CHEMICAL HERITAGE FOUNDATION

HENRY EARL LUMPKIN

Transcript of an Interview
Conducted by

Michael A. Grayson

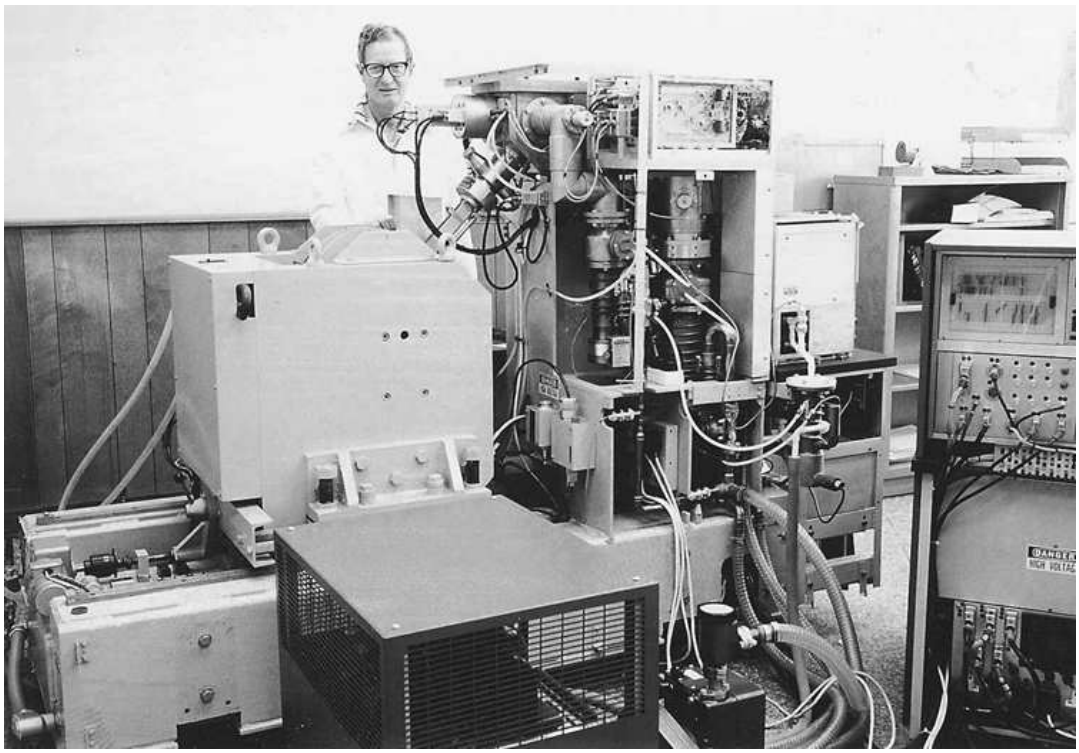
at

Round Rock, Texas

on

2 January 1992

(With Subsequent Corrections and Additions)



Henry Earl Lumpkin

ACKNOWLEDGMENT

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

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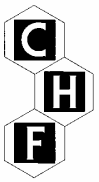

HENRY EARL LUMPKIN

1920 Born in Ingram, Texas on 1 March

Education

1941 B.S., chemistry, Southwest Texas State University
1942 Graduate work in meteorology, University of California, Los Angeles
1944 Institute of Tropical Meteorology, University of Chicago, San Juan, Puerto Rico Campus

Professional Experience

United States Army Air Force
1942-1943 Second Lieutenant
1943-1944 First Lieutenant
1944-1945 Captain

Humble Oil and Refining Company
1945-1948 Physicist in Research and Development
1948-1960 Research Chemist
1960-1963 Senior Research Chemist
1963-1965 Research Specialist

Esso Production Research Company
1965-1968 Senior Research Specialist

Exxon Research and Engineering Company
1969-1980 Research Associate

Honors

1938 Freshman scholarship , Southwest Texas University
1963 Secretary & Treasurer, ASTM E-14
1964 Director of Analytical Group, American Chemical Society, Southeast Texas Section
1967 Principle Speaker on Mass Spectrometry, Seventh World Petroleum Conference

1972	Chairman, ASTM E-14
1972	Vice President [Data and Standards], American Society for Mass Spectrometry
1982	Organizer and first President, “Young at Heart” Round Rock Social
1985	President, Cedar Park Chapter, American Association for Retired Persons

ABSTRACT

Henry Earl Lumpkin begins the interview by discussing his family background and how he learned to value education. His family had a tradition of attending Southwestern Texas State University and becoming school teachers. Watching his father's career persuaded him not to be a teacher and Lumpkin decided to major in chemistry and minor in physics and mathematics at Southwestern Texas State University. After graduating in three and a half years, Lumpkin joined the US Army Air Corps. In the Air Corps, he took graduate classes in meteorology at the University of California at Los Angeles. During World War II, Lumpkin was promoted to a captain and served as a Base Weather Officer. After the Air Corps, he went to work for Humble Oil and Refining Company as a mass spectrometrist. He then describes the state of instrumentation during his career at Humble and the innovations that were being made in that field. Lumpkin speaks highly of Humble's professional development program. Humble Lectures in Science Series allowed employees to take time away from work and attend classes taught by esteemed professors. Lumpkin also talks about the freedom he was given by the company's administration when submitting publications to research journals. He feels that the company's encouraging attitude towards scientific pursuit was very important. Along with his many achievements in the development of mass spectrometry, Lumpkin also played key roles in the American Society for Testing and Materials [ASTM] and the American Society for Mass Spectrometry [ASMS]. He was chairman of ASTM E-14 and Vice President of ASMS. Lumpkin ends the interview with thoughts on his colleagues and the future of mass spectroscopy.

INTERVIEWER

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

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INTERVIEWER: **Michael A. Grayson**

LOCATION: **Round Rock, Texas**

DATE: **2 January 1992**

GRAYSON: One of the things I think is particularly interesting about mass spectrometry at the time you entered the field is that it was an unknown area. In a way, you're a pioneer. Your parental background, education, and attitude toward science are important because you shaped a whole new field. Would you tell me a little bit about your background?

LUMPKIN: Let me start off with family background. It seems that most of the Lumpkin people were supposed to be school teachers. My parents, aunts, uncles, and even their spouses were all public school teachers, and they almost invariably went to Southwest Texas [State University] in San Marcos. Obviously, I was also expected to go there! [laughter]

I've got one older brother and one younger brother; and the oldest one is still alive. He attended Southwest Texas and became a chemist. That persuaded me to go to Southwest Texas and take chemistry. After being reared in a school teacher's family in Texas during the Depression, I quickly learned that I did <u>not</u> want to be a school teacher! Things were very difficult in those days. My father [Charles Everett Lumpkin] was always a superintendent or principal of an elementary or high school, and in spite of his high ranking, my family still had to move around.

These days, people spend their entire careers working for the same school district. In the past, however, teachers were elected by the board of trustees and had to move around. As a result, I saw what the life of a school teacher was, and did not want it. When I enrolled in Southwest Texas, I deliberately did not take any courses in education. That way, I could never be tempted to teach in a school. [laughter]

GRAYSON: Your older brother's interest in chemistry influenced you to study it as well.

LUMPKIN: Yes, he and my younger brother felt the same way I did about becoming school teachers. We were three boys living in a school teacher's family during the Depression days. We always had a garden, chickens, and a cow—all the things that we needed to do to get by—but living on a school teacher's salary made my brothers and me realize that we did not want to be in that profession.

As a result of our constant moving, I went to school to five different elementary and high schools. I finally graduated from a relatively small high school called Harlandale High School in San Antonio, Texas. I'm a central Texas man.

In my life, I guess I was most influenced by the fact that my parents were educated. All of my forbearers on my father's side were educated, and I learned the value of an education from them. In fact, my father's oration on his graduation from high school was titled, "Ignorance a Menace to the Republic." I've still got a paper he wrote in 1912 entitled, "The Value of an Education." There were thirteen people in his graduating class. E.T. Butt was one of them. His brother, Howard E. Butt, was the founder of the HEB chain of grocery stores now found all over central Texas. Another member was Dora Nimitz, sister of Admiral Chester Nimitz of World War II fame. It always was a given that my siblings were going to go to college.

GRAYSON: Did you have the choice to pursue whatever you wanted?

LUMPKIN: No, not whatever we wanted, but different areas. I always wanted to go to the University of Texas, but finances were such that I ended up in San Marcos.

GRAYSON: The [University of] Texas was a state school, right? It wasn't the cost of the education, *per se*, it was the cost of living, really.

LUMPKIN: Right. Back in those days, the state schools were reasonably cheap for residents.

GRAYSON: So if you'd been able to stay with relatives or something similar, then it would've been all right?

LUMPKIN: Yes. Let me tell you about something that impressed me. In the old days, from about 1910 to 1925, an ambitious young man or woman graduating from high school with good grades could go to one summer of normal school and start teaching in the public school systems the next fall. My father started teaching school right after he graduated from high school, but he still wanted to continue his own education. Every summer, no matter where we were, he would pack us all up, rent a house or some rooms in San Marcos, and go to summer school for a few semester hours of credit. Then, he would go back and teach at his school for the long term. The next summer, he would come back to San Marcos again. Every year, I went back and forth with him. He finally got his Bachelor of Science degree in 1929, about 10 years after he started teaching school; he got his master's degree in 1932. I spent one summer in a tent in San Marcos on college property because he couldn't find a place to rent! Even now, when I hear the sound of cicadas, I am reminded of that summer. I was probably seven or eight years old. That's my idea of impressing on a little child the value of an education—a man moving his entire family every summer.

GRAYSON: That does make an impression! [laughter]

LUMPKIN: That was one of the reasons why I went to Southwest Texas, but primarily, it was a financial issue.

GRAYSON: Did you decide to follow along after your older brother in the study of chemistry?

LUMPKIN: That's pretty much right.

GRAYSON: He seemed to be doing well with in the field, and you thought you could handle it. So you didn't really have any burning desire other than that?

LUMPKIN: I guess I was interested in science as a kid. When I was in the fifth grade and earlier, only people who had some affluence could purchase a radio. So what did Earl do? He built himself a crystal set radio. There was an old gentleman named Algie Deviney who lived in the little town of Martindale, about seven miles outside of San Marcos. He would come by the school, sell us galena crystals, and tell us how to make a crystal set radio. So during my boyhood days, I was interested in science and radio, electricity, and things of that nature, which bent me towards chemistry. In fact, when we moved from Martindale, in central Texas, down to Oak Island just below San Antonio, we moved into a teacherage which had no electricity in the home. That meant kerosene lamps. We had an old Atwater Kent [Manufacturing Company] radio, but no electricity. I wired the entire house—three bedrooms, a kitchen, and a bath—for radio reception.

I remember my Aunt Josie gave me fifteen dollars for Christmas one year, and I bought a pair of headphones. The rest of them had to listen to the radio with a telephone receiver, a hand-held ear piece. I set up the crystal set radio in my brothers' and my bedroom. I would run wires to a drop cord where I could plug in the headphones or receivers. Everything was fine until four or five were plugged in; then the signal got too faint. With those fifteen dollars, I also bought a variable capacitor. Finally, I was able to tune in WOAI and separate it from KTSA. Those were the two strong San Antonio radio stations. Before, it was kind of a mish-mash, and I only heard the station with the loudest signal.

That's a lot of the reasons why I was involved in science. When I was at Southwest Texas, I took practically every science course that they offered in the fields of physics, chemistry, and mathematics.

GRAYSON: Did they actually have a curriculum with certain requirements for a chemistry degree, or did you just take a large spectrum of basic science courses with a little extra in chemistry?

LUMPKIN: I took all the courses in chemistry that were offered, so it was a Bachelor of Science degree with a major in chemistry and a minor in physics and mathematics. There were great professors there. Dr. C. [Cecil] L. Key was the department head and probably my favorite chemistry professor. Dr. Herschel Hopson taught freshman chemistry and physical chemistry. Mr. R.R. Rush was my physics teacher, and Dr. S.M. Sewell taught mathematics and astronomy. I had happy days as an undergraduate living at home. I was a townie; I wasn't on campus a great deal.

GRAYSON: Were you able to complete your undergraduate education in a standard four-year period?

LUMPKIN: It took me three and a half years because I lived there and went to school in the summertime as well.

GRAYSON: Right—as opposed to your father, who took a long time. [laughter]

LUMPKIN: Yes! He took twenty or twenty-five years to get his education.

GRAYSON: How modern was the chemistry? Was it pretty much up to date with regard to the tools you ended up using, or did they not even mention them?

LUMPKIN: Even though this was a teacher's college, they had very up-to-date laboratory apparatuses. But mind you, no one had ever heard of a mass spectrometer in a college laboratory in those days! Before World War II, they didn't have anything like what we call modern instrumentation. It was primarily wet chemistry.

GRAYSON: It was basically a good, solid founding in chemistry, but it didn't give you any inkling of what you were going to get into? [laughter]

LUMPKIN: Exactly! In fact, I had never heard of a mass spectrometer until I started to work for Humble Oil and Refining Company.

I graduated in February, 1941 at the age of twenty-one. Obviously, those were the days of the draft. I was 1A, which meant that if my number came up, I would be in the military, most likely in a role that I didn't particularly care for.

I went to work for the Texas State Department of Health in Austin, and a recruiting group from the Army Air Corps came through there. I took off half a day, and ate about three or four pounds of bananas in order to up my weight because I only weighed about one hundred and thirty to thirty-five pounds.

GRAYSON: Sounds familiar!

LUMPKIN: After eating, I went down and took the physical exam and forgot about it. That was in July, and sure enough, I got messages saying that I was going to go into the Air Corps in October. That was fine with me; it was a better assignment than the infantry. So I spent my military career in the Air Corps. I went to UCLA [University of California, Los Angeles] and took courses in advanced meteorology there.

GRAYSON: Were they interested in you because you had a bachelor's degree?

LUMPKIN: Right.

GRAYSON: I assume that you graduated high in your class, and so they wanted to continue your education.

LUMPKIN: Yes, in a new field.

GRAYSON: But the field had to focus on something that would be helpful to them.

LUMPKIN: Everyone who came into the early classes in Air Corps meteorology school had to have science degrees before they even started into the meteorology.

GRAYSON: Right. So it was kind of like a graduate study program.

LUMPKIN: Yes. This was a graduate course. I ended up in the Caribbean and South America. I then took some additional graduate courses in meteorology at the University of Chicago for a few months. They had set up a campus in San Juan, Puerto Rico.

GRAYSON: An extension campus? [laughter]

LUMPKIN: Exactly.

GRAYSON: They just needed people to do weather forecasting?

LUMPKIN: The field of meteorology is a science. It's not an exact science like mathematics. There's some guessing involved. We still know that. Particularly when you're trying to forecast weather in south Texas, but [laughter] I've forecasted weather in the tropics, where it's a little bit easier to do that!

GRAYSON: What was the state of the art of weather forecasting at that time? Why was the military interested? Was it mostly for airplanes?

LUMPKIN: Yes. Back when I graduated as a second lieutenant from UCLA in November, 1942, the big push was getting planes over to North Africa—that was during the invasion of North Africa. They needed a lot of weather casters along the route from Miami down to Puerto Rico on down to South America, on down to Belem in Brazil, across to Ascension Island, then on across to Dakar in North Africa. The Ninth Weather Squadron had weather forecasters stationed at every air base all along the route, ferrying war planes over and war-wearies back. We were forecasting route information for pilots.

GRAYSON: Weather forecasting is particularly critical for overseas flights where, if you have a problem, you've got a problem!

LUMPKIN: Yes. I usually served as a base weather officer. I started off as being the only weather officer there and then moved to another location where there were three or four officers. By that point, I was a captain and had some second lieutenants under me, along with many enlisted technicians. We had a pretty good sized operation.

GRAYSON: How long was your tour of duty during the war?

LUMPKIN: It was from 1941 to 1945. It cut into my education.

By the end of the war, I already had two children! I was in luck. O'Dell [M. Lumpkin], my wife, got pregnant while we were at UCLA. She was out there with me. We got married at mid-term during cadet school, and the good old Army Air Corps had a rule that every cadet who got married during school was immediately sent overseas. Those who did not get married were given state side assignments near their home towns. I was sent overseas, but I was able to make some trips back once in a while. By the time the war was over, I had two children and had to earn a living. I could no longer go back to school to complete my education.

GRAYSON: But you did have a bachelor's degree and the post graduate education in meteorology by that point, right?

LUMPKIN: Yes.

GRAYSON: You had intended to go on in science and complete further graduate work, but you had other responsibilities.

LUMPKIN: Exactly. The Air Corps wanted me to stay in as a career meteorologist, but I decided very quickly that I did not want a permanent career in the army. Back in those days,

soldiers got out of the service on points—your age, how many years or months you had served, and how many children you had. I was one of the very first people to be released, which was very lucky for me. When I came back with my degree, I was probably one of the first returning soldiers who were able to get a professional job. That way, it was easier for me to find a job.

For my first job, I applied to the Texas State Highway Department because I liked the central Texas area, but they were not hiring. Next, I went to the big Texas oil company, Humble Oil and Refining Company.

I went down to the Humble building in downtown Houston and handed in my application. They took it and sent it to their research and development division in Baytown. About a week later, I got a telephone call to come down for an interview. I hadn't even been discharged from the Air Corps yet! I was only on terminal leave—still in uniform, with captain's bars, and looking very neat and trim. I interviewed with the people at Humble R[esearch] and D[evelopment], and about three weeks later I got a telephone call saying that I was hired. I went to work with Humble Oil and Refining Company in the fall of 1945 and was immediately assigned to mass spectrometry.

I took a general tour of the labs and saw the facilities. I'm sure that they asked penetrating questions on my knowledge of physics, electricity, magnetism, and electronics. However, I don't think they offered me a specific job at the time that we interviewed. They told me that I would be assigned to the mass spectrometry laboratory after I reported in to work. There was one technical man in charge, who was called Ben Thomas or B.W. [Benjamin William] Thomas. He was all right. In about 1940, the first publication about industrial mass spectrometry was a paper by Herbert [C.] Hoover, Jr. and Harold [W.] Washburn, describing the CEC [Consolidated Engineering Corporation] mass spectrometer (1). It turns out that a lot of petroleum people were impressed with what the instrument's analysis of methane, ethane, and propane. It could also separate iso-normal butane. All the oil companies then said, "Hey, this beats the old low-temperature distillation procedure—we must have one!"

When I went to work for Humble Oil and Refining Company, they already had a CEC model 21-101 mass spectrometer and two Westinghouse [Electric Corporation] mass spectrometers. They were just getting a group of people together to exploit the use of these instruments because we used recording oscillographs back in those days. You took the photographic paper, put it into a dark sleeve, and went to a darkroom to develop and dry it. Then, people went through them and numbered the peaks on the record and measured the peak heights. Afterwards, we had to go through an extensive calculation, which CEC very tediously taught us how to do with beautiful manuals. We had inverted matrices, for the solution of simultaneous equations.

As time went on, we expanded all the way from hydrogen through the C5s and ended up sometimes with eighteen or twenty components in an inverted matrix. We used an old Friden

[Calculator Company, Inc.] or Marchant [Calculating Company] electric calculator to get the matrixes. It took about two or three days to invert a twenty by twenty matrix using the Kraut method. We could use that as long as the calibration data remained constant. Back in those days, the ionization chamber temperature was not controlled. It was very difficult to control the pattern and sensitivity of these various compounds, and we were analyzing for individual components. Not for types.

GRAYSON: What was the highest component you normally looked at?

LUMPKIN: With the 21-101, in one mixture, we could do hydrogen, CO, CO_2, methane through the butanes, nitrogen, oxygen, H_2S, water, ethylene, propylene—the whole works. However, we were limited by the stopcock grease and the volatility of the material. If we actually had a separated material, we could analyze for individual C_6s and individual C_4 and C_5 olefins, but we couldn't do that all in one big mixture.

The Westinghouse machines were different. First off, the CEC machines were 180 degree magnetic sector instruments. Westinghouse got in the business when Harold Washburn and CEC began to sell these mass spectrometers to very eager petroleum companies along with a few chemical companies. CEC made companies sign a contract stating that any scientific breakthroughs made using their mass spectrometer would be assigned to CEC. The oil companies were in a bind, so they signed the contracts on the first instruments that they bought. They all thought, "We have to get out of that contract." A consortium of people formed, including Ben Thomas, John [H.] Hipple of Westinghouse, Norman [D.] Coggeshall of Gulf [Oil Corporation], and D.P. Stevenson from Shell [Oil Company] Development in Emeryville.

The group went to Westinghouse, famous electrical company in Pittsburgh, and said, "We need to build a mass spectrometer that we can buy instead of a CEC to break this patent business." After two years, they came out it with a Westinghouse Type LV 90 degree instrument. Again, when I went to work at Humble, they had two Westinghouse mass spectrometers. Those were laborious instruments! We read the signal with a light beam galvanometer. We set on the peak and the light beam would deflect, and we would balance it down to a null and read a Leeds and Northrup [Company] K-2 potentiometer to measure the signal.

That was in 1945 and 1946. We used Westinghouse instruments until the late 1950s and early 1960s—we were still using the Westinghouse then, but with a pen and ink recording oscillograph. It had limited resolving power—120—but with certain materials which vaporize at room temperature, that's all we needed anyway. We could vaporize them and introduce them through a gaseous inlet system until we got up into the liquids. Of course, we could put a bit of

liquid in and let it vaporize at room temperature, enough vapor pressure to read on a manometer. When we got up in the C_6s and C_7s and C_8s, we had difficulties. From there, we began to realize that a heated inlet system was needed.

The CEC 21-101 and the 21-102 were both room temperature inlet systems, gaseous only. Finally people began to realize that if we heat up these inlet systems and get rid of stopcock grease, then higher molecular weight compounds could be vaporized. Jack [M. J. Jr.] O'Neal and T.P. Wier, Jr. from the Shell Oil Company refinery in Houston built the first elevated temperature inlet system.

GRAYSON: What was the problem with the stopcock grease?

LUMPKIN: Well, there were two main problems. First of all, there was absorption, which caused inaccurate results; second, there were memory effects for the next sample. There was always the business of having to shut down for half of a day once a week. We had to shut down the entire system and grease all of our stopcocks. Otherwise, they began to get draggy and leaky.

I wanted to say this about the stability of the spectrum: normal butane was used to determine the sensitivity and the relative cracking pattern, like the m/e [mass to charge] 58 to 43 ratio, to determine what we call pattern and the reproducibility of pattern. When these changed, our analysis was off, and we had to recalibrate for all of these twenty gases or so. It was thought that ionization chamber temperature variation caused these changes.

In response, CEC came out with an ionization chamber temperature controlling device. It was a glass envelope. In order to change filaments you had to turn it off, cool the mercury diffusion pumps, vent the whole instrument to atmospheric pressure, take the analyzer out of the magnetic field, break a wax seal, take out the ionization chamber, put in a new filament, and insert the whole thing again! Anyhow, it was the glass envelope that allowed this system to work. On the outside of the vacuum there was a parabolic mirror with a bright filament bulb sitting in front of it.

There was a thermocouple on the ionization chamber. We tried to maintain the temperature slightly above what just the filament alone would provide. When the thermocouple said, "Heat me up a little bit," a bright light would come on, the parabolic mirror focused on the chamber and would shine light through the glass envelope over the ionization chamber. It would heat the thing back up again. It seemed to take hours to reach temperature stability. It was crude, but it worked.

GRAYSON: Was the heated inlet system a kind of a natural evolution to get into the heavier compounds?

LUMPKIN: Yes. There were all sorts of natural evolutions. As far as I'm concerned, the developments in mass spectrometry in the petroleum industry, and probably throughout all of industry, started with CEC because they made the first instrument that was reproducible. Mass spectrometers were used before that time for isotope measurements and elemental atomic weights and things of that nature, but that was mostly in the physics laboratory. The chemistry laboratory finally had an instrument that had a reproducible pattern, and could be used it for actual analyses and identification of compounds.

GRAYSON: Were you going to say something about the stability of the instruments?

LUMPKIN: Yes. CEC realized that they had to improve things, so they did. They not only increased the resolving power, stability of magnetic fields, stability of electronics, and recording devices—we finally got into digitizers instead of the old recording oscillographs. The entire field just kind of moved along together in the petroleum industry. It moved to higher and higher boiling ranges. I would say that probably the C_8s and C_9s were as far as we could go for individual component analysis in complex mixtures. After that, people started doing type analysis. In other words, total C_5s, C_6s, C_7s, C_8s, paraffins, naphthenes, aromatics, olefins—things of that nature.

GRAYSON: Where did the requirement for type analyses come from and why was it important?

LUMPKIN: Petroleum is extremely complex! They would latch onto any useful technique they could find. They could take feed stock and run it through some sort of reforming process, look at the feed stock and products, tell you that you've converted this much naphtenes to aromatics, and high molecular weight paraffins into lower molecular weight olefins. All that sort of feed and product type analysis studying various pressures, temperatures, catalysts, and so forth—that was what we were involved in, and that was what the petroleum industry really used a mass spectrometer for.

Except for lower boiling ranges, the only real need for individual component analysis now is when you get into specialty products like toluene. If you're going to make a nitration grade toluene to make TNT [2,4,6-trinitrotoluene], then you want to be able to analyze specifically for that component. Likewise, if you're going to make high grade gasoline with lots of iso-octane in it, you need to be able to measure the branchiness of the paraffins that contribute to high octane ratings. Type analysis helped as you went to higher and higher boiling ranges. The mass spectrometer was a glove that fit a needed hand.

GRAYSON: How long did Westinghouse last as a commercial business?

LUMPKIN: I don't think that Westinghouse really ever had their heart in making money at it. I think they did sell quite a few of them. Humble used them to good effect.

GRAYSON: Were they also 90 degree instruments?

LUMPKIN: Yes. They worked for materials which had enough vapor pressure at room temperature.

GRAYSON: Did they serve to break CEC's strangle hold? [laughter]

LUMPKIN: Exactly. [laughter] In fact, Frank [H.] Field and Sam [Samuel H.] Hastings used the Westinghouse for their work in publishing the first paper on low-voltage analysis. As far as Humble and Exxon [Mobil Corporation] were concerned, the low-voltage procedure, both at low resolution and high resolution, was the mainstay of our major contributions in the characterization of petroleum. That is, being able to identify and quantitatively analyze for benzenes, naphthalenes, phenanthrenes, chrysenes, and other polycyclic aromatics all the way from C_6 to C_{50} including aromatic oxygen, nitrogen, and sulfur compounds. Our main contribution was identifying and analyzing for them and being able to tell people what was happening when compounds went through the different processes by analyzing feed and product.

GRAYSON: When was the low voltage work done?

LUMPKIN: I think Sam Hastings and Frank Field's article came out in about 1956 (2). I published my first paper in 1958 and extended it (3). Theirs was very limited. I don't think they were primarily interested in the analytical field; they were trying to show the possibility of the technique. Because we were in the same laboratory, I took it and extended it on to analytical procedures.

GRAYSON: What was your initial work when you first started working in this laboratory?

LUMPKIN: The first thing they did was hand me the CEC manual and say, "Start reading. You need to understand this instrument, what it can do, and how to use it."

GRAYSON: Were there any other employees at CEC who knew how to run the mass spectrometer?

LUMPKIN: Yes. Ben Thomas worked at CEC, and we always had great instrument technicians that maintained and repaired the electronics. We also had many laboratory technicians and most were college graduates. There were three shifts a day in which we read all the charts, ran the instruments, and worked on all the calculations. I'd be in a 20 by 40 foot room with four or five Friden calculators and six or eight women and men sitting there, reading charts, and running through calculations. It was a big operation!

GRAYSON: Was that how the place was run when you first got on board?

LUMPKIN: Yes, but they needed technical help because Ben Thomas was the only professional before I was hired. Humble needed technical help to supervise its technicians and keep a quality control. They needed someone who knew what he or she was doing. Most of the employees had Bachelor of Science degrees; we rarely hired anyone as a research technician

without their bachelor's degree. I think that was a smart thing to do because our technicians were high-quality people. I was very lucky to come into the company as a professional with only a B.S. degree. I was also fortunate to have some people involved in the laboratories at Humble that were greatly interested in the field of mass spectrometry.

Joe [L.] Franklin was the first president of ASMS [American Society for Mass Spectroscopy], organized in 1969. He was one of the men that I interviewed with in 1945. Although Joe wasn't a mass spectroscopist, he was a scientist, and one of his major interests was mass spectrometry. Over the years, there were Joe Franklin, Fred [W.] Lampe, Burnaby Munson, Thomas Aczel, and myself. Frank Field and John H. Futrell, who were also presidents of ASMS, were also there. A lot of these people would come and work at Humble for a few years and then move on to some academic area. In fact, Joe Franklin himself left Humble and accepted the first Welch Foundation chair at Rice University. He spent the rest of his career there. Frank Field went to Rockefeller University.

GRAYSON: An impressive group, to be sure! Do you remember how the company changed from Humble to Esso to Exxon, and what the significance of that was?

LUMPKIN: Well, Humble Oil and Refining Company was founded in about 1917 on the handshake of the few men in Humble, Texas, where an oil field had been discovered. That's the reason why it's called Humble and not 'Umble. [laughter] You pronounce the "H." By the 1920s, the company needed some money for development, so they sold about 53 percent of their stock to John D. Rockefeller's Standard Oil Company. Humble still maintained its independence as a Texas institution. Standard Oil Development Company and Humble Research and Development Division had an agreement in the research area called "General Research Agreement," whereby they exchanged information. As the years went by, I think Standard Oil Company began to realize, "Hey! This is a valuable piece of property. We ought to buy some more of it."

They began to buy more and more stock of Humble Oil and Refining Company in 1953. I owned Humble Oil and Refining Company shares but didn't own any Standard Oil Company shares for many years. In 1965, Standard Oil Company acquired over 95 percent of Humble. Somehow, they had the legal power to say, "All right, we can now call in all the rest of Humble," and they did. Standard Oil Company gained 100 percent control. Don't take all of this for gospel, but that's what I remember happening.

Afterwards, we became Esso Research and Engineering Company. They changed the name Standard Oil and Development Company to Esso Research and Engineering Company. This was the research part of Standard Oil.

GRAYSON: Was "S-O" an abbreviation for Standard Oil?

LUMPKIN: Yes, they changed S-O into Esso. Then in 1970, they changed the name to Exxon because the company was having trouble marketing under Esso in two or three states. I think Illinois was one of them, along with Kentucky or Tennessee. The Standard Oil Company of New Jersey, the old Rockefeller Company, was broken up in 1911 by an anti-trust action. Socony Vacuum Standard of Indiana, Standard of California, and Standard of Ohio became separate companies.

[END OF TAPE, SIDE 1]

LUMPKIN: Let me tell you something about the organizational area of Humble. When I started working at Humble, I was hired as a physicist. It wasn't long after that when they changed my title to chemist—research chemist, or something of that nature.

GRAYSON: They classified mass spectrometry as physics?

LUMPKIN: Primarily. Within about two years, they just dropped that all together. Humble always had two parallel ladders, professional and administrative. People could, by choice, stay on the professional ladder or switch to admisnistration. I stayed in the technical field rather than administrative field. I imagine there are a lot of good mass spectroscopists, all over the country, who were promoted into the administrative area. Maybe one of the reasons why I stayed so long in the field of mass spectrometry was because I didn't have good administrative skills and was never tapped to go into the administration, so I remained in the professional field. Joe Franklin did the same thing, as well as Thomas Aczel and Frank Field. They stayed mostly on the professional ladder.

I think an important thing about Humble was that they had a program for personal development, called the Humble Lectures in Science Series. It was one of Joe Franklin's ideas. Every year or so, he would set up a series of courses. He would contact people from all over the world who were the tops of their fields to teach graduate courses that Humble's technical people

could sign up for. Instead of lasting an entire semester, the courses were condensed into two or three weeks of intensive work at the graduate level. While employees took these courses, they were relieved of all other duties. They didn't even have to check into the office or answer any correspondence or telephone calls!

I want to read to you a list of some of the courses that I took in the Humble Lectures and Science Series. I'm sure you'll recognize some of the names of professors that taught them. "The Spectra of Polyatomic Molecules," by Dr. Kenneth S. Pitzer, University of California; "General Physical Chemistry," by Dr. F.A. Mattson, University of Texas; "Modern Theories of Organic Reactions," by Dr. Paul [D.] Bartlett of Harvard; "The Chemical Bond," by Dr. Linus [C.] Pauling, University of California; "Free Radicals in Photochemistry," Canadian National Research Council, B.W.R. Stacey; "Electronic Circuits," by Dr. Robert Watson from Texas A&M; "Mathematics," by Professor B.C. Moore from Texas A&M; "The Chemistry of Organic Sulfur Compounds," by Dr. F.G. Bardwell from Northwestern; "Free Radicals in Solution," by Dr. Cheves [T.] Walling from Columbia University; "Electronics for Chemists," University of Illinois—I forget the professor there; "Estimation of Physical Properties," by Dr. R.C. Reed of MIT; and "Free Radical Chemistry," by Professor Michael Swartz from Syracuse University. These courses were great for continuing one's education and self-improvement.

GRAYSON: How many people typically took those courses?

LUMPKIN: They were limited by the size of the classroom, so, generally 10 to 15 people.

GRAYSON: Did they all work at Baytown?

LUMPKIN: No. Some people came from the Houston office and the Technical Service Division, which was responsible for the technical side of operating the refinery. Those were courses with three hours of lecture in the morning, a noon break, and another three hours of lecture in the afternoon. The assignments for the evening and the final examination determined the grade.

GRAYSON: Wow! There were grades?

LUMPKIN: Yes. The professor went through the whole process like you were in university. We even had to buy special text books. About half of my technical library is made up with books from those courses.

GRAYSON: These courses took place over a period of how many years?

LUMPKIN: Well, my first one was in 1947 and my last one was in 1964. It was fourteen years. I took courses every year or every other year, according to what was going on in my field and whether the powers that be thought that I would be helped by taking a course.

Franklin was the dean of the school, so to speak. He would make the arrangements and normally during the course, two of the students, Joe Franklin, and the professor would go out to lunch. The next day, two more students, Joe, and the professor would go out to lunch some place else. We'd end up on Friday night, on the last week of the course, at a big banquet held at some place in Galveston, the big Humble building in downtown Houston, or some place in Baytown.

The day that was my turn to take Linus Pauling for lunch, we went to the Old Rebel Inn in Baytown for lunch. Mrs. [Ava Helen] Pauling was having lunch with us, also. We went in to order, and I think we had some canapés in advance, with some jalapeño peppers. Mrs. Pauling was not afraid of jalapeño peppers. She lived in Southern California and is used to jalapeño peppers, Mexican food, and things of that nature. She picked one up, and I said, "Mrs. Pauling, they're quite hot!" She said, "Oh, no trouble, no trouble!" She bit down into it, and it seemed like she could not breathe for about thirty seconds. We started to get frightened. Finally, she got it out of her mouth, gasped, choked, sputtered, and grabbed a glass of ice water! [laughter] She didn't have any more after that. When the Friday evening banquet came, it just turned out that O'Dell and I were sitting at the same table with Dr. and Mrs. Pauling. I very courteously did not mention that incident at all.

GRAYSON: Yes! [laughter] I guess she was just visiting.

LUMPKIN: Yes. She was just down there for the two weeks, and I'm sure that Mildred Franklin toured her around all over the area.

GRAYSON: It is amazing that the company bought into that package! How long were the courses?

LUMPKIN: They took two to three weeks.

GRAYSON: You guys weren't working during the class at all! You were being paid to take this course. Do you know if Franklin had any trouble with that up the ladder?

LUMPKIN: Our professors were also the top people in their field, so they were probably paid a pretty good hefty remuneration.

As far as I know, Franklin was respected and didn't have any trouble with the program. Actually, higher-up administrative people realized the value of scientific research. They wouldn't have bought a mass spectrometer in the first place if they hadn't realized that we'd come out ahead. I had been there since 1945; and in about 1949 or 1950, my section head decided that we should publish. I think that's one of the reasons why Humble Baytown R & D Analytical became fairly well known in its field.

GRAYSON: Frequently, when a chemical company goes to publication, they tend to be somewhat restrictive about what's published. Was that the situation in your case?

LUMPKIN: It always had to be reviewed, and we had to be selective in examples that we made. In the analytical field, we always liked to take an example of a feed and a product that showed what happened in the process that is of some value to the company. They looked at the material carefully, but they did allow things.

GRAYSON: If you submitted a paper for review, was it done in a timely fashion?

LUMPKIN: A lot of the time it was done right there in Baytown. Occasionally, something would have to go up to New Jersey for approval, but most of the time the patent people in Baytown would make a decision.

GRAYSON: What types of journals did most of these articles go into?

LUMPKIN: Most of what I was involved with went into *Analytical Chemistry*.

I know that you can't see this on the tape recorder, but I'm showing Mike a bound volume called *Earl Lumpkin on Mass Spectrometry* that was presented to me when I retired in 1980.

Whenever possible, I'd get reprints of the publications that I was author or co-author of over the years, and I had some in a drawer. Apparently one of my colleagues came in my office, sneaked out a copy of each of these papers, and put them all together in a beautiful bound volume.

Here's *Analytical Chemistry* and *Journal of Chemical Physics* with Joe Franklin. Here's *Journal of the American Chemical Society*. They are mostly *Analytical Chemistry* (4).

GRAYSON: Yes. These are all review journals and that type of thing, where you'd have had to deal with people sending back reviews and then trying to answer their questions.

LUMPKIN: Yes, but there were never major difficulties. We hardly ever had to do much changing.

GRAYSON: But the decision to go ahead and publish the work was made, more or less, at a fairly low level?

LUMPKIN: At the time, when I thought that I had something of value that I wanted other petroleum mass spectroscopists to know about, I would write it up and submit it to my boss. He'd say, "Sure! Fine. Go ahead and publish it."

Actually, the higher-ups at Humble were not very hands-on. We were quite free to pursue whatever direction at whatever pace that we thought was correct. I can recall monthly sessions where my boss would come in and sit down for an hour to talk about things. He wouldn't be telling me what to do; I would be telling him what I had been doing.

GRAYSON: You had a great deal of freedom.

LUMPKIN: Exactly. This was the type of administration we had; it was great!

GRAYSON: How long ago was this?

LUMPKIN: Well, I spent three and a half years with Exxon Production Research Company [EPR] in Houston. Otherwise, I was in Baytown for my entire career. I went to EPR in Houston as a section head. There, we had mass spectrometry but worked with another side of it, the isotope ratios—carbon 12, carbon 13 ratios for geologic work. We had potassium/argon, age dating with the mass spectrometer releasing the argon, melting the rock, and so forth. I spent three and a half years there.

After Thomas Aczel came to work with Humble in 1959, I spent 1965 to 1968 with Exxon Production Research Company in Houston. When Prudhoe Bay oil field was discovered in about 1967, there was a big push for mass spectrometry in Baytown, and so I was transferred back. I was Thomas Aczel's office mate. We were office mates off and on for many, many years and had a great relationship.

Thomas was a big man, physically, but he had very little manipulative ability as far as the laboratory was concerned. He was an idea man and handled data, but he didn't handle equipment or instruments. He knew which end of a screwdriver to grab a hold of, but that was about the extent of it. He was not proficient with his hands. I'm sure you've seen these rolling swivel chairs where you can rotate a collar on a spiral shaft and lift the chair up and down to various heights. When we got new chairs in our office, we set them both at just the right height. About a year later, I got the idea, "I think I'll begin to move Thomas's chair up a little bit." So

about once a week, I would take a hold of its collar and move the seat up about an eighth of an inch. The next week, I'd raise the seat another eighth of an inch. So his chair kept getting higher and higher. Finally, he came in and tried to sit at his desk, and he had to spread his legs as wide as he could because he couldn't get his knees under the desk any longer! [laughter] He didn't know what to do. I suspected that he knew I was doing it, but he very calmly said, "Earl, I think that my chair is too high. Would you please lower it for me?" [laughter] I very calmly replied, "Yes, Thomas, it does seem to be a bit high." So I rolled it back down to where it was before, and he never said a word about it except that! [laughter]

GRAYSON: He must have suspected somebody was playing games on him.

LUMPKIN: Yes! It happened so gradually that he just all of a sudden, wham!

GRAYSON: He couldn't get to his desk. [laughter]

LUMPKIN: He couldn't get his knees under his desk! That was not very kind of me to do something like that to somebody like Thomas, who was so sincere and so – naive, you might say. But I did it!

GRAYSON: Do you feel that Exxon still has the same attitude towards scientific pursuit?

LUMPKIN: Yes. It seems that whenever the Humble R & D Division wanted a piece of capital equipment, like an MS-9, it was bought. We got the first MS-9 in the country in 1963, costing 100,000 dollars plus all the training, which included sending me on a three-week trip to England to get trained. From my point of view, it wasn't hard to get them to fund this because we had done good work with the older CECs and the Westinghouse mass spectrometer. As we went further and further into the barrel, with higher and higher boiling ranges and more and more complex materials, we finally realized that we needed more resolving power. That was when Humble bought the first MS-9. Esso Research and Engineering Company later purchased the first MS-50 in the industry.

GRAYSON: Was there a definite interest in doing analytical work, or was it done only out of necessity? Were these people in high enough positions in the company that you didn't have to go through a lot of hoops and what not to justify the purchase?

LUMPKIN: No. We wrote a one-page justification. The request would go up to New Jersey and, about two weeks later, come back down approved. Then, we'd write out a purchase order. Nugent [F.] Chamberlain bought a big fancy nuclear magnetic resonance instrument. We had the top line of equipment in a petroleum research laboratory because Humble was willing to support its scientists. In fact, just think of the work of Franklin and Field on ion molecule reactions. There's practically nothing there for an oil company, as far as making money. Field spent practically a career in the area of ion-molecule reactions and protonation of ions for the advancement of the knowledge of chemistry. He started that in Baytown. He had a machinist in Houston that built his chemical-physics mass spectrometer according to his design. Now, what was there in that for Humble as far as process development? Nothing! It was to advance the knowledge of chemistry.

GRAYSON: Humble was willing to foot the bill for that?

LUMPKIN: Right, and provide a big lab and laboratory technicians and all that's required to do that. Later, Exxon gave him the machine, and he took it with him to Rockefeller University.

I'm talking about 1992. Exxon closed the analytical laboratories in Baytown. There were still some analytical labs, but not the big analytical research and development lab that we had there in the 1940s through the 1980s. Exxon has its own major research organization in New Jersey now, and they still are backing up research in all fields, not only mass spectrometry.

GRAYSON: When you started out, you used a 101, an oscillographic light beam with a photo paper, and a Westinghouse, which was even more primitive. What machines did you use after that?

LUMPKIN: MS-9 and MS-50. Obviously, as time goes by, as instrument manufacturers make more complex and better instruments that turn out more and more data, and you end up with computers. Eventually, computers handled our data. Initially, we were still getting the data by hand, and punching cards and reading them into a computer. Then, we finally got computers to

get the data directly from the instruments. One day, I had a dedicated computer on a high resolution mass spectrometer. For the last several years, I have seemed like more of a computer man than a mass spectroscopist! [laughter]

The data was coming from the MS-50 at such abundance that we were trying to make heads and tails of what all of the spectra meant. We would write computer programs to boil it down to some useful information. For instance, we analyzed crude oils from all over the world, and the engineers wanted to know, "What components does this crude have in it that are different from that crude? Should this me made into a lubricating oil or gasoline? What should we do with these various crudes and how should we classify them?" We ended up with thousands and thousands of bits of information from the mass spectrometer and tried to reduce it to maybe ten little indices to tell us what we wanted to know. We wrote computer programs to reduce this mass of data down to some useable information for the engineers.

GRAYSON: You worked during a good portion of the technical development of the instrumentation itself – all the way from unheated or unregulated heated sources and detectors that required a lot of manual labor to computers. You mentioned that Thomas was not an experimentalist.

LUMPKIN: Right. I was in the laboratory and was primarily an experimentalist. Any time there was any difficulty with any machine, I made the decisions about what to do. In fact, I suspect that one of the reasons why I'm fairly meticulous at the things that I do now in my own workshop is because I was always working on hundred-thousand dollar machines and didn't want to goof things up. We had to take things slowly and easily when we were cleaning up an electric sector and didn't want to break the quartz pieces. So, yes, I was hands-on and developed a lot of the things that were used.

I published a diagram of a high temperature solids inlet system. Before then, there were major difficulties getting solids into an instrument. What can you do? You can put it in solution, but then you get the spectra of the solvent and the sample, unless the solvent can be pumped away before the sample vaporizes. Terephthalic acid is a good example of that kind of solvent. Nobody could get a spectrum of terephthalic acid. We tried to heat it up to melt it and get it into a micropipette, but it would solidify before we could get it into a gallium covered fritted disk. To solve this problem, I built the solid inlet system, and we got a beautiful spectrum of terephthalic acid and a lot of other things. The system featured a glass dry ball and socket valve. No stopcock grease. It was completely dry, polished for days, until we could take them out in the sunlight and put the socket over the ball and see interference fringes. Then we could close the valve, open an atmosphere across it, and it would not leak.

It must have been about 1958 or 1960 when I made the trip out to California to visit CEC. This is when they were just beginning to work on the 20-110 Mattauch[-Hertzog] double-focusing mass spectrometer. Esso New Jersey had signed a contract to buy one. They were going to buy two, one for them and one for us in Baytown. We were out there in California talking about it one time, and I can recall being in the CEC research laboratories and talking about an inlet system for this instrument that they were designing that would have resolving power of 5,000.

I went to the blackboard and drew a chalk diagram of an inlet system with which you could introduce gases, liquids, and solids with a one-inch pump out valve on it. Part of it enclosed in a heated chamber, part of it not enclosed in a heated chamber. After the conference was over, with all the scientists around, that drawing was still on the blackboard. About two years later, I heard that the drawing I made on the blackboard stayed there for months and months. Finally, when CEC came out with their newest instrument, there was my inlet system!

Of course we'd already built one like it the year before. We never bought an instrument with the inlet system, except with the first few CECs. Later we told them, "Don't ship it with the inlet system. We'll provide our own."

When the first MS-9 from Associated Electrical Industries [AEI] came over in 1963, they shipped it with their inlet system, because they insisted that it passed the specification test with their inlet system. We said, "All right. When your man finishes, we'll take it off and send it back to you with credit for the cost of that return. We install our own." They said, "Fine." When the MS-50 came over, AEI [Associated Electrical Industries, Ltd.] didn't even send the inlet system. We had one already made to go on it by the time it got here. So that was hands-on for me – a lot of things like that.

GRAYSON: Did you want to get into higher territories and higher molecular weight materials than the inlet system designer could come up with?

LUMPKIN: Yes. Finally, they ended up with fairly good inlet systems, at least most of the manufacturing people did. I was only involved with the CEC and AEI at that time. So yes, I was very much of a lab, hands-on person into all sorts of things, such as separation procedures, silica gel, alumina gel, and percolation fractions. If you look through the publications that I was involved in, I did an awful lot of separations, and later on, we got into gas chromatography in combination with mass spectrometers. I also helped write our own computer programs. In the latter few years, when mass spectrometers had a dedicated computer program of their own, we used the computer program provided for us only for data acquisition. For the handling of the data on down the line, we wrote our own programs. We'd took the data, fed it into a tape read-

off and selected the masses that we wanted and what we wanted to do with them. I'm sure you've heard some of the papers that Thomas and I presented in the seventies and eighties.

GRAYSON: Yes, you basically did a lot of the work that, at the time, was done by the vendor. You were more advanced and actually did things that were different than they had anticipated with their general purpose inlet systems and software.

LUMPKIN: Yes, I'm sure. For instance, take the business of low voltage, where you are stressing a filament and trying to get enough ion and electron current across the ionization chamber. We would work with people and tell them what we needed, and they would make suggestions or make changes in their filament assemblies in order to give more constant electron voltages and get more electron current. We would tell the manufacturer what we needed and make suggestions on how to do this because they were not manufacturing hardware, like [E.I.] DuPont [de Nemours and Company] did for a while. We would suggest to them some additional things that could be done with the instruments.

GRAYSON: Did they see that it was to their advantage to take those suggestions to heart?

LUMPKIN: Yes.

GRAYSON: You've been pretty intimately involved with The American Society for Mass Spectrometry and its predecessor ASTM [American Society for Testing and Materials] over the years. Could you discuss ASTM, what the people were doing, and important things that you saw in its development?

LUMPKIN: Coming up in 1992 is their fortieth annual conference. It's not forty years of ASMS. What they're doing is backing up to when ASTM E-14 started in 1952. But as far as I'm concerned, you have to back up even further to 1946. That was when I went to my first mass spectrometry conference in Houston. I attended every mass spectrometry conference from 1946 until I retired. I did not miss one! My first conference was called the CEC Group Meeting; it was the forerunner of ASTM E-14. They had a meeting each year, and ASMS is still following the same format – that is East, Midwest, West; East, Midwest, West—it was either Midwest or South; I can't remember which. The first one was held in 1945 in Chicago.

CEC would announce the meeting and invite people to present their technical papers, and it was approximately a half technical conference and a half user's clinic. Obviously, it was their meeting, and they introduced new concepts, new calculation procedures, new hardware, etc. This went on until 1952.

GRAYSON: How long did each meeting last for?

LUMPKIN: They were generally two to three day meetings. They were pretty short. In 1947, it was held in Los Angeles, California, and I went out there from May 13th to the 19th. It wasn't just a one-day meeting. It was worth making a trip to California, New York, Pittsburgh, or wherever you went to for the meeting. I was always the technical man who went and got a lot out of it. Obviously, the company thought so too or they wouldn't have sent me.

We learned about new products, new ways of doing things, and what other people in the field were doing because we were giving technical presentations. D.P. Stevenson from Shell, Emeryville, would talk about his ionization appearance potential measurements and what he was learning from those things. Archie Hood from Shell in Deer Park would talk about the procedures that he used, and Ralph [A.] Brown with Atlantic Refining Company would talk about things that he discovered with the mass spectrometer. These meetings were forerunners of ASTM E-14 and ASMS conferences.

GRAYSON: Do you feel that the other petroleum companies had as much invested in the pursuit of scientific information as Humble Research and Development?

LUMPKIN: We felt that Humble was among the top. We thought a major competitor was Shell, in Emeryville, California. I think we were ahead of most everyone else as far as development in the field. But there were people active in American Oil Company, Pan American Oil Company, and Shell. Also General Petroleum [Company] in Torrance, California, and Getty [Oil Company] in Philadelphia. It turned out that a major number of these oil companies were in Texas.

We found out that there were a lot of Texans going to these meetings. It must have been in the late forties in the Jung Hotel in New Orleans on Canal Street. CEC would foot the bill for the end of conference banquet, and they hired a girl to play some songs on a Stomach Steinway, an accordion. Of the people who attended the conference, maybe 25 to 30, half of them were

from Texas, so we stood around and sang songs after a beer or two. We sang all kinds of songs, "Oh My Darling Clementine" and the whole works. We always ended the night by singing "The Eyes of Texas." It had nothing to do with the University of Texas. It was just the fact that we were all Texans, whether we went to school at the University of Texas or some place else. That was the song that everybody in Texas knew. It was the song that we all had in common, so it became a tradition that we sang. This went on all through the days of the CEC conferences.

In 1952, Westinghouse started a separate annual conference of their own. Meanwhile, CEC began to realize that their conference was becoming too big and expensive for them to carry on and that it should be put under somebody else's wing. That year, we held our group meeting in Pittsburgh. That was when the Pittsburgh meeting [Pittsburgh Conference on Analytical Chemistry and Applied Spectroscopy, Pittcon] was still being held in Pittsburgh. We all met in one little low sitting room. There must have been about fifty people in a hot room filled with cigarette smoke. We talked about whether or not we wanted to join a group called ASTM. Most of us had never heard of ASTM, even though it had been working in the petroleum industry for years. There was a man at the CEC Pittsburgh meeting from ASTM who said, "You ought to form your own committee. You'll practically be on your own. You won't even know that you're tied to ASTM. We'll let you form your own little technical group."

That night, it was decided that CEC would drop their group meetings, but they would still be involved in the field with ASTM. In 1953, we all met again in Pittsburgh, but we were now called the ASTM Committee E-14 on mass spectrometry.

GRAYSON: At the time, were there some other options besides ASTM? Could you have fit in with the Pittsburgh conference?

LUMPKIN: No. I think the Pittsburgh conference offered that possibility. In fact, we went there for two years in a row, but most of us did not like the idea of going to Pittsburgh in February. [laughter] We liked sunny California, San Francisco, Denver, Atlantic City, or Atlanta! That's why we went to ASTM, and I think it was a fairly happy marriage. Later on, the members working in academia didn't like the idea of being tied in with ASTM, which is standard's organization, so ASMS was formed in 1969 in the Dallas ASTM E-14 Conference. People were saying, "We have an awful lot of academic people coming to the conference now, and mass spectrometry has evolved into so many areas away from procedures, which is what ASTM primarily was involved with." ASTM had been very tolerant with us. They didn't push us to write procedures; they just let us go our own way, collect our own monies, elect our own officers, and once in a while report to them. I was secretary of ASTM E-14 from 1963 to 1964 and helped run the conference at the Sheraton-Palace in San Francisco in 1963 and at McGill University in Montreal in 1964.

After ASMS was formed, I was chairman of ASTM E-14 for 1972-1974, which made me Vice President for Data and Standards of ASMS. Mass spectrometry expanded into so many different directions of biologics in extraterrestrial, looking at meteorites, and it just evolved into so many different things that it had nothing to do with a standard's organization. That was the main reason ASMS was created. Of course, I was a charter member. I was there in 1969, and I voted for Joe Franklin to become the first president of ASMS. We started in 1969, and I've maintained my charter membership ever since then.

I wrote this next story up once for the ASMS journal, but somebody might look into these archives and not ever see that. I mentioned the proliferation of Texans in the CEC group meetings, and later on ASTM E-14. We decided that even if we couldn't sing all evening long, we'd at least have one song, so we would sing "The Eyes of Texas." During the group meetings and conferences, we would arrange to meet on Wednesday evening just before the banquet at a certain corner of the building. All the buzzing was still going on from the cocktail hour. We could hardly hear anything, but finally there might have been a little faint music showing up in one corner of the room. There we were, all gathered together singing "The Eyes of Texas." This kept on until I retired.

[END OF TAPE, SIDE 2]

LUMPKIN: Amidst all the hubbub of the cocktail hour before the annual Wednesday night banquet, there would be a faint rendering, according to how many voices we had available. Texans were beginning to get thinned out, and some of them were a bit shy. Frank Field never sang with us, but Joe Franklin did. Joe Franklin would sing "The Eyes of Texas" with us. Eventually, we began to recruit some good song voices. John [H.] Beynon was one of them. John Beynon could sing a good song! He would learn the words quickly too, so we made him an honorary Texan! [laughter] John Beynon sang with us for several years, and Fred [W.] McLafferty sang with us also. He had a good lusty voice; Joe Franklin's voice was kind of squeaky. He knew the words, obviously, because he was a graduate of the University of Texas.

We would always arrange to sing "The Eyes of Texas" very close to the doors that would open up into the banquet room from the cocktail room so that, when the doors opened, we would have our choice of tables. Then during the banquet, we would all sit together at a table of eight or ten. This was a tradition that went on even after I retired in 1980.

At the ASMS conference in San Antonio in 1983, Catherine [C.] Fenselau, who was then President of ASMS, knew that I had retired, but she still invited me to come to the conference. I

came and spent Monday, Tuesday, and Wednesday there. There was an eclipse of the sun at noon during that conference. All those guys had their mirrors—shinning eclipses up on the walls of the buildings nearby. It was a great day, really. That Wednesday evening, Catherine asked me to direct the entire group in singing "The Eyes of Texas," which I was very happy to do. Burnaby Munson joined me. Fred McLafferty rushed up and said, "Hey, you didn't let me know in advance! I got here too late!" [laughter] Thomas Aczel was also there, so that was a great evening.

One thing that kind of left me feeling funny about this was that within about three minutes all the Texas A&M grads had to go off and gather into one little place and dance around in a circle and sing the Aggie War Hymn. They took it as being a school song, rather than a tradition of the group, but I joined them. That was a great evening! I got in there and joined arms with them—of course I'd had two or three beers by then, too.

GRAYSON: ASMS came along and took over in about 1969. So your involvement with ASMS was as ASTM chairman?

LUMPKIN: Yes. For the first few years, the chairman of ASTM E-14 was automatically a vice president of ASMS and a member of the board of directors. I was president of the ASTM E-14 and vice president of ASMS. Harry [Harold J.] Svec was president at that time, and Frank Field was program chairman and later on the president of ASMS. They were a very good group of men to work with, and during the time that I was on the board of directors, we rewrote the constitution completely.

GRAYSON: This was also at the time when they didn't have a professional group do grunt work?

LUMPKIN: No. We did it ourselves.

GRAYSON: You say you served as secretary of ASTM E-14. Did you keep tract of mailing lists and all that kind of stuff as secretary?

LUMPKIN: Yes. That was back in 1960. I was treasurer of ASTM E-14 in 1964 when we had our meeting in Montreal. Jack [A.G.] Sharkey was Vice President [Arrangements Secretary], I think Russ [Russell E.] Fox was president, and Norman [D.S] Coggeshall was program chairman. Those were the three other officers.

Operating in a foreign country for the first time was a big hassle, especially because it was mostly Americans coming to Montreal for this conference. They didn't have any Canadian money. I didn't want to have to make change, and the Canadian dollar was worth 5 percent more than the American dollar at that time. The registration fee was something like six dollars American, which would have been five dollars and eighty cents Canadian. Most people would pay no attention and just say, "Here's my six bucks!" A Canadian would come in and give me his six Canadian dollars. However, someone would occasionally want his change, the difference between the American and the Canadian dollar. I just said, "Six dollars American or Canadian! Sorry, I can't be worried about this kind of stuff!" They took it very well.

Early in the week, when people were coming in, Jack Sharkey and I went down to the Bank of Montreal and opened an account. It was about a four-block walk down the hill from McGill University to the bank to make a deposit. We went down there and made a deposit twice a day because we didn't want the money hanging around the hotel. We would go down, and the bank assigned us a little cubicle. There, Jack and I counted the money out and then took it to a cashier to make the deposit.

From time to time, the conference would include foreign speakers, or some symposium chairman would arrange for a notable speaker to be paid to be on the program. In that case, Jack and I would write notes to ourselves and would put one hundred dollars cash into an envelope to give to the symposium chairman to give as a fee to the speaker.

We were fairly loose and easy. We kept count of everything, but we didn't go through putting it into a bank account and writing checks. Jack and I handled the conference on a cash basis, including paying off the hotel. A lot of times we would put the money in the hotel banking system, you might say the hotel cash box area, and have a private key to that. We would collect more money than we expected to spend, so we would always have some money left over. After paying the hotel bill in cash, Jack and I sent whatever money was left to ASTM E-14 headquarters.

By the time we decided to go from ASTM E-14 to ASMS, ASTM E-14 had quite a healthy bank account built up. I was chairman of ASTM E-14 then, so I had the charge of the monies and making reports to ASTM headquarters. It wasn't a rancorous meeting, but there was some dissension about how we should divide the money because ASTM E-14 was going to be a continuing organization, and ASMS was brand new, not a penny to its name! We ended up giving ASMS approximately three-fourths of the cash that ASTM E-14 had saved from 1952 to

1969, which ended up being fifteen thousand dollars. So, we had ASMS started off on a good cash basis, and I was involved in that.

GRAYSON: What did you do about the paperwork?

LUMPKIN: Humble, again, was very lax about this. ASTM would furnish the stationery. Every time you became president, you got a big batch of paper and envelopes. I just used the company secretary and from one office to the next one, one president to the next president passed it on. They would pass a card file of names, addresses, telephone numbers, and a list. Of course that was always changing—names were always being added and subtracted. Exxon gave the time of its secretaries, some postage, and other things like that to ASTM E-14. It was a good relationship, and no harm done to the company. It was piddling to them but a major help to a young growing field.

GRAYSON: Can you talk a little about Archie Hood?

LUMPKIN: Archie Hood was my counterpart with Shell in Deer Park—the Shell Oil Company, not Shell Development Company. In other words, he was in the refinery or the research laboratories like we had research laboratories housed with the refinery in Baytown. Jack O'Neal was the head of the group, and Archie Hood was about my age. We golfed together, sang together, and went to meetings together. When one of us ran out of gallium—which we used for inlet systems—we would call across the channel, and they could ship over ten grams of gallium. He was just a delightful person.

GRAYSON: And also, a worry-free competitor?

LUMPKIN: Well, he has used my lab, and we were willing to share mass spectroscopic information and ways to do things. However, we did not reveal company process information. We'd talk about how to get spectrums, calculate, and standardize procedures. That was our relationship.

GRAYSON: You also worked with Jean Futrell, right?

LUMPKIN: Jean Futrell had been in the area of mass spectrometers, and I can recall him telling me about working when they were first doing the magnetic separation of uranium isotopes with these big, old, walk-in mass spectrometers like Saddam Hussein tried to build recently. I'm taking about massive things that you run all day and all night, separating U235 from U237. You shut the whole thing down, break the vacuum, go in there, and scrape it off the ion collector to get a few milligrams. I think he was involved in that before he came to Baytown.

GRAYSON: You already mentioned Burnaby Munson. Would you like to say anything else about him?

LUMPKIN: Burnaby is a Texan. He's from Wharton, Texas—near my wife's hometown, actually. He worked with Joe Franklin and Frank Field doing research work on the chemical physics mass spectrometer that Frank had designed and built. Burnaby came to Humble a year or two after Futrell left. He stayed there for quite a few years and works at [University of] Delaware.

GRAYSON: You mentioned Thomas Aczel a number of times. What about Waldron?

LUMPKIN: John Waldron used to be the chief mass spectroscopist design engineer with Associated Electrical Industries [AEI].

I already mentioned that Humble needed resolving power. We're talking about resolving power in the league of five thousand, which would help us to do things like separating twelve hydrogens from one carbon, as far as mass is concerned. It wouldn't get us any nitrogen or oxygen break-down differences, but we could get quite a bit with that, so we entered into a contract with CEC.

It was in 1960 that CEC had the 21-110 up and operating for us as a demonstration. Phil [Phillip J.] Klaas was the spectroscopist with Exxon in New Jersey at that time. I met with CEC in Pasadena to review the design and operation of the 110. I took my whole family out there,

including my teenage children. They had a great time all over Pasadena and Santa Monica. They went to all the museums, and it was a great three weeks.

But at the end of that period, I saw that the resolving power of the 21-110 was not what CEC expected. As far as I could tell, they had made a mistake in the design and failed to take into account the width of the slit in calculating the resolving power! In spite of the many years of good products from CEC, I recommended to Humble that it should not purchase the CEC instrument. I guess I was more hard-hearted than the people up in New Jersey were because they went ahead and bought one that they never did very much with.

About that time, John Beynon, who was with ICI [Imperial Chemical Industries, PLC], reported on instruments of higher resolution manufactured by AEI, a new company that came out of the old Metropolitan Vickers Company. These were the MS-7 or MS-8. So we entered into correspondence with them, probably Vickers were the first few letters that I got. I kept those letters for many years, but I don't have them any more. Then it became AEI, and finally we entered into a contract with AEI to provide us with the MS-9. That was their newest instrument after the MS-8s that John Beynon had made us salivate for. He was getting resolving power up around two or three thousand, and we were still stuck at six hundred. I went over to England for a check out on the MS-9 being built for us in the summer of 1963, and met John Waldron for the first time.

CEC was pretty unhappy because Humble, one of the prime mass spectrometry laboratories in the country, turned them down, but CEC continued to work until they turned out a good instrument. However, the prototype that we were looking at just didn't do it, and the MS-9 really was quite a way ahead of the CEC 110.

GRAYSON: Yes, there's also that underlying dichotomy with photoplate detection versus the electrical detection, too.

LUMPKIN: Yes. MS-9 had an electrical detection, even though it was a laborious, recording oscillograph. At least we didn't have to run it through a dark room; it recorded on UV-sensitive paper. While I was in England, John Waldron took me under his arm, and we spent several evenings together. In fact, one weekend, he drove me from Manchester to the old Roman city of Chester. It was about an eighty mile drive. We went down there for dinner, and he and I walked along the old Roman wall at the city of Chester.

I've been back to Manchester since then, but I can remember the experience of being with John Waldron. In the mind of a young spectroscopist, like I was, here was a top man in his

company—a noted scientist who had designed those instruments. Sure enough, in the fall of 1963 when we got the MS-9, he came over and visited my family in our home and had dinner with us. He was a fine man. The last time I saw him was in Nebraska, where I gave a paper at a conference on coal chemistry at the University of Nebraska in 1976. It was the presidential election year. I remember being at a cocktail party for the presenters in the home of Michael L. Gross and watching TV as Gerald Ford lost to Jimmy [James E.] Carter.

GRAYSON: Humble got one of the earliest MS-9s.

LUMPKIN: We got the very first one that left England. I think ICI in England got two of them, of the first batch of five. I had to go to a training school on the MS-9 in England. There, I met Brian Green, who helped with my hands-on training. He helped me learn how to twist some of the knobs on the MS-9 over in Manchester.

Nigel Bean was an erection engineer. He came over in the fall of 1963 to install the MS-9 and run the acceptance tests, so he was in Baytown for a long time. It takes quite a few months from the time you unpack one of those things to when you get it all set up and can get it into operation. My children fell in love with this young, unmarried Englishman. Of course, he was in his twenties, and my children were in their teens. He has a beautiful singing voice, and after having had him out for dinner one evening, we finally decided, "To heck with you living in a motel, just come move into the room with my son." So Nigel Bean just came and lived with us. He became a member of the family in that time.

GRAYSON: What about Martin Elliot?

LUMPKIN: He was also British. I always thought very highly of a lot of those learned Englishmen and their ability to understand and express themselves. Martin Elliot and Syd [Sydney] Evans were favorites of mine. Syd Evans came to AEI after John Waldron. Syd Evans, as far as I understand, was the main designer on the MS-50. That man knew more vacuum technology, electronics, and physics than any man that I ever spent much time with. He was involved in the installation of the MS-50 and made quite a number of trips to Baytown when we would get in trouble and call for help. At the ASMS conferences, for many years, we would always meet and have dinner or go to a museum together.

I hadn't thought about all these people for a long time, and yet, they're part of my mass spectrometry. You have to remember, I've been out of the field for eleven years. This is quite a cultural shock for me to come back into it. Not only for this session today, but Thomas Aczel died last year, just before the national conference. There is a symposium planned in his honor on the mass spectrometry of petroleum chemistry, on Monday morning, 1 June 1992, in Washington, D.C.. [Chang Samuel] Sam Hsu died within two weeks after Thomas' death. People from the ASMS called me and asked if I'd be willing to give an eight or ten minute talk about my relationship with Thomas. Later, they called again and asked if I would expand that to a thirty-minute talk about some of the things that we're talking about now—the early days of mass spectrometry and how mass spectrometry intersected with the field of petroleum chemistry over the years. When I retired, I thought at first that it might be a good idea to do a little bit of consulting. A lot of scientists did consulting when they retire.

I retired in June and in July I was already up in Bartlesville with [Stewart] Shepley. I met with his group and his bosses at the Bureau of Mines Energy Research Center. Actually, they got me up there to pick my brain on the MS-50 because they wanted to buy one, and they wanted me to tell them how it could be used and what wonderful information they could get from it. Presumably, I helped them out, and they did buy an MS-50. I went up there for a while, and then I went to another conference on petroleum and gave a talk on how mass spectrometry is of value in that field. Afterwards, I went to New Jersey and talked with them. After completing three trips, I was thinking, "What did I retire for? I don't want to do this! A hundred bucks, hundred-fifty bucks a day for two or three days, all this traveling up there and back! I'd rather sit here in Round Rock and prune my fruit trees and forget about mass spectrometry!" [laughter] That's what I did! I forgot about it. I went back to the conference in 1983 at Catherine Fenselau's request. Then, I went back to Nashville at your request, for this group session that we had with Frank Field and Jack Sharkey last year. Other than that, mass spectrometry has been out of my mind.

I called the main library in Austin and asked them if they had *Chemical Abstracts* and the lady said, "Huh?" [laughter] I said, "*Chemical Abstracts*, all the technical information of journals from all over the world." She said, "Oh, no. We don't replicate anything that the University of Texas Chemistry Library might have." So, last Friday I went to the University of Texas library in the chemistry building, Welch Hall, and I've got page after page here of reminders for myself for the paper that I've got to write for the ASMS conference. It did me good to go back and realize that I could still find my way through *Chemical Abstracts*. [laughter]

GRAYSON: I think we've covered quite a bit. I was wondering if we can wrap up with some grandiose ideas about where we came from and where we're going.

LUMPKIN: I think I've already talked about the past of mass spectroscopy. I certainly have been happy to have been a part of it and to have gotten into the field at a time that I did because there are very few people of that era still active in the field. There are very few people still alive in the field who remember back to those days. I'm really happy to know, even though it was fifteen years ago, that mass spectrometry is really beginning to make headways in the fields of medicine and biology. Hopefully they can find out something about what causes cancer. I know that mass spectrometry is so ingrained now in all the biological and government laboratories involved in medicines, that this is a hope for the future. O'Dell and I have talked about this many times. I feel that people are getting to be a bit too narrow. I had all types of experiences, and they weren't all in one narrow field. I worked in chemistry, physics, mathematics, and computers—a whole gamut of work. My feeling is that a lot of students are coming out with very narrow outlooks and fairly narrow desires about what they really want to do.

GRAYSON: I think the business of specialization is the reality, and I've seen that myself.

LUMPKIN: Yes, the old business of knowing more and more about less and less! It might be good in some areas, but I think we still need a lot of broad-minded thinking.

GRAYSON: Yes, I agree. I think that's one of the reasons that I enjoy the ASMS conferences. They cover such a broad spectrum of disciplines. I see it as a meeting where people—they may be mathematicians, geologists, MDs, biologists, chemists, or whomever—have one thing in common, and that's mass spectrometry.

LUMPKIN: Right. I hardly ever attended a paper presentation on a completely different area of mass spectrometry, listened, and not gained something from it—either a technique or an idea. Even though the conference is on a very broad topic, people should not just attend their day, but to be there to learn from what their peers are doing.

GRAYSON: One-on-one interactions are very good. That's harder to do as the conference gets bigger, but it's still a very interactive group. Well, I think we have probably covered the broad areas and topics, unless you have any other things that you want to throw in.

LUMPKIN: No, let's call it off. I've enjoyed the conversation, and I probably talked longer than I should have. I don't think I revealed anything that I shouldn't! [laughter]

[END OF TAPE, SIDE 3]

[END OF INTERVIEW]

NOTES

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Lumpkin and Thomas Aczel using an Associated Electrical Industries [AEI] MS-9 mass spectrometer, June 1974

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