

CHEMICAL HERITAGE FOUNDATION

FRANK H. FIELD

Transcript of an Interview
Conducted by

Michael A. Grayson

at

Durham, North Carolina

on

9-10 December 2009

(With Subsequent Corrections and Additions)



Frank H. Field

ACKNOWLEDGMENT

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FRANK H. FIELD

1922 Born in Keansburg, New Jersey on 27 February

Education

1943 B.S., Duke University, Chemistry
1944 M.S., Duke University, Chemistry
1948 Ph.D., Duke University, Chemistry

Professional Experience

University of Texas, Austin, Texas
1947-1949 Instructor
1949-1952 Assistant Professor

Humble Oil and Refining Co., and Esso Research and Development,
Baytown, Texas
1952-1953 Research Chemist
1953-1960 Senior Research Chemist
1960-1962 Research Specialist
1962-1966 Research Associate
1964-1966 Section Head

Esso Research and Engineering Co., Linden, New Jersey
1966-1970 Group Leader
1966-1968 Research Associate
1968-1970 Senior Research Associate

Rockefeller University
1970-1988 Director, Rockefeller University Extended Range Mass
Spectrometric Research Resource
1970-1988 Professor, Laboratory of Mass Spectrometry & Chemistry of
Gaseous Ions
1988-1989 Camille and Henry Dreyfus Professor

Honors

1963-1964 John Simon Guggenheim Fellow
1970-1972 Vice President, American Society for Mass Spectrometry

1972-1974	President, American Society for Mass Spectrometry
1974-1976	Past President, American Society for Mass Spectrometry
1973-1975	Member, Petroleum Research Fund Advisory Board
1987	Fellow, American Association for the Advancement of Science
1988	Frank H. Field and Joe L. Franklin Award for Outstanding Achievement in Mass Spectrometry

ABSTRACT

Frank H. Field was born in Keansburg, New Jersey. Orphaned at an early age, he was raised in Cliffside Park, New Jersey, by an aunt, an uncle, and a grandmother. Somehow when he was a young teenager he saw a chemistry set that he desperately wanted. He did get the set, and he found what he wanted to do with his life. He had a good, solid public school education, which enabled him to be a candidate for college.

Field entered Duke University, placing a year ahead in chemistry. He had very little money, and to meet his expenses he worked in the school dining hall and graded math papers. When World War II began, Duke's chemistry department had a contract with the federal government to do research work for defense purposes; during his junior and senior years Field held a full-time position as a lab technician, in addition to being a full-time student. Things were going well for Field at Duke, and they asked him to enroll in graduate school there. He worked on using fluorocarbons as hydraulic fluids to replace hydrocarbons on warships. In addition he took pictures of experiments on solid rocket propellants. He received his PhD for work in magnetochemistry.

Field accepted an instructorship, at that time a tenure-track position, at University of Texas. Funding from the National Institutes of Health and the National Science Foundation did not exist, so his funding was very skimpy and came from the University. He had worked in magnetochemistry, but the magnet he needed was too expensive for the University of Texas, so he looked around for something else to do. Humble Oil & Refining Company gave an early mass spectrometer to the University, who gave it to Field. He had to rebuild much of the machine, as all the glassware in the machine had broken in transit to Austin. So began his mass spectrometry career. He worked first on measuring the ionization potential of cyclopropane, which had not previously been measured.

To encourage development of basic science at Humble Oil, Joe Franklin persuaded Humble to set up summer courses for professors from various Texas universities, and Field attended one such program. He and Humble liked each other, and Field left the University of Texas to work with Franklin at Humble Oil. Field and Franklin wrote their first book together. Standard Oil Company had bought Humble Oil, and Field eventually moved to Linden, New Jersey, to Esso Research and Engineering Company, where he continued his work on chemical ionization.

Feeling "out of the mainstream" at Esso, Field became receptive to the idea of working elsewhere. He was recruited by Rockefeller University as a full professor. He shifted into biochemical mass spectroscopy to be more in keeping with the biomedical orientation of Rockefeller. He built the second Californium-252 mass spectrometer in the world. A talk in Bordeaux, France, excited his enthusiasm for matrix-assisted laser desorption/ionization (MALDI) and he persuaded his postdoc, Brian Chait, to build one. Biomedical mass spectroscopy has been able to grow wildly as result of desorption technique.

In 1989 Field retired and moved with his wife to Oak Ridge, Tennessee. In 2004 he felt the need for a continuing care establishment, and the Fields moved to The Forest at Duke near Duke University. In 2009, Field was diagnosed with pancreatic cancer. He talks a little about his treatment and prognosis; Field hopes to recover enough to die of old age, as he says. He then continues with the interview topics. He says his only philosophy of science has always been to get a good job and do agreeable, useful work. He believes, however, that a considerable

amount of scientific innovation arises from chance observations. He agrees that mass spectroscopy has contributed significantly to biology, but thinks that it is probably at its limits. He says the United States needs to be scientifically competitive, particularly against fast-rising societies like China's. He then summarizes his interest in ionization and talks about other scientists in the field.

INTERVIEWER

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

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INTERVIEWER: Michael A. Grayson
LOCATION: Durham, North Carolina
DATE: 9 December 2009

GRAYSON: I push this button. I'm going to start out by saying that this is 10 January 2009...

FIELD: Oh, December.

GRAYSON: Very good. [laughter]

FIELD: It's 9 December.

GRAYSON: Okay. It's 9 December 2009. I'm in Durham, North Carolina interviewing Frank Field. So, I know that the time stamp on this digital, audio file will say something about 2003, but it is 9 December 2009. Okay, Frank, I guess, we can just start at the very beginning and that would be back to your early days, your parents' background, their education, and where you grew up, and that type of stuff.

FIELD: I'll give you a relatively brief summary because it's not much interest to anybody but me. I was born in Keansburg, New Jersey ...

GRAYSON: Keansburg, is that...

FIELD: Keansburg, which is a small resort town on the Raritan Bay in New Jersey. My parents were not much with me as a child. My father died when I was two months old and my mother raised me in Keansburg until she was stricken with cancer. Because of that we had to move in with relatives in Cliffside Park, New Jersey which is a commuter town just across the Hudson River from 96th Street, New York [New York]. My mother died there in 1933. So, I was an orphan at age eleven.

GRAYSON: So, you were born in 1922.

FIELD: 1922.

GRAYSON: And your birthday was...?

FIELD: 27 February 1922. I was then raised by an aunt who was my mother's sister, unmarried. Also living in that the house was a brother, who would be an uncle, and an aged grandmother. So, that made four of us in Cliffside Park, New Jersey. We were working class people, German immigrants, [perhaps a generation removed from the old country]. I haven't spoken of my father, because I never met a paternal relative until I was about fifty.

GRAYSON: Wow.

FIELD: Because they lived up in Massachusetts, and in those days Massachusetts was light years away from New York. But anyway, the people I grew up with were working class people, but with a Germanic feel, especially in my aunt, for education and culture. Coming from St. Louis [Missouri], you would appreciate that. My aunt saved me really. I would have been lost without her.

GRAYSON: Well, it's a responsibility for her, and she was unmarried?

FIELD: She was unmarried.

GRAYSON: So, that was [...] really something that she didn't [...] have [to do], that responsibility. But she accepted it.

FIELD: She accepted it, but in my later years, I have come to realize that there was a real quid pro quo involved. I mean, she got a son, which she never would have had, and it was meaningful to her. Well she, my aunt, [never] got beyond the eighth grade, and she worked as a stenographer/secretary in various New York [companies...]. My pre-college education was a great [success. It occurred in the public schools] in Keansburg [and] Cliffside Park. [This was during] the Depression, but it was a marvelous education really, as I look back on it. Then [my aunt managed to supply me with a little financial support to get started in college (Duke University), and later I worked very hard and supplied an increasing fraction of my expenses myself]. I went all the way through to a Ph.D. The actual degree was granted in 1948, but I finished the work in 1947.

GRAYSON: Were there any teachers that influenced [the] direction of your education with respect to like say, math, chemistry, physics, type thing? Or was it something that you evolved an interest in and followed?

FIELD: I've always been pretty much self-directed. I had valuable professors, but none that I would say that particularly influenced me personally.

GRAYSON: Okay. So, the interest in chemistry...

FIELD: I've been thinking about that and as best I can tell, in 1937, there was a store in Cliffside Park that had toys and Christmas gifts [...] in [its] window. In it was a Chemcraft Chemistry Set, and I wanted that Chemcraft Chemistry Set worse than anything. I don't know why. I tried to remember, but I can't remember why I wanted it, but I know I wanted it. I would go up to that window and stare [through it] numerous times in the weeks before Christmas, and my aunt bought [the set] for me. I played with it, and I [found] a friend who also had an interest in chemistry, and we expanded the chemistry set. We could go over to New York and go into the chemical supply houses for amateurs and buy chemicals, and we did experiments. I did them on top of the washtubs in the basement where I grew up. So, I was very devoted to chemistry, and have never given it up.

GRAYSON: Do you have any idea of the year that you really got into this...this chemistry set really caught your fascination? Was it...

FIELD: 1937.

GRAYSON: 1937. So, you were like fifteen then.

FIELD: Yes.

GRAYSON: What was your aunt's name...did we get your aunt's name? We should probably get that.

FIELD: Her name was Anna Fleischmann, good German name.

[...]

GRAYSON: Two “N’s.”

FIELD: And an issue was made of that, because the Fleischmann with two terminal “N’s” was not a Jewish name. Fleischman with one terminal “N” was a Jewish name. The anti-Semitism [of the time] was such that we didn’t want to be incorrectly associated with the Jews. And those were the times...

GRAYSON: And that was well understood by everyone in that era that the two “N’s” meant that you were...

FIELD: Well, I got the information from my aunt, and I never questioned it. So, I can’t really answer with any intelligence. But it was an issue.

GRAYSON: Yes. [During] that era, [...] definitely. Did you do any experiments with that chemistry set? [...] I like the idea that you ‘expanded’ the chemistry set.

FIELD: Well, we did.

GRAYSON: Did you do anything that you regretted doing with it or that caused problems? Fumes, or consternation among the adults?

FIELD: Well, one of the things that my friend and did...the friend is now a lay psychoanalyst out on the west coast, still practicing. We’re the same age.

GRAYSON: You want to give us his name?

FIELD: His name is Ken Helfant, but that is not the name by which I knew him. He had been adopted. He was the son of a woman who married a second time and he took [...] the name of the stepfather. But then when he reached maturity, he reverted back to [...] Helfant, [which was] the name of the biological father.

GRAYSON: You both were interested in chemistry at the time, but [...] you're the one that stayed with chemistry, and he moved onto other things.

FIELD: He moved onto other things, yes. You asked about events, and an event that sticks in my mind is that in those days there was a magazine called *Popular Science*. You know *Popular Science*?

GRAYSON: Yes.

FIELD: It used to have, every month there would be an article about amateur chemistry. We did, Kenneth and I, quite a number—in our basements, quiet a number of experiments—based on those articles. One of them was an experiment where one makes luminous calcium sulfide.

GRAYSON: Luminous?

FIELD: Luminous.

GRAYSON: This must have been really exciting to do.

FIELD: Well, you take oyster shells, break them up and mix them with sulfur and heat them. Well, we did this in Kenneth's basement. It was a cold February day, and of course, the sulfur caught fire producing...and now, his parents were out for the afternoon. It was a Sunday afternoon. The burning sulfur of course, produced copious amounts of sulfur dioxide. The house was full of it. We opened all of the windows in spite of the weather and went outdoors. There, I experienced something I've never seen before or since and that is, it was snowing and there was a thunderstorm accompanying the snowstorm. Well, we aired the house out and got by without getting skinned alive by his parents. But obviously, I remember this experience. I don't think we ever did get luminous calcium sulfide. [laughter]

GRAYSON: So, when you got to college, everything was at Duke then.

FIELD: Everything was at Duke.

GRAYSON: So, you also contributed to your education at the time. There were no scholarships at the time for you? Or did they have scholarships or did you just earn...?

FIELD: There were five scholarships for the whole university, two hundred dollars each. Now don't put that down, because that memory is a little bit vague, but the days of "need free" admission were decades in the future. So, what I did, I got a start from my aunt's money. I immediately applied for a job working, slinging hash in the dining room, which I got and I started doing. In those days, there was the NYA [National Youth Administration]. Does the NYA mean anything to you?

GRAYSON: No.

FIELD: Okay. One of the [Franklin Delano] Roosevelt New Deal Agencies, the National Youth Administration...

GRAYSON: Okay. I haven't ever heard of that one.

FIELD: And they provided money to do useful things. The useful thing that they did for me was they subsidized my being an assistant to a math professor. So, I would grade papers at twenty-five cents an hour...thank you. That was the going rate also for the, working in the dining room. You got your meals, and it was the equivalent of twenty-five cents an hour.

GRAYSON: So, tuition at the time at Duke was considerably less than it is now.

FIELD: Two hundred and fifty dollars a year.

GRAYSON: Two hundred and fifty a month?

FIELD: A year.

GRAYSON: Oh, a year.

FIELD: Or well, two semesters.

GRAYSON: So, that still is...given the wages that, was a lot of money.

FIELD: Oh, boy it was. I mean it was. You can't imagine how hard money was to come by in those days.

GRAYSON: So, did you live in the dorm?

FIELD: Yeah, I lived in the dorm.

GRAYSON: Why did you choose Duke? Do you remember?

FIELD: Oh. It's a long story, which I really don't care to go into.

GRAYSON: Very good, okay.

FIELD: Let's just say I chose Duke and I did what I was telling you for the first year and part way into the second year. Then, the war came along. By the time the war came along, by the time it got to be 1941, I was a junior. I had to take, not entrance examinations, but placement examinations when I first got to Duke and I did well enough in the chemistry placement examination that they put [me] right directly into the second year course, into analytical chemistry, so that I was a year ahead in my chemistry all the way through my college career. The university—the chemistry department—got war research contracts, and these were staffed by [graduate] students and by some undergraduates, but mostly graduates. But I was one of the undergraduates. So, I worked as a research worker for the whole second half of my education. That would be my junior year. It was part of the junior year, my senior year, and then into graduate school.

That was the hardest time in terms of work that I've had in my life because I was working forty hours a week as a lab technician. And I was at one time stupid enough to try to take twenty-one semester hours of academic work. I couldn't do it, so I had to cut back to fifteen, which was the normal load. But there I was working forty hours, and carrying a full academic load. It was tough, but I did it. I feel that I got a good education at Duke. It was not the academic powerhouse then that it is now. It was a football school, in fact. We had winning seasons and went to the Rose Bowl twice [1939 and 1942] and that sort of thing.

GRAYSON: So, as a lab technician that paid a little bit better than the twenty-five cents an hour you were...

FIELD: Oh, yeah. It sure did. Yeah.

GRAYSON: Okay. So, you were able to live a little more...?

FIELD: Yeah. It wasn't easy street, but it surely was better than it...now, I wasn't living solely on the twenty-five cents an hour. I was still getting help from home. But as time went on, as I progressed, I became more and more self-sufficient. This is not to be represented as a 'Horatio Alger' story, because it wasn't. But I did work awfully hard. It was tremendously important to me to get an education because I grew up in what, as I have told you, was a lower middle class environment and I wanted out.

GRAYSON: And you saw education as the way.

FIELD: And education was the way. Entrepreneurial activities in the late 1930s and the early 1940s were not very popular, because people were going broke. Businesses were going broke. It was a tough, tough time. I think I am down in item four—particularly influential teachers and mentors. I would say that the teacher that I think of most [...] was Fritz London.

GRAYSON: London.

FIELD: Yes. Now, he was a very famous theoretical physicist. He was the coauthor of the Heitler-London Theory of the covalent bond. So, he's very well known.

GRAYSON: How do you spell that Heitler, "H," the Heitler-London.

FIELD: That was Walter Heitler.

GRAYSON: Heitler.

FIELD: And it was Fritz London, but Heitler was...all of this was in Germany in 1927. London was a Jew, and got caught up in the [Adolf] Hitler anti-Semitism. He was an Austrian Jew. No, I guess he was German, but you know they shuttled back and forth. [London was German, born in Breslau, now Poland.] He had to get out of Germany. So, he went to France and worked at the Sorbonne, but that was just a temporary position. The man who probably did

as much as anything to make Duke what it is today [was] the Chairman of the Chemistry Department, named Paul [M.] Gross.

GRAYSON: That's Gross.

FIELD: Yes. He eventually became Provost of the University and I don't know what all. Now, this is not related to me, but it's interesting enough, that I'm going to tell you it anyway. At this time—the time I'm talking about which would be sometime in the 1940s—the president was a man named [Arthur] Hollis Edens. But as I say, this doesn't have anything to do with me, but it is of interest. Edens had the view as President that Duke should be a genteel southern regional institution. Gross, who was a New Yorker, graduated from Columbia [University]. [...He] was a hot shot. He was a very ambitious man. He wanted Duke to be a world-class university. They got down on the mat and battled it out and Gross won. As a consequence, Duke University got oriented on the path that it has followed to become in fact, the world-class university. So, that's interesting.

GRAYSON: That would have been in the early 1940s, probably, while you were there.

FIELD: It might have been in the mid to late 1940s or later. I was gone, so I...

GRAYSON: [...] Okay.

FIELD: Or at least, I was busy enough with my own affairs that I wasn't very concerned about the university. Anyway, London gave me an insight into some real high class science. You know, here was a man who was an intimate of [Erwin] Schrödinger, and [Werner] Heisenberg, [Arnold] Sommerfeld, all of those German giants, and he talked about them and taught about them. Now, he taught two courses. I'll interrupt myself. One was quantum mechanics, and the other was statistical mechanics. [You] spent three years taking the two courses. The first year you spent trying to learn to understand London's English, which was awful. Then the second year you took...let's say if you took a statistical mechanics the first year, which is what I did, [but I] didn't understand much of it because I couldn't understand the English. Then I took quantum mechanics. By this time, I knew the English better, and I understood the science. Then I went back and took the statistical mechanics again. But I had the feeling the way London taught the course that I was sitting right on the forefront of science. That I...

GRAYSON: You probably were at the time. I mean, this was...

FIELD: Well, no, no, no. It was about ten years delayed because the forefront of quantum mechanics was 1925 to 1928, starting with Heisenberg in 1925 [...]. But London took us back there and we just sat there and drank it in. So, that was exciting.

GRAYSON: So, you took all his physics, but you ended up doing chemistry.

FIELD: Well, this was in the Chemistry Department. I don't know. I talked about Gross, but I guess I didn't tell you the important thing. That is that Gross, as the Chairman of the Chemistry Department in 1938, somehow or other learned that London was in need of a job. So, he went to Europe and recruited him to come here and be Professor of Chemistry at Duke, which London did. The Londons are still an influence in this town; that is Fritz died in about [1952].. But he had a wife who was [artistic] and became a rather well known artist. [Because of their fame, perhaps] they got some money, and endowed...various things around here were named for them or endowed by them, so there are London endowments [the Fritz London Memorial Lecture and the Fritz London Memorial Prize]. There's a son, Frank London, who lives in the area, and does good things. Damn interesting. Gross made a big contribution.

[As for my] philosophy of education: very simple. I wanted to get some knowledge that could earn me a good living. I didn't have any high-flown ideas about culture or being a well-rounded person. I wanted a good job, so it worked.

GRAYSON: At the time at Duke there was this battle between being a genteel southern institution and being a world-class institution developing. When you were attending the school, was there an emphasis on the liberal arts or maybe were there more kids doing liberal arts curriculum type thing?

FIELD: Well, the first thing I should say is that this conflict was somewhat after my time and miles above my level.

GRAYSON: Sure.

FIELD: So, I wasn't concerned with that at all. What I will say is that when I was here, Duke was a "Johnny Come Lately" school. You know, the west campus here...there are two campuses. The west campus here was only completed in about 1931-1932. It became Duke University rather than Trinity College. It started as Trinity College. [James B.] Duke gave the money, and it became Duke University [...]. But another thing that was smart was that the man who convinced J. B. Duke, the tobacco magnate, to give the money to Trinity College [was William Preston Few, which Duke did with the understanding the name would be changed to Duke University [... Few was the President of Trinity College at the time].

Few was smart enough to see...he had this money, and he went out and raided as many good eastern colleges of their professors as he could, and he was pretty damn successful. He brought in a faculty that was really first rate. It was instant academic quality. And the recruiting of London was just part of that, although somewhat delayed. But, I had good professors. A number of them pretty well known, doing good research work. The atmosphere in the Chemistry Department and indeed, in the whole university, was very good, the academic atmosphere. I feel that I got here a good education. Now, without the aura that a Harvard [University] education would have given at that time, but in terms of a practical education—and I don't mean "practical" in the usual sense of the word—it was great.

GRAYSON: Did you...

FIELD: Shall we move on? You have a question.

GRAYSON: Well, I was wondering if you...when did you decide to go to graduate school? I assume that, when you started you were looking at a bachelor's degree as your end point. Then somewhere along the way you decided, well, maybe I want to do graduate work.

FIELD: Well, look, I did well.

GRAYSON: Were you encouraged?

FIELD: To do well academically in science and you wanted to do high level science, you'd need a Ph.D.

GRAYSON: Well, okay.

FIELD: That was it.

GRAYSON: So, it was pretty much like you really just [moved right into it].

FIELD: Grew into it and they wanted me to stay. Then another factor, a tremendous factor was the war came along. So, that at the time that I was about ready to get out of undergraduate school, get my undergraduate degree, there was the war, the draft. There was the research

projects at Duke that needed manpower, and so I stayed here through the war, and worked in several laboratories.

GRAYSON: Can you say anything about those research projects that you did back in those days?

FIELD: Oh, yeah, I could say something. I did some work on a project which investigated the use of fluorocarbons as hydraulic fluids. The idea being that, if you're on a warship, a battleship, and it is damaged, then one of the sources of the conflagration would be hydrocarbon hydraulic fluid. If you could replace those with inert fluorocarbons, it would be better. This was a project at Duke, because one of the professors was a man named Lucius Aurelius Bigelow. [...] He was an organic chemist and his research interest of many years was the direct fluorination of hydrocarbons, which is a difficult and not particularly safe operation. But he was making these fluorocarbons and the Navy got interested for the potential use as hydraulic fluids. So, that was one project. Now, I don't think it came into fruition. My job...you know, I'm a low level guy at this point. But my experiment was...here's a test tube. [Gestures with hands]. [...Here] is a muffle furnace with a quartz tube in it, [and] here's another test tube. The whole thing [is sealed together and] evacuated. One distills the...one puts the fluorocarbon, or hydrocarbon, or whatever you were investigating in one test tube, and you allow it to distill through the furnace at increasingly higher temperatures until it decomposes. Sure, the fluorocarbons were much more stable than hydrocarbons. I don't think anything much came of the project for reasons that I don't know. But it went on for a while, and I worked on it, and did my job. Another one was interesting. You want to hear about it?

GRAYSON: Oh, yes.

FIELD: If I keep blathering on like this, we're going to be here forever.

GRAYSON: Well, you know I'm here to capture what doesn't show up in [the] literature.

FIELD: Well, now look, shut me up if you think you need to. One of the jobs I had was trying to abstract—now this would have been 1941 say—trying to abstract British reports about solid rocket propellants. The interesting thing is the following. This is a solid propellant rocket. [Gesturing with hands]. Here's the rocket up here, and the engine is this part at the back which is filled with grains of cordite. Now a grain [of cordite was] a stick of [nitrocellulose, a plastic, about three-quarters of an inch in diameter and eight inches long. When the rocket is fired, the cordite] burns and releases all sorts of gases which provide the propulsive thrust. The British developed this, and they tested their rockets in England. The rockets worked fine. So, then the

British got into the war in North Africa and took the rockets down there, they'd blow up in the course of being launched.

What was happening is that as the rocket burns it produces the gas which produces the thrust, and the rocket accelerates but the rocket grains are compressed up against the grid at the back of the rocket chamber. The thermal mechanical properties of nitrocellulose were such that the ability of the grains to withstand stress is much less at higher ambient temperatures than at low temperatures. Of course, it's hotter in North Africa than it is [in England] and so what would happen is these grains would be pressed against the grid, and they would retain their form in England where it was cool, but in North Africa they'd bust up and you'd get a branched chain reaction and the thing would blow up. What we did to try to illustrate this, and this is Marcus Hobbs' idea, was because of...

GRAYSON: Marcus Hobbs? [...]

FIELD: Hobbs, yes. He ended up being another big shot here at Duke. At the time I was there, he was an assistant professor of chemistry. But to see what happened with these grains, we had an interesting experiment. That is he had fabricated a pipe, a copper pipe, oh, an inch and a half in diameter, and maybe thirty feet long. At one end of it, slots were milled out [along each] side.

One put the grain of powder on a carriage with a spring at the front end and the grain [behind it].. This carriage was propelled down the tube by compressed air and ran into a backstop just [beyond] these slots. Over here, looking through the slots was an ultra high-speed motion picture camera. If I recall correctly, it was three thousand frames per [minute].. We took pictures of the behavior of these rocket grains under this very strong deceleration mimicking the acceleration in the real rocket. Boy, those...we got some pictures too. It was exciting. These grains just danced and distorted. You could see the stress that they were under, so we easily understand how they would break up.

GRAYSON: Interesting. So, that helped [fund] your education as you were going all the way through your doctorate doing this research.

FIELD: Now of course, when I really got into graduate school, then I could get a graduate fellowship which means you taught lab. That's really the way I got through the latter part of the education, by teaching lab. Where are we?

GRAYSON: Well, I was looking at the idea that you're...contrasting your experiences in school with what today's student might experience. I don't know if there's a whole lot of difference or not. You still have graduate fellowships with people teaching labs in modern day

situations. Prices are different, but I suspect that students [...] there [were] tougher economic times during your student years?

FIELD: Oh, yeah, very much so.

GRAYSON: But as regard to the educational experience, I suspect it's pretty much the same.

FIELD: I suppose. Yeah. I think that's right. I don't really have a philosophy of education, except as I say for me, I just wanted to learn enough, so I could get a good job, which I think as a matter of fact, is a perfectly valid reason for getting an education. It may in fact, be the best [reason] for it. Well, my experience with today's student you know...I don't wish to speak to that.

GRAYSON: Okay.

FIELD: Shall we go on to five?

GRAYSON: Sure. So, you have your Ph.D. degree. Did you think at all about going into academia at the time or did you?

FIELD: I did. I did. I got a position as Instructor at the University of Texas [at Austin].

GRAYSON: Oh, okay.

FIELD: I was one of the last Instructors in American academe, because very shortly thereafter, the entry level was Assistant Professor.

GRAYSON: So, as an Instructor you essentially were a teacher. You didn't have any research...

FIELD: Oh, no, no, no. The four levels were at that time, Instructor, Assistant Professor, Associate Professor, Professor, and then, you know, Distinguished Professor and that sort of thing. But that wasn't part of the regular...

GRAYSON: So, how was the Instructor different from the Assistant Professor?

FIELD: Less money.

GRAYSON: Okay. But you were...

FIELD: It was a tenure track position.

GRAYSON: It was tenure track.

FIELD: It was tenure track and you were expected to produce research and promotion was based on research results just the way it is now. [Bells ringing.]

GRAYSON: The bell's ringing.

FIELD: Well, it'll stop.

GRAYSON: Amazing. So, what did you teach as an Instructor at University of Texas?

FIELD: Well, I had a lecture session. I lectured in freshmen chemistry. Then I had a quiz section, [which] was the usual assignment. I was expected to do research. My research allotment from the university was two hundred dollars per annum. In 1947, when I started this, there wasn't any government [sponsored, academic] research. There wasn't any NSF [National Science Foundation] or NIH [National Institutes of Health], none of that stuff. Well, it wasn't enough.

GRAYSON: What attracted you to UT? There was a position there and you decided to go.

FIELD: There was a position there; [they] advertised it, and I didn't see any new...

GRAYSON: At the time, the University of Texas was probably not the powerhouse it is today.

FIELD: That's true, but it was still a reputable university in a big state. I'm not very fond of Texas. They've got a bunch of assholes down there, especially in politics. But it was a reputable position.

GRAYSON: So, you went to University of Texas in Austin.

FIELD: In Austin, yeah.

GRAYSON: Okay.

FIELD: Now comes the part that is of interest for mass spectrometry, because I did my doctor's thesis here at Duke on the subject of magnetochemistry, that is investigating the magnetic properties [of materials], in my case cobalt and nickel ions. In the late 1930s and early 1940s there was a flurry of interest in magnetochemistry. As I was going through Duke in my graduate work at the time of the war, all sorts of [restrictions] applied; I didn't have a whole hell of a lot of choice.

So, the most promising research project that I could find was one with a [professor] named Warren [C.] Vosburgh, who was a relatively well known inorganic and analytical chemist. But, he dabbled in this magnetochemistry, but he really wasn't very knowledgeable about it. He was interested in complex ions, which is, I guess, why he got involved in it. When I got to the University of Texas, magnetochemistry was [the only research work] I knew, so that's what I wanted to continue doing. It would require a magnet, and a suitable magnet would have cost five thousand dollars. The University of Texas couldn't come up with the money. So, I improvised, and I [measured the optical spectra of some cobalt and nickel complexes]. I got a paper out of it,¹ [...] but it was pretty clear that I wasn't going to get any money for my magnet, and about that time, a great stroke of good luck hit me.

[...]

There was a colleague at the University of Texas who was consulting for the Humble Oil & Refining Company in Baytown, Texas. It came to pass that, oh, in the mid 1940s, mass spectrometry began to penetrate into the petroleum industry for the analysis of hydrocarbons, which you certainly know about. One of the first commercial mass spectrometers was made by the Westinghouse Corporation, and that was the Westinghouse LV instrument. It served good purpose, but in a few years, it was displaced by the technically more sophisticated CEC (Consolidated Engineering Corporation) [...] instrument. Now the Westinghouse was a 90°

¹ Frank H. Field and W. C. Vosburgh, "Complex ions. IX. Magnetic susceptibilities of nickel and cobalt complex ions from 0 to 80°," *Journal of the American Chemical Society* 71 (1949): 2398-401.

sector instrument and the CEC was a 180° instrument. Slightly better resolution, but much better stability and accuracy for analytical work, which is of course what the petroleum industry wanted.

So, Humble had this extra mass spectrometer on its hands. And it's not a piss-ant outfit, so it thought in terms of well, can we give this to somebody? [Frederick A.] Matsen, understanding the penurious financial situation at the university, was right there, and he said, "Yes, give it to the University," which they did. They gave it to Matsen.

GRAYSON: Matsen, who's he?

FIELD: Well, he was either an assistant or associate professor of chemistry at the time I was there, a colleague [...]: Frederick A. Matsen.

GRAYSON: So, he wangled this magnet or this...

FIELD: No, no, no, the whole mass spectrometer.

GRAYSON: Mass spectrometer.

FIELD: It sat in the hall outside his office, because he didn't know what to do with it. He was just scavenging [...] for the university. But there it was. This was 1948. I'd been there since—there being UT—since 1947. I was getting nowhere with research. So, we got together and he said, "Why don't you take this machine and try to use it?" [Well], I didn't know mass spectrometry from a hole in the ground; it wasn't very well known at that time, but I said yes [...].

Now, where I [could] set this bodacious instrument up? It turns out that there wasn't any space in the Chemistry Department for a machine of this size, not that it was all that big. But this was the end of the war. The University had undergone a tremendous expansion to take care of the G.I. Bill students. It went from a student body of twelve thousand in 1945 to eighteen thousand in 1947. So, everything was at sixes and sevens, and difficult. Well, the University had as a property a donation from the government, and that was the so-called magnesium plant [...]. The government during the war had built a factory [ten miles northwest of Austin] for the production of magnesium, which was used in flares, aerial flares. [Now the University of Texas Balcones Research Center.] The reason for [locating this plant where it was] was that going through Austin, going through the area [we are discussing] is the so-called Little Colorado River, not the big Colorado River, but a Texas river that flows into the Gulf of Mexico and goes through Austin. It had enough water flow that a series of three dams were built on it, thanks

largely to Lyndon [Baines] Johnson. These were primarily flood control dams, but of course, they generated power, and the power could be used to [electrolyze] magnesium salts to produce metallic magnesium. The end of the war came and the demand for metallic magnesium dropped to just about zero. So, this plant was closed down, but rather than just dump it, the government gave it to the University of Texas. [The University] never made use of the manufacturing facilities, but there was a laboratory, [which the University did use]. I was allotted space in this laboratory building.

Now, this was ten miles out of town. I was a poor boy. [I don't mean to harp on this fact, but it was a tremendously important factor in my life and work]. I mean, I got through college, but I didn't have any money. I didn't have any family to support me. I was being paid, thank you, three thousand dollars for nine months of work. Maybe for summer school, I might get another five hundred dollars for teaching summer school. So, I was living with a family on thirty-five hundred dollars a year, [and it] wasn't easy. The only car I could afford was a [used] 1929 Model A, which I bought in 1947. [It was a wreck, but] that was the transportation I had to get out to the magnesium plant. The radiator leaked, so I had to fill it up every morning. Well not quite. It leaked out enough that I couldn't...it didn't make sense to put antifreeze in it, and the February of 1949, was a particularly severe climate. The temperature got down to -8° in Austin, Texas, an all time record.

The way I got about working with this was, I would drain the radiator of this bucket every night, and fill it up in the morning, which I did. [That cold February morning] I [got] my car started, and all of the other, faculty members who lived in the faculty complex around there couldn't get their cars started. So, I took them to work in my 1929 Model A [...].

So, I moved this mass spectrometer [...]out to my lab at the magnesium plant. It was all packed up in boxes that had been shipped up from Baytown. They had given me a lab assistant at the magnesium plant, so my assistant and I started opening up these boxes. It was a disaster, because the gas handling system...and this was primarily a gas mass spectrometer, all glass...[was completely broken]. Every single piece of glass was broken. You know, big glass diffusion pumps, and so forth, and so on. You get the picture. So, what to do? Well, fortunately, there was a little bit of money in this magnesium plant operation, and they financed my getting the glassware repaired in Houston [Texas]. That was the nearest glassblower that would handle the job. Fortunately, the mass spectrometer tube itself was not damaged.

GRAYSON: That was a glass tube also.

FIELD: Yes, although it had a metal liner. So, I got the thing set up and I got it working. Not a bad trick for somebody who was starting absolutely from scratch, but that's the way I got into mass spectrometry. Now it didn't seem to make any sense to try to do analytical work. I didn't have any analytical work that needed to be done. Furthermore, do you know what the ion readout procedure was, what the method was?

GRAYSON: Is that the old turn the crank and mark the ion current [...] type thing?

FIELD: Have you ever used a suspension galvanometer?

GRAYSON: Yeah, in physics labs a long time ago.

FIELD: I read out the ion currents on a suspension galvanometer that was connected to an amplifier, an FP54 amplifier. An interesting vacuum tube that operated at a plate voltage of six volts, and the reason for that is you couldn't...it was a high sensitivity device, and you couldn't allow for any photoemission from the plate, from the anode. The good old days. Anyway, I got the thing going. I had to decide, well, what can I do with this thing now? [At this time] there was just the beginning of some papers by the handful of mass spectrometrists who existed at the time. The two that were most influential for me were John [A.] Hipple and Dave Stevenson, [David] P. Stevenson [...]. Hipple was on the staff at Westinghouse in Pittsburgh [Pennsylvania] and Stevenson was a Princeton [University] graduate who was doing an industrial postdoc with Hipple. They put out some of the early papers on electron impact phenomena and ionization potentials and appearance potentials.

Other names that are meaningful, there was Walker Bleakney, who was at Princeton. Perhaps, one of the most influential ones was a man named J. [John] T. Tate at the University of Minnesota. There were others that don't come to mind, but those were important ones.

So, I figured that I could use this apparatus for measuring appearance potentials, because I [wouldn't] have to do a lot of scanning. I mean, scanning a spectrum meant changing a voltage, step by step, and reading the deflection of this on the [graduated] scale, labor intensive to the point where it was just not practical. But what I could do in measuring appearance potential is to focus on the ion. The machine had enough stability that it would stay pretty well focused on an ion. The accelerating voltage of the electrons [was changed] to see where the ion current either appeared or disappeared, and that would be the appearance potential or the ionization potential.

So, I got started doing that. It was pretty [...] damned tedious, but I did it. The first thing that I worked on was, of all things, cyclopropane because it was a small molecule and nobody had ever done anything with it. So, I decided to do it, and I measured the ionization potential of cyclopropane, and published it.² That's one of those first publications in there from the University of Texas.

² F. H. Field and E. A. Hinkle, "The ionization potential of cyclopropane by electron impact," *Journal of Chemical Physics* 18 (1950): 1122-3.

There was an interesting development, which is that this innocuous little publication... I think it was in the *Journal of Chemical Physics*, was in conflict with a measurement of cyclopropane that was made by a man named P. [Peter] J. Dyne, who I think was at Oxford [University] in England. He had measured that ionization potential using a spectroscopic method and we had significantly divergent results. So, he wrote me and told me, I was wrong and that didn't go down very well—sort of an arrogant Englishman. We corresponded back and forth, and made some measurements and one thing or another. I think I was right, as I recall I was right. He just didn't interpret his spectra correctly.

GRAYSON: So, what would this spectroscopic method for the appearance potential measurement be using? Like, radio frequency spectroscopic...?

FIELD: No, no, the optical spectra.

GRAYSON: The optical spectra. So, if somehow...

FIELD: I'm not sure, but I think that it's a matter of looking for a convergence of a spectral series. Convergence as a function of frequency. You have to choose the right series, and but, I'm not knowledgeable about that.

GRAYSON: Yeah, but I can see that an optical spectroscopic method could be [used]. I can understand how that might be used as an appearance potential measurement. But as you say, [...] you've got a lot of series to choose from, especially something like cyclopropane. You know, I mean hydrogen's one thing, but cyclopropane is...

FIELD: Well, cyclopropane has a very well defined ionization potential. There's nothing in that.

GRAYSON: But, the spectroscopic series to select and measure it might be a little bit more complicated than for...

FIELD: But that's possible. I can't really speak with any degree of accuracy to that.

GRAYSON: So, you're duking it out with this guy at Oxford, and basically, you never really...he didn't come to your position, and vice versa.

FIELD: No, there was never a meeting of the minds, but I think that, in retrospect, I was right. I think that the ionization potential that's listed in the table now, is the one...close to the one that I got.

GRAYSON: Yeah.

FIELD: Well, then I went on from there and measured a few other compounds. Then another very good thing happened to me. That is that there had been this relationship [generated by Al Matsen] between the Humble Oil & Refining Company and the University of Texas [...]. Humble, set up a summer scholars program under the direction of Joe [L.] Franklin, who was a very effective sort of man, in his own way, brilliant. He pushed and pushed and pushed for the petroleum industry to become—or for the Humble Oil & Refining Company—to be more interested in basic research and in academic concepts. So, he set up this summer scholar program where people at several universities in Texas would go down to Baytown and work for a summer. He also set up the Humble Lectures in Science, where he would [...] identify the best research workers in the world in fields that might be of interest to Humble. [They would be brought] to Baytown for a two-week period where they would teach a course to fifteen students who were chosen from the scientific staff of the company. They would take a course. So, I took a course in free radical chemistry, two weeks from Cheves [T.] Walling. [...] He was a famous free radical chemist at the University of Utah in the 1950s.

GRAYSON: First name was?

FIELD: Cheves. And Ingold, Christopher [K.] Ingold was brought over from England. Michael [J. S.] Dewar was brought over from England. I mean this was a remarkably enlightened point of view and was all Joe Franklin's doing. Well, I was invited for one of these summer sessions and I went, gratefully, because I'm...you know that was money—salary.

GRAYSON: Oh, yeah.

FIELD: They liked me, and I liked them. I let them know indirectly that my situation at Austin wasn't very good, and [I was open to an offer....They made one, working with Joe Franklin, and he] and I had a very productive collaboration that lasted until 1965. He got an appointment as the Welch Professor at the Rice University. A year after that, I transferred to the Linden Unit of Esso Research and Engineering Company...

GRAYSON: In New Jersey?

FIELD: In Linden, New Jersey, yes. I was there until 1970, when I got an offer from Rockefeller [University] which I saw fit to take, and I went then to Rockefeller until I retired in 1989.

GRAYSON: Yeah. I'd like to backup just before we go too far forward. I probably should have stopped you sooner. Why...I mean, Humble apparently [had] an extra mass spectrometer that they didn't need and they gave it to the University of Texas.

FIELD: They replaced the Westinghouse with a Consolidated [model].

GRAYSON: Ah, okay. Okay. So, they [had] used the Westinghouse instrument and basically, they were surplusing it.

FIELD: In effect, yeah.

GRAYSON: Because they'd gotten a CEC instrument that was a better, okay.

FIELD: It was better for their purpose.

GRAYSON: Very good, okay. I understand now. So, finally, you've [...] when did you dust off the University of Texas from your shoes and move on? That would have been 1950?

FIELD: 1952.

GRAYSON: 1952. [...] You say Joe Franklin was the dominant force behind setting up this very, really kind of academic environment at Humble. Do you know how he came to be at Humble?

FIELD: Yes. Well, Joe Franklin is something of a phenomenon. He is a native of Natchez, Mississippi. You never think of anybody intellectual coming out of Natchez, Mississippi. But the two Franklin boys, Joe, J. F. Franklin, and his brother, B., which I think stood for Beaufort, both went to the University of Texas. That would have been in the late 1920s or early 1930s. Graduated in chemical engineering and went to work for Humble. Joe became the first

Research Associate at Humble, and B. became... I think he became the manager of the plant. Smart, smart people. Joe started as an engineer, and he did good work as an engineer on practical problems. He developed what is known as the propane deasphalting technique, which I dare say you don't know anything about. But what it amounts to is that asphalt in motor oils is very deleterious. It's desirable to get rid of it, and the way you get rid of it is you take the motor oil and dissolve it in liquid propane. The motor oil will dissolve in the propane, but the asphalt is too big, and it precipitates out, and so you filter it, and then reconstitute the motor oil. Joe developed that and several other things. He got interested in the course of his very practical work, in alkylation. Alkylation of hydrocarbons by olefins, although I always felt it had to be the other way around, but nonetheless you know what I'm talking about, to produce high octane gasoline.

Joe had a theoretical bend to this mind. He wondered why it was that certain things happened, and this gets fuzzy in my mind, but it has to do with energies of ions. Why when you alkylate, does the ion rearrange in a certain way, rather than in another way? This turns out to be a matter of the energies of the ions, and that got Joe interested in the question of the energy of the ions. He was thinking in terms of liquid phase phenomenon. But there wasn't any evidence, any knowledge about the energy of carbonium ions except [those] in the gas phase, [and this knowledge was] a result of those early mass spectrometry investigations made by Hipple and Stevenson and John Tate. So, he got interested in mass spectrometry, in particular in the energy of ions. Well, that's what I had started. I had started measurements of that sort up at Austin quite independently. But Joe heard about it and got me down to Baytown for the summer. One thing led to another and so I just left Austin, and joined Humble. I spent about 95 percent and maybe even 99 percent of my time in the Standard Oil organization, that would be at Humble and at Linden, doing basic research. It was just absolutely remarkable. Now, that had more to do with Joe Franklin, getting it started than it did with me. But if I hadn't done well with it wouldn't have gone anywhere.

GRAYSON: Can you explain this business with Standard. Standard Oil, I guess, was kind of like an octopus type company. I mean, you think about Humble, and then there's Esso, and there's.

FIELD: In the beginning, there was John D. Rockefeller. [laughter] He was a Cleveland [Ohio] boy. Of course, about ninety miles from Cleveland are the Pennsylvania oil fields. So, he got interested in oil. Now, this would have been 1870. He set up the Standard Oil Company which was incorporated in Cleveland. He got into the oil business. My recollection is that he was primarily concerned with shipping the oil. They used to put it in barrels and put the barrels on [railroad flat cars], and he cornered the market on that. Developed the Standard Oil Company into what was called the "octopus." He was a very, very shrewd, ruthless sort of guy and made himself rich as Rockefeller, which used to be a phrase. You know, we don't think of it anymore, but he was a dominant economic figure or one of the dominant ones. J. P. Morgan was probably equivalent. Until 1906, it was a monopoly. It was a cartel. It was a trust. Teddy [Theodore] Roosevelt came along and busted the trusts. Standard Oil Company was broken up

into a number of different companies. The direct descendant of the Standard Oil Company was Standard Oil Company (New Jersey). That became known as Esso. That was *the* Esso. Then there was Standard of Indiana, and Standard of Ohio, and so forth and so on. But, Standard Oil (New Jersey), also referred to as Jersey Standard, was in effect the direct descendant.

Now, it was basically, a refining and distributing company. It didn't have any significant amount of production facilities, so that in the course, in the fullness of time, it struck up agreements with regional oil companies to develop production capabilities. One of these was the Humble Oil & Refining Company which was a Houston outfit. The peculiar name came from the fact that the Humble Company was formed, I think 1917 or 1919, something like that, in the town of Humble, Texas, which is about thirty miles north of Houston. It was a well-known town. But the company is proud of the fact that there were three partners who had oil properties. They met on the porch of the General Store in Humble, Texas and shook hands on a deal that made the Humble Oil Company. In the 1920s, in the early 1920s, Standard needed production facilities. So, it came along and bought a portion of Humble.

Now, when I worked for Humble, it was only owned 70 percent by Standard. There was still 30 percent that reported to the Board of Directors in Houston, and boy, didn't Joe Franklin and the research people make use of that, because these...I talk too much.

GRAYSON: No, no, no, no.

FIELD: Well, I've heard all this before, you know. [laughter] Anyway, I lost my train of thought.

GRAYSON: Joe Franklin worked [at] Humble. Well, did he work under [the] 30 percent that was Humble?

FIELD: Yeah. Well, there was, you know, a lever or two to use to get more basic research. The Humble people, the men that founded, were wildcatters. You know what a wildcatter is? So, they were used to taking risks. The Standard Oil people up in New Jersey, they were businessmen and a completely different outlook. We got to do basic research in part because of this [divergence] of outlook. So, Joe and I worked together doing basic research, writing papers, having a pretty good time, really, until as I say, he left in 1965 to take a Welch Professorship. I left in 1966 to go up to Standard Oil (New Jersey), and then I left to go to Rockefeller. I was going to say something else, but it's...

GRAYSON: Well, during that period, most of the work that you did was related to the...I think what would be referred to as the fundamentals of ionization, the fundamental gas phase ion chemistry, maybe.

FIELD: Well, all of the papers that came out between...well, I went there in 1952, between 1952, or not all, but virtually all, were devoted to energies of gaseous ions. But also in there, the company supported the writing of our book that I have made mention of which I'll speak about, if you wish.³

GRAYSON: Yes, let's. I'd like to explore that. I mean, what prompted that? [...] Here's two guys in an industrial laboratory, but granted they're doing basic research, but they're not normally the kind of people that you would think in today's world, who would write a book on something as esoteric as electron impact phenomenon, gaseous ions.

FIELD: But you see, that illustrates the point I'm making. That is this was Joe's idea, basically, to write something because the fact of the matter is that there had been a flurry of interest and work in gaseous ion energies, and chemistry starting in the mid 1940s with Stevenson and Hipple and John Tate, extending up to the time I'm talking about which would be about 1953. We, Joe and I...I think it fair to say, we were in the forefront of that, along with other people, but we were a factor in the area, but it was a disorganized area. There were these several papers, and several workers, but we got advice...we had Henry Eyring as a Humble lecturer in science. I told you about that Humble Lectures in Science.

Henry Eyring came down and we talked with him about what we were doing. He was enthusiastic. He said, "You people need to write this up." Well, when Henry Eyring tells you, you need to write your work up, that means something. Who could say no? So, Henry thought that we should write a review paper [...] the name of which escapes me.

GRAYSON: There [were] a bunch.

FIELD: Anyway, a review paper. So, we set out to write a review paper, but it turned out that there was so much material that our review paper ended up too large to go into the review journal. So, then came the thought, well, why not publish a book? So, somebody had some contact with Academic Press and suggested we submit it there, which we did and was accepted and published. It had a—I would say—profound affect on the discipline of gaseous ion chemistry because it pulled it all together. In the introduction, which I wrote—I wrote most of it, as a matter of fact. Joe had other things to do. Besides, I wrote better than he did. I said something to the effect...we ourselves are surprised that the discipline of gaseous ion chemistry has as many implications as in fact, it does. That was the case. We made a science out of gaseous ion chemistry, in effect.

³ F. H. Field and J. L. Franklin, *Electron Impact Phenomena and the Properties of Gaseous Ions*, 1st Ed. (New York: Academic Press, 1957)

GRAYSON: By bringing all the work together and showing ion...

FIELD: Yeah, by putting it all together...

GRAYSON: Yeah. Now, it was published in 1957. So, in addition, there were tables of data in there, and you reviewed...

FIELD: Well, one of the big things that we did was to tabulate all of the gaseous ions energies that we could find in the literature at the time. Well, we cut it off at 1955, but then we added a little of 1956 and 1957, when we had to go through a revision of the manuscript. But, our things, table... what the hell table was it, the table at the end of the book where we list all of the ion chemistry or ion energies that we could find. We made our choice. We made a recommendation as to which would be the most reliable.

GRAYSON: That was kind of gutsy.

FIELD: Well, you had to.

GRAYSON: Because there were so many.

FIELD: But there was enough of a scatter and enough different kinds of techniques used to make the measurements that...yes, critical evaluation was essential.

GRAYSON: That took a lot of time.

FIELD: It did take a lot of time, but the interesting thing is here's good old Humble Oil & Refining Company supporting me full-time doing this. Joe did some other things. But that was my job. I'd sit in front of my typewriter and type and stew. We made some intellectual contributions in all this too, because at the time, one of the real controversies in science was, what is the heat of sublimation of carbon? There was a low value of 125 kilocalories per mole, and a high value of 170 kilocalories per mole, and I think there might have been an intermediary one too, but it wasn't taken very seriously. One of the pieces of evidence for the heat sublimation of carbon can be found in the electron energy spectrum of methane. You know methane has a certain ionization potential. Methyl radical, methyl ion has a certain energy, and

so forth and so on. Put all these together and come up with the value of heat sublimation of carbon.

We had to look at all of this and make a choice as to what was the best value. In fact, we argued against...we decided against the electron impact value which was 125 in favor of the 170 which was done by the very direct method of putting a crucible into a mass spectrometer and you put carbon in the crucible and heat it and see how the vapor pressure varied with the temperature. [Then you applied] the Clausius–Clapeyron equation. Okay, where are we?

GRAYSON: Okay. Well, the book and the work that it engendered, the fact, that you actually did it as an employee of a commercial, industrial laboratory and [created] the book. I assume that it was relatively well received in the circles of people that did that kind of work. Now, how many...do you have any idea how many copies were printed for starters? You know it's got to be...

FIELD: I don't know, five hundred, five-seven hundred, something like that. It went into a second printing.

GRAYSON: Yeah. Oh, a second printing, okay.

FIELD: No, actually it was second edition.

GRAYSON: Okay. Now, second edition.

FIELD: Revised. We talked about this on the phone. Academic called it a revised edition.

GRAYSON: Revised edition. But the revised edition is almost twice as many pages, did you just...

FIELD: And the reason for that is in the first edition, the energy table, the ion energy table was the one we had revised. But then, after that the National Bureau of Standards volume of ion energies came out⁴ and that's public domain, so what Academic Press did was just to take the values from that and incorporate them into the back of our book. That's what caused the difference in the number of pages.

⁴ Charlotte E. Moore, *Ionization potentials and ionization limits derived from the analyses of optical spectra* (Washington, D.C.: U. S. Dept of Commerce, National Bureau of Standards, 1970).

Now, something else I want to say is that most of what I'm known for is this ion energy business and other things as well. But one of the early papers that I wrote with a fellow named Sam Hastings is also very important.⁵ That is [about] low voltage ionization mass spectrometry. Because what we did was to determine that you could quantitate unsaturates in petroleum just by running a mass spectra at a low enough voltage that only the unsats ionized and didn't fragment to any significant amount. That found immediate [...] attention and utilization, because it turned out that you could get information about olefins or aromatics in a petroleum sample. And even [in petroleum] crude by a five minute mass spectrogram rather than requiring a twenty hour distillation.

GRAYSON: So, by lowering the ionization potential you limit the hydrocarbon types that are going to be...

FIELD: Ionized. Only the unsats get ionized.

GRAYSON: So, the olefins and the basic...

FIELD: And the aromatics.

GRAYSON: ...olefins and aromatics are ionized and since most of the samples probably [are] constituted [of] saturated hydrocarbons [in large part].

FIELD: Well, it all depends.

GRAYSON: So [...] take [peaks from] those out; and then the other advantage [...] is that the fragmentation of those unsaturated compounds is mostly reduced.

FIELD: What you get in principle, it never works out quite that way, but you get a series of peaks that just represent the molecular ions. And consequently, the molecular distribution of the hydrocarbons, of unsats, in the mix. That has been used very extensively in the petroleum industry or it was. I don't know the way it is now.

⁵ F. H. Field and S. H. Hastings, "Determination of unsaturated hydrocarbons by low-voltage mass spectrometry," *Analytical Chemistry* 28 (1956): 1248-55.

GRAYSON: Yeah. [...] There's a fellow by the name of Ron [Ronald D.] Grigsby, who actually still does this hydrocarbon analysis where you make you – the matrix calculation for the PONA [paraffins, olefins, naphthenes, and aromatics] or PNA [polynuclear aromatic hydrocarbon] analysis. You end up taking a normal...I think a regular high voltage mass spectrum and then do a matrix inversion and [...] you get the different hydrocarbon types out. He actually runs that analysis today out of his basement, at the CEC 110 instrument that he has in his basement, if you can imagine that. [laughter]

FIELD: Hard. Can you imagine doing that matrix calculation using a Marchant calculator which is the way it used to be done?

GRAYSON: I pecked away at a Marchant calculator quite a bit in my day. I guess those are past or don't exist anymore. Are they relics?

FIELD: You know, we used to say that the worst thing a man could face is falling into a Marchant calculator while it's doing a division. [laughter]

GRAYSON: So, you were in industry for, how many years, about ten, twelve?

FIELD: From 1952 until 1966. No, wrong. 1970. Yeah, from 1952 to 1970. That's all with the Standard organization.

GRAYSON: Okay. Any other things you want to tell us about your industrial experiences? I mean, they were different than most places, particularly than any place today.

FIELD: Well, what I have to say is that [...] I fell into clover.

GRAYSON: Oh, yeah.

FIELD: I was just very fortunate. Joe Franklin was a big part of it, but then I did well enough at it, once I got started, people started looking for me and took me as I was.

GRAYSON: You've also done some collaborative work with Burnaby Munson. When did Burnaby show up on the scene?

FIELD: Well, now, this was at Baytown. Joe Franklin and I were working. We had a little group there and Jean Futrell was part of it. But Jean left, and who else left? Somebody...oh, I know who it was. Fred Lampe was part of the group. So, the little group was Joe, and me, and Fred with Jean Futrell sort of on the edge, but he left fairly early on to go to the Air Force to Wright-Patterson [Air Force Base]. Fred was recruited by Penn State [Pennsylvania State University], so he left and took a job at Penn State [Pennsylvania State University]. He was an associate professor, I think when he went, and became full professor and chairman of the department and so forth.

To replace Fred, we brought in Burnaby Munson, who had been working at Humble. He had been working in Baytown in the research laboratory doing some work with separations for about a year. Two things were obvious. One was that Burnaby was a smart guy, but he wasn't happy. He wasn't doing what he wanted to do. He was just farting around. So, Joe had more to do with this than I did, brought him in and we started—the three of us—started working together. Of course, Burnaby, we were right about Burnaby's being a smart guy. When Joe left, [Burnaby] and I worked together only about a year, year and a half, something of that sort, until I left to go up to Linden. Shortly thereafter, Burnaby [...] took a job at the University of Delaware, but he got a number of publications out that were of some importance.

GRAYSON: So, [Franklin] kind of got the group to a critical mass for a number of years, until everybody, kind of, bailed out. Did people leave, because they were being sought or did people leave because the atmosphere at Humble was changing you know with regard to the attitude...

FIELD: No, they left because they were recruited. [...] I mean, that's why Fred left. That's why Joe left. That's why I left. Burnaby left, because there was nobody left. [laughter]

GRAYSON: Yeah, yeah. So, the move up to Linden, what prompted that...up to New Jersey?

FIELD: Well, the manager of the Research and Development Division at Baytown was talking with the director of the Long Range Research Division in Linden. He made the mistake of saying, "Well, now, is there anybody that we have that you would like to have?" [The Long Range Research director], [Jack Ludwell], said immediately, "Frank Field."

GRAYSON: So, you got recruited away to a different part of the organization.

FIELD: To a different part of the organization, yeah.

GRAYSON: Of course, then moving back to New Jersey, probably was not all that bad for you, since you were familiar with the area, and you had grown up close [...] to the east coast area.

FIELD: Well, it wasn't bad for me in some ways, but it was in others, because it was a part of New Jersey that I wasn't familiar with, and I had a Texas wife, and Texas family, and they didn't understand it at all. Then the other thing is that—I'll bet, it's still this way now—there is a vast, or was a vast difference in living expenses. So, that the company as you know undertakes to keep you whole. But it didn't, because I sold my house in Baytown, which was perfectly adequate for my family. They gave me twelve thousand dollars for it. The only house that I could find that would do in Summit, New Jersey was forty-five thousand dollars. [Esso had to give me] supplemental moving expenses so I could live. That wasn't easy. I mean it wasn't an easy experience. We lived in a hotel for a month.

GRAYSON: Of course, the price of real estate probably went up a lot more when you moved to your next destination, New York.

FIELD: Oh, I guess. I mean we didn't buy in New York. We just rented. At Rockefeller, it was a subsidized apartment. So, that, yes, it was high. We were paying three hundred and fifty dollars a month rent. Well, that wasn't all that trivial in 1970, but it wasn't backbreaking. They paid me enough that I could cover it easily.

GRAYSON: While you were at Linden, only a couple of years, you still continued the same type of research routine?

FIELD: Yeah, um-hmm.

GRAYSON: Okay. So, eventually, Rockefeller recruited you. I mean that seems like an unusual connection. Well maybe not. I don't know. But, I mean, [you would be] doing basic research in gas [phase] ion chemistry [and] a university [wants] you in medical school...you went to the medical school, right, at Rockefeller?

FIELD: No. Rockefeller was The Rockefeller Institute for Medical Research. And in 1966, it became the Rockefeller University, but the same administration, same everything, except changing name. A little bit in function, because they could take on students.

GRAYSON: Oh, okay.

FIELD: The way that worked out, I was getting a little bit uneasy about my situation at Esso, because basic research is fine, sounds good. But I got increasingly of the opinion that this isn't really making the company all that much money. It really isn't mainstream, and I really would like to be meaningful—mainstream. I didn't know what to do. [...] I was also on a trip giving a lecture in Washington [D.C]. Of course, Hank [Henry M.] Fales was in Washington...

GRAYSON: Okay.

FIELD: I happened to let on that there was a problem with my situation at Esso. I was worried that I wasn't doing what really I needed to do. Hank was sympathetic with this. It just so happened that at that time, Rockefeller had started some negotiations with him about [coming to Rockefeller] and setting up mass spectrometry [...]. Because there was a guy at Rockefeller named [Lyman R.] Craig, who recognized that mass spectrometry had become a major influence, a major force in biochemical research. He felt that Rockefeller, as a leading institution, should partake of it. So, his first thought was to talk to Hank, who was at the National Institutes of Health, and was a good deal better known in biomedical mass spectrometry than I was. [...Rockefeller] offered him the job, but for personal reasons, he couldn't take it. He had some sort of disabled wife or maybe a child or something like that, and couldn't move. But he suggested me.

So, I got a rather enigmatic letter one day from [Lyman] Craig suggesting that I come over to Rockefeller and give a lecture about my ionization. That's what he said, "Your ionization." He meant chemical ionization, of course. So, sure, I mean, I had got all sorts of invitations to speak. So, I went over, just across the river. Then it turned out that they were recruiting me. He [only] said, "I want you to go to [talk to] the president." [When I did] the president offered me a job. [laughter] I was, I think, the only person taken on at Rockefeller as a full professor with a background in industry.

GRAYSON: I can imagine. Well, [...] you just said something that rang a bell with me [...]. When you spoke about your ionization, so chemical ionization, we need to explore the history of that. I kind of let it slip by.

FIELD: Well, that of course, is my big contribution, although as I said earlier, I think the book and the low voltage ionization...I won't say are of equal importance, but they're pretty high importance, [although] not as well known or celebrated. But, at Baytown, we built a new mass spectrometer. We had been doing this work, this ion energy work with converted and abandoned commercial instruments. The first work we did was with a Consolidated, CEC 620, which was a cycloidal instrument, but a little one, just a Cracker Jack idea, that cycloidal

instrument, but small. We did [some valuable] work with it, [but] in about 1962, 1963, something like that, we realized that we needed a better instrument.

[...] So, I started designing an instrument that I thought would do the job for us. One of the things that I felt was very much needed was an ability to go to higher pressures, because in the mass spectrometers that we had been using, if you got up to a pressure in the ionization chamber of as much as 10 microns...is that the word I want?

GRAYSON: I remember the ion sources were all pressure sensitive, so they didn't blow out a filament.

FIELD: [...] They were designed to operate 10^{-5} torr. But I felt that we wanted to get to higher pressures than that. So, I went whole hog in the design and put [...] a four inch diameter diffusion pump on the source area at a four-inch diameter diffusion pump on the mass analyzer with a small slit in between. With it we were able to get the pressure up to two torr.

GRAYSON: Wow.

FIELD: We saw all sorts of interesting phenomena. One of these was that when we made an elaborate study of methane, just increasing the pressure, increasing the pressure, and looking at the different ions that formed, and how they varied in intensity. What we discovered was that in methane in our apparatus [at] something like one [torr...], the relative [intensity] distribution of the ions didn't change [as the pressure was increased further].

Now of course, the [absolute] intensity would drop some, even in spite of the differential pumping. But the relative intensities of the ions did not change, which meant that these ions were stable in methane. The major ions would be the mass 17 which is CH_5^+ , 29 C_2H_5^+ , 41 C_3H_5^+ , and then smaller ones. That was interesting, because nobody knew anything very much about [the reactions of gaseous ions...]? Well, the answer is not much. [But we showed that the ions formed in methane at high pressure did not react further; that is, they were stable].

But then we discovered that in order to get this [...] stability of this spectrum, we had to make sure that we had [to have] bone dry methane. It couldn't have any water in it at all or else we ended up with hydronium ion, mass 18...mass 19, sorry. One thing led to another, and all of a sudden, it clicked. Well, let's see what happens if we add other substances to this methane as a carrier at high pressure[...]. Of course, what we found was that a new kind of spectrum developed and that was the chemical ionization spectrum. We wrote that up, and submitted it to *Journal of the American Chemical Society* and got a scathing review back.⁶

⁶ 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.

GRAYSON: That was with Munson?

FIELD: That was with Burnaby, yes. Yeah. The reviewer thought we were crazy and recommended that [the manuscript be rejected and returned] and so forth, and he made some idiotic remarks which we argued with. Fortunately, the editor at that time was a man named A. B. F. Duncan. I never met him. I think it was sort of an elderly photochemist, but he believed us, instead of the reviewers. The paper was published, and it was a wild success. We ran out of reprints in no time. Those were the days when you had reprints, rather than copying machines. Chemical ionization was a real contribution.

GRAYSON: Well, it allowed the investigation of a lot of compounds that you couldn't otherwise look at.

[...]

FIELD: [...] The problem with electron impact ionization is that you get much fragmentation, and there are quite a number of compounds and compound types that don't give you molecular ions. One of the things that we—and by we, I mean me, Joe Franklin, Fred Lampe, [...] Burnaby...this whole Esso group, Humble group—felt was important was molecular ions because we had to deal with complex mixtures. If we could pick out molecular ions, it told us a lot about the mixture.

Now, by contrast, there are people like Fred McLafferty, bless his soul, who didn't have that background. He devoted his attention to trying to interpret the fragmentation and to do the structure of the molecule from the fragmentation. You can do that, if you're working with individual compounds, and Fred was working at the time that he got involved with all of this with Dow, which is a chemical company, not a petroleum company. So, he wasn't faced with this complex mixture problem to the extent that we were. So, he put the emphasis in [the fragmentation] direction. We put the emphasis in the [molecular ion direction]. That's why we were so intrigued with the fact the chemical ionization gave much stronger molecular ions, which we called quasi-molecular ions, because they were protonated or [...] hydride [ion] depleted. We were very gratified that the chemical ionization was much better at that than the electron ionization.

GRAYSON: Yes. So, then apparently, the commercial companies started coming out with chemical ionization sources [...] pretty soon after that.

FIELD: Oh, it was an interesting story there. As soon as we discovered what we had as good industrial scientists, [we] talked to [our] patent people and got a patent. [But there was a bit of a problem, and that was that] Esso is Standard Oil (New Jersey), now Exxon Mobil. It's so [...] big that if a patent doesn't bring in just oodles of dollars per year, they're not interested in it. For all its value, chemical ionization was never going to make anybody that kind of money. It's not like digging petroleum out of the ground. So, in the fullness of time, Esso sold the patent rights to Marvin [L.] Vestal. You know Marvin Vestal?

GRAYSON: I know Marvin.

FIELD: Marvin was running an outfit called Vestec. Marvin's a smart guy, and he saw the value of [chemical ionization]. Esso sold [Marvin] that patent for something like five thousand dollars. Broke my heart. [But] Marvin took it and ran with it, and he built chemical ionization [attachments], things that could be used to make a machine [do] of chemical ionization [...]. Well, [eventually] Marvin was bought out. Now how did that work? Marvin was bought out by [G. D.] Searle [& Company], but the only [asset] that Searle wanted was the chemical ionization patent, or something like that.

GRAYSON: Yeah. That's the pharmaceutical house, Searle?

FIELD: Yeah. G. D. Searle. I think that Marvin was bought out by Searle. Anyway, the patent had some monetary value, but not for a company as big as Esso.

GRAYSON: Then of course, I think other companies, I don't know if some of these things are done by license or they just create a chemical ionization source and do it slightly differently than the patent, so that they can bypass it.

FIELD: It's easier just to [...]pay royalties.

GRAYSON: So, that's the story on the chemical ionization development. Just curiosity.

FIELD: I would say not for development of a technical field. When you keep working in it, in something, and if you keep your eyes open, you discover something. You know there's an old saying, "If you work with anything long enough, you'll bust it." But there's another face on that, and I found this when I was trying to do a little bit of electronics well, after I retired, and I didn't know much about it. But, I discovered then, what I've really known all my life without recognizing it, is that if you've got a problem, and you work at it and continue to work at it with

as much intelligence as you can bring to bear, you may very well fix it. It's something like that. You know we had this phenomenon. We didn't understand it. We just kept poking with it and kept our eyes open, and made an important observation. Where are we?

GRAYSON: Well, I think we've probably covered quite a bit of your career up through [...] Linden...your time [in] the commercial side of the house. We talked a little bit about patents and [...] a little bit about scientific innovation and so on.

FIELD: All right. Well, how about...we'll we're in [question] five—employment experiences. We've done government, academia.

GRAYSON: So, when you went to Rockefeller, it was then more like...it was a university. You actually had students that were attending [...] Rockefeller.

FIELD: Students in a very peculiar sense. Just graduate students, although there were some M.D./Ph.D. students. They gave just the Ph.D. degree. The education [at Rockefeller] was working in the laboratory beside the professor, a few courses, not many, not very important, and a superb place to be. [...] Oh, I did a very, a minimal amount of teaching. I didn't really have any classroom obligations. I didn't have many students either [...]. I did my best [...] to make myself relevant, useful. So, I switched over to a very considerable extent [...] to biochemistry—biochemical mass spectrometry. My successor in the position, who [is] Brian [T.] Chait, has gone farther than that, and [...] is doing actual biochemical research. So, that worked out all right.

GRAYSON: So, what was the main difference in going from industrial, essentially, petroleum chemistry, mass spectrometry into biomedical, biochemical, and biomedical mass spectrometry. It a little bit of a leap there. The interesting thing is that Rockefeller would recruit you, because you're, you know the work experience is totally different than, what they were hoping to get into. You know the mass spectrometry part. They must have recognized that your intellect was...had a broader than...

FIELD: Well, I was going to say that, though it sounds rather immodest, but Rockefeller recruits on the basis of quality. No, I went over and I talked to them. I had a C.V. that was pretty impressive, really, and they hired me. I lived up to it. I modified it. I did not become a biochemist. But, I did a good deal of biochemical mass spectrometry. I made an effort. If you look at the C.V., you'll find that there are some pretty important biochemical studies.

GRAYSON: Did I get a C.V. from you, yet?

FIELD: I don't know.

GRAYSON: I don't recall either, but I need one. So, I assume you have one filed away on a computer or a copy somewhere.

FIELD: I'll have to look, Mike.

GRAYSON: Okay, because that's something [we need]. There's always little details of things that we want to get together to put with this. We'll have to get a C.V.. I'll make a note of that that we need to get a C.V.. So, when you went to Rockefeller, they gave you what...money, space, or was any instrumentation onboard?

FIELD: Yeah, all of those things.

GRAYSON: So, but you had to buy the equipment that was...

FIELD: Yeah.

GRAYSON: So, it was your, basically...

FIELD: Well, I took what came to be known as the Humble/Esso mass spectrometer with me. So, I was able to continue [the gaseous ion chemistry work]. I worked on it in two different ways, one continuing the basic research in gaseous ion chemistry. The other doing biomedical [mass spectrometry].

GRAYSON: So, that instrument that you designed in Baytown went to Linden.

FIELD: Yes. And then went to New York.

GRAYSON: And then it went to New York. Okay.

FIELD: Yeah. So, it's a peripatetic mass spectrometer. [laughter]

GRAYSON: Then you were able to buy what you needed for the medical side.

FIELD: Well, I started out...Rockefeller gave me money, and I bought a DuPont 492 Model mass spectrometer, which wasn't much of a mass spectrometer. Then, several years later, the NIH came along with a program to provide instruments for biomedicine and I got a VG. What was the number on it? Anyway, it was a Vacuum Generators mass spectrometer which [came] with the data system and everything.

But, what I really did at Rockefeller of value, was I went to a meeting, one of these [...] I went to a meeting. Where the hell was it? Oh, it was in Washington. It was in Washington. It was an NIH meeting of 1980, something like that...before that...1975. I heard Ron Macfarlane talk about his Californium-252 machine. I thought to myself...my God, we just have to have that. So, I built the second one, certainly in this country, maybe in the world, after Ron, and put it to use. Then somewhat after that, then it was 1986 or so, I went to Bordeaux [France] to an international meeting. I heard [...]

GRAYSON: Oh, Franz Hillenkamp?

FIELD: ...talk about the laser desorption mass spectrometer, the matrix-assisted laser desorption, and I came back. I was about to retire, but by this time, Brian Chait had been working with me for a number of years, and obviously was going to be my successor. So, I gave the information to him, and he worked like a beaver and came up very rapidly with the matrix system. What's it called?

GRAYSON: MALDI [Matrix-Assisted Laser Desorption/Ionization].

FIELD: MALDI. MALDI, which is an extremely useful device for biochemical work, and all the while, we were using these instruments to do analyses and do some research. Well, look. Shall we defer and go and see what Carolyn has provided [for lunch]?

[...]

[END OF AUDIO, FILE 1.1]

GRAYSON: [It looks like we're recording].

FIELD: That's an awful long lasting tape recorder.

GRAYSON: It's a digital machine. It's got 1 gigabyte of memory stuck in there somewhere, so it's actually converting our conversation to digital form, and storing it on there.

FIELD: So, it's much more compressed.

GRAYSON: Yeah. I've got Klaus Biemann on there, and Fred McLafferty and a whole bunch of other people are on it. [laughter] So, you're in good company.

FIELD: Yeah, well, good, good.

GRAYSON: I assume that's good company.

FIELD: Top fellows, all of them.

GRAYSON: One of the things that you mentioned as we were wrapping up before lunch, was that you had built an instrument at Humble. You'd built an instrument—two instruments—at Rockefeller, one for the Californium desorption work and another one for MALDI.

FIELD: Well, Brian Chait built the MALDI. I came back from Switzerland...not Switzerland, from Bordeaux with the news of the Hillenkamp advance, and described it to Brian and urged him to try to get some money from NIH. NIH was immediately responsive and put up enough money to build it.

GRAYSON: Do you remember the year?

FIELD: Beg pardon.

GRAYSON: Do you remember the year that...

FIELD: 1988.

GRAYSON: 1988?

FIELD: Yeah, um-hmm.

GRAYSON: Okay, because there's all this kind of controversy around MALDI, and who deserves to be given the credit for matrix assisted laser desorption ionization.

FIELD: No. I don't have any doubt about it. Hillenkamp.

GRAYSON: Well, I think that's probably the feeling in the community, but then, I mean you actually were designing and building these instruments. [...] They were built from scratch.

FIELD: Yeah, but two of them. The third one too, because it was Brian built from scratch. Now by then we'd had a fair amount of experience with time of flight, impact time of flight mass spectrometry. So, it was a just a matter of putting a laser on the front of a time of flight mass spectrometer. Brian did that with a great deal of alacrity and got into the racket and has done very well with it.

GRAYSON: So, on the Californium desorption work that was [...] a time of flight instrument also, wasn't it?

FIELD: Yes.

GRAYSON: [...] When you got that going, that was a completely different kind of experiment, because you had a non-synchronized time event...it wasn't repetitive and it was something had to be triggered to get the time of flight measured.

FIELD: Well, the Californium was the trigger.

GRAYSON: Right. So, what mass range were you able to get results [with] that instrument, do you remember? [...]

FIELD: Oh, we were able to get pretty good results on peptides. My recollection is somewhere...we might have got up to about five thousand, in the low tens of thousands.

GRAYSON: Okay. That was pretty [good].

FIELD: Not anywhere near as effective in getting stuff up as the laser desorption.

GRAYSON: Yeah. But at the time, that was pretty advanced.

FIELD: Oh, at the time, it was just incredible.

GRAYSON: Because most instruments, kind of, ran out of steam way below that.

FIELD: Well, without the desorption, you were limited with derivitization procedures, and to get three thousand was just absolutely remarkable. You know, it wasn't really a very feasible sort of thing to do. So, the desorption techniques really provided the means by which mass spectrometry made very meaningful contributions into biochemical, biomedicine in that then you could look at polymers. Now, of course, one used mass spectrometry for metabolites and the sort of thing that the [Evan C. and Marjorie G.] Hornings did at Baylor [University]. But until you could look at proteins, big peptides, sugars, the biopolymers, you weren't in the forefront of or the mainstream of biomedical mass spectrometry.

GRAYSON: Now when you first went to Rockefeller, did you start the Californium instrument then, when you first went there or [at] a [later] period of time? You said you had a VG instrument for a while.

FIELD: It was when I went there. I went to Rockefeller in 1970. As I say, I went to this meeting in...I guess, it was in Bethesda [Maryland], and heard Ron Macfarlane in about 1973, and immediately started building the instrument. Got it built and operating 1975, something like that, it took a while. It's such a new technique for me, but anyway.

GRAYSON: Yeah. So, what kind of work did you do before you had the Californium—the desorption instrument? [...].

FIELD: As I said, I had a two-fold line of research at Rockefeller. One was a continuation of the gaseous design chemistry. Then the other was biomedical mass spectrometry, which was pretty much in the way of a service function. People would come in...that is, coworkers, but not only from Rockefeller, but you know the corner of 68th Street and York Avenue is pretty much a hotbed of biomedicine. There's Rockefeller on one corner, New York Hospital at Cornell Medical on the other corner, and Memorial Sloan-Kettering on the third corner, then of course, quite a number of other institutions. So, I was an NIH Biomedical Research Resource, so we did work for anybody who was getting government grants.

GRAYSON: Right. [...] Mike Gross has that research resource in mass spectrometry at Washington University in St. Louis [...]. He used to be at Nebraska, Mike Gross, and then he moved to Washington University probably about twenty years ago.

FIELD: Yeah, I know.

GRAYSON: To manage the research resource that had been at Washington University for a number of years prior to his arrival. [...] They wanted someone with mass spec experience to take it over. Okay, so similar to, what do they call those things, P50 grants or something. I don't remember the name, but it's not an R01 or any of that kind of stuff [...].

FIELD: Not R01.

GRAYSON: [...] So, once you got to Californium desorption, then you can start doing research in a biomedical side that was [in the higher mass range].

FIELD: Well, I did a little research before that, but it was not high molecular weight research work.

GRAYSON: So, you left right about the time the MALDI instrument was coming together and Brian Chait was taking it over.

FIELD: Yeah. Well, he didn't take it over. He built it.

GRAYSON: He built it. Okay.

FIELD: I brought back the idea from Europe, from Bordeaux. I said, “Brian, we’ve got to do this or you’ve got to do this.” He did it. In very, very short order, very effective fellow is Brian Chait.

GRAYSON: Well, I’m sure aware, if you don’t move quickly in this field then...

FIELD: You’re dead.

GRAYSON: Yeah. You’re behind. You fall behind very quickly. I’ve gone to the last couple of PittCons [Pittsburgh Conferences] and what’s going on now with the instrument development is just amazing. These instrument companies are like – they must pull their hair out every time PittCon comes around, because there’s so much new technology coming together.

FIELD: Well, I decided once I retired, I wasn’t going to try to keep up with it. It didn’t make any sense. One of the reasons I retired is things were coming so fast, and I was getting older and slower intellectually. You know, I had been fighting that battle for thirty-some odd years. I just decided to hell with it. There are phases of life. You move from one phase into the next phase, and don’t try to carry the former one with you, which is one what I’ve done.

GRAYSON: So, you retired in 19...

FIELD: 1989.

GRAYSON: [...]. Then you moved to Oak Ridge [Tennessee], right?

FIELD: Yeah. We went to Oak Ridge, and lived in Oak Ridge until 2004, when we came here, because I felt that as an aging man with no real relatives close at hand, this is what’s known as continuing care community. That I wanted some help. Now, Carolyn was very reluctant to leave Oak Ridge, but I think that she’s more or less reconciling herself to it.

I’ll tell you a little bit about my current history which is, that in June I was diagnosed with pancreatic cancer. That’s awful. So, I started in with the medical group at Duke Hospital—Duke Medical School. In the course of the summer, I underwent a treatment of radiation therapy along with chemotherapy. Then on the 30 August, I had a surgical procedure called the Whipple Procedure, wherein they go in and get as much of the cancer off the pancreas

as they can. The reason that pancreatic cancer is so lethal is the pancreas is nestled in there with all these other vital organs and metastasis just goes like wildfire.

But my cancer fortunately, was in such a position that I'm told that a great deal of it was removed. I've been sewn back up together. Also, in the process, portions of neighboring organs are also removed. So, my digestive system was rearranged and the exit from my stomach is put further down in my small intestine and I lost my gall bladder and so forth. So, a major, major operation which I'm still trying to recover from. I haven't completely yet. Next week, if all goes well, I'm scheduled to have another course of chemotherapy, which is no fun. But the idea is to try to kill off any cancer cells that the previous treatment didn't. I'm eighty-seven years old. I'm close to eighty-eight, so as I put it, and think about it, what we're trying to do is to keep me alive long enough so I can die of old age. We're talking in terms of, at best a few years, and few means five or less. But at eighty-seven, what can one expect?

GRAYSON: Yes, but you...

FIELD: I mean, that's a rhetorical question, I believe.

GRAYSON: But I mean you're intellectually capable of being with the program, so that's pretty valuable.

FIELD: Well, I'm better off than a hell of a lot of people my age.

GRAYSON: Oh, a lot of people a lot less than your age.

FIELD: And a lot less than my age. So, hell, I don't have anything to complain about.

GRAYSON: But by being here, you were able to get the expertise in medical care that...

FIELD: Here the way it worked, I got diagnosed incidentally by a neighbor who lives two doors down the street here, who is a retired physician. I told him some symptoms I was having, and he said, "Let me look at your eyes." He said, "You've got jaundice." He set up a course of appointments for me with his former colleagues at Duke. So, I went over there and in no time at all, I was diagnosed, sure enough, with pancreatic cancer. Did what I told you, was in the hospital for ten days after the surgery.

But then I [was transferred back] here to The Forest [at Duke]. We have a medical department, [...] there's a Health center and a Wellness center. The Wellness Center is for ambulatory people and the Health Center is for non-ambulatory people. They moved me into the Health Center and kept me there for ten days. You know physical therapy, and general care, medical care. After ten days, they considered that I was well enough to come home, and so, here I am. But, I still have to go down there every day to get the dressing on my gut changed. The people are there. There's not a physician on duty full-time, but she's there periodically, and you can make appointments with her. But there are nurses, and they're in touch [with the physicians]. [Even at home, The Forrest] feeds us. We take one meal a day with them. They clean the house, look after everything, if there's anything wrong here, just get on the telephone. There's sponsored activities and so forth. The thing that may be even better than anything else is that there are here a group of very smart pleasant people. So, it's a nice society.

GRAYSON: Yeah. That's important.

FIELD: But that's the present situation. Let's continue.

GRAYSON: Yes. So, we were working our way down through your employment experience, because I think obviously, management in industry and academia are different. Of course, in your particular situation, your industrial situation was kind of unique.

FIELD: Well, it was.

GRAYSON: The only one that compares somewhat similar is Sy [Seymour] Meyerson]. He had a pretty good arrangement there at...

FIELD: Yeah, he did at Amoco [Corporation].

GRAYSON: At Amoco, up in that part, in the...

FIELD: But he didn't get as much basic research done as I did. Most of his work was very tightly related to practical petroleum chemistry and problems.

GRAYSON: [...] Did you have any difficulty publishing stuff in industry at the time?

FIELD: No. The Humble and Esso procedure was that you wrote...if you wanted to write a paper, first it had to be submitted to the Patent Department for vetting, and you know all about this. Since I was so much involved with basic research, it was very seldom that there was any problem, even with a chemical ionization paper, because by that time we had a patent on it.

GRAYSON: Oh, okay.

FIELD: So, that went through pretty easily. It was a new sense getting the – submitting the stuff to the patent lawyers and trying to explain to them what you were talking about. But, it was not...I had no major difficulties in that regard.

GRAYSON: So, [...] obtaining the patent was fairly straightforward, so you got the idea across to them when it came to getting a patent.

FIELD: Oh, yeah. The patent went very easily, yeah.

GRAYSON: That can be a hassle sometimes you know.

FIELD: Well, it can in two directions. One is the attorneys, and the other is the patent office. By and large, I didn't have any problems. Now, I want to tell you something that's of interest, and that is I talked about the book. Of course, there were royalties on sales of the book. Humble took the very reasonable position, that since I was paid and Joe Franklin, both of us were paid for working on the book, we shouldn't get any extra [publication] royalties. So, the company setup the Field-Franklin...or Field-Franklin Award [for Outstanding Achievement in Mass Spectrometry] where the royalty money was put into an escrow trust and was given out on a year-by-year basis to Baytown High School students who excelled in science. Joe and I made the decisions [as to who received the award].

GRAYSON: Oh, wow.

FIELD: That went on for about ten years that we gave it out scholarships. Not a great deal, but enough...

GRAYSON: To encourage...

FIELD: It was a pretty good thing to do.

GRAYSON: Yeah. Well, [you acknowledged] the student and offered encouragement ...]. I was wondering about what happened to whatever royalties came from that book. But like you say since you'd already been paid for doing the work, and I don't know how much the royalty [added] up to, but book royalties can be...

FIELD: Oh, it added up to...oh, I don't know maybe five thousand dollars, something like that.

GRAYSON: [...] I think [...] all [...] along your management people had an idea that the stuff you were doing had relevance and importance. [A] lot of times I think one of the issues in modern business is that the people who are actually managing the operation don't understand the value of the work.

I think there was a period after World War II, when technical people with enough background had risen to management positions, where they could understand the technical importance of what someone below them was doing. But now, most of the management in these companies has come to business schools, and they don't really have a clue.

FIELD: They don't really know much about it. Yeah. At Humble and at Esso, the management came up through the scientific ranks. Now, of course, that means different things in different companies, with different people, but we didn't have business school graduates coming in trying to tell us what to do. They were all people who had been there.

GRAYSON: Yeah, who had had the first [hand] experience. It looks like you had worked on [very] effective teams in both your industrial and academic environment.

FIELD: Well, the thing about the academic, if we're talking about Rockefeller...I was the team at Rockefeller. It's an unusual institution. It doesn't have departments. It is organized on the basis of the laboratory. The laboratory head, which I was, is the boss, and really the boss. The way things worked out, you got a little bit of money from the university, but you raised most of it from the government. So, you raised the money. You spent it as you saw fit, on what you saw fit. You hired people that you wanted to do it.

It gave rise to some very interesting things. I had a colleague who was a photochemist, and after a while he got tired of being a photochemist. So he decided he was just going to write popular scientific monographs, which he did while he was paid by Rockefeller, and he put out a couple of very interesting books. You know, that was all right. It was independent workers.

The reason it worked was that the administration of Rockefeller of course, is the president and such like, [and they were] smart enough to pick people for whom that would work. So, it was just a remarkably interesting institution. I thoroughly enjoyed it.

GRAYSON: So, you were able to hire people there, right.

FIELD: A graduate...not graduate students, so I had one or two graduate students, but postdocs mostly.

GRAYSON: I see. There was a guy by the name of Ron [Ronald C.] Beavis, [who] I think was [there towards the end of your tenure].

FIELD: He was a postdoc who came in, [but he] was mostly Brian Chait's protégé, because Brian came to me from Ken [Kenneth] Standing in [University of] Manitoba in 1979. We worked together until I retired. Ron Beavis was a fellow worker [of Brian's] at Manitoba, about the same age as Brian. He went to the University of St. John's in Newfoundland [Canada] which didn't work out very well, so he down and worked with us at Rockefeller for a couple of years. Then he went on to NYU [New York University], and then he went somewhere else that I'm not really familiar with, some...I think he took an industrial position, maybe with Searle...I don't know.

GRAYSON: So, [...] you did have a group of people with which you did [...] research [as an academic]?

FIELD: Yeah, sure.

GRAYSON: [...] That puts you in a management position [...at Rockefeller].

FIELD: Simple as...

GRAYSON: I think we've covered most of the first page, unless there's something there that you see that you want to [cover] in particular.

FIELD: On seven, I don't really understand...I don't have anything to say about evolution with time over career.

[...]

Well, you heard what we were just talking about R&D support, you know about that. Driving forces, what can one say? Eight, well, you know about the evolution of time, and R&D support. I told you about it, and so I think we're finished with that.

GRAYSON: Yeah. Some of it's a little bit redundant.

FIELD: And product development experiences, if any. Well, we talked about publishing. We talked about patents. We used to get something like a hundred dollars or two hundred dollars for a patent, which was fair enough. Same argument as with the royalties from the book, the company wanted patents. They put in that money as an incentive to file the patent, not to do the work, but just to file the patent. So, that's what you were being rewarded for.

Scientific innovation, what does it mean to you as derived from your experience? Well, certainly, there is scientific innovation. For the most part, I think it comes from chance observations, the real scientific innovation. Now, there are the [Albert] Einsteins who can sit down and conceive of thought experiments that result in the theory of relativity, but how many are there? I mean that's...he's such a sport that, he doesn't count. But most of the time, you're working in a field and you observe something and it triggers off a thought or a concept. If you're in a position to do so, you pursue it, discover, and then develop something. That's what happened with me.

GRAYSON: So, there's a lot of what you might call [...] serendipity involved and just also being observant.

FIELD: I think so, but it's not just serendipity because you have to have placed yourself working in a field or in an area where there was a likelihood of something useful occurring. I was lucky, as I told you this morning, to get into mass spectrometry in a very early stage. So, that was for me, where the serendipity was.

GRAYSON: Yes. It's interesting, when I interviewed Fred McLafferty, you know he was at Dow [Chemical Co.]. [...] He, kind of, abandoned mass spectrometry from his perspective [...], he had done what he could with it, and didn't think that there was much more future for it, and took a position at their research laboratories on the east coast. Really just, kind of, moved away from mass spec, but then [he] came back obviously. And it's a field that many people feel is [confirming], but it never seems to be confined. It always keeps [expanding]...

FIELD: Well, mass spectrometry, I would say since 1940 has been on a roll. Now it was discovered by J. J. [Joseph John] Thomson. When was that [1897]? Something like that.

GRAYSON: A little bit later, but I mean, he was working with the instruments.

FIELD: These parabola rays, parabola rays experimented with...about the turn of the century. Then [Francis William] Aston came along. When was Aston, the first decade of the...

GRAYSON: Yeah. He was his protégé or I think he worked for Thomson for a while then took over, basically.

FIELD: As a matter of fact, he was the early 1920s, was Aston with his anode rays where he did all the molecular weight measurements. [...] But it really started in, in my opinion, with the invention of these—or the construction of—these mass spectrometers by the commercial firms. That would be Westinghouse on the one hand and CEC on the other. Of course, for a while, General Electric was in it, and Bendix was in it. But, it was easy to get a mass spectrometer. You could buy it, and it had some instructions as to how to use it. First, the petroleum industry discovered its utility. Mass spectrometry wouldn't be anywhere without the petroleum industry. Then it moved over into the biomedical business. I leave out solid state, spark mass spectrometry, because I know nothing about it.

But it turns out that being able to examine a material of interest on an atomic or molecular basis and to measure the mass of the elements of the particles that are formed, and their intensity is going to be of use in any sort of scientific activity or technical activity. I'm not going to speculate about the future of mass spectrometry, but I do think that a very large fraction of the fundamentals of it had been discovered. Now, there are people who were doing things like using mass spectrometry to study surface reactions and that sort of thing. But in my opinion, that is probably more of niche application than anything else.

If I were starting out in a scientific career now, I don't really think I'd go into mass spectrometry knowing what...if I knew what I know now, which of course I wouldn't. The real excitement in science is in biology, and mass spectrometry has made significant contributions to biology. Well, once again, you know they're pretty much made. Now it's a truism and trite at that to say well, something very exciting may come along. Well, you know you may get hit by an asteroid and the whole thing. So, that's a footless sort of speculation.

GRAYSON: It's definitely an interesting field, that's for sure. I think it draws in people from [other disciplines]...

FIELD: Well, it's because of the widespread application of it. As I say almost – well many, many scientific and technical disciplines profit from knowing the mass of the fundamental particles that are involved in it, the intensity of them. Mass spectrometry provides such information.

GRAYSON: Yes. Well, we're probably going to talk about professional networking a bit more, when we talk more about specific topics [...].

FIELD: Well, twelve, my current work and interest, I told you what they are. I'm trying to stay alive for another five years. [laughter] What is important for the future vitality of chemical R&D? Well, I think that the answer to that is the same as it was thirty-five years ago or more, when I started this racket. When was it? This is 2009.

GRAYSON: This is 2009.

FIELD: I got started in mass spectrometry in 1947.

GRAYSON: So, that's fifty [or] sixty...

FIELD: Fifty-nine plus, three is sixty-two years ago. Any sort of R&D is necessary because new things have to be discovered in order to—for societies. I won't just say companies, but societies to stay [...] competitive. It's a competitive world and world societies are competitive societies. So, I think, as we were talking at lunch, we have to do R&D to keep ahead of the Chinese and they're damn well working hard to keep ahead of us.

GRAYSON: So, I think we've talked [quite] a bit about the chemical ionization work before and how you, kind of, more or less discovered the whole process.

FIELD: Yeah.

GRAYSON: Basically you wanted to get to higher pressures in the ion source [...] because of your interest in gaseous ionization processes.

FIELD: Yes.

GRAYSON: So, what motivated you to [...] go to higher pressures? Just curiosity?

FIELD: Well, a better question would be, why was I interested in ion molecule reactions? The answer basically, was Victor [L.] Talroze, because in 1951, he came out with the startling announcement that CH_5^+ existed. Who would have thought that...this violation of a fundamental concept of the quadra-covalence of carbon? We read that [paper], “we” being Joe Franklin and I, read that [paper], and we thought, [...] that’s something we ought to get into just because it was fundamental and interesting. As I have said to you, Joe had an interest in carbonium ions because of his background in alkylation. So, we started doing very elementary ion molecule studies [...] with the Consolidated 21-620, cycloidal instrument, and also a few with the Westinghouse.

GRAYSON: Westinghouse, yeah.

FIELD: But one thing leads to another, and you want to see what’s going to happen. I was tired of trying to look at ion molecule reactions and increasing the pressure in the ion source and have the whole apparatus swamp out. So, I decided we’d get some real strong differential pumping, and see what would happen. That was a good insight.

GRAYSON: So, in order to get the [reaction] cross-section up, you needed more pressure in the instrument.

FIELD: No, not so much the cross-section, but get to higher order reactions. One of the first papers we wrote [in this area] was an ultra high pressure study of ethylene where we saw six [consecutive] of reactions, six in a row in the ion source.⁷

GRAYSON: You could see it just as a matter of increasing the pressure.

FIELD: Yeah.

⁷ F. H. Field, J. L. Franklin, and F. W. Lampe, “Reactions of gaseous ions. I. Methane and ethylene,” *Journal of the American Chemical Society* 79 (1957): 2419-29.

GRAYSON: Okay. [...] When you say you're doing ion molecule reactions [in the] CEC 620, you were creating ions of a specific kind. Then you were interacting with other compounds or with themselves [...]

FIELD: No. The way it started out, is you have a compound of interest, let's say methane. You put it in the mass spectrometer and bombard it with electrons. You don't have any choice over the ions that are formed by that electron bombardment. That is the consequence of [the nature of] the mass spectrum. But if you raise the pressure high enough, then those ions formed initially by electron impact will interact with [...] molecules of the [...] substance in the ion source. So, what Talroze did, was he raised the pressure of methane in his ion source until the methane ions reacted with methane, and abstracted a hydrogen from them to form the CH_5^+ . He could identify that by a mass [analysis]. Now, if that happens, who knows what other startling things might happen. So, that's what we were looking at.

GRAYSON: So, why was Talroze doing that? Do you have any idea? He was operating in Russia at the time, right.

FIELD: Well, yeah. He's a Russian or was, he's dead now.

GRAYSON: I was just wondering how that work was accepted originally, because [...as] you say, it's a startling observation to see CH_5^+ ion, because the tetrahedral bonding of carbon is accepted as gospel. [...] I was wondering if he had a hard time getting people to accept that result.

FIELD: I don't know. It was published in *Doklady Acad. Nauk* in Russian. I don't know anything at all about the difficulties of Russian science. At the time, he was a member of one of the Russian scientific institutions. I guess he just did it. He had a certain amount of autonomy, but I'm just guessing. I don't know the answer to your question.

GRAYSON: I'm just curious because, as you are aware, when you publish something that [...] doesn't [...] fit inside the box, people don't tend to look at it quite kindly.

FIELD: Well, he was smart though, because you know, he got this mass 17 ion. He got it [again] in a high resolution machine with high enough resolution that from the mass he was able to deduce the formula, that it was CH_5^+ , which is very convincing evidence.

GRAYSON: Yeah. So, [he] couldn't be accused of having an OH radical or something else going on in there.

FIELD: Yeah.

GRAYSON: [...]. Then of course, all this business with chemical ionization, gas phase ion chemistry and fundamentals of ionization, they're all tied together...

FIELD: Because the chemical ionization comes from the two underneath.

GRAYSON: So, what do you think is the impact of the particle desorption ionization technology on mass spectrometry in science?

FIELD: Well, it's moved into a mass range that was absolutely inconceivable when I started. We thought we were doing well to get the mass spectrum of butane, molecular weight 58. When Consolidated came out with its CEC [103], and there were several additions of the CEC down the road, they could get up to mass 300. Then when somebody discovered heated probes for introducing samples [the available mass range went up], and what particle desorption mass spectrometry did was to just move that way on down the road. So, that I think that the exciting thing about the paper at Bordeaux is [the tremendous mass range now available]. I think [Hillenkamp] mentioned getting a spectrum of a protein with molecular weight 500,000. Now, you know that's incredible.

GRAYSON: [...] It's just the idea that you can push the mass range on up into areas that previously were inaccessible.

FIELD: What was interesting about that is that then got you into the region of polymers, both inorganic polymers which was interesting of course, but even more important, about chemical or biomedical polymers like proteins which are of almost infinite importance.

GRAYSON: Yeah. About the same time, were you doing any Fourier transform.

FIELD: No, I never did any Fourier transform.

GRAYSON: Okay. So, maybe we can move onto the personal interactions.

FIELD: All right.

GRAYSON: These are just some names that I came up with, so if you have any other names or ideas, then [hop in]. We can just start at the top, John [H.] Beynon was a force in English mass spectrometry for a number of years. Did you have any interactions with John?

FIELD: Oh, yeah. I knew John very well. We exchanged Christmas cards for a long time. I suspects he's died, not quite sure.

GRAYSON: I interviewed him last year ago, last April.⁸

FIELD: Well, it would have been...if he's dead, and I don't know that he is, it would have been fairly recently. But I don't know this [O. P.] Tanner at the bottom of your list.

GRAYSON: That's O. P. Tanner. [...] I got involved in mass spectrometry through O. P. Tanner. He was at Monsanto. He used to work for Lion Oil [...]. Monsanto bought Lion Oil and [...] he had a CEC 103 [that he moved] up to St. Louis. [...] At the time, I was in undergraduate and wanted to do something interesting. So, I got on as kind of like a co-op type student, where you work some, etc, etc. We actually used the low voltage ionization technique quite a bit in his lab.

FIELD: Good.

GRAYSON: On his 103. So, he also introduced me to my first ASTM [American Society for Testing and Materials]...

FIELD: Say again.

GRAYSON: He introduced me to my first ASTM Meeting, Dallas 1966, I think it was.

⁸ John H. Beynon, interview by Michael A. Grayson at Swansea, Wales, United Kingdom, 22 April 2008 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0420).

FIELD: Yeah, that was great.

GRAYSON: Those were crazy days [...]. What about Klaus Biemann? Did you have much interaction with Klaus?

FIELD: Well, I knew Klaus. We were never close friends. He was rather a reserved man, got along well, with his students. His students were very fond of him, and very, very loyal, and very numerous and very numerous and very productive. What can you say about Klaus Biemann, one of the giants?

I will tell you this, a name here that might be added, and that's John [D.] Waldron, who worked for Metropolitan-Vickers in England and was pretty important in mass spectrometry in its very early days. While I've spoken of Joe Franklin, do you need anything about Joe?

GRAYSON: I think you've given me a pretty good insight into Joe, and apparently, he had a lot of influence at Humble to promote the kind of intellectual atmosphere that you experienced there.

FIELD: Very much so, yes.

GRAYSON: It's to his credit that he was able to do that. I mean even when times were not as focused as they are today, it still would be quite a challenge.

FIELD: No, he did very well.

GRAYSON: He did go to Rice [University] eventually, Rice University, right.

FIELD: He became a Welch Professor, the first Welch Professor at Rice University.

GRAYSON: Yeah. Welch [was] a big oil baron or made a lot of money in the oil business in Texas, [didn't he...]?

FIELD: No. He was not in oil.

GRAYSON: Oh, okay.

FIELD: I know...but it's disappearing to the back of my head.

GRAYSON: Well, I'll try and get a little bit more information. I think that's great. So, that's [an endowed Chair at Rice University].

FIELD: Yeah, it's a Chair at Rice.

GRAYSON: Yeah. So, and [...] that was...the remainder of his career was at...

FIELD: At Rice.

GRAYSON: ...at Rice. That was [in] the Chemistry Department?

FIELD: And eventually he retired and then died old. About 1980, as a matter of fact. Been dead a long time.

GRAYSON: Now, was he significantly older than you were do you think?

FIELD: About fifteen years [...].

GRAYSON: [Henry] Earl Lumpkin?

FIELD: Earl Lumpkin is interesting in that he's an east Texas boy. He went to East Texas University or something like that, maybe it...was it East Texas or Central Texas [Southwest Texas State University]? It doesn't matter, a Texas boy, got a Bachelor's Degree, and went to work for Humble. Was working not as a technician, but not very much higher than a technician, and he was an analytical chemist. He was working as an analytic chemist running the CEC. The way he got into the research business was through Joe Franklin. Because Joe, as I told you, was interested in carbonium ions and found out that the information about the carbonium ions was in the gas phase. So, he set Earl Lumpkin to work trying to make some measurements on the energies of carbonium ions. Well, at that time, I was doing the same thing at the University

of Texas on this Humble machine, the one that Humble had given the University of Texas. So, Joe sent Earl up in effect to pick my brains.

GRAYSON: Oh, okay.

FIELD: Which [he] did, I mean I gave him such information as I could. He and Joe produced one or maybe two papers on ion chemistry.⁹ Then I came on the scene at Baytown. Earl went back into analytical mass spectrometry and really made his reputation as an analytical mass spectrometrists. One of the things that he did was...a fellow worker at Humble named Sam Hastings and I developed a low voltage ionization mass spectrometry, and Earl picked it up and applied it to the Consolidated instrument, and started using it for very practical purposes. That's Earl.

GRAYSON: Then of course, Burnaby would be a little bit younger than you are, I guess.

FIELD: Yeah. He's younger than I am. He was born in...he's about ten or eleven years younger than I am. I think he was born in 1933. I was born in 1922.

GRAYSON: Okay. So, mostly you know him through interaction at Humble before you went on to New Jersey.

FIELD: Yeah. Well, that was our interaction was at Humble, before I went onto New Jersey, and he went on to Delaware. He's a smart guy, productive, and he's resisting retiring. I think he ought to. But he's a single man, and he has made himself into the Mr. Chips of the University of Delaware. He's a personable sort of guy in a peculiar kind of way. He has made his students at the University of Delaware his life. Henry [M.] Rosenstock was a ...can I say anything more about Burnaby?

GRAYSON: Well, you can say...yeah, sure. You have anything...if you have anything else to say, then you know this is...

FIELD: Well, Burnaby went on and got the Distinguished Professor Award at the University of Delaware for his work with students, and such like. As I say, he was a very interested in that and very good at it. He made some contributions after he went to Delaware, but there were, in

⁹ J. L. Franklin and H. E. Lumpkin, "Lack of resonance energy in gaseous carbonium ions," *Journal of Chemical Physics* 19 (1951): 1073-4.

my opinion, with all due respect to Burnaby, it wasn't really great, because his interest really was in the students.

GRAYSON: So, his interest in mass spec was not as intense in doing research and...

FIELD: Oh, intense enough...well, I can't say more than I've said. It was intense, but his interest in his students, I think was greater.

GRAYSON: Okay. This is probably good for someone at a school like Delaware [...].

FIELD: Yeah. Rosenstock was a student of Henry Eyring at the University of Utah. Rosenstock...it's the third guy, and then Eyring, wrote the paper on quasi equilibrium theory of mass spectra which came out in 1951 or 1952, I forget which year the original report came out.¹⁰ In which Eyring applied rate process theory to the decomposition of gaseous ions. Up until that time, we had all of these spectra. The NBS [National Bureau of Standards], started publishing spectra in 1947. Number One NBS spectrum [appeared in] 1947. It was of methane. But nobody understood the spectra, how they were formed, or why or what the considerations were. Eyring with the help of his students Rosenstock and [Austin L.] Wahrhaftig and Merle [B. Wallenstein] and there's another one, but I've forgotten.

GRAYSON: I can get the reference.

FIELD: Yeah. I think it was it was in the *Proceedings of the National Academy of Science* [where they published] this rather abstruse theory, and explained [the spectra of polyatomic molecules]. Henry then went on to...when he graduated from Utah, went to the Bureau of Standards in Washington. He spent the rest of his career there at the Bureau of Standards doing one or another, mostly in mass spectrometry until he died at a relatively early age on a scientific meeting in, I think, it was Lisbon, Portugal. He had a heart attack and just conked out.

GRAYSON: Any other names that you want to pull up from the memory of that era of troops?

FIELD: Well, I told you about one, John Waldron, who I knew, but never very well. The ocean was pretty wide at that time.

¹⁰ H. M. Rosenstock, M. B. Wallenstein, A. L. Wahrhaftig, and Henry Eyring, "Absolute Rate Theory for Isolated Systems and the Mass Spectra of Polyatomic Molecules," *Proceedings of the National Academy of Science* 38 (1952): 667.

GRAYSON: Oh, yeah.

FIELD: That's enough.

GRAYSON: Okay. Like I say, if there's any that do come to mind.

FIELD: Yeah, okay.

GRAYSON: So, we have this whole business with ASTM, [Committee] E14, and ASMS [American Society for Mass Spectrometry] converting it to...

FIELD: Okay. You want...where are you?

GRAYSON: Oh, I...

FIELD: The next page.

GRAYSON: No. I'm sorry. I cheated a little bit. Did I send you the upgraded...

FIELD: I don't know.

GRAYSON: There's this one issue here, one topic here I added. I remember that you were involved in ASMS business, the society business for a while there.

FIELD: Oh, I have this ASMS activities on my list.

GRAYSON: [...] I got involved in [...] about 1966 with mass spec society. I remember there was a move afoot to go away from being associated, affiliated with ASTM and to set up an independent scientific society [...].

FIELD: [...] Joe Franklin was very important in that. In fact, my recollection, although I'm not privy to all of the details, but my recollection is that the establishment of ASMS was Joe Franklin's idea. Now, I think he had collaborated with...Rosenstock may have been involved in that. But that's just a very vague notion on my part. But, as you say, I think that ASMS...no, wrong. ASTM first got interested in mass spectrometry in...I'm going to say 1950 [...] and it set up a committee. That's the way the ASTM is organized in terms of committees. There was Committee E14 on mass spectrometry. The first meeting of that committee, if I recall correctly was in New Orleans in 1951. Earl went to it—Earl Lumpkin. I don't know if anybody else did. I did not. They went on for a couple of years.

I got to go to my first one in 1954, also at New Orleans [Louisiana]. Then it went on, as such, until the early 1960s, when it was quite clear that this was a case of the tail wagging the dog. That is, the E14 [committee] got to be too big for ASTM, which has a very specific limited focus. As I say, Joe was very influential in seeing that, and helped very much in getting the ASTM started. Now, my activity in it was that I went to the meeting in 1954, and then skipped a couple, and then went in 1958. Joe Franklin became Program Chairman and then...what is it called, President, I guess. Sometime after that, I became Program Chairman. Of course, you know there was an automatic sequence. You were Program Chairman one year, and then, President the next couple of years or something like that. But, somewhere around 1965, I'm going to say, I was elected Program Chairman and went on to become President. I think I was President about 1970 or 1971, a two-year term, but somewhere in there. It was when first I went to Rockefeller, so it was in the early 1970s, when I got actively involved in the ASTM administration. Of course, up until then, I'd been involved in submitting papers for the meetings and got to be known through that activity.

GRAYSON: So, that meeting back then was probably five [or] six hundred people, do you think or at the most or three [or] four hundred?

FIELD: Started out at maybe a hundred.

GRAYSON: Maybe a hundred, yeah.

FIELD: In the first, when I went to in New Orleans in 1954, I think they had three hundred fifty people, and that was considered to be a big meeting.

GRAYSON: Yeah.

FIELD: So, the growth of mass spectrometry in the period we're talking about was absolutely phenomenal, which is the consequence of the fact of the utility of it.

GRAYSON: So, right now, I think that the last meeting attendance is running right at six thousand...

FIELD: Really?

GRAYSON: ...for the attendance, yeah. The biologists have moved in.

FIELD: Wow.

GRAYSON: I go to present a historical perspective poster. [...] I'm not doing science anymore, not that I ever did that much science. But [...] I've [...] worked out an arrangement with ASM as that if the Board agrees and they have a topic that they think is interesting, then I put together a nice poster to try and get some of the new people coming in to realize two things. One is it's been around for a while, and the other is in some cases they're reinventing the wheel. You know, one of the things that Barbara [S.] Larsen has done, she was a past President of ASMS, she works at Dupont [E. I. du Pont de Nemours and Company], has arranged for all the past bound volumes to be digitized and put on computers [...] so, that you can access them online.

FIELD: Good.

GRAYSON: [...] That just happened this last year, and I said we should require that whoever submits a paper, research all those volumes to find out who did [what] before...before they submit a paper. Because so much of this stuff...it's amazing, looking back at those bound volumes in the early years. It's tremendously amazing how much fundamental good work was done back then.

FIELD: Well, that's why the society grew to have a six thousand attendance annual meeting.

GRAYSON: [...Now] we have to meet at cities that have a convention center venue, because with that many people, there's no way [to fit into a hotel]. The number of sponsors, corporate sponsors is up to [...] a hundred or whatever. Basically, they're using that as a funding mechanism, as well, because the corporate sponsors pay a considerably larger fee.

FIELD: Good deal of money.

GRAYSON: The membership fee is still below a hundred bucks. It's seventy-five bucks or sixty-five bucks, I guess [for] students [...]. And you still get the journal, so they're doing a good job of making it very affordable.

FIELD: Like gangbusters.

GRAYSON: So, [...] did you have any particular [...] controversies going on during your presidency of ASMS or was it pretty much just keeping all the troops in line.

FIELD: No, I didn't have any controversies. I don't know if there were any controversies. Not while I was familiar with the situation.

GRAYSON: Yeah, okay. Usually, somebody always has an issue, but whether it boils up to something big enough to be a controversy is you know...

FIELD: Not to my knowledge.

GRAYSON: So, important publications. I think you point out the book is definitely important.

FIELD: The book is definitely important. The low voltage ionization is definitely important, although not as well known. The chemical ionization is important, and some of the early papers on Californium fission fragment are important.

GRAYSON: I know [that] was an important development, but how many other people were doing it besides you and McFarlane? Do you have any ideas, if there were many?

FIELD: What, the Californium?

GRAYSON: Yeah, Californium.

FIELD: Well, I was the second. I guess other people that came in, but it never was really very widely used.

GRAYSON: Well, once MALDI was made available, it precluded the...

FIELD: Well, the Californium impact procedure was in competition with what was called “fast atom bombardment,” where an ion was formed in a mass spectrometer or in an ion source and allowed to undergo an interaction. It was accelerated and allowed to undergo an interaction with a residual gas, where it picked up the electron and became a neutral particle. The bombardment was with the neutral particle. There was quite a splurge in that a lot of people got involved in it. There was some controversies about that. The [person] I associated with fast atom bombardment is a guy named Micky [Michael] Barber, who was English. I tried a little bit of it, but I found it to be a rather tricky procedure, because you had to do this charge exchange business. I always found the fission fragment procedure to be more straightforward, but it required the time-of-flight. As I recall, didn't they do fast atom bombardment with sector instruments.

GRAYSON: Yes.

FIELD: Also, it didn't involve handling Californium-252, which scared lots of people because it's pretty vicious stuff.

GRAYSON: Oh, really.

FIELD: But both of them were swept away by the invention of the matrix desorption mass spectrometry, [for] which incidentally, Brian Chait figured—and Ron Beavis—figured out the mechanism for it. If I say so myself, I had an idea that I just turned over in my mind to explain what was going on. I called it the Field Mashie-Niblick theory of desorption mass spectrometry. I thought to myself, this situation is analogous to blasting a golf ball out of a sand trap. You take a mashie or a niblick and you hit at the ball. You don't hit the ball. You hit the sand underneath the ball, and the sand lifts the ball out. If you hit the ball, with the edge of a mashie-niblick you cut it, I guess. I mean, it seems reasonable. So, the idea is for the environment to provide the mode of power to get this heavy object up in the air. I thought something like that must be happening, which of course, it was or is.

GRAYSON: So [...] I'm not familiar with those terms.

FIELD: You're not a golfer.

GRAYSON: I'm not, no. I never swung a club.

FIELD: There are about nine irons in a set of clubs. They progress in the pitch of the head. The mashie and the niblick are way up at the eight and nine. They've got a very large angle, so that you come along, and you scoop underneath the ball, and it goes up in the air. It doesn't fly very far, but it goes high, because you want to clear obstacles and that sort of thing.

GRAYSON: Oh, okay.

FIELD: One is mashie, and the other is niblick.

GRAYSON: So, it's a golf analogy of MALDI desorption.

FIELD: Yes.

GRAYSON: [...] Field[...] - mashie-niblick [...].

FIELD: Pardon? Nobody knows about this except Brian Chait and a few other people at Rockefeller with whom I discussed it. I mean, it wasn't a well enough established hypothesis even to be talked about in any sort of public literature.

GRAYSON: Well, I did something you might find amusing, maybe not. This is a bar graph showing your productivity in number of papers published as a function of year.

FIELD: Well, it's sort of a jagged profile, isn't it?

GRAYSON: But obviously, you had some years better than others. There's some years when you didn't have any publications early on [...].

FIELD: Yeah, it figures.

GRAYSON: You were, kind of, getting organized. Then it looks like after the high period there, they kind of tailed off slowly, although there were a couple of papers.

FIELD: Well, here in 1989, I retired.

GRAYSON: Okay.

FIELD: I don't know what that fellow doing out there is.

GRAYSON: I think it's a reprint of an earlier important paper.

FIELD: Okay, but that's trivial, really not meaningful. Well, that's the way we saved trees you know. Have a good year and...

GRAYSON: [...]. It's interesting though, when you look at this publication record for some of the people that were involved in the Manhattan Project. There's a little bump down here, and then there's this quiet period for [...] the mid 1940s to late 1947, 1948 and...

FIELD: Then it suddenly jumps up.

GRAYSON: Because just about everything they were doing was classified, and they couldn't be touched. I don't know, if you have any other topics that you want to cover at this point in time. We've got [...your thoughts on] your important publications. I don't know if you want to submit yourself to videotape recording or not. That's really your choice.

FIELD: I leave it to you, Mike.

GRAYSON: Well, I think it would be important for people in the future to be able to look and see a person who's made a significant contribution to the field speaking to them. The idea of these questions, if you look at them, they're [...] less so focused on the oral history [...], but more on trying to give people...

FIELD: Well, as you said philosophical topics.

GRAYSON: Yeah. So, I think that that would be a good thing to have. I don't see the value of doing a videotape of the oral history interview, because I don't think that's particularly [useful]. This would [involve] spending [...] maybe fifteen or twenty minutes [...] depending on how things go, just talking about these topics in general, and your attitude towards them. [...] Some of them would be a little bit redundant [to] what we've done, but it'd more [...] speaking to someone [...in] the future so to speak.

FIELD: You decide.

GRAYSON: I would like to do it, if it's okay. We could maybe do it in the morning, tomorrow [...] ten o'clock, say?

FIELD: I suppose so. Sure.

GRAYSON: [...] You'd be fresh, and [the time] gives you a chance to think about those a little bit more, if you want. [...] Hopefully, it'll be nice and bright, sunny. I think it's good to use sunlight [...], natural light for lighting. So, we can set up maybe in the dining room [...]. It depends. Spend a little bit of time sorting [things] out, and making sure you look good. All right, and the other thing is [...] if any other topics come to mind, in the meantime that you [...] would like to [talk about].

FIELD: Well, what comes to mind, Mike, is that I've done an awful lot of talking in the last four or five hours. I think I've said pretty much all I have to say about anything. [laughter]

GRAYSON: Okay. Well, in that case then, I guess we push the off button.

FIELD: I can see that.

GRAYSON: Consider that as a concluded or oral history interview.

[END OF AUDIO, FILE 1.2]

[END OF INTERVIEW]

INTERVIEWEE: Frank H. Field
INTERVIEWER: Michael A. Grayson
LOCATION: Durham, North Carolina
DATE: 10 December 2009

GRAYSON: Recording. So, we're talking about the future of mass spectrometry and you were saying about the future.

FIELD: Well, I was saying that I think it has a future because it has fundamental information, fundamental in the sense that it's information of importance in the real understanding of physical and chemical phenomena. It will improve [...]

GRAYSON: So, you don't think too much of [R.] Graham Cooks' idea about a mass spectrometer for [...] every person.

FIELD: No, I don't. I don't care how simple the mass spectrometer, it's still going to be very complicated piece of apparatus. There's a whole group of people who can't even work computers, and a mass spectrometer's a whole lot more complicated than a computer.

GRAYSON: But you have to admit, Graham [is] a very creative fellow [...].

FIELD: No, I told him...I don't admit it. I proclaim that he's...when you say, "admitted," the implication is that I'm opposed to the notion—I have an issue. I'm not. I think Graham's a genius.

GRAYSON: Yes, lovely fellow [...].

FIELD: And Brian is South African too.

GRAYSON: Oh, I didn't know that.

FIELD: So, South Africa has done a very good job of turning out some important scientists.

GRAYSON: Yes, indeed. Graham's a very interesting fellow. I guess he's really responsible for that Purdue program, keeping it going, the analytical chemistry program there because he's a major force in it.

FIELD: Well, I guess I'm not that familiar with it.

GRAYSON: Well [...] most academic chemistry departments don't have much emphasis on analytical chemistry. It's almost, all synthetic chemistry and all these other [chemical disciplines]. So, there're only a few schools in the country that have a strong analytical chemistry [program]. Graham's presence at Purdue is, I think it's one of the few schools that have that strong emphasis. It seems as though analytical chemistry is looked on as kind of a [...] second rate...[discipline]. A person getting into that usually [is] not likely to get [...] tenure track positions when they go into the academic environment.

FIELD: Well, I think that's right. I think chemistry used to be a four-legged stool. That is there was physical chemistry, organic chemistry, inorganic chemistry, and analytical chemistry. That's the way it was when I was in college. Of the four, analytical, has always been somewhat inferior to the other three in interest, at any rate. One can argue whether it has been inferior to the other three in importance. Because if you don't know what you have, you don't know what you're doing and back to mass spectrometry again. But it doesn't have the glamour of the other three.

GRAYSON: It's, kind of, treated as a service.

FIELD: Yes.

GRAYSON: If you do analytical chemistry [...] you have a service function.

FIELD: It's a service function. There is research in analytical chemistry. One can discover a new technique for quantitating a given material.

GRAYSON: A new technique for ionization.

FIELD: A new technique for ionization. But the main utility of it is the information it gives about the samples that are fed into it. It's the same way nowadays. We had at Rockefeller an NIH Biotechnology Research Resource. One of the requirements was that you did service work, and so we did service work. But you got a lot less credit for the service work you did than for the paper that you published. And nobody ever got tenure in a major university doing service work.

GRAYSON: [...]. Well, I don't know if you have any other words of wisdom about mass spectrometry. [...].

FIELD: I'm not sure I've given you any words of wisdom up till now. [laughter]

GRAYSON: No, no, no. [...].

FIELD: Well, we hope for the best.

GRAYSON: Yes [...]. So, actually there [were] a couple of things that occurred to me that I [...] missed yesterday and that is, [...] you didn't tell me your father's name or the your mother's name [...].

FIELD: Well, my father's name was Field. And his name was—full name was Frank Aretas Field. Although, there was a family argument about that. But let's leave it at that. My mother's name was Mary Louise Fleischmann. A good German name. But the Fields showed up on this continent in 1629, so they were here all along.

GRAYSON: Oh my, okay.

FIELD: And the Fleischmanns came as, I think mid-nineteenth century immigrants.

GRAYSON: So, is Field an English [name]?

FIELD: English, yes. Yes.

GRAYSON: Okay.

FIELD: Well, they're my parents. They were...my father, and the Fields are a Massachusetts family. Somehow or other my father immigrated down to the New York City area, and met my mother. The Fleischmanns lived in New York City [...]. They got together and produced me. Sort of, peculiar. They were married for I think twenty years or so, before I came along. Then my father died and so...no other offspring.

GRAYSON: Last night you mentioned that when you were born...your father was how old?

FIELD: Seventy-one. Let's see, he was born in 1851, and I was born in 1922, so that's seventy-one.

GRAYSON: Wow. This is after they'd been married for quite a while.

FIELD: Oh, I have in mind ten or fifteen years, something like that.

GRAYSON: Interesting. [...] I think that's probably it. I think we're pretty good, Frank. So, it is Frank. It's not Francis?

FIELD: No, just Frank.

GRAYSON: Frank, okay. Sometimes...I know it's Joe Franklin...he went by the name Joe, not Joseph, is that correct? I put Joseph Franklin, I think for something, and someone changed it to Joe Franklin.

FIELD: I don't know, Mike. But I would just go with Joe.

GRAYSON: Joe, okay.

FIELD: It's conceivable that he was Joseph. But he never went by Joseph. He was always Joe L. Franklin.

GRAYSON: The ones that get me are those who have chosen, particularly, the English do this, their second name [to] go [by]: like R. Graham Cooks and W. Alan Wohlstenholme [...]. So, many of the English people I know give up their first name [...].

FIELD: Suppose the first name was Algernon. Give it up.

GRAYSON: I guess so, but it's sometimes a little messy when you're doing literature searches and you're looking for somebody, and all of a sudden, you discover, well, his real name is his first initial, and then he goes by his second name. It makes [...] research a lot more complicated. [...].

[END OF AUDIO, FILE 2.1]

[END OF INTERVIEW]

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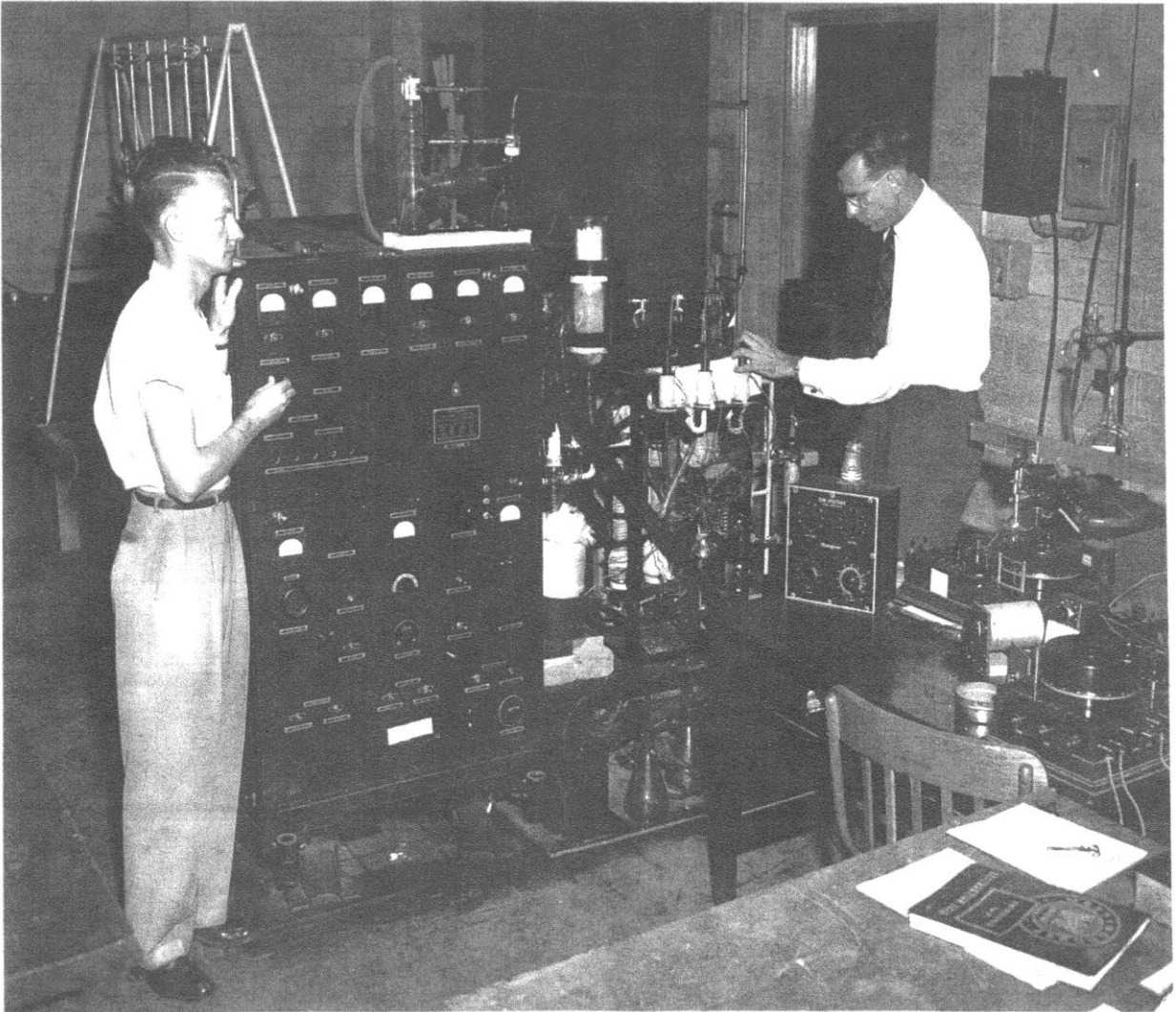
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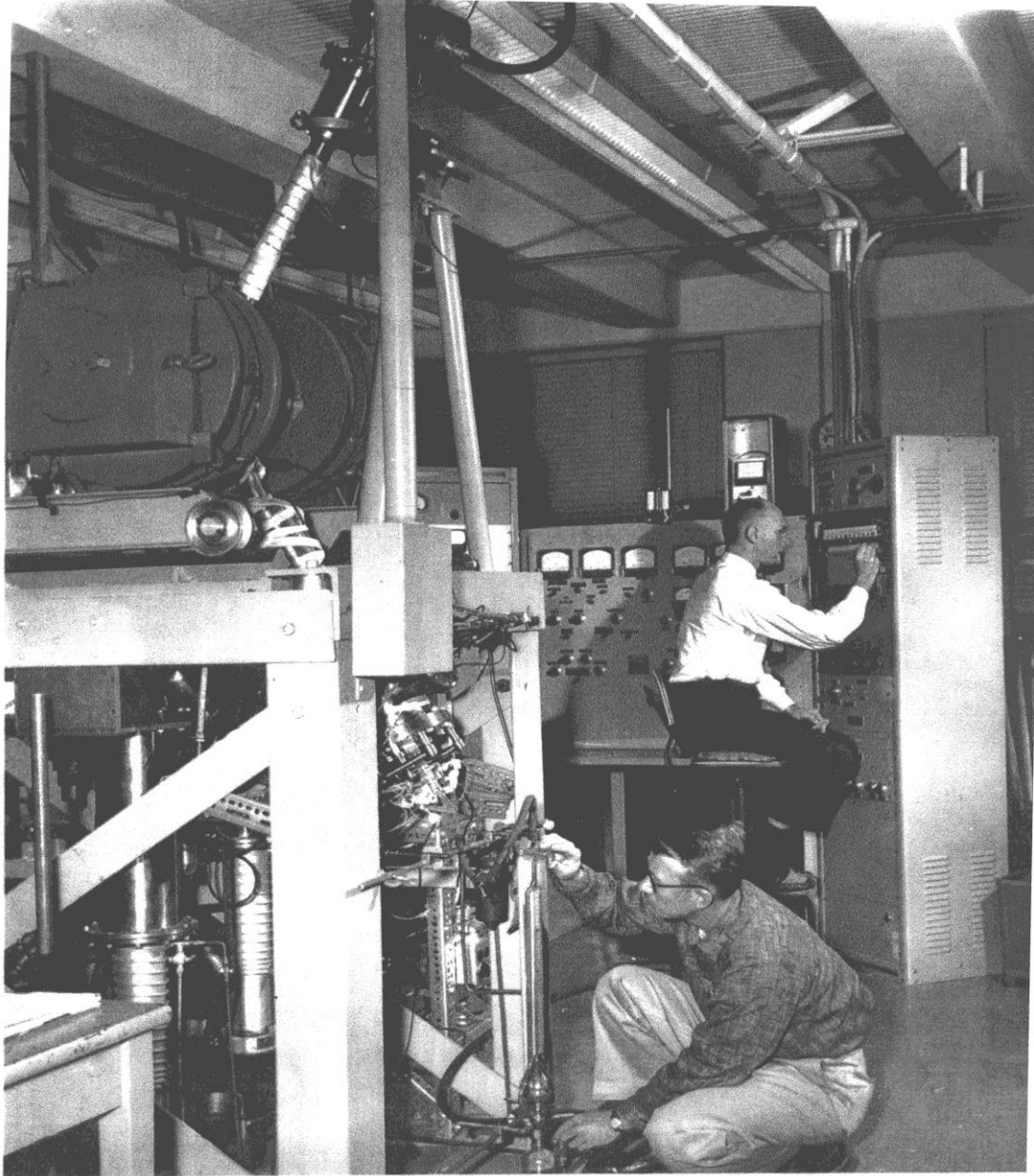
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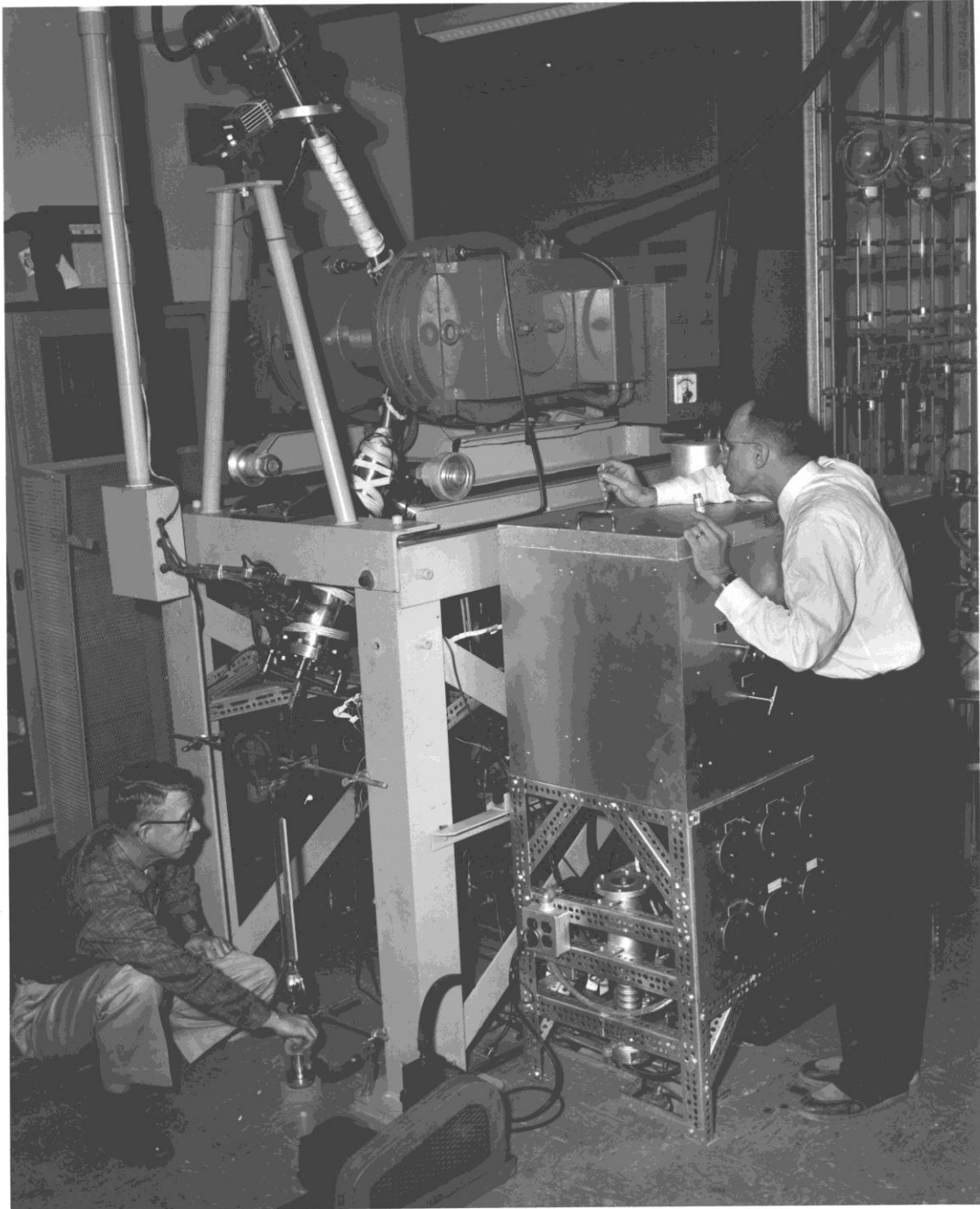
APPENDIX



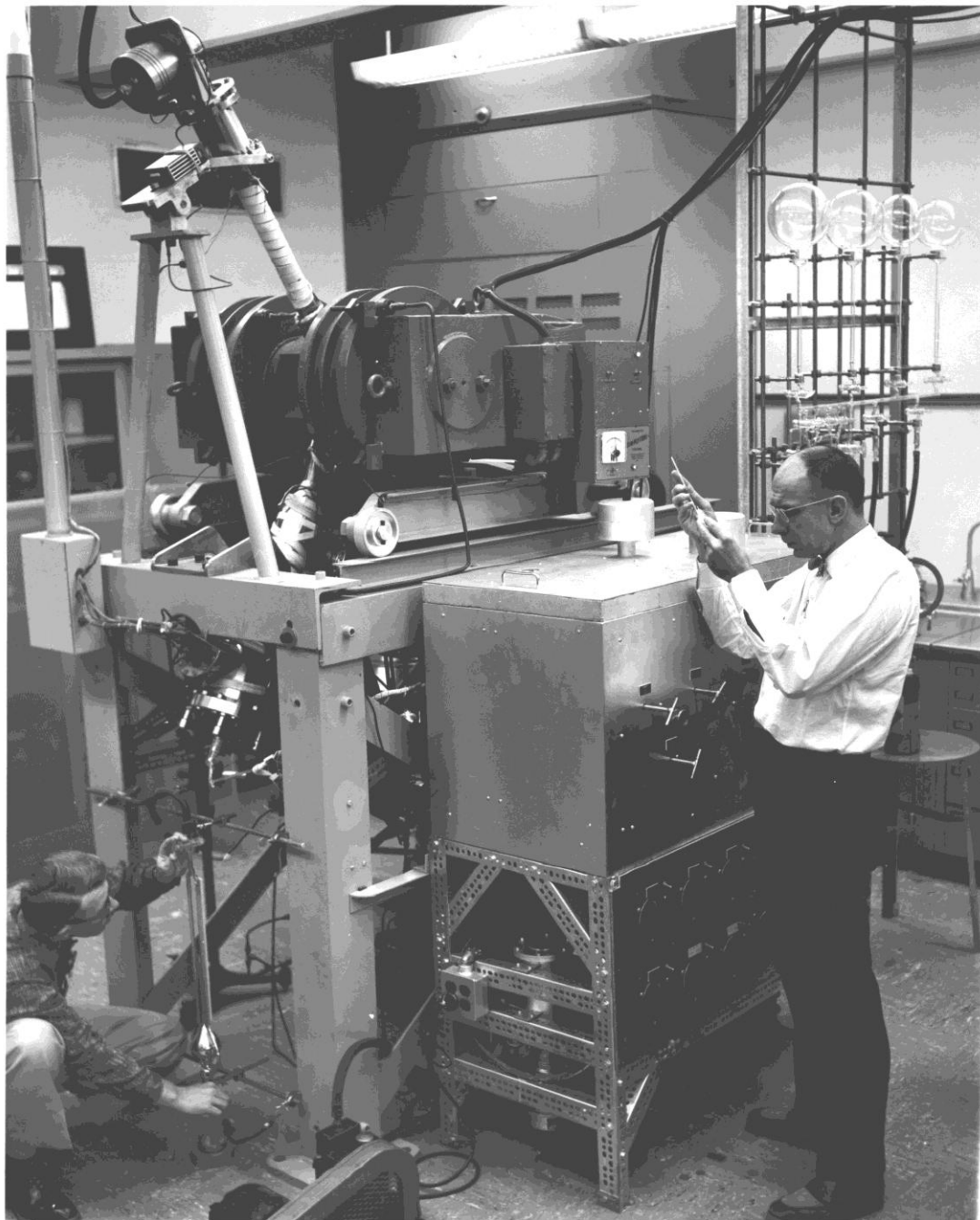
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