did. Why I chose Humble, I don't know. I was down to two places, Humble **<T: 15 min>** or…rats.

GRAYSON: Was it another petroleum…?

MUNSON: It was another oil company in Dallas, [Texas]. Mobil.

GRAYSON: Oh, okay.

MUNSON: Mobil. And at Mobil, I knew what I would be doing, and was, sort of, interested in it. It was *in situ* combustion. As I said, I was a pyromaniac. For secondary oil recovery. But at

Baytown—Humble—I didn't know what I was going to do. And as I said last night, at that time, they were not hiring people to do anything specific. They were hiring people. And when I went to work, I think they decided what I was going to do shortly before I came. And I was on a project in solution thermodynamics, trying to extract paraffins from aromatics using fluorocarbon solvents. Well, it didn't work, but it was some solution thermodynamics, about which I had no experience whatsoever, but fortunately had done a little bit of reading on Hildebrand solubility. It was fun, and I enjoyed it. I even got a paper out of it.[1]

GRAYSON: I want to back up a second. It was a black sheep career of going into chemistry. How did that go over with your lawyer family?

MUNSON: My father never expressed an opinion as to what it was I should do.

GRAYSON: As long as you got out and did your own thing, that was okay with him, then?

MUNSON: He never expressed an opinion. My grandmother was quite sure, however, that I was going to go to become a lawyer. But my father never expressed an opinion as to what it was that I was going to do.

GRAYSON: So, that was nice, that you got to follow your interests as you wished.

MUNSON: I never…as I said, my life does not display tremendous amount of planning and obvious goals. It just seemed interesting at the time, so I did it.

GRAYSON: And they went along with it, so that was…so, other than the fact that there was really a strong background, strong interest in being an educated person in your family, they really didn't care…

MUNSON: No.

GRAYSON: …what you did, as long as you…it was just understood that you would get the degree and you'd proceed.

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[1] M.S.B. Munson, "Solutions of Fluorochemicals and Hydrocarbons," *Journal of Physical Chemistry,* 68 (1964): 796.

MUNSON: Yes, it was a given.

GRAYSON: Okay. So, you then started at Humble in…it would have been 1959 or 1960?

MUNSON: 1959 Yep.

GRAYSON: Humble Oil and Refining.

MUNSON: Oh, yes.

GRAYSON: That was a small outfit, I think.

MUNSON: Well, it was a small…yes and no. It was still called Humble, but it was owned mostly by Esso at the time. […] And I don't know the exact ownership game, but at that time, we were connected with Esso Research and Engineering, but a lot of it was still autonomous in the research center in the Baytown refinery.

GRAYSON: So, Baytown was where they had their primary refinery?

MUNSON: Yeah. The Baytown refinery was one of them. There was another one up in Bayway, New Jersey and there were other refineries. But the Humble refinery was in Baytown. And the research center was in the refinery, just sort of in a little bit of a corner of the refinery. It was there.

GRAYSON: How did the research center support the refinery, or did it? Was there any interaction…I mean, did anything you did in the research center help the refinery?

MUNSON: Oh, yeah. A lot of the research in the research center was associated with projects in the refinery, obviously. But at that time, Humble, and I suspect most companies, were doing different kinds of research. I didn't know anything about it when I went there, but Joe [L.] Franklin and a small group were doing really basic research in ion chemistry. The other projects were what we called long-range research, in which there was no immediate goal. And then there were projects that were basically associated with things that were happening. We need something, an answer right now. And I think that the policy was generally that when somebody came in with a Ph.D., you would put them in a long-range project, and then get them to convert them to doing something for the immediate benefit of the company.

I worked on my solution thermodynamics project for a year or two. I don't really remember how long. And then Fred [Frederick W.] Lampe left to go to Penn State [University], and there was a group **<T: 20 min>** of Joe Franklin, Frank [H.] Field, and Fred Lampe who had been doing ion chemistry. And I became Fred Lampe's replacement. My way of describing it is that I was Joe Franklin's first postdoc, because he was the leader of the group, and the oldest in the group, but Frank and Fred were comparable age and stature. I knew nothing about mass spectrometry when I went into the group, and as I said, I was Joe's first postdoc. Why I was chosen, I don't know. I'm glad I was.

GRAYSON: Yeah.

MUNSON: Because I've been doing it since 1962, maybe, 1961 or 1962, something like that. I think my first publication was in 1962 on a really practical thing, the reactions of rare gas atoms and rare gas ions.[2]

GRAYSON: Yeah. That must have been useful to the petroleum industry.

MUNSON: Oh, very.

GRAYSON: So, Joe Franklin was kind of like an institution or a person of note in the organization there.

MUNSON: Yes, he was. I think a lot of things happened in the research center through Joe's insistence, through Joe's persuasion. We had at that time a lectures in science series, and these were notable chemists who would come in and would give, I think, a two weeks short course. People would take that. I had an inorganic chemistry course with Eugene [G.] Rochow. Wasn't terribly related to anything that I was doing, but that was all right. I did it.

GRAYSON: And what…how do you spell that name?

MUNSON: R-O-C-H-O-W, a noted inorganic chemist at the time. I had a two-week course in irreversible thermodynamics with Ilya Prigogine, which was also not closely related to anything that I was doing […].

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[2] J.S. Dahler, J.L. Franklin, M.S.B. Munson, and F.H. Field, "Rare-Gas Molecule-Ion Formation by Mass Spectrometry, Kinetics of Ar_2^+, N_2^+, and He_2^+ Formation by Second- and Third-Order Processes," *Journal of Chemical Physics* 36 (1962): 3322.

GRAYSON: Okay. Who would attend these courses?

MUNSON: Different people in the research center. If you had an interest in it, you could do it. And there were maybe…we'd get maybe one a year or one every other year, something like that.

GRAYSON: And you said these were two weeks long?

MUNSON: Yeah. These were two weeks. And you weren't doing anything else. You were doing that all day.

GRAYSON: So, I mean, this was all-day lectures?

MUNSON: Well, it was lectures, discussions, problems, interactions, going on with it. Sort of, a short course.

GRAYSON: For two weeks?

MUNSON: Yep. For two weeks.

GRAYSON: Wow. They paid these individuals, obviously, to come in and do this?

MUNSON: Yes.

GRAYSON: And you think Joe was behind getting…

MUNSON: Oh, I know he was. He ran it.

GRAYSON: They did this approximately once a year, and how many years do you think this went on?

MUNSON: I don't remember how many years it went on, but it went on for several, primarily because of Joe's insistence on it, with the idea that this would keep people up to date on new areas of the things that were going on. Because one of the things that you could see fairly easily was that if you were working on a specialized project, you became the company's expert on this, and you narrowed in, and that was what it was you did. Well, when the company decided they weren't interested in that anymore, which happened periodically, then you were sort of out of it.

GRAYSON: Yeah.

MUNSON: This was the idea of continual training that was going on.

GRAYSON: Trying to keep you from getting boxed in a [corner].

MUNSON: Yeah. Trying to keep you from getting stale.

GRAYSON: Yeah. How many people were in the research operation at Humble?

MUNSON: I don't know.

GRAYSON: Altogether? You think it would have been twenty, fifty?

MUNSON: I don't remember. Fifty to 100. Something like that. But I really don't remember the number.

GRAYSON: So, a fairly large operation for what probably wasn't that big of a company, was it, at that time, right?

MUNSON: Well, it was a fairly hefty-sized company, because they had an awful lot of oil down there, and it was still a huge refinery. I think at that time there were 250,000 barrels a day or something like that. But at that time, we were part of…before that, they'd been completely independent. Humble had been taken over by Esso, and we were then connected with Esso Research and Engineering, and would go up to New Jersey for periodic meetings. **<T: 25 min>**

GRAYSON: Okay. So, Esso had its own operation, but that was up on the East Coast?

MUNSON: Yep. Esso's main labs were in Linden, [New Jersey]. They had a Baton Rouge, [Louisiana], lab, but the main research was up in Linden.

GRAYSON: Well, it seems like a very interesting thing that would be done in a really industrial…I mean, petroleum…the company is really heavy in industry type operations, and they have a research group that would then bring in prominent scientists to lecture for two weeks. I can't imagine a modern company allowing someone to take off two weeks and…

MUNSON: Unfortunately, I can't either anymore.

GRAYSON: Yeah. I mean, it's pathetic, but it shows a certain change in the mentality of how things are done in corporate America.

MUNSON: Very much so. Well, at that time, the Baytown research center was like a lot of companies then. If you went to work for them, you retired there. When I was interviewing at that time, companies talked about their retirement plans, with the idea that you would stay there for forty years. And I stage-managed some forty-year parties—some retirement parties—while I was there, for people who'd been there and were going on to that, in the research center. I left in 1967 to come to Delaware…about that time, the company had the major change. They were having layoffs—things that they had never done before. And they've never gone back to the original model. Somewhat paternalistic, but it was fine. And there were people who—you went to work for the refinery and you were going to stay there for the remainder of your work life.

GRAYSON: Yeah. Well, that's obviously changed quite a bit.

MUNSON: Yes.

GRAYSON: I mean, I don't know, maybe for the better or the worse. Who knows? But it seems that at least having a job in perpetuity, so to speak, if you…I assume that you had to perform…

MUNSON: Yes, you did. Oh, and there were people [who] left sometimes of their own volition, sometimes encouraged to leave. But nobody was ever really fired. If they left, it was encouraged to go someplace else.

GRAYSON: So, you were drafted into this organization with Lampe, Field, and Franklin. And this was your first exposure to mass spectrometry.

MUNSON: Yes. At that time, mass spectrometers were relatively very expensive, and there weren't very many universities that had them. The University of Texas in Austin did not have one. I'd actually had a little bit of indirect…a friend of mine who graduated a year before me was working down in Baytown, in the research center, and he ran some mass spectra of some of the material, the pyrolyzate from acetylene, aromatic hydrocarbons, polynuclear [aromatic hydrocarbons] ones, and so forth and so on. He ran the mass spectra, and we just made some guess as to what the compounds were. But I didn't know a thing at all about it. You know, the mass spectrometry was not offered and was not discussed in my graduate analytical course that I recall anything at all about.

GRAYSON: Sure. No. What kind of instrumentation were you faced with when you got into the group?

MUNSON: Frank Field had built…you talked with Frank.[3] He had built—well, he didn't physically do it himself, but he had it built—a high pressure mass spectrometer, which was a huge thing in the lab. It took up the whole lab.

GRAYSON: So, by high pressure…

MUNSON: Oh, this was high pressure for mass spectrometry. And it began working with maybe…the ion chemistry began in the 1950s, [Victor] Talrose, [D. P.] Stevenson, and [D. O.] Schissler, and Field, Franklin, and Lampe were the three major groups, two of them in petroleum refineries, because they had the mass spectrometers. And I don't know the exact sequence of the events. I think Talrose actually did it first, increasing the pressure in methane, and if you do that, then you see that the 17 to 16 ratio changes with pressure. And my way of describing it, Talrose was a physicist, so CH_5^+ seemed perfectly reasonable. CH_5^+ has never seemed reasonable to any chemist, and then it went on. And Stevenson and Schissler were doing this. **<T: 30 min>**

GRAYSON: How do you spell Schissler?

MUNSON: S-C-H-I-S-S-L-E-R.

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[3] Frank H. Field, interview by Michael A. Grayson at Durham, North Carolina, 9 and 10 December 2009 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0636).

GRAYSON: Two Ss.

MUNSON: Yeah. They were at Shell Development in Emeryville, [California]. And at that time, you took a standard mass spectrometer and just put too large a sample in it, and you could see these reactions. Analytically, it had happened before. If you put in too large a sample, you could see the M+1 ions for polar compounds. And then you tried to build up the pressure deliberately, and Frank's instrument was built [and] was designed to go to high pressures, to operate at high pressures, with differential pumping. And the time I was there, we began going up to pressures of 2/10ths of a millimeter, 200 microns in the illicit units of the day.

GRAYSON: Illicit units?

MUNSON: Yes.

GRAYSON: Two hundred microns?

MUNSON: Two hundred microns And at the low pressures [in the micron range], what you could see are the product ions. You could see mass 17 and mass 29, because there wasn't anything there in methane. At high pressure, you could see the product ions and you could see the reactive ions disappear. So, you could study the kinetics. And this was the case, that you studied the kinetics by varying the abundance of the ion at a constant—essentially [constant time, which you couldn't vary much]. You had an electric field, and it ejected the ions. And so you got these funny-looking curves. Catherine [Fenselau] called them spaghetti curves, reactant ions decreasing, product ions increasing. And then you could match that, and it and get the reactant product sequence by relative abundance and by measuring appearance potentials.

GRAYSON: This was Catherine Fenselau?

MUNSON: Oh, no. Catherine didn't have anything to do with it. But she called my things spaghetti curves.

GRAYSON: Oh, okay.

MUNSON: And at 200 microns, you could see lots of reactions. You could see many things that were going on. And then later on, we increased the pressure to about two millimeters, and then you could see a lot more things that were going on. Now to some extent we were rediscovering what the physicists had learned a good while ago, and that is, if you were doing discharge experiments in gases, you get products…at higher pressures the products were not what you started out with, and the effect of impurities was fairly obvious in that one, but we didn't really know that.

GRAYSON: And not the first time that something has been rediscovered.

MUNSON: Right.

GRAYSON: Were there any other mass spec instruments that you interacted with there, besides this high pressure piece?

MUNSON: Well, there were a couple of other instruments that were there, but I spent all my time…[phone ringing]

[END OF AUDIO, FILE 1.1]

GRAYSON: We were talking about other instruments besides the high pressure machine that you had.

MUNSON: There was an old…there was a CEC 10X, or something like that [CEC 21-102 or 103C], one of the early instruments that was used for routine analyses. I spent all my time with the mass spectrometer. I didn't get in to use that one. That one ran…technicians ran that, and it was used extensively for analyzing samples for the research center. I mean, for the plant.

GRAYSON: It was in the research center environment, but it was used for routine analysis to support the…

MUNSON: Routine analyses, for projects in the research center, and for analyzing things in the plant. And then there was…[Henry] Earl Lumpkin was there, and he was using that, and Tom [Thomas] Aczel, A-C-Z-E-L, was there, and they got an MS9. I think that was the first MS9 in the country, doing high resolution analyses of samples, again, in the research center and for the refinery. And I remember they had the oscillograph paper with the multiple traces. We worked extensively with oscillographic paper. And I remember one of the technicians, whom I still see occasionally, Mary Frances Bonds, would get down this huge 20 feet, of chart paper that came in, it was slowly recording, and would take a ruler and would measure the peak heights for all the multiples of a given mass number for the analysis of the aromatic composition, and the heteronuclear aromatic composition.

GRAYSON: So, it was a high resolution scan?

MUNSON: Yes. A high resolution scan. Very slow.

GRAYSON: Yeah. So it must have been a ton of peaks to measure.

MUNSON: Oh, there were. There were.

GRAYSON: Well, you know that one of the old CEC 110s is up at Iowa in Ron [Ronald] Grigsby's basement, and he still does PONA [paraffins, olefins, naphthenes, aromatics] analysis with it, and—through his PetroMass company.

MUNSON: Yes. The one that they had was not a 110. It was the earlier version. So, low resolution, that the petroleum companies had been using for years.

GRAYSON: So, there was this, kind of, analytical side…

MUNSON: Yes.

GRAYSON: …that was separated from the stuff that you guys were doing in the research center.

MUNSON: Yes.

GRAYSON: So, you interacted primarily with this high pressure instrument?

MUNSON: Yes. That was my mass spectrometry.

GRAYSON: Where did these publications go?

MUNSON: *Journal of Chemical Physics, Journal of American Chemical Society, Journal of Physical Chemistry*.

GRAYSON: Okay. And so, you're studying the chemistry of the…ion chemistry.

MUNSON: Yes.

GRAYSON: They accepted these publications without too much trouble?

MUNSON: Yeah. There was a small but continuing ion chemistry group at the time. The first paper that I worked with Joe and Frank on was, as I said, the reactions of rare gas atoms and rare gas ions.[4] And if you take an electric discharge you produce, the rare gas, excited rare gas atoms, which will react, collide with, and react to form the dimer ion—the so-called [John A.] Hornbeck-[J. P.] Molnar process. And we measured the appearance potentials to get thermochemistry and bond dissociation energies out of that.

GRAYSON: Hornbeck…

MUNSON: Molnar, M-O-L-N-A-R.

GRAYSON: Did Frank have any practical use for this in his ion chemistry studies, or was this just an experiment that…

MUNSON: This was just an experiment to see what was going on.

GRAYSON: Shake down the…see if there's something interesting that might be going on that you could capitalize on.

MUNSON: And it didn't occur to us that you could actually make compounds in the rare gases. That was not one of the things that even seemed reasonable.

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[4] J.S. Dahler, et al., "Rare-Gas Molecule-Ion Formation by Mass Spectrometry, Kinetics of Ar_2^+, N_2^+, and He_2^+ Formation by Second- and Third-Order Processes."

GRAYSON: But you came very close to doing it.

MUNSON: Yep. We made ions.

GRAYSON: You made ions.

MUNSON: We made ionic…yeah. And the rare gases would react with other species, because you could make $XeCH_3^+$ and other species, again, ions, not neutral. But most **<T: 5 min>** of the work really was on the reactions of hydrocarbons, and my recollection of it, and I wasn't involved with the high level decisions as to how it was that Joe was allowed to do this. To some extent, I think he was allowed to do what it was he wanted to do. But he convinced the management that the reactions of gaseous ions—gaseous hydrocarbon ions—was relevant to the reactions of acid catalyzed processes in solution—reactions of carbonium or carbenium ions, whichever way you want to talk about it were important. And they were. They did help in the mechanisms of the processes. So, we were then doing reactions in methane, ethane, and propane, to go back and study what was happening, because at that time, nobody had…we didn't, and nobody had any real idea as to what chemical reaction…[the chair] rocks. Sorry about that.

GRAYSON: Okay.

MUNSON: As to what chemical reactions were going to occur. You took ethane, increased the pressure, and you got mass 29 appearing. That was easy. Production of that. Propane, you got mass 43. So, you got hydride transfer reactions that were beginning to occur. And that was fine, because hydride transfer reactions were chain transfer reactions in the acid catalyzed processes [and] this is going back and seeing them. And you could tell that, yeah, the lower molecular weight species would extract the hydride from the …the lower molecular weight ion would extract the hydride from the higher molecular weight molecule. And we observed the thermochemistry in the pattern that was there.

GRAYSON: He was able to at least justify it to management, because he had to do at least that much, but they were willing to support him and let him…

MUNSON: Yeah.

GRAYSON: …pretty much do what he wanted as long as he…

MUNSON: Yeah. I think that was the case.

GRAYSON: Because I don't know how much…they must have invested a significant amount of money in the high pressure instrument.

MUNSON: Oh, yeah. I don't know how much it cost. It was done before I got involved with it. But at that time, it was…I'm sure it was expensive. I'm sure it was expensive. And it took up a *whole* room. It took up a whole room. Frank moved it to New Jersey when he moved up to New Jersey, and then he took it to Rockefeller [University]. [laughter]

GRAYSON: Yeah. So, somewhere along there, the whole business of chemical ionization became a topic.

MUNSON: We did a fair amount of chemistry, and the pressures were the order of 2/10ths of a millimeter. And you could see consecutive ion molecule reactions. If, for example, you took acetylene, which we did, you'd get ions in acetylene, and they will react to produce dimers, which will then react to produce trimers. And you can see these consecutive reactions that were occurring. And then I think it was the year that Frank was on—essentially an academic—an industrial sabbatical leave, that Wilburn Geiger, who was the technician who really kept that instrument going…

GRAYSON: How do you spell his name?

MUNSON: G-E-I-G-E-R, I think.

GRAYSON: G-E-I…

MUNSON: It's either E-I or I-E. I don't remember. Wilburn and I, Frank was gone, and so we just increased the pressure by another factor of ten. We took methane and put it in and raised the pressure until—by the—so we had two millimeters, I think. And it took a very long time before we finally managed to figure out what was happening, because we got very irreproducible results. We got mass 19 and 31 appearing. And mass 19 doesn't belong in methane. The reactions of methane were CH_4 giving CH_5^+, 16 to 17, and CH_3 giving $C_2H_5^+$, 15 to 29. And there were some minor reactions there. And we periodically saw 19 appearing as a relatively abundant peak in the high pressure, and 31 coming and going. And we finally figured out that we had to completely get all of the water out of the instrument to eliminate the mass 19, and there was a little bit of an air leak, which gave the mass 31. And then we did, and finally we got the thing to show that if going up to high pressure, you'll get CH_5^+, and it just comes up and flattens out, and **<T: 10 min>** doesn't react in methane, doesn't do anything at all. And we had no idea what CH_5^+ was going to do, because it had no known chemistry or anything at all like that.

GRAYSON: Sure.

MUNSON: And then subsequently, Mike [Michael L.] Gross with the ICR [ion cyclotron resonance] went back and did it, and you do…the ICR is the standard way of doing things, because that way you study the kinetics as a function of time. And they went back, and yes, if you have CH_5^+ low-pressure methane, it just sort of goes up and doesn't do anything. People believed that. I don't think they believed our kinetics experiment one way or the other. But yeah. That was the case. So, we went back and both CH_5^+ and $C_2H_5^+$ were essentially inert in methane. And having discovered the real problem from trace levels of impurities, having very carefully, using Philips research-grade methane that had been doubly distilled in a vacuum manifold that hadn't been opened for ages, then we went back and deliberately put small amounts of impurities in. And then you could see the subsequent reactions: CH_4 coming down, CH_5^+ coming up, coming back down again, and the final product, coming up.

GRAYSON: So, your impurity.

MUNSON: Yeah. That was the impurity that you put in. If you put in a little bit of methane…sorry, a little bit of water in the methane, percent, tenth of a percent, something like that, at two millimeters, you could see that you had…by increasing the pressure as you went up, you could see that you had converted 16 to 17 to 19, and then you pick up a little bit of 37. And at high pressures, the dominant ion was the trace impurity that was there. And we played around with that for a while. And there was another connection. That was around the period of time when radiation chemistry was one of the things of interest. The idea that you would take [a hydrocarbon] stream and put it into a reactor, and you'd get all kinds of products. And also, methane was sort of a drag on the market. Nobody had that much interest in methane. It was flared from the oil refinery. So, maybe this was a way of converting methane into something else.

We were then studying high pressure reactions to methane, and put in a bunch of inorganic additives, and began putting in other hydrocarbons. And the reactions, CH_5^+…after a little bit, it became obvious that CH_5^+ was an acid, reacting by proton transfer. $C_2H_5^+$ was an acid, reacting by both proton transfer and hydride transfer—Brønsted-Lowry acid—but CH_5^+ was predominantly a proton transfer reagent, with a lot of dissociation. Because then you go back and say, all right, if CH_5^+ is an acid, CH_4 is a base, and the proton affinity was known. But most people don't think of methane as a base.

And then we added the other hydrocarbons to it, and were just, sort of, unimaginatively putting in different hydrocarbons to see what was happening, because you had no legitimate business for making a prediction as to what the reactions were going to be on there. We put in isomeric hexanes, because they were available from Philips Research in high purity, and noted that you got similar reactions but different ratios. And put in a couple of other isomeric hydrocarbons, and then what you see is yeah, you had similar reactions, but different ratios.

GRAYSON: Ratios of…

MUNSON: Of product ions. And the ratios of the product ions were sufficiently different that you could tell the difference between branch and normal hydrocarbons, and you could tell some information about the structure, and that was the beginning of CI [chemical ionization].

GRAYSON: That was something you just stumbled onto…

MUNSON: Yeah.

GRAYSON: …by playing with the equipment and then seeing what happened if you stick this in there? And so, there was that famous publication that came out in *JACS* [*Journal of the American Chemical Society*] that…[5]

MUNSON: 1966, I think.

GRAYSON: This ended up being a fairly well-referenced publication over the years, I would think.

MUNSON: Yep.

GRAYSON: It was probably the **<T: 15 min>** first description of the chemical ionization process.

MUNSON: It was.

GRAYSON: But the JACS editors didn't…

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[5] Milam. S.B. Munson and Frank. H. Field, "Chemical Ionization Mass Spectrometry. I. General Introduction," *Journal of the American Chemical Society* 88 (1966): 2621.

MUNSON: [laughter] Did Frank tell you about that?

GRAYSON: We didn't talk about it in great detail.

MUNSON: He's the best. He would be the best one to do it. When I talked about it, I…he saved the reviews. I would not have saved the reviews. If Frank had not been the coauthor on the paper, chemical ionization would never have made it, because the review was very negative about the paper, about the technique, and about everything connected to it. I don't know who wrote the review. It was obviously one of our senior colleagues in mass spectrometry at the time. I don't know who it was. But it was a short review, very derogatory, all kinds of things. But because Frank was well-established and was a very strong character… [laughter]

GRAYSON: Oh, not Frank.

MUNSON: We made minor changes to the paper—I think they were minor changes in the paper—resubmitted it, and I think as much as Frank having influence, it was published, and that was it. But if it hadn't been for that…nope, it wouldn't have been published.

GRAYSON: I wonder if there was…if one of the reviewers had come from an academic position, and, you know, figured that these guys in industry had no idea what the hell they were doing, and…

MUNSON: My guess is it was. I don't really know who it was, but I would guess one of a half a dozen people, because nobody else would be allowed to do it. It was not obvious at the time who it was. And yeah, there were some problems, we couldn't explain all of the peaks that were there. But we could explain most of them.

GRAYSON: And the major ones.

MUNSON: And we explained the major ones. And we had plausible mechanisms for that. We didn't have verification for the mechanisms. The techniques for verifying those didn't come through until a good deal later on. But the first paper had a bunch of compounds in it. Actually, the first thing that we did was just to take a series of esters, because we had those, and go back and do that. But that wasn't the first paper, because the first paper was one which had different compounds in it. And the technique was particularly useful for aliphatic esters, because they didn't give…they give essentially no molecular ion [by electron ionization].

GRAYSON: Right.

MUNSON: And we could see molecular weight plus one in the products that were there. Alcohols gave some molecular weight plus one, some molecular weight minus one, which you could go back and you could tell, hey, it was obvious that it was an alcohol, and it was obvious that it had this many carbon atoms. And so, actually the technique was useful, and that was what we did. The other thing that was there that came out that…the first papers that we did were with methane. Methane gave a lot of fragmentation, so you get structural information. We did some with isobutane, because the t-butyl ion—the t-butyl ion is a much weaker acid. It [gives] much less decomposition. And that was what the technique was used for a lot at that time, was selective ionization, so that you would get one peak per compound, and you could go ahead.

GRAYSON: So, how rapidly did the rest of the mass spec community adopt or become interested in this as a method of ionizing compounds?

MUNSON: Not instantaneously. It took a while. I think what helped a lot was when Hank Fales did…well, one problem was you couldn't do this in most mass spectrometers.

GRAYSON: In most instruments.

MUNSON: Because you had to go to high pressure. And so none of the conventional mass spectrometers at the time could do that. You had to then rebuild the sources. And AEI [Associated Electrical Industries, a British company,] Marvin [L.] Vestal, I think, had…I can't remember what the company was.

GRAYSON: Was it Vestek?

MUNSON: No, this was well before then.

GRAYSON: Well, he had another company before Vestek?

MUNSON: But this wasn't his company.

GRAYSON: Oh

MUNSON: Bill Johnston's company in […]

GRAYSON: [Oh, ARL or something like that?] I was just interviewing Marvin.[6]

MUNSON: Yeah. They built a high pressure source for the MS9, and Hank Fales got it. **<T: 20 min>** And then Hank began doing CIMS, [(chemical ionization mass spectrometry) on compounds], showing things on compounds of more interest to more people—that it worked for biological molecules. And then after that, it began to pick up, because you had to have an instrument which could operate at high pressure.

GRAYSON: It was a serious instrumental issue in terms of getting the technique installed, so to speak, as opposed to FAB [fast atom bombardment]. I think most people…

MUNSON: Yeah.

GRAYSON: …could do FAB pretty much, pretty easily, without a whole lot of modifications. You obviously reported these results at the ASTM E-14 [American Society for Testing and Materials] conferences back then. Was there much interest exhibited, or was it primarily just the fundamental boys that found the ion chemistry and the chemical ionization interesting?

MUNSON: Well, at the beginning, it was primarily the fundamentalists, because that was the group we talked to.

GRAYSON: But eventually, with other people starting to use it, it took off, and as I recall…you know, I was just coming into the business about that time, and that was a hot topic, the whole idea that you could form ions in a method other than electron ionization, which had its obvious limitations and its obvious advantages, compared to gas discharge. But chemical ionization seemed to offer an opportunity to study whole ranges of compounds that were beyond…well, you say the alcohols, the molecular ion normally didn't show up unless you got C5, C6 alcohol, and here's the chemical ionization providing the opportunity to […]

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[6] Marvin L. Vestal, interview by Michael A. Grayson at Orange County Convention Center, Orlando, Florida, 3 March 2010 (Philadelphia, Chemical Heritage Foundation, Oral History Transcript #0680).

MUNSON: And I think the other thing that was there that made the technique useful for people is that you could predict the reactions relatively easily with acid-based chemistry because you transferred a proton into a place in the molecule, it did its business right there. It wasn't until later on that you came back and you discovered…yeah, you do this, and you have reactions occurring. But most of the time, a reaction occurred at that center.

GRAYSON: That was done while you guys were still at Humble, right?

MUNSON: Yes.

GRAYSON: In the mid [to] the late 1960s?

MUNSON: Yeah.

GRAYSON: You said that this was something that you and the technician had worked on while Frank was away on an industrial sabbatical.

MUNSON: Yeah. That was the high pressure methane. Then when he came back, that was when we started. Having gotten the high pressure of methane and then going back and putting the impurities.

GRAYSON: Was that something he was aware you were going to do, you were going to increase the pressure by an order of magnitude?

MUNSON: Oh, that was always the goal, was the idea that you built an instrument to go up to high pressure. The lot that you could do at 200 microns, and 2,000 microns, it was a factor of ten in there, and you could see then more products. You could see then the multiple reactions, sequential reactions, and see more amounts of…

GRAYSON: When he came back, you had already determined that the water…the 19 peak came from protonating water?

MUNSON: Yeah. It was about that time.

GRAYSON: This must have been an intriguing idea to him, what was going on with all this chemistry. Oh, then you say he went to [University of] Leeds?

MUNSON: Yeah. He spent a year at Leeds.

GRAYSON: Leeds in…?

MUNSON: In England.

GRAYSON: In England. Okay.

MUNSON: Working, I think, with Michael Henchman.

GRAYSON: Okay. He didn't say anything about that in our interview, so I don't know what that was about. And this industrial sabbatical concept seems like it was another thing that was being done in industry then that was definitely…

MUNSON: It was not common, but it happened every now and then. Somebody else from the research center, Dave Walker, no connection to mass spectrometry, he took off a year and went to Spain.

GRAYSON: So, the operation at Baytown pretty much folded up shortly…well, I guess, Esso took over and the whole operation got moved up to…

MUNSON: No. It didn't fold.

GRAYSON: Oh, okay.

MUNSON: It continued there. There still is a research center that goes on there. But different parts of it were moved **<T: 25 min>** up to make what became obviously the main research center at Linden. And Frank moved up there. Why he moved up there, why it was decided to move him up there, I don't know. That was at a level well above me, that I didn't know anything at all about.

GRAYSON: Now he packed up his gear and took it to New Jersey.

MUNSON: Yes.

GRAYSON: And where did you end up in that process?

MUNSON: I was there and that was about the time I decided I might want to try something else. For a little while after that I was doing some radiation chemistry experiments…again, the idea of can we take this chemical ionization concept to produce new products? Well, it didn't turn out that way. I don't think it would have ever been successful. But I have a recollection…we did some experiments in which we took, I think, it was tertiary-butyl benzene in methane and irradiated that in the Van de Graaf generator. Tertiary-butyl benzene in nitrogen, and irradiated that. And the ion chemistry was very different in methane than it was in nitrogen, and we wound up getting very different neutral products from that, which suggested, again, that the ion chemistry did have significant effect on what was going on in it. But nothing ever happened.

GRAYSON: You stayed at Baytown after Frank had gone to New Jersey?

MUNSON: I think Frank left in 1966, and I left in 1967. Joe Franklin left in I think 1963.

GRAYSON: Is that when he went to Rice [University]?

MUNSON: He went to Rice. Fred Lampe left the group in I think it was 1962, when he went to Penn State. Joe left in 1963 or 1964, something like that, to go to Rice. Frank moved up to Linden in 1967 or 1966. And then I left in 1967. That ended the fundamental research in ion chemistry at Baytown. Mass spectrometry was still going on. Thomas was there. Earl was there, still doing things.

GRAYSON: Okay. So, when you departed Baytown, you came here [University of Delaware]?

MUNSON: I came here.

GRAYSON: You've been here for a long time. [laughter]

MUNSON: I've been here for a long time. I've been here for a long time.

GRAYSON: Why did you choose to go into academia?

MUNSON: I think I had always wanted to do that, and 1967 was a reasonably good time to be job hunting in academia. There were places that were around that wanted…I came to Delaware…again, I don't really know why. I liked the place. I liked the then chair. They had a mass spectrometer. Again, this was a long time ago. And Bill [William A.] Mosher was the chair, and he'd gotten a grant for a mass spectrometer. They had the mass spectrometer and they were looking for somebody to use it. They had a time-of-flight, but this was the 110 that they had just gotten, and they were looking for a mass spectrometrist to use it. And I think I came to Delaware because it was what I would describe as a teaching university. I enjoyed doing research, but I also really wanted to do teaching in the sort of archaic sense of undergraduate teaching. And that was the opportunity that was here. Now I didn't really know the difference, the standing between the University of Delaware, the University of Pennsylvania, the University of Maryland. I didn't look very carefully at all these things. I didn't look at the rankings or anything like that. I came up here.

GRAYSON: Did you look at a variety of schools?

MUNSON: Yes.

GRAYSON: You chose Delaware from a selection that you had…

MUNSON: I had offers from [University of] Georgia, Georgia Tech [Georgia Institute of Technology], and here. I suspect now most people would be surprised that I [came] here, but I did.

GRAYSON: Given your aversion to cooler climate, I would have thought you'd have stayed in the South. You know, Delaware is not Wisconsin, but it is…

MUNSON: No, it's not Wisconsin. I think I interviewed here around **<T: 30 min>** Christmas, something like that. It wasn't terribly bad.

GRAYSON: That would have been, what, 1960…?

MUNSON: I came here in the fall of 1967.

GRAYSON: You came here as a tenure-track position, or did they grant you tenure on arrival?

MUNSON: No. I came as an associate professor, but it was a tenure-track position, which is fine, because I don't think…I wouldn't have survived starting out as an assistant professor and doing all the things that were there. But this was forty years ago. It was much easier. It was easier to get money. It was easier—you didn't have to document everything. My tenure was granted with minimal fuss. I didn't have to provide ten books of this, that, and the other. Now I don't know why anybody would do it, because the tenure documents are two notebooks, three notebooks, four notebooks, and I really…I really want to insist that the promotion and tenure committees sign a piece of paper saying, "I have read these documents." [laughter] They haven't. I know they haven't. There's no way.

GRAYSON: I have two sons who are in that process now, and one of them, the youngest one, says essentially that the various things that have been added to the process of getting tenure over the years has grown to the point where those that got it years ago don't quite understand that it's become a real a bear.

MUNSON: It's a monumental process. It's a monumental process. It's ridiculous.

GRAYSON: Scott's [M. Grayson] working on his tenure at Tulane [University], and Matt's [Matthew Grayson] working on his at Northwestern [University], and it sounds like it's…I guess, it'll be worth it eventually, but right now, they're working like crazy. I mean, keeping ridiculous hours. And they'll make it. I hope they make it. So, you came in [and] you wanted to teach when you came here. What did they assign you to teach?

MUNSON: Teaching freshman chemistry. I taught freshman chemistry. I taught other things, but I've taught freshman chemistry nearly every year that I've been here.

GRAYSON: Okay. So you must be considered as a very nice person among the faculty to want to teach freshman chemistry, because I would assume a lot of the people aren't interested in dealing with that particular…

MUNSON: Yeah.

GRAYSON: …class, and it's probably a large…are these large sections?

MUNSON: Well, yes and no. We do teach a lot of students. We have multiple freshman chemistry courses—five or six, something like that—aimed for different markets. We have a large freshman chemistry course for science and engineering majors, but not chemistry majors, chemistry and chemical engineering majors. Then we have a course for chemistry and biochemistry and chemical engineering majors. We have another course, a lower level course, for people who nominally need some chemistry but don't really like it, and so on. So, it's divided an awful lot, and it takes really different skills to do it, because if you're teaching chemistry to students in the [School] of Nursing, they're really not interested in quantum numbers. You need to go back and tailor the course to the market that is there. But I've taught physical chemistry at the undergraduate level. I've taught graduate courses in kinetics, analytical chemistry as well.

GRAYSON: And these freshman chemistry classes are I imagine fairly large.

MUNSON: Some of them are. For the past thirty odd years…the University developed an honors program in the fall of 1976, and since I had said that I wanted to teach, and I had gone to a small junior college and liked doing that—the interactions with students—I figured I should either put up or shut up. I began teaching the honors program in the fall of 1976. And when that program began, it was done in Dover [Delaware] , which is about an hour from here. And I drove down to Dover two or three days a week and taught classes there. And the classes were smaller, but there was need. And in fact, for the first three years when we were down there, I taught my own labs. And when I had three labs a week in addition to the lecture…the third lab, it was, "Truly kid, I've already heard this. I don't need any of this anymore." [laughter] But it was fun. It was fun. I enjoyed it. Then the program moved up here in 1979, and I have been teaching **<T: 35 min>** the honors general chemistry or honors quant, or sometimes both of them, since then.

GRAYSON: So, these honors kids are more serious, I would assume, in their academic pursuits?

MUNSON: Some of them are. Some of them are. But they're still college freshmen. The thing that was interesting was that *we* learned. The program began as an early admissions program. Students came and rather than completing their senior year, and they took their freshman year in college, and they were taking college courses, and that was fine. And we anticipated that we would have a group that was all highly motivated and all kinds of things like that. And then we discovered that we had a group of freshmen, and that's a special category. And there were some of them who were really very good and very motivated, and all the things, and there were others who were not, and this is characteristic of the young freshmen. They are in general more willing to work, more interested in doing it. But the course I've been teaching is for chemistry and chemical engineering majors, so these are the ones who better want to do that.

Now there is a modest or sometimes more than modest attrition rate as people discover, “This is work. I don't really want to do that.” And that's fine, because the students now get pushed into things way too early. And the general…well, not general, but it's frequent…if you're good in math and science, go into engineering. Well, they discover that they really don't like engineering. And they discover that they don't necessarily like math and science, because the honors group are the ones who did well in essentially everything in high school. And if you do well in things, you like it. Then when you discover, I don't really like that very much. And that's fine. That's fine.

GRAYSON: I guess the reason the honors program started in Dover was to separate these pre-freshmen from the rest of the…

MUNSON: Yes.

GRAYSON: …college kids.

MUNSON: That was part of it. And there was another reason that…again, done at a level beyond me. I think it was an idea that the University of Delaware would then have somewhat of a presence in Dover, and we were using Wesley College campus, and I don't know whether they needed something or not. But that was at the presidential level and I'm not at the presidential level.

GRAYSON: Yeah.

MUNSON: It was fun, but it was a problem because the students…we offered a whole range of courses, but not necessarily all the courses that they needed, so that the students would be commuting back and forth a couple of days on it. And so in 1979, the program moved up here and changed emphasis. It then became no longer an early admissions, because you couldn't recruit simultaneously for juniors and seniors. So, then it was just an honors program, beginning as a freshman year, and then gradually extending so that it's a complete honors program over all four years now.

GRAYSON: And so, the qualifications for getting into the program is a grade point average?

MUNSON: It's a grade point average. It's an essay. To some extent it's recommendations. I think the recommendations probably count more than they would do in general.

GRAYSON: So how big is the program now? Your entering classes, you were saying the entering class would be what, about 4,000?

MUNSON: Oh, the entering class at Delaware is about 3,500, something like that. And the honors program has gone up…it's down a bit. I used to pay more attention to what it…I was director of the honors program for a while. I haven't paid much attention to it. I think we may be at 400 now.

GRAYSON: It's a little bit of a select group, and it's not…

MUNSON: It is a select group.

GRAYSON: You had your opportunity to teach. Were you also required to do research? I mean, obviously the mass spectrometer was here, and you were expected to do something with it.

MUNSON: Yeah. I had graduate programs.

GRAYSON: Okay. So, what did you do with your mass spectrometer?

MUNSON: Well, we had two. We had an old time-of-flight and the new 110 [CEC 21-110]. We did some ion chemistry with the time-of-flight. It was a departmental instrument, so it was used for analytical spectra for other faculty. And we had time on it for doing research. We modified the 110 to go up to high **<T: 40 min>** pressures, and doing chemical ionization studies with that. And then we got—later on—we got another instrument, an old instrument donated from Du Pont. I can't remember which one it was now, what the number was.

GRAYSON: This would have been in, what, the mid-1970s?

MUNSON: Yeah. Something like that. And again, modified and used that, partly for department use, and partly for research in chemical ionization and general ion chemistry.

GRAYSON: So, the primary thrust of the work here was in the fundamental ion chemistry stuff and chemical ionization?

MUNSON: Yep.

GRAYSON: Your graduate research program, how many students did you end up getting over time?

MUNSON: Oh, somewhere in the twenties.

GRAYSON: And these came from…did they come out of Delaware, or mostly from other parts of the country?

MUNSON: Mostly from this region. Delaware has been a predominantly regional university. Most of our undergraduates come from the immediate region, more of them from out of state than in state. There are not that many Delawareans who are going to go to graduate school…and again, if they come here as an undergraduate, we suggest they go someplace else. But there are a large number of schools on the East Coast. You name it, there's…

GRAYSON: Oh, yeah.

MUNSON: …there's a large number of whatever it is. Most of our graduate students at that time came from the schools out here. We've gotten more of an international population now, but still, a reasonable fraction of them come. And this weekend, we are hosting here the Intercollegiate Student Chemistry [Convention], an undergraduate conference that we've taken undergraduates to for years. It's been going on for sixty years maybe, or something. I've gone to it for 40 years. It's not officially intended to be recruiting, but it's a way then of making sure that the small private schools in the Pennsylvania area, and there are a lot of them, know about Delaware, and we get a fair number of students coming.

GRAYSON: And so, it's an opportunity for undergraduates to show research?

MUNSON: Yes. Yes. It's an opportunity for the undergraduates to talk about their research, because undergraduates have done research in chemistry for years.

GRAYSON: Most of the experiences that I've had, that's not a common thing. But then, you know, it's probably something that's more regional than national, although I think that it's becoming more common in most schools for undergraduates to be involved in research programs. And at least I wasn't…well, maybe I was, I guess, and I didn't think of it so much as research. So, basically the research that you conducted primarily followed in the lines of the chemical ionization studies.

MUNSON: Yeah. I was continuing what I'd started in Baytown—what Frank and I had started in Baytown.

GRAYSON: And were there any significant advances that came out of that, that you think were important to the area in terms of the stuff that was done here?

MUNSON: We were trying what I felt was some exotic chemistry. I'm not sure how successful it was. We were doing both exotic chemistry and thermochemistry, trying to get heats of reaction, heats of formation of ions, and proton affinities, reactions of…again, trying to get selective reagents. Isobutane is a selective reagent. Well, tetramethylsilane is an analog to that, and tetramethylsilane if sufficiently volatile, if you put tetramethylsilane in, you get the trimethylsilyl ion. You have to fiddle around with the conditions on that. And you can then use that for selective detection in certain classes of compounds. You can put other reagents in so that you have trimethylsilyl atoms, and you can get a displacement reaction that increases the selectivity of the process that you're doing. If you use ammonia, and **<T: 45 min>** there was…high pressure of ammonia was fine, but there are lots of things it didn't react with, because the ammonium ion wasn't much, and the reactions got, sort of, complicated.

And the people at Allied—oh, Lord, years ago—discovered accidentally that if you put ammonia in before you use methane, there'll be a little bit of ammonia that's left behind, so you will have then in your reagent gas a mixture of methane ions and ammonium ions. And it was referred to in a phrase that one'd better not say these days, Polish ammonia, because the technician who developed it was Polish. But it worked fine, because you had then both 17, 29, and 18 to react, and you could see the adduct ions for that. Then we played around with that, trying to work out what were the best conditions, how one did this, and the mechanisms of the reactions that followed.

GRAYSON: So, chemical ionization had a reasonable run of application in the broader mass spec community, up until probably about the time that FAB came along?

MUNSON: Well, no. It's continued. I don't know that FAB completely replaced it. It's just that […] for a while I, sort of, kept track. Chemical ionization used to appear in the titles of the papers. Well, it doesn't anymore, because it's old hat. It's just there. You just routinely use it for that. It's still there.

GRAYSON: So, the number of people that use it, that use chemical ionization, is still fairly constant over time?

MUNSON: I don't know…I don't really know. I haven't gone back to look. But, sort of, looking at it, there are still…I was looking at, what is it, Science Citation Index on there. There are a few thousand papers that have chemical ionization somewhere in the abstract, talking about that. So, the technique is still used.

GRAYSON: And then this whole business of negative chemical ionization…did you have anything…did you do anything along those lines?

MUNSON: No. Unfortunately, I never did, because I never had an instrument that would determine negative ions.

GRAYSON: Ah, yes. A requirement. [laugher]

MUNSON: Yeah. Don [Donald F.] Hunt…well, he wasn't necessarily the first one, but Don did a lot in the negative ion CI.

GRAYSON: Other than the instruments, the two that were here when you got here and the one that was donated, did you end up purchasing other instruments during your tenure as a research mass spec type?

MUNSON: We got a couple of other instruments, but I never built any. We modified them.

GRAYSON: So, standard operating procedure, I guess, is to operate or modify the instruments after they got here?

MUNSON: Yep. Done some things with Doug [Douglas P.] Ridge on the ICR [ion cyclotron resonance]. Again, mechanisms of chem CI reactions.

GRAYSON: How do you spell his last name?

MUNSON: Ridge, R-I-D-G-E.

GRAYSON: Oh, Ridge. Okay. Ridge. I want to back up a little bit. When you were working at Esso, did you have any issues with publishing in house? I mean, normally the industrial concerns, they have some kind of vetting of what goes out. Typically I guess, in my experience, it's kind of a lawyerly issue. But I don't know if when you were at Esso…

MUNSON: Well, not really. And again, this was, sort of, vestigial from Joe Franklin. The work that we were doing was not company confidential of any significance. In fact, everything had to be approved by the lawyers, but it really wasn't a major problem with that. We never had any difficulties with presenting papers at meetings or submitting things for publication. Chemical ionization was not what they considered a major proprietary…there's a patent on it.[7]

GRAYSON: Yeah. I was going to ask you about that, if there was a patent on it and if…

MUNSON: There was a patent. That was my sole patent at Baytown. The company never did anything with it, and it went to SRI [Scientific Research Instruments Corporation]—the company that Marvin [worked for]—Bill Johnston's company down there. And they then used it.

GRAYSON: So, SRI purchased the patent?

MUNSON: Yeah. From Exxon—from Esso.

GRAYSON: License, or whatever you want to call it.

MUNSON: Yes.

GRAYSON: As a result, did you get any…I mean, was there any financial **<T: 50 min>** remunerations in terms of the patent? There wasn't anything?

MUNSON: Nope.

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[7] M.S.B. Munson and F.H. Field, "Process for Chemical Ionization for Intended Use in Mass Spectrometry and the Like," U.S. Patent 3,555,272. Issued 12 January 1971.

GRAYSON: So, what's the use of patenting things? You had to obtain your own research funds when you were here. NSF [National Science Foundation]?

MUNSON: Yeah.

GRAYSON: Primarily?

MUNSON: I got a grant from…well, the University has a small fund, and I got a grant from them. I got some funds from the Petroleum Research Foundation, the National Science Foundation, and the NIH [National Institutes of Health] over the years.

GRAYSON: How has that changed over the years?

MUNSON: It's much more difficult to get money. And again, you have to be more specific. To some extent, you do some research to get enough so that you can write the proposal to get the next grant on it. I'm uncharitable. I'm not sure that the NIH has ever funded research. I contend that they fund development.

GRAYSON: I see.

MUNSON: You have an idea that you're reasonably sure how it's going to come out, and you have to present what you think it's going to be. And the National Science Foundation has gotten more like that over the years. It's very difficult to get undirected funds that are there. And they need to be tied into this goal or that goal or the other one.

GRAYSON: So, it's more […] towards the development of an application, or a…

MUNSON: Yes.

GRAYSON: …highly directed result. The opportunity for serendipity is not really…

MUNSON: Is not as high.

GRAYSON: …very good. Well, since serendipity counts for so much in the research world, it's unfortunate that's—that's the way it's working out. So, you've had an opportunity to work in both industry and academia, but I think your industrial experience is from an era that no longer exists.

MUNSON: It doesn't.

GRAYSON: So there is no way of comparing it.

MUNSON: That no longer exists at all, at all.

GRAYSON: And what about in your tenure here of what, forty years? Close to?

MUNSON: Yep. It'll be forty-three years.

GRAYSON: How has your employment experience in academia, changed over that period? Other than you've become an honored faculty member.

MUNSON: I don't like to say it's become more difficult, but to some extent it has become more legalistic, more of a problem about things. Quite a bit is reasonable. I suspect all of it's reasonable, but it doesn't make any difference. It's going to happen, anyway. The safety issues of things. I agree that when I was in graduate school, safety was not one of the things that was even discussed or considered. Friends and I in graduate school…benzene is a very low level carcinogen—because I'm still alive and I know some colleagues of mine who are still alive—I washed my hands in benzene a few times a week for a couple of years, because I was getting rid of polynuclear aromatics, which I knew were worse.

GRAYSON: That's a good solvent for polynuclear aromatics.

MUNSON: Yes. But there are things that we can't do now in the lab, and there's so many things that we can't do in the lab. Increasingly, the paperwork—you have to document all of the things that are there. And it just gets to be tedious. I don't know that we're any better off for it. But I don't know that there's anything that's going to happen. I think it's going to go…you have to continue justifying everything it is that you're doing on a continuing basis. There's less and less of a presumption that you actually know what you're doing and you're trying to do it right.

GRAYSON: It's somebody watching over your shoulder, or helping you watch over your own shoulder…

MUNSON: Yes.

GRAYSON: …to see that you do it right. Yes, well, I hear that, and I understand it.

MUNSON: Your sons are likely to be very, very cognizant of this.

GRAYSON: Yes. Well, Scott's in chemistry. Matt's an electrical engineer-physicist. So, **<T: 55 min>** they both do lab work, so they have to deal with laboratory issues. My youngest son at Tulane is concerned about the funding mechanism for research being not supportive in the coming years. I don't know. He sees the burden of the…funding from the government being…it's where primarily almost funding for research comes these days, as not being supportable into the future. He thinks it's going to run out of steam. I mean, the number of people doing research that want money, and the amount of money, the number of people is going up. The amount of money is going down.

MUNSON: Oh, I agree with it.

GRAYSON: He's concerned that getting funded is going to become more and more difficult with time.

MUNSON: I agree with him. I think it's going to become more and more difficult in time and the number of sources go down. My objection to the universities is they claim they support research, and they lie. They don't.

GRAYSON: They support overhead.

MUNSON: They support overhead. You go out and you get the money, and you do the things to bring it in, and you're supposed to bring in money. If you don't bring in money, you won't get promoted. You won't get retained. And looking at it, if you pay attention to what's done, the different government agencies tend to support things in a particular area that they want. You have to be working in that area.

GRAYSON: So, what is the overhead rate here?

MUNSON: I've been out of the research game for a few years, so I don't [know].

GRAYSON: Okay. But I imagine it's like 50 percent or something like that.

MUNSON: Something like that. Yes.

GRAYSON: It's typically large. Yes. Well, that's an issue.

MUNSON: And if you go back, I'm not sure that there really is a need for the amount of…the number of Ph.D.s that we produce. Du Pont has cut back…we see things here. Du Pont has cut back significantly over the years in the research that it does. It's mostly development of things that are oriented around a particular project. They used to have…Central Research Division used to do lots of things. They don't do that much anymore. AstraZeneca and the pharmaceutical companies are beginning to cut back, and they're doing things that are there. So, I'm not sure that there's a need for that many PhDs […]. And the academic game is based on that: that you need graduate students to get the work done.

GRAYSON: One of the things that the United States has as an export, I suppose, is education. I mean, obviously, you're aware of the fact that we're educating a large number of foreign students.

MUNSON: Yeah.

GRAYSON: However, many of them stay instead of going back. But there is still that demand and recognition that the United States provides good education for people who want to pursue highly technical fields, and I guess they can always go back to their native country if they, you know, can't find…

MUNSON: Yeah. I don't know enough about that, as to the ones who go back. Of the ones who've I've seen at Delaware, most of them stayed here rather than going back. And I don't know how the University is trying to develop…our new president [Patrick Harker] is trying to develop more of an international reputation, and so we're getting some foreign undergraduates. I don't know how that program is going to work. It's sardonically amusing that the science and engineering departments have been faulted in the past for foreign graduate students who are teaching labs and the undergraduates cannot understand them, but at the same time, we're supposed to be increasingly diverse and international, and so forth and so on. Methinks they talk out of both sides of their mouths.

GRAYSON: Well, I know at least at Washington University [in St. Louis], the graduate students that come in the chemistry department are almost…I would say 70 to 80 percent from outside of the country. It's a very high percent of…

MUNSON: We're not that high, but it's increasing. It's increased over **<T: 60 min>** the years.

GRAYSON: As a matter of fact, one of our faculty members recruits in China. They sent him to China for…

MUNSON: We haven't done that yet. We haven't done that yet. And it is a problem, because most of the graduate students are supported for a year or two as teaching assistants.

GRAYSON: Yes.

MUNSON: And I can without any problem at all imagine a student taking a general chemistry lab who isn't either terribly knowledgeable about chemistry or terribly interested in chemistry, having difficulty understanding the lab instructor explain what's going on. And I don't have any…I don't know what to do about it. And the universities talk about diversity and all the things that are involved with that. But then we do have orientation meetings and all kinds of things for graduate students, not just for the foreign graduate students, but for graduate students, to make sure that they know what it is that they're doing in the labs, and to improve the quality of lab instruction. But we've never done anything useful, I don't know that anybody has, of trying to take advantage of the fact that you do have people coming in from a different culture, and to talk about things. It's easy to watch. I go to all my labs, so I know. It's easy to tell the difference. The domestic students can talk about things that they did, and that's helpful and useful to the freshmen—to the students. The foreign students aren't comfortable about doing things, because their educational experience was very different from what it is here. And they can't talk about what it is that they did with the idea that it's going to…or they don't talk about the idea of what they did in their undergraduate degree, as to how it's going to affect the current students.

GRAYSON: Yeah. Well, things are changing, that's for sure, and this…what is it, global economy, global…

MUNSON: Yeah. Might as well start learning Chinese.

GRAYSON: Yeah. Well, my ability to learn new things is really rapidly diminishing.

MUNSON: So is mine.

GRAYSON: Now you've been associated with mass spectrometry since the sixties, some-odd, and you've been active in the ASTM E-14 organization, and ASMS [American Society for Mass Spectrometry], over the years. How did t you get involved in all that good stuff? Were you kind of drafted or nominated?

MUNSON: The first mass spec meeting that I went to was probably in 1963, and I went because neither Joe nor Frank either wanted to go or could go. And I think it was a meeting in San Francisco, [California], and it was a plum. It was great to go to the meeting, because I think that was…that was probably my first meeting to do that. I can't remember what I talked about, but I think it was Fred Lossing who came up afterwards and talked to me about what it is, about the paper that I had given. And that was one of the things that I noticed about the early mass spec meetings, a collegiality, a geniality, a general interest about them.

GRAYSON: They were smaller.

MUNSON: They were much smaller. They were much smaller.

GRAYSON: Probably 1963, what, maybe 300 or 400?

MUNSON: Yeah. They were much smaller. And only [shortly before then they went] to two parallel sessions. The old-timers complained about that, because they couldn't go to everything. People tended to go to everything. And then continued going to the E-14 meetings on a fairly regular basis. And then I can't remember when it was that they decided to form ASMS, but I was there at that meeting, that one. I went to most ASMS meetings from then until fairly recently. I don't go anymore. I've stopped…essentially, I've essentially stopped research, and I don't like traveling.

GRAYSON: So, your involvement in the organization began when—of ASMS?

MUNSON: Oh, I was a charter member of ASMS. **<T: 65 min>** I suspect I was nominated for president…again, because I had been working with Joe. [laughter] And in those days, you were…if people had heard of you, you had a chance of being elected, if people hadn't heard that much about you. I'm not sure that policy or anything like that for ASMS was one of the things…we ran a meeting, and that was what it was we were doing.

GRAYSON: So, somehow your name came up and you were nominated?

MUNSON: Somehow my name came up and I was nominated and I was elected.

GRAYSON: And this was after it as ASMS, I think, wasn't it?

MUNSON: Yes. It was after it was ASMS.

GRAYSON: So, that would have been in the…well, you did the whole six-year tour, though, didn't you?

MUNSON: Yes.

GRAYSON: You start out for programs…

MUNSON: Yes. Vice president for programs. I was vice president for programs in 1979, at the meeting in Seattle, [Washington]. And that was the first time that we had posters. And it was a local mom-and-pop group that did it. They had difficulty…at that meeting, I saw every paper that was given. I heard every paper that was given, *briefly*, because I continued circling around through the entire meeting, on there. I didn't hear any content of any meeting, of any talk. I didn't stay to one…

GRAYSON: They were doing content…

MUNSON: It was just going around, looking, and make…I was just hyper about making sure how things were going to be, because there was a United Airlines strike just before that. And if it hadn't been settled, we would have been in real trouble, because people couldn't get to Seattle. And we had all kinds of…Jim [James A.] McCloskey was president at the time, and we were nervous about lots of things.

GRAYSON: And so, how were the poster sessions at that time?

MUNSON: They were okay, but they…like I said, that was our first time to do it, and the lighting wasn't terribly good, and you had to set things up and all kinds of things. They got better, obviously, as it went on. And there was also the problem at that time that posters were considered second class. You gave a talk. I changed—I haven't given—I've given a couple of invited talks, but other than that, all my presentations have been posters. I much prefer posters. They're much more fun to do. I contend that if you have a have a large audience, give talks, but a third of them came to hear the talk before you and are just staying because they don't want to get up and leave. A third of them came to hear the talk after you, and that's fine. Of the third that came to hear your talk, half of them will decide about halfway through they're not interested in it. So, you're talking to a large group, most of the people who are not there, but if you have a poster…and my ground rules for posters were I warned people that if they slowed down, I would talk at them. I don't just have the poster there, that those are my slides, those are my talks…and they were fun. I was exhausted when the days were over, because I'd stay there all day talking about the poster. And I would see some people sort of avoiding me because they weren't interested. [laughter] It was fun. I liked doing that.

GRAYSON: And I think it has been used wisely by the Society, and they do have a good set of guidelines for helping people to put posters together. They've all gone to reduced size overall because of the printing limitations of some of the software, some of the four-way poster boards that are kind of not really totally used by most posters, because they don't get…

MUNSON: No. I know they're not.

GRAYSON: They don't get printed out.

MUNSON: And I'm not sure that…well, it's not going to change. People are going to do it that way, and that's the way it figures. The advantages of the earlier things, put them up on 8-1/2 by 11 pieces of paper and you put them together, was that you could prepare your poster, finish your poster the Friday before you left. Now, you've got to get it done well in advance, and it's sort of hard to make the last minute changes. The other one…there's something wrong on this one. I can change it.

GRAYSON: Yeah. No. Yeah.

MUNSON: But nobody's going to do those anymore.

GRAYSON: Well, I have one of these last minute operating persons for one of my sons, and he actually ships or brings his poster on a thumb drive or whatever to the meeting, **<T: 70 min>** and runs to Kinko's and has it printed out, you know, at the last minute.

MUNSON: [laughter]

GRAYSON: Get all the changes and stuff done, and then…

MUNSON: Yeah. Because that's what you do at home, now. Again, that's changed, because earlier, you were stuck with the slide.

GRAYSON: Right.

MUNSON: You couldn't make the last minute changes.   It took a while to get the slides prepared. Now you've got your PowerPoint presentation. You can go in it, you can edit it the morning that you give the talk.

GRAYSON: It's kind of…like they say, it's changed between your posters, you have to prepare ahead of time, and…well, actually, in the old days, you had to have your slides, you know, done. I mean, obviously, at McDonnell Douglas, when I was working there, everything had to be done by their group, the technical art people. They did all the artwork, and they produced the slides, the whole nine yards. So it was a big deal. You had to get it in plenty early, so that you'd, you know, get it done.

MUNSON: Yeah. Well, when I came here, we didn't have any good way of doing it in the…you had to get your slides ready and take them to a commercial place to get them done. And yeah, they could get them done overnight, maybe.

GRAYSON: When did you sign off on the research end of things?

MUNSON: I'm trying to remember exactly when. Maybe four, five years ago. I've been threatening to retire for ten years. I will retire one of these years, or die. I'm not sure which. And graduate students are a commitment of four or five years. And I wasn't really sure that I was going to stay that long. So, I just got out of the game.

GRAYSON: And did someone else take over the mass spec operation here?

MUNSON: Well, we have multiple mass spec groups.

GRAYSON: Okay.

MUNSON: For a while, we had Jean [Futrell] and his group, Jean Futrell.

GRAYSON: Futrell?

MUNSON: Futrell. Me and my group. Doug [Douglas P.] Ridge and his group. And Murray [V.] Johnston and his group. Okay? A lot of mass spectrometry was going on here. Jean left. I stopped. So, we have Doug and Murray who are still doing the mass spectrometry there. So yeah, mass spectrometry is still going on. I'm just not involved with it.

GRAYSON: When did Jean come here, Futrell? He was at Wright-Patterson Air Force Base for a while, I think?

MUNSON: He was at Wright-Patterson, and then he was at [University of] Utah. He came here from Utah.

GRAYSON: Oh, okay. He went to Utah.

MUNSON: He came as the department chair. Now when did he come? That is a good question, the answer to which I don't know. He was the chair after Wahrhaftig for…but that doesn't really do me any good. [Donald B.] Wetlaufer was chair from 1975 to 1980-something, and then Jean probably came in the late eighties, maybe.

GRAYSON: Okay. Thirty—twenty years ago or so.

MUNSON: Yeah. And he was here for **<T: 75 min>** ten to fifteen years.

GRAYSON: And he brought a research group with him from…

MUNSON: Yeah.

GRAYSON: …Utah, with equipment?

MUNSON: Yes.

GRAYSON: And so, he was always interested in MSMS type things, with induced association stuff.

MUNSON: Yeah.

GRAYSON: Carried all that stuff on. Did you guys collaborate on anything?

MUNSON: We didn't collaborate on any papers. We had one of these National Science Foundation multidiscipline, industry, university programs that Jean got organized on that. But we never…we're not coauthors on any papers. I was on the committees of his students and vice versa.

GRAYSON: So, it seems like coming in as department chair doesn't give you much time to do research? Or is this, you know, a low level department chair job?

MUNSON: Oh, I'm not department chair.

GRAYSON: No, I mean Futrell. You say he came in as department chair?

MUNSON: Yeah. He came in as department chair, but he had Anil [K.] Shukla as a postdoc, and so his group was going on. It wasn't huge, but mass spectrometry groups are not huge, because you're sort of instrument limited. And he continued to build, and he was here for ten years, ten to twelve years, before he went out to Pacific Northwest National Laboratory.

GRAYSON: So, he was he chair during that whole period?

MUNSON: Yeah. He was chair for two five-year terms.

GRAYSON: Okay. Typically, the job of a department chair is not looked upon as something you want to get involved in, and most…

MUNSON: No, but Jean liked administration.

GRAYSON: Okay. I guess so, if you like it, it works okay. Now he's still at Battelle, I think, in Northwest?

MUNSON: Yeah.

GRAYSON: I had forgotten that he was up here. So, you've seen a lot in the mass spec business. What do you think about the recent developments…I guess they're recent - about the last ten years, fifteen years - of these new innovation techniques that open up the world of biology?

MUNSON: I think John Fenn deserved his Nobel Prize [in Chemistry, 2002], because the electrospray is so different from every other kind of ion formation. All of the others—the fast atom bombardment, the fission fragment, CI, FI [field ionization], FD [field desorption]—all consist of taking something and producing the ions from the neutral species. And electrospray, you start out with the ions and you get rid of the other stuff. And it's just such a tremendous difference in the way of thinking about that. I don't know […] Marvin's thermospray…

GRAYSON: Thermospray?

MUNSON: …was relatively close, and I don't know whether Marvin feels aggrieved [about] that? [laughter]

GRAYSON: Not really. We talked about that a couple of months ago. He thinks it's great, you know, that the electrospray came along and superseded his technology. Even though, I mean, it displaced his company. But, you know, Marvin is not one to worry about the past. He's ready to move on to the future.

MUNSON: Okay. But the thermospray was the only thing that was even remotely close, because the electrospray was just discontinuously different than everything else, and it has certainly tremendously changed mass spectrometry. I can remember Catherine had a paper in

Analytical Chemistry many years ago, twenty years ago, something like that, talking about middle molecules…[8]

GRAYSON: Yeah. […] I remember that.

MUNSON: And middle molecules were a couple of thousand, and talking about the thing. And at that time, getting up to molecular weights of a couple of thousand, that was a heroic effort. That was a heroic effort. And Hank Fales had some things—some fluorinated polymers—that were used…mass range in there. But it was just a formidable task to get up there. And now you can do whatever it is that you want.

GRAYSON: Yeah. It's pretty amazing. The other thing I find truly strange or amazing is the phoenix-like rise from the ashes of time-of-flight mass spectrometry. Obviously, there was a time-of-flight [machine] here, when you came here, which was a Bendix machine, I'm pretty sure.

MUNSON: Yep. There was no other kind. [laughter]

GRAYSON: Yeah. That was it. And its performance mass spec-wise was certainly nothing to be…

MUNSON: No. Had a mass range of about 250, something like that. One of my first graduate students and I hooked it up to a GC [gas chromatography], because, again, you could get the fast spectra. And so GC, CI, MS…not terribly good, but it worked. You could show that you could GC CI MS that way. But yeah, the time-of-flight, since then, it just improved tremendously. Well, same way with the magnetic instruments. When we had the 110, it was intended for photographic plates, but the photoplate reader cost six thousand dollars, and we never had a photoplate reader, so we always used it for electrical recording. The magnet was slow and in order to go back and do things you had to recycle the magnet, you had to cover a certain range, and so forth and so on. **<T: 80 min>** And when our electronics guy put a computer on it, we would have it set up so that it would scan for…you'd have it cycle for a while, and not a short time, but cycle for a while, so that you could go back then and get it to cover the…to get reproducibility for what you wanted.

GRAYSON: So, you actually end up having your own computer MS down here, with…

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[8] Catherine Fenselau, "Mass spectrometry of middle molecules," *Analytical Chemistry* 54 (1982): 105A-16A.

MUNSON: Yeah.

GRAYSON: …local talent, and that kind of thing? Yeah. The time-of-flight thing, I guess the reflectron is one of the things that moved it forward in terms of resolving power, and then the electronics have evolved to the point where they can get the kind of and sizes of pulses they need in the ion source to make it work.

MUNSON: Well, and the same way for the quadrupoles. The quadrupoles initially were a GC instrument, because they would go up to 250 or 400, something like that. But they scanned fast. And now you can get quadrupoles that…

GRAYSON: Up in the thousands range.

MUNSON: Yeah.

GRAYSON: What do you think about biology taking over mass spectrometry?

MUNSON: I'm not in that part of it. [laughter]. You could see it in the ASMS meetings, because so much of it is biological applications. So, those of us that have been around for a while, the physical organic chemists were taking it over for a while. The fundamentalists are there, and they're still there, but it's a decreasing fraction of the group. And we were always concerned as to whether or not they were going to go form their own splinter group, and to some extent they have. But there are still fundamentalists that are there. The physical organic were in there for a while, and then the…

GRAYSON: Environmentalists. The environmental applications.

MUNSON: Oh, the environmental…the metabolites…going back, GCMS [gas chromatography mass spectrometry] for the metabolites. But then the complete burgeoning of things, high molecular weight and biological systems that are there. I don't do any [of that]. I never did any of that, and have no…I understand its practicality and its utility, but I like small enough of molecules that I can see, and think of them as their own piece.

GRAYSON: I'd like to just touch base on some of your personal interactions with individuals that have, you know, established themselves in the field. I don't know how much interaction you had with John [H.] Beynon during your career.

MUNSON: I met John Beynon and visited with him on several occasions at ASMS. And in fact, there have been a few cases where he and Fred Lossing and I would go out and take an afternoon off for the mass spec meetings. I enjoyed meeting with John. I used to have his book, and I recommended his book to everybody, because that first book was absolutely one of the best things that was written about mass spectrometry, with fundamentals and details of things that were there.[9] I enjoyed very much knowing John.

GRAYSON: The table of mass spec…

MUNSON: Accurate mass.

GRAYSON: …accurate mass calc in the back, apparently he calculated all those, and his wife checked them, by hand.

MUNSON: Okay. That I would believe. That I would believe.

GRAYSON: Which is pretty amazing. I mean, it's a classic. It's a book to make sure you've got on your bookshelf, if you're in the mass spec business. What about Klaus? Klaus Biemann?

MUNSON: I never knew Klaus [until] recent years. When he was more active, I didn't really know him, because we were working in different areas, on the fundamentals. Attended the fundamental section, the ion chemistry, thermochemistry, and so forth, so I didn't really know Klaus at all.

GRAYSON: And of course, we talk about Joe Franklin. Unfortunately, Joe really…it's unfortunate we don't have an opportunity to talk to him.

MUNSON: Yeah.

GRAYSON: But I think through individuals like yourself, we get some insight into his character, personality, and importance in…

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[9] John H. Beynon, *Mass Spectrometry and Its Applications to Organic Chemistry* (Berkeley: University of California Press, 1960).

MUNSON: Oh, I think he was very important, certainly in ion chemistry and mass spectrometry in general. He was the second president or the first? [Franklin was the first ASMS president 1968-1970.]

GRAYSON: For **<T: 85 min>** ASMS?

MUNSON: Oh, yes.

GRAYSON: I've got the records. He was an early…he was one of the early ones.

MUNSON: He was certainly one of the instigators in forming the Society, and he had a major influence over things. And he was a very good friend. He was a very good friend and a mentor to me. My mass spectrometry came from him and Frank. And he was very gracious. He was a good cook.

GRAYSON: Oh, really?

MUNSON: He was a very good cook. He wasn't…at the last he wasn't allowed in the lab.

GRAYSON: Yeah. I was wondering if during your tenure there at Humble, if he was a hands-on kind of guy, or was he a management level?

MUNSON: No. He was management level, and he didn't…he had been at management level. [laughter] He had ideas to help you, and he wasn't a dominating personality. It wasn't like, “You do this, and you do that, and you do the other.” He was a very good mentor.

GRAYSON: He just invited the process of interaction on a scientific problem? He, you know, guided it, but he didn't…

MUNSON: Yeah. He guided. He didn't direct it.

GRAYSON: He didn't say, you know, “This is what we need to do.”

MUNSON: No.

GRAYSON: He's going to help you see that that was the way that you needed to go. Yeah. Frank Field spoke very highly of Joe Franklin as a super individual.

MUNSON: Oh, he was. He was, very much. And after I moved up here, and he was at Rice, I would come down and I would generally meet them once a year, a couple of times a year, something like that, when I was down. And we would go out to dinner. We were eating our way through the restaurants in Houston. Joe was a gourmet cook, and he enjoyed his food, and knew all about it. And it was interesting, because we would go out, and I would be telling the waiter what to do, the check, so forth and so on, so that it was obvious to anybody that I was the host, but Joe got the check. I managed to get the check back frequently, but Joe got the check on that. We took a couple of long trips, went out to one ASMS meeting, and then drove up the California coast to Canada on that one.

GRAYSON: Oh, wow.

MUNSON: And it was fun. I enjoyed that.

GRAYSON: So, why did he leave—when he left Humble to go to Rice? Was he sought by Rice?

MUNSON: Yes.

GRAYSON: So they wanted him?

MUNSON: They wanted him. They wanted him. He went as the first [Robert A.] Welch Professor [of Chemistry] at Rice. And [they] set up a lab for him there.

GRAYSON: So, do you recall what kinds of things he was doing with the lab there?

MUNSON: Ion chemistry.

GRAYSON: More ion chemistry stuff?

MUNSON: Yeah. Ion chemistry.

GRAYSON: I guess, Rice has always been a fairly prestigious institution, and it's probably much smaller than…but it was an important school, one of the most important schools in Texas.

MUNSON: Yeah. It was smaller then. I'm not sure that Rice has grown that much in size. It was a private school always.

GRAYSON: Yeah.

MUNSON: They've diversified, I think, over the years. It initially was Rice Institute.

GRAYSON: Right.

MUNSON: Primarily science and technology. I think they've expanded, call themselves Rice University, but I think it's still Rice Institute. But no. They went to get Joe.

GRAYSON: And unfortunately, he didn't have a very long tenure there before he died, did he, when he was…he died, kind of, young in age.

MUNSON: I'll look at that, because I have something on Joe. Or at least I think I have something on Joe that's over here. 1982.

GRAYSON: 1982. So, well, I guess he was there for…

MUNSON: He was at Rice for ten years.

GRAYSON: Ten years.

MUNSON: Oh, no. Longer than that. Twenty years, because he went to Rice in 1963. He was the chairman for **<T: 90 min>** probably five to ten years, something like that.

GRAYSON: Is it possible I could get a Xerox of that? Because it would be I think valuable information. He was an important person, and we don't have anything like this about his…this would be great. We'll try and get a copy of that. So, have you got a file on Joe?

MUNSON: Mm-hmm.

GRAYSON: Do you have any…well, do you have any good pictures, besides the…

MUNSON: That's the standard picture.

GRAYSON: Yeah.

MUNSON: He liked that. I can't remember the name of the…there was a photographer in Houston who took standard pictures like that.

GRAYSON: Yeah. That'd be great to have something to document this and get that file to CHF [Chemical Heritage Foundation].

MUNSON: Yeah. Looking at this […] it was at the Seattle meeting, I guess, 1979. So, after the meeting, we went up into Canada. Joe and Mildred [Franklin] made the arrangements, and I did the driving. And they were great fun. They were great fun. And Mildred also was a very congenial, very helpful. And the pair of them did all the things that you would think. They routinely had dinners. They would invite people over, graduate students, postdocs, visiting faculty, and what have you.

GRAYSON: Now of course you mentioned Earl Lumpkin, but then Earl was primarily involved in the analytical side of the lab there.

MUNSON: Yeah. Earl and Tom were there, developing analytical procedures. And practically all analytical chemistry as such. But some interesting things that were done in that group. I don't know whether Earl is still alive or not.

GRAYSON: I'm not sure. If he is, he's living in Round Rock, [Texas], north of Austin, [Texas].

MUNSON: Yeah. I haven't seen him in a long time.

GRAYSON: Yeah. Henry Rosenstock.

MUNSON: I knew Henry slightly, again, through ASMS and through Joe. But that was about it.

GRAYSON: So, they were doing…somewhere I heard that Rosenstock was at the National Bureau of Standards doing these fundamental studies…

MUNSON: Yes.

GRAYSON: …in appearance potential and that kind of thing.

MUNSON: Yeah. And calculations of rate constants of reactions.

GRAYSON: O.P. Tanner? Did you have any interactions much with O.P.? He worked at Monsanto.

MUNSON: No. I don't recall him at all.

GRAYSON: Actually, O.P.'s the guy that got me involved in the field, and took me to my first ASTM conference. That was fun. Out in Dallas. And of course, we talked about Frank Field…interaction with Frank.

MUNSON: Yeah.

GRAYSON: So, he was, kind of, a mentor for you as well as Franklin.

MUNSON: He was. I said I started out and contend I was Joe's first postdoc, and then I was a junior colleague with Frank, but I was a colleague. By Joe's definition, I was a colleague. In reality, I was his first postdoc.

GRAYSON: So, it seems like the ion chemistry group had dissipated pretty much about the time…well, Franklin left first, right, and then…

MUNSON: Yeah. He left in 1963. Frank left in 1966. I left in 1967.

GRAYSON: So, it was just a gradual evaporation of the troops?

MUNSON: Yeah. Oh, of course, the other interesting thing was that Jean Futrell was in Baytown.

GRAYSON: Oh.

MUNSON: For a while. For a year or two, maybe.

GRAYSON: Okay. I wasn't aware of that. Did you have any interaction with him then?

MUNSON: No.

GRAYSON: He was off doing something different, I suppose.

MUNSON: I didn't. Yeah. I think he was there before he went to wherever it was in Ohio.

GRAYSON: Wright-Patterson.

MUNSON: Wright-Patterson. I think he was there, because he did have a…I think he had to have been **<T: 95 min>** there for a short time in radiation chemistry…for a short period of time.

GRAYSON: Now you mentioned Marvin Vestal. Did you have any interactions with him in any way?

MUNSON: When Marvin was at, I think it was SRI, in Baltimore, I went down and, sort of, consulted briefly with them, because we had made a…they were designing the source for the

MS9, and we built a high pressure source—made a high pressure source—and something like that. And then I kept up with Marvin over the years. He's just a delightful person to know.

GRAYSON: Yeah. He's a very energetic fellow.

MUNSON: Very much so.

GRAYSON: And full of ideas.

MUNSON: Now he has an interesting history. [laughter]

GRAYSON: Well, I know. I just met with him for a day. He's being granted patents even as we sit here on, you know, his ideas about how we're going to revolutionize the proteomics business.

MUNSON: Oh, yeah.

GRAYSON: And his vision is one that he holds dearly and is promoting and working hard to make happen. So, it'll be interesting to see in a year how things turn up.

MUNSON: Yep.

GRAYSON: Oh, okay. What other ideas and things do we have to talk about here? What would you consider your most significant publication, out of everything that you've done?

MUNSON: Well, I guess I'd better consider the first CI paper as the most significant publication.[10]

GRAYSON: I guess that kind of probably put you on the map, didn't it, amongst a whole bunch of other people?

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[10] F.H. Field, J.L. Franklin, and M.S.B. Munson, "Reactions of gaseous ions. XII. High-pressure mass spectrometric study of methane," *Journal of the American Chemical Society* 85 (1963): 3575-83.

MUNSON: Yeah.

GRAYSON: And that pretty well…did you ever keep track of the number of reprint requests you got for it, or did you run out of…

MUNSON: No. I ran out of that a long time…

GRAYSON: Yeah. That was back in the days when they'd send postcards [for reprint requests], and [you would] send them off to people all over the place. And that was kind of fun. Nowadays, everything is online before…you can hardly get it out of your lab. Well, I think we can take a break here. I'd like to do a little bit of video recording. Is there anything else that you want to [say]—words of wisdom that you want to bestow on us while we're at it?

MUNSON: No. You're in charge of this. I'm just here.

GRAYSON: Okay. Well, we've talked about your experience in industry and academia, and we've kind of reviewed the differences that have occurred over that period of time. And it's interesting how things have changed, and I…

MUNSON: Oh, they've changed tremendously.

GRAYSON: It's hard to see they'll ever change back, that's for sure.

MUNSON: No. They won't change back.

GRAYSON: You never really had to do any research management in industrial environment, and then most of what you've done academically has been in mentoring graduate students, which you…

MUNSON: Yeah. I was chair of the department for a while. Didn't like it. Director of the honors program for a while. And gave that up when I had a heart attack.

GRAYSON: I understand you have something of a connection to Texas. I mean, you go back there regularly?

MUNSON: Yeah. I'll put on my best southeast Texas accent for you. [laughter]

GRAYSON: So, is the plan to retire to Texas?

MUNSON: I don't know. Life gets more complicated. As I said, I've been threatening to retire for about ten years now, and I'm going to have to in a bit, because I'm beginning…I'm tired. I can't do the things that I've been able to do. And I don't know how to decide whether I'm going to stay here or whether I'm going to go down to Texas. It's a trivial thing, but I've lived in Delaware longer than I've lived anyplace else, but I never really paid much attention to it. Since I don't have a family, I haven't been involved in things. I'm—my connection with Delaware is through the University, and over the years, through the honors program. These are the people whom I know, and they're here. I'm not really planning to join the retirement community to do anything else. And I have the house down in Texas, and I have friends down in Texas. But the problem down there…. The house is fine, small town, and I can get around there, but the friends whom I visit don't live terribly close, and realism sets in every now and then, and I don't know how much longer I can drive 300 miles or how much I should do these things. I don't mind killing myself, but I don't want to kill anybody else on that.

So, I don't know. I've had more connections with the medical profession in the past five or six years than I ever wanted to, and I like my doctors up here, and I don't really want to change to go down there, **<T: 100 min>** but I don't know what I'm going to do. A colleague retired from here to the Houston area some time back, and his comment was, you don't have to shovel heat. [laughter]

GRAYSON: Yes. Yes. Yes.

MUNSON: And that's true. That's true.

GRAYSON: So, whereabouts in Texas is this property that you have?

MUNSON: Southwest of Houston. Wharton. It's my father's house.

GRAYSON: So, you're getting close to the Gulf again. Southwest.

MUNSON: Yeah. It's nominally the Texas Gulf Coast. It's not close enough to the coast that I have to worry about things. When [Hurricane] Ike came through [in 2008], he was sufficiently far from the coast that there wasn't…it would be wind damage, and wind damage is generally minimal, unless it's really outrageous.

GRAYSON: That's something to be decided in the future.

MUNSON: Yeah. I mean, and I don't have time [laughter].

GRAYSON: Okay. I think what I'd like to do at this point is maybe take a little break and then do a short video talking about some specific topics.

MUNSON: Okay All right.

GRAYSON: I'd like to pick your files for any pictures that you might have that would be interesting to—to pull out.

[END OF AUDIO, FILE 1.2]

[END OF INTERVIEW]

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