CHEMICAL HERITAGE FOUNDATION

SEYMOUR MEYERSON

Transcript of an Interview
Conducted by

Michael A. Grayson

at

Gary, Indiana

on

7 March 1991

(With Subsequent Corrections and Additions)

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

SEYMOUR MEYERSON

1916	Born in Chicago, Illinois

Education

1938	S.B., University of Chicago, Chemistry

Professional Experience

	Deavitt Laboratories, Chicago, Illinois
1941-1942	Chemist
1942	Army Ordnance, Kankakee Ordnance Works, Joliet, Illinois
	United States Army
1943-1946	Army Signal Corps
1945	Army Corps of Engineers, Manhattan Engineer District
	Standard Oil Company (Indiana)/Amoco Corporation
1946-1951	Chemist
1951-1954	Assistant Project Chemist
1954-1957	Project Chemist
1958-1960	Senior Project Chemist
1960-1962	Senior Research Scientist
1962-1972	Research Associate
1972-1980	Senior Research Associate
1980-1984	Research Consultant

ABSTRACT

Seymour Meyerson was born and raised in Chicago, Illinois and attended the University of Chicago from which he received his undergraduate degree. Unsure of what he wanted to pursue as a career, Meyerson decided to take additional courses in a variety of disciplines from the University of Chicago, as well as from George Williams College, which he attended for one year. After a brief time in a Chicago laboratory, he began working as a civilian inspector for the U.S. military. By 1943 Meyerson began active service with the U.S. military, spending the majority of his time with the Signal Corps; he also performed important work as the technical liaison officer between the Manhattan District and Standard Oil Company (Indiana).

Though he had studied chemistry as an undergraduate at the University of Chicago, Meyerson's wartime work removed him from a laboratory setting for many years. His time with the military, however, afforded him the opportunity to be trained in electronics, to encounter his first mass spectrometer, although simply as a black box, and also to make important contacts with Standard Oil Company (Indiana). In 1946 Meyerson started what would become a nearly forty year career with Standard Oil Company (Indiana) (later the Amoco Corporation). From the outset, Meyerson was involved with the mass spectrometry group, first conducting quantitative gas analysis on gases and low-boiling liquids, consisting of hydrocarbons and fixed gases. His extensive career gave him the ability to witness the development of mass spectrometry at Standard Oil Company (Indiana) and the movement of his laboratory to newer buildings with more space and newer instrumentation, as well as the transition from human calculators to early analog computers.

Much of Meyerson's research was conducted in collaboration with others in his company and in the larger mass spectrometry community. He and his colleagues were able to make advances in mass spectrometry techniques because Standard Oil Company (Indiana) supported basic research and there was a commitment of corporate management to the larger scientific community. Throughout his oral history Meyerson detailed the instrumentation with which he worked, mainly from Consolidated Engineering Corporation. Additionally, Meyerson discussed the early history of mass spectrometry as a discipline and as a community.

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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INTERVIEWEE: **Seymour Meyerson**

INTERVIEWER: **Michael A. Grayson**

LOCATION: **Gary, Indiana**

DATE: **7 March 1991**

MEYERSON: It's going to take a lot of digging through my own memory to even try to reconstruct a lot of this stuff.

GRAYSON: Well, I think that the issue that we're trying to get to...this is for the meeting in Nashville [Tennessee], the agenda, and it's an attempt to give people a framework within which they can think about, that activity for the national meeting. And, as we discussed when I first mentioned it to you, you'd want to have some kind of an agenda, rather than just to sit around and shoot the breeze. And so, that's really...

Your kind of input forced us to think about creating the agenda, so that's really the product of your comments when we talked last month. Mostly what we're trying to do is get the feeling of the people in the environments that they dealt in and interactions they had and those types of things.

MEYERSON: The other thing that struck me here is that, of the five people here, three of them all come from Humble Oil. They've heavily dominated the group, they are the majority.

GRAYSON: Yes, that's correct. That point was also mentioned by Al [Alfred L.] Yergey.

MEYERSON: They are very different people.

GRAYSON: Sure.

MEYERSON: And they all have different recollections. And, of course, Frank Field, on the one hand, and Tom and Earle, on the other, were in very different kinds of business.

I still have a vivid recollection of a conversation, over several beers one evening with Frank Field (it must have been about 1959) at a mass spec conference in Los Angeles [California]. At that meeting, a guy named Gene Levy, whom I haven't seen for a long time now, who was at that time with Atlantic Refining in Philadelphia [Pennsylvania], had expressed such pleasure (a real victory to us all) that he had, just before coming to the conference, gotten word that his management had agreed to do something that he'd been pushing for a long time.

He had come to feel that the demands of service work, of analytical service, on him and his laboratory were so considerable that they really didn't leave him any time or energy or instrument time to do any kind of exploratory work he would really like to do. And, apparently, he had petitioned his management to separate the mass spec operation into two separate groups; one of them for service and the other for what at that time was commonly fashionable to refer to as chemical physics. And he'd just gotten word that they had agreed and he was going to soon be set up with his chemical physics laboratory. He was delighted. Frank and I were talking about that, that evening.

My reaction was that, that doesn't jibe with my notion of how I'd like to see such an operation work. There in my laboratory, all the years that I was around, I was most comfortable using the same people and the same instruments for service and for exploratory work. And it seemed to me that there was a complete feedback loop there, that each of them contributed a great deal. So many of the most fun kinds of problems to work with came out of real analytical work. And then the time came when it happened frequently that I was able to be of some real solid help to somebody with analytical problems, because I had been doing my homework in the exploratory end of things for years before that. So that that loop arrangement is something that made eminently good sense to me.

On the other hand, Frank was thoroughly in agreement with Gene. And he said that, as far as he's concerned, I mean there at Humble, all the time that he had been there, they had two very separate establishments. The one that he and Joe Franklin were in, that's the chemical physics end of things, and the analytical thing which was Earle Lumpkin, and later Tom [Thomas] Aczel came into that. And he said, as far as he's concerned, there's nothing that has ever happened in analytical there that's of the slightest interest to him. And with the single exception of low-voltage electron impact, he didn't think there was anything that had ever happened in their laboratory that was of any interest to Earle Lumpkin. Now, Earle might put it very differently, and my guess is that he would. But I remember I was really taken aback by that kind of attitude. It was so at odds with my understanding of how things work and how people function.

GRAYSON: That probably relates to the fact that some people want to maybe just do pie in the sky stuff. Or to feel not exactly irresponsible, but not responsible for real world problems. That's an issue that comes up in our own research laboratory, where people are concerned about doing research that they can publish, and they're not as closely concerned with how it may affect the company's products or things that the company is working on.

And so it's a dichotomy and approach to your work that exists. And it's not only in the case you mentioned there with Frank and that situation. I think it's fundamental to all research laboratories throughout the world. There's a subset of people who would rather not be bothered with real world problems, and would just as soon...

MEYERSON: I remember quite some years ago, over a long period of time there seemed to be a controversy that continued to rage, and we're reminded of it frequently by letters to the editor of *C&EN* [*Chemical & Engineering News*], for example, and other places about how one defines basic research. And of all the various and assorted criteria that were offered, the only one that made good sense to me was that if you are trying to gain new understanding of a phenomenon, that makes it basic research. And that doesn't mean that it has to be separated from practical applied problems. It could be just as basic and have a useful application as not. That's irrelevant to whether it does or does not qualify as basic research.

GRAYSON: Well, some people feel that... An impression that I get is that if they don't have control of their work, if they can't direct it... In other words, they feel almost as if... If someone comes in with a problem and then they have to work on that problem or address, use their talents on that problem, they feel then that they don't have control of the work that they're doing. And consequently, it's not research, even though it may be. They may, as you pointed out, they would have to use all the knowledge and background and information that they've learned through all of their experience to solve the problem. And they may even learn something new in solving the problem. It's not research because they didn't choose the problem; the problem was presented to them. And this is the real issue in our laboratory right now where I'm working. And I think it's an interesting commentary that you brought up. This happened in 1959?

MEYERSON: Yes.

GRAYSON: And I suspect that it's going to happen twenty years from now and thirty years from now for people who are working in this environment.

MEYERSON: Incidentally, I should add that I haven't heard anything from or of Gene Levy for years now. I don't know what's happened to him, but I would be pretty sure that he has not been working with mass spectrometry, whether he calls it chemical physics or anything else. It's been a long time now. And it wasn't too many years after that, that Humble started to dismantle that whole establishment where Frank Field and Joe Franklin and a number of other people were doing work that was pretty much separate from anything applied. They just gradually, over a period of some years, dismantled that whole thing. And at least the people that I knew who had been working there, all, one after the other, moved into the academic world, where they seemed to feel much more comfortable.

GRAYSON: Well, that is the dichotomy that exists in... I think it really depends on the individual and their own personal attitude towards work. My feeling is really more along your lines. And in our environment, I have a pretty strong interaction with people outside the research laboratory. But—I wouldn't say I'm unique in that—but I probably interact a lot more than many people in a research laboratory.

At this particular point in time, with the problems that McDonnell Douglas is having with cash flow and so on, my interaction is valued by research laboratories. But several years ago it would have been considered to be a waste of time or not a good thing to do. Right now in order to preserve the laboratory, it's observed as something that's valuable because there's a very strong interest in... Probably our company is doing what Humble did then is that you either are going to help direct your focus on a problem or else we're not going to be able to afford to pay you to sit out here and do pie-in-the-sky things. So it's pretty clear that our research laboratory will have to be focused more closely on company problems in the coming years.

I don't know if that's all bad. The issue, once again, really, I think, in our environment, is who controls the work, and that's who decides what to do. There's been an ego in there. There's always that ego that is involved in a lot of this, particularly with people who are in a research environment. I mean they've all done a lot of work, and they have a lot of intellectual capability, and they've got to establish a reputation. They're known. They feel they should be able to do what they want to do, and they're not really interested in working on a problem that comes in. But that's people. And that's an interesting area of the world.

Things seem to be working pretty well here. My machine, I think, has warmed up. What I'd like to do is back up to the beginning of this outline, and if you want to develop anything in more detail. I'd like to get some personal history information before we get too far afield.

We know, of course, and I usually announce this at the beginning of the tape, but since we started out so smoothly, I ought to mention this now, that this is an oral history interview with Sy Meyerson, and it's being taken on the 7 March 1991.

So, Tell me, when were you born?

MEYERSON: I was born in 1916 in Chicago [Illinois].

GRAYSON: So that would put you...I always hate to do these mathematical calculations. What is that, seventy-five?

MEYERSON: Seventy-four.

GRAYSON: Chicago, as I learned during the meeting, is a very fascinating city; a lot of different things going on all over the state at any given time. What kind of interest did you have in school—in grade school, high school—that would bear on the career that you ended up in actually pursuing?

MEYERSON: I'm not sure how to answer that question. But whatever came out of grade school and high school, by the time I got to the University of Chicago I found maybe a dozen different areas of study all fascinating. And I could have been happy with any one of them. Sometimes, thinking back as to how I happened to land in chemistry, I think that somebody must have given me a twelve-sided coin, and I tossed it, [laughter] and it came up chemistry.

Unlike a great many people in the physical or natural sciences, I took a fair amount of work, there at the university, in the social sciences as well. In particular, after getting my degree in 1938, my bachelor's, (it was a very bad time, jobs were very scarce) I took a few graduate courses in chemistry, and then switched to another area, group work education, which was defined as informal education and recreation. I took background work in the social sciences, sociology, social psychology, comparative religion—several such areas at the University of Chicago—and then spent about a year at George Williams College. That was the old YMCA training college.

And then somewhere along the line I went back and found my first job as a chemist, around 1941 I think, in a consulting laboratory in downtown Chicago.

GRAYSON: So between 1938, your graduation, and 1941 you were actually pursuing almost a totally different academic career?

MEYERSON: A good part of that time. That's correct.

GRAYSON: Then in 1941, what evolved from that point?

MEYERSON: Well, I worked at that job for maybe six months to a year. I don't recall exactly. From there, the war came along, and I went to a job as an inspector of powder and explosives at an explosives plant near Joliet, Illinois.

GRAYSON: Was this as a civilian?

MEYERSON: As a civilian. I was employed by the Army as an inspector. And, from there, I enlisted in the [Unites States Army] Signal Reserve Corps for a training program in electronics and related things...

GRAYSON: Really?

MEYERSON: ...leading into radio and radar. So that I spent some five or six months in intensive training there. Most of that time was in course work that was organized by the physics department at the University of Chicago, with some contribution from the mathematics department. And following that, I spent several weeks in radar school at a local establishment that arranged a training program on contract to the Signal Corps.

GRAYSON: That's interesting because we have an interview of John Beynon—a short one, but nevertheless an interview—and he had a very similar career. After he graduated from school and became involved in the military, they sent him to radar school. And people who seemed to have any science or technical background were automatically stuffed into radar school or whatever in those days.

So after completing this course work, then what happened? You're still not quite getting into mass spectrometry here. There is a gap that we have to cover.

MEYERSON: No. From there I went into active service in 1942 in the Signal Corps. They detailed me with the Air Force for basic training in Miami Beach [Florida]. And after basic training there, I was still with the Air Force but at radar school at West Palm Beach, Florida. From there up to Fort Monmouth [New Jersey] for more training of one kind or another. In time I got into Signal Corps OCS and got my gold bars. And then somewhere along the line, I was detailed in the Corps of Engineers and assigned to the Area Engineer’s office in Chicago, where I found myself as part of the Manhattan Engineer District.

The office was hidden in Washington Park on the south side of Chicago, right in the neighborhood where I had lived most of my life until then. My job was technical liaison officer between the Manhattan District and Standard Oil Company in Whiting [Indiana] on a project that Standard Oil was doing on contract to the District. As technical liaison officer, I chased back and forth constantly between the Area Engineer’s office and the refinery. I wore civvies during that time and was furnished a civilian car. But I carried paperwork to identify both myself and the car as belonging to Uncle Sam.

That was my first contact with Standard Oil. The job that Standard Oil had undertaken there was the separation of the boron isotopes. And that's because the rare isotope (and I think that's boron-10 if I remember rightly) is about 20 percent in nature...

GRAYSON: Yes, it's the one that's a lot less...

MEYERSON: That's the one that has a very large cross-section for neutron absorption. And that's what it was needed for. The project run by Standard Oil was undertaken to prepare that rare isotope in a minimum concentration of 95 percent. That was a huge distillation operation.

GRAYSON: Why don't we stop here and see how this works with a little background. Ok, I think the furnace shouldn't represent a problem.

So this is all for essentially the Manhattan Project work?

MEYERSON: That's correct.

GRAYSON: But the Manhattan District really wasn't associated with the Manhattan Project, was it?

MEYERSON: Yes.

GRAYSON: So, when you say Manhattan District here...

MEYERSON: That's the Engineers' organization and operators of the project. Now, in the laboratory at the pilot plant that was operated where I spent my time, there was the first mass spectrometer that I ever met face to face.

GRAYSON: This was 1940...?

MEYERSON: This was 1944 or 1945; 1944 I would guess. I really had no direct contact with any of the technical work there. My job was to facilitate the interaction between the Engineers' office and the operation there.

GRAYSON: Would it be like a contract, what they call a contract monitor type of a job, where the government gives someone a specific task of monitoring the performance of a contracting organization type thing?

MEYERSON: Well, I suppose it was related to that. It wasn't exactly that. I wasn't checking on anybody, but there was really a good deal of facilitating that was necessary. For example, classified shipments of one kind or another often had to make their way from the Engineers' office in Chicago to the refinery in Whiting. And, when that happened, I had to be at the Engineers' office when the shipment was put on the truck. And I signed the necessary papers in my capacity as shipping officer. Then I drove out to the refinery to be there when the truck arrived to make sure that we got the stuff off the truck all right and sign the necessary papers as receiving officer.

GRAYSON: You did everything but ride along with the shipment, then?

MEYERSON: That's right. I had a command of one man. There was one young fellow, a PFC, who had been a student of, I believe, Harold [C.] Urey at Columbia University, who was assigned, as his military assignment, to work there at the plant. But, again, he was in civvies. And I was his commanding officer; he was my command.

GRAYSON: Do you remember his name?

MEYERSON: Not offhand. I might or might not be able to reconstruct it. I'm not sure, but not offhand. Other than that, there were about fifteen or so technical people—chemists, engineers, possibly other categories—in the crew that ran that plant and operated it. And it was a very young group, so that most of them had military deferments. And part of my responsibility was to help prepare and to sign all the paperwork to keep getting these guys deferments. There was a little bit of irony there with my being in the service with a background not terribly different from those of these guys. But part of my job was to keep them out of the military service.

GRAYSON: Was your being in the service something that you volunteered, or signed up to do, or were you drafted?

MEYERSON: No. I had enlisted in the Signal Corps, enlisted in the reserve corps and this grew out of that.

GRAYSON: You were more a volunteer than a draftee, then?

MEYERSON: Yes, I was not drafted. Now, part of my responsibility also had to do with security around the place. And there were some curious aspects to that. The structure within which this whole operation was carried out had been built as a pilot plant, and it was known as number four process laboratory. The man in charge, group leader, was Art Conn.[1] And years later he authored a paper describing the operation. I cited it in my reminiscences paper,[2] I believe.

The process laboratory area was surrounded by a tall fence with barbed wire above it. And I recall that some of my toughest problems as security officer had to do with dealings with the people from the Security and Intelligence Division of the Manhattan Project. They would send out somebody once a month to do a security inspection of the entire installation. And as near as I was aware, these guys must all or nearly all have been lawyers by training. They didn't seem to have anything in the way of technical background. For example, one of these guys would make a security inspection, and then his recommendations would come through. And he was disturbed because a part of the yard within the enclosure was not fully visible from the two guard shanties at the two gates, at different ends of the place. And so he recommended that additional big floodlights be mounted on top of the fence surrounding the area to ensure that the guards could have a good, clear view of everything out there. And so I had to sit down and write a letter to somebody in the Manhattan District organization, pointing out that here we have what is perhaps the most highly classified single operation in the entire Calumet area, and in order to ensure that security is adequate, we're surrounding it with a tall fence with big spotlights all around it. This kind of thing happened constantly. In any case, I recall vividly that even the technical people on that project had essentially no information. They were given no information, except the technical aspects of their particular job there.

GRAYSON: They had no idea what..?

MEYERSON: At least formally, they had no idea. Obviously, people did a lot of speculating, and several of the technical people had a pretty good notion of what it was all aimed at, what it was part of. But they were not encouraged to talk about their speculations. On the other hand, there was one operator, a non-technical man, involved in running the stills, who was pretty well read and very sharp. And who did a lot of speculating and he shared his speculations with the other non-technical people there. And I recall being present at one time when Art Conn had called this guy in and instructed him in no uncertain terms to keep his speculations to himself. [laughter]

GRAYSON: The guy was getting too close to home.

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[1] A.L. Conn and J.E. Wolf, *Ind. Eng. Chem.* 80 (1231) (1958).

[2] Seymour Meyerson, “Reminiscences of the early days of mass spectrometry in the petroleum industry,” *Organic Mass Spectrometry* 21 (1986): 197-208.

MEYERSON: He was getting very close to home.

GRAYSON: So that the total focus of this operation was to enrich the boron as a moderator for nuclear...

MEYERSON: Well, not so much as a moderator, I don't think, but for use in the Geiger Counter devices, using it in the form of BF_3 in the Geiger Counter. And I'm not aware of any other use that was made of that boron isotope at that time. That doesn't mean that there wasn't some other use made.

GRAYSON: So basically it was going into instrumentation for detecting, measuring radiation?

MEYERSON: Yes, that's correct.

GRAYSON: So the mass spec that you saw in this operation, was that part of this project, or was it just...?

MEYERSON: Oh, it was part of this project...

GRAYSON: So the mass spectrometer was actually...

MEYERSON: Within the laboratory, in the process laboratory, it was used to monitor the isotopic composition.

GRAYSON: A quantitative measure of...

MEYERSON: That's correct. As far as I was aware, all I knew about it was that it was a big black box. I had a vague sort of notion of what was inside that box, because I think during an introductory physics course, some years earlier, my textbook had, I think, one paragraph about mass spectrometry. So that it was something I had encountered before, but I really knew nothing about it.

GRAYSON: Do you know whose black box it was? Was it a . . . I guess CEC [Consolidated Engineering Corporation] was building instruments then?

MEYERSON: No. There certainly were no commercial instruments at that time. And who built that, I don't know. I'm pretty sure that it was not built there at the refinery, but I don't know who...

GRAYSON: I'm wondering, it almost seems at that point in time it would have had to have been one of [Alfred O. C.] Nier's machines. That was part of the thing that he did, was build a series of instruments for various...

MEYERSON: It's possible, but I don't know. I am not sure. I'm trying to think who actually worked with it, but I'm not sure. The man in charge of the laboratory work was Bill Rittschof, whom I haven't seen now for many years. I think he's living not too far from here, in Michigan City [Michigan], or thereabouts; possibly New Buffalo, Michigan. And Charlie [Charles M.] Judson, who has been working on mass spectrometry for a long time now, was part of that crew. Now, it's possible that Judson may have been responsible for that operation, but I don't remember that for sure either.

GRAYSON: So then you continued in this position until the end of the war?

MEYERSON: No, I was there for about six months, then went back into active service and some more training at Fort Monmouth. And in December 1945, I went overseas and was part of the army of occupation in Germany until about August or so of 1946 when I came home. I spent all of that time in various Signal organizations. I never had my hands on any piece of technical equipment. I was in a company, a Signal Service Company, in Berlin [Germany] during a good part of that time that had much of the responsibility for U.S. Army communications into and out of Berlin. And at various times, I had a title of construction officer and telephone and teletype officer.

My training, of course, had all been in radio and radar, so they didn't let me near any radio or radar equipment. [laughter] And I knew nothing about construction or telephone and teletype. During the time that I was telephone and teletype officer, I found a young fellow in my outfit who had been working for some time for Bell Telephone somewhere in Texas. And I found that this guy was very bright and competent, and he knew what he was doing. And I just gave him all the responsibility that he was willing to take. And I figured my part of the job was to get him promotions as fast as I could. [laughter] That was really it.

GRAYSON: So then at the end of your . . . we’re up into 1946 when you came back to this country. It's been thirty years and you still haven't gotten close to a mass spectrometer yet, right?

MEYERSON: That's correct. I spent several months looking for a job, trying to figure out what I wanted to do. And the spot in which I found myself was not an easy one. As I told interviewers in various places, I had what I felt was a decent academic record with a degree and a few graduate courses in chemistry. And the Army had given me some pretty good training in electronics, ultra-high frequency techniques, but it was very, very rushed, a very heavy schedule. And I hadn't been in a chemistry laboratory, by this time, for some years. And I hadn't worked with any electronic gear, for that matter, from the time that they sent me overseas.

So I felt I had a fairly decent background in two areas, but both were rusty, and I wasn't quite sure what to do with them. For the most part, the response I got from interviewers was, “Well, probably, you ought to go back to school and get yourself another degree and decide what you really want to do with yourself professionally. And then come back, and we'll be glad to talk to you. But until then we really don't have much to talk about.”

Until I went out to Standard Oil, where I had been on that Manhattan District assignment. When I left that assignment, the group leader there, Art Conn, had urged me to consider working for the company when I got out of the service. So I went back there for an interview. They set up three interviews during the day, three people for me to talk to. The first one was with E. B. Tucker, who was the group leader of the analytical group. This was the entire analytical organization, which consisted of maybe six or eight technical people, and maybe a dozen support people. And for the first time, I got a very different kind of response then I had encountered anywhere else.

I don't think I mentioned earlier —maybe I did—that there was a period, from about 1939 to 1940, during which I had taken work at George Williams College and at the University of Chicago in the social sciences. Anywhere else I had been for an interview, and people heard about that, they said, “Why, as a chemist, would you want to take work in the social sciences? What a waste of time.” But for the first time, I got a different kind of response from Tucker. What he said was “That's great”. He said that almost nobody who comes to work here at technical work has had anything in the social sciences. But, “as I see it,” he said, “anything that you have ever experienced, whether it's job experience, just living, or academic work that might help give you a little more insight into what makes people function they way they do, is great.”

GRAYSON: This guy was probably a little advanced for his thinking, even...

MEYERSON: I know he was in many ways. As the years went by afterwards, I had a lot of occasion to have that driven home to me over and over again.

GRAYSON: This was Tucker?

MEYERSON: E. B. Tucker. I think his training was in chemical engineering. I don't know just when it was, but it had to be sometime during the war, I think, or immediately before the war that his first analytical group was put together. Somebody tapped him on the shoulder and told him that he was a group leader in charge of analytical. Further, when I put it to him that I had the background that I've just described in chemistry and in electronics, I felt that they were both fairly sound, but both were pretty rusty. And I would guess that if I went into a job in which I can utilize both of them, then it would maybe help to increase the probability that whoever gambled on hiring me might begin to get a return on the investment a little sooner. And that seemed to make good sense to him. And he described the need for a chemist to work with a mass spectrometer. Took me down and introduced me to Henry Grubb, L. J. Schmauch, Grace Marsh, and the other people working with this equipment, and left me there for several hours. By the end of the day, I was feeling very comfortable with this equipment and with the people and with the prospects there and Tuck evidently felt pleased also and I never did see the other people that I was...

GRAYSON: You never got to the other two interviews.

MEYERSON: I don't know who they were. Never got to them. I was assured by the personnel man that I would be receiving an offer from the company which came promptly after that. It was for a salary of 325 dollars a month, which was pretty respectable at that time. I was told that the company evaluated my graduate work—the graduate work I had taken—and the experience both on the job and in the military service as roughly the equivalent of a master's degree. And they used that to set the starting offer. So I started to work at Standard Oil with the mass spectrometer. I accepted in late November of 1946.

GRAYSON: So, finally we get to mass spectrometry. After all this time we finally get there.

MEYERSON: Now, that machine that I worked with was, of course, a Consolidated [Consolidated Engineering Corporation] 21-101.

GRAYSON: Ok, this was 1946?

MEYERSON: 1946. It had been installed in 1943.

GRAYSON: Ok, so it had been there for a little while?

MEYERSON: It was the second machine that Consolidated had sold and installed.

GRAYSON: Number two?

MEYERSON: Number two. I still have the nameplate from it, which has a much higher number on it, 1464. And I gathered, somewhere along the line I was talking to somebody who had been part of Consolidated at that time who confirmed, that they put those much higher numbers on the nameplates to mislead their competition.

GRAYSON: This was the Dempster design 180 degree magnet. I suppose this machine had a resolution around several hundred?

MEYERSON: Oh, I would guess the resolution was maybe as high as one-hundred and fifty. For the most part, we used it for quantitative gas analysis. That took us through the C-5s, light hydrocarbons and fixed gases. Took us through the C-5s with all of the C-4 olefins lumped and all the C-5 olefins plus cyclopentane lumped. We felt we could tolerate maybe one- or two-tenths of a percent of C-6s without screwing up the analysis badly. And we just wrote it as C-6 and heavier on our analytical reports. We also did quantitative component analyses of C-6 paraffins, which had to be a narrow cut taken off the Hydrobot distillation apparatus.

GRAYSON: The analyses were done in conjunction with the pilot plant operation, or full plant operation, or...?

MEYERSON: Both. They came from all over the refinery.

GRAYSON: The use of the technology in the refining business at that time, it seems that already, it's received a certain amount of recognition in terms of solving particular problems in the petrochemical industry.

MEYERSON: We didn't talk about petrochemicals in those days. I think a large part of the drive that prompted Consolidated to do the necessary development work with this first instrument came through the machinery that was set up through the Office of Rubber Reserve at the Reconstruction Finance Corporation. This was a governmental body that was set up to do everything possible to establish and develop a synthetic rubber industry in this country during the war, because natural rubber supplies were largely cut off. They simply weren't available.

So there were companies making rubber. They were companies—and then again this was largely petroleum companies—that provided the supplies of butadiene. They were chemical companies involved, to some extent, with largely the petroleum industry. And the needs for establishment and development of the rubber industry came at the same time and got mixed up with the analytical needs for aviation gasoline. The petroleum companies were faced with analytical needs to improve upon analyses that were simply far, far too slow, too complex, and too incorrect, until then. So, in about 1944, I think twice during that year, in the spring and fall, the Rubber Reserve had called together conferences.

GRAYSON: The Rubber Reserve is a governmental...?

MEYERSON: Is a governmental office; it owned many of the rubber plants itself. They were operated under contract by chemical companies. And it was a tight partnership between governmental agencies. Several other governmental agencies were also involved between the various governmental agencies and private industry. Circumstances made cooperation absolutely mandatory and they functioned very effectively.

The first of these conferences on analytical methods that was called together by the Rubber Reserve dealt almost entirely with optical spectroscopy. The second one in 1944 included a handful of papers on mass spectrometry. This met somewhere in Los Angeles [California] or Pasadena [California] or somewhere out in that area. I believe I have a copy of their proceedings with all the various papers that were presented there. And sometime during the several-day conference, the Consolidated people invited participants there who had any interest in mass spectrometry to come out.

[END OF TAPE 1, SIDE 1]

GRAYSON: We were talking about the first meeting of the Consolidated plant in Pasadena.

[TAPE PAUSED]

MEYERSON: That was the first group meeting of users and potential users of Consolidated mass spectrometers.

GRAYSON: So this was in 1940...?

MEYERSON: 1944.

GRAYSON: 1944.

MEYERSON: That's correct. Either in 1944 or 1945 there were two such very informal meetings. And then in late 1945 there was the first group meeting set up by Consolidated as a full blown technical conference.

GRAYSON: Now you were still in Europe at the time of the 1944, 1945 conferences?

MEYERSON: That's correct.

GRAYSON: So you had not then been exposed to these until after you started with Standard Oil in, I guess by 1947 or so. You were involved in these?

MEYERSON: I'm not sure when I went to my first conference; it might have been 1948. I would guess that it was probably 1948, and if I remember rightly...No, I'm not sure where that first conference was. I could find out. I have notes on all conferences that I've ever attended.[3]

GRAYSON: Now, just as a little bit of an aside while we're talking here. I'm kind of struck by the similarity of events driving things that I got from Al Nier in our interview, and that is that the technologies of the war—the technologies of demanding your finding a source of rubber, the technologies that were needed to create atomic weapons—drove the development of analytical instrumentation because, basically, what I feel is that the way of doing this was known. I mean, technically, the chemistry and the physics, etc. of how one would go about creating these end products was known, but the difficulty was in knowing when you had gotten there. In other words, you had to measure, you had to find some way of measuring how well you were doing in your process. And so, here, again, we see that the need to measure the feedstock for the rubber industry and determine when you had what you needed to put in was an important problem that drove the technology of developing instrumentation for measuring these things, in the same way that mass spectrometry was used as a technology for measuring enrichment in uranium plants.

MEYERSON: Correct.

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[3] The first Consolidated Group Meeting took place in May, 1950, in New Orleans, Louisiana.

GRAYSON: So, it's a technology that was really driven to a very large extent—perhaps more than we even realize—by the needs of the war in terms of creating high tech things—making your own rubber, synthetic rubber, and making atomic weapons. And without that driving force, it might still be a kind of curiosity in physics because up until really in the very early forties, mass spectrometry was —at least as I perceive it— in the purview of the physicist and only an occasional chemist might do something with it.

MEYERSON: Yes. That's correct. I think the Consolidated people were really the first people who were prepared to do the necessary engineering development to make a feasible instrument that could be commercialized. And they did not arrive easily at the decision to invest what they had to put into that. It was a very chancy kind of thing.

I think that younger people who have come into the field over the last couple of decades may have heard the name Consolidated but don't really know anything about what the organization was or what it did. But even quite a bit earlier in the game, I suspect that not very many people had anything like an adequate appreciation of the enormous contribution that Consolidated made in order to make mass spectrometry a feasible, practical, useable experimental technique. These instruments had been physicists' playthings as you put it, and just a few chemists here and there had begun to use it to make measurements on organic compounds.

GRAYSON: Is it fair to say that most of these chemists were in industry?

MEYERSON: I've been going through my files and tried to run down these early beginnings recently in connection with a paper that I had agreed to try to prepare for *JASMS* [*Journal of the American Society for Mass Spectrometry*] which right now I think I'm going to back out of, incidentally. But as near as I can tell, the first mass spectrum of any organic compound that was recorded appeared in the literature in 1928 by a man in Germany named R. Conrad—I guess it would be pronounced “Cone'rod”—who worked for I.G. Farben Industrie.[4] That's industry.

GRAYSON: Do you recall if the spectrum was of?

MEYERSON: Methane. Perfectly good compound to start with, couldn’t have found a simpler one. And he was working with a parabola mass spectrograph, so he couldn't do anything very quantitative with it. But, qualitatively, he was the first guy around who collected, and was able to detect, a whole slew of ion beams, and established unequivocally their nominal masses, assigned them and was able to report that we start with methane, pass it through an electric discharge, and then use electric and magnetic fields appropriately to separate them by mass so

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[4] R. Conrad, *Physik Zeitschrift* 31 (1930): 888.

that you get ions with a composition CH_4, CH_3, CH_2, CH, and C. In addition, you get C_2 ions, and you get some C_3 ions and maybe even a few C_4 ions out of a mess like that. And, this was a starting point.

In 1930 and 1931 there were a few more people who were beginning to publish. Conrad was publishing a little more work. [Thorfin R.] Hogness at the University of Chicago published one of those very early papers. Now Hogness was on the faculty at the university chemistry department when I was in school there.[5] I don't remember whether I ever had a course with him or not. I certainly was not aware of anything he would have done in mass spectrometry. I wasn't aware of mass spectrometry at the time that I was...

GRAYSON: How would you spell his name?

MEYERSON: Hogness. I think his first name was a good Scandinavian name, something like Thorfin.   I'm not sure if that was right, but that's pretty close, I think.

GRAYSON: So he was in the academic environment?

MEYERSON: Yes, he was academic. And then of course about 1935 or 1936 in his work... Well, let me back up and say there was another team, H. R. Stewart and A. R. Olson. If we interrupt it for a moment, I think I could tell you where they were located [they were at Berkeley, presumably at the University of California].

GRAYSON: Ok. Sure.

MEYERSON: In 1928 was by T. R. Hogness and H. M. Kvalnes. That was 1928 published in *The Physical Review*.[6] Then Conrad came along and he published in 1930 a paper dealing with methane along with a collaborator, a man named O. Eisenhut.[7] He extended that from methane to include ethane, ethylene, and acetylene. In 1934 Conrad published a more extensive piece of work, in which he pinned down a set of rules that were obeyed by these ions.

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[5] For at least one such paper, coauthored with H. M. Kvalnes, the authors were at the University of California, not Chicago. See T. R. Hogness and R. W. Harkness, “The Ionization of Carbon Monoxide by Controlled Electron Impact, Interpreted by the Mass Spectrograph,” *Physical Review* 32 (1928): 936-41; and T. R. Hogness and H. M. Kvalnes, “The Ionization Processes in Methane Interpreted by the Mass Spectrograph,” *Physical Review* 32 (1928): 942-5.

[6] Ibid.

[7] O. Eisenhut and R. Conrad, *Z. Elektrochem* 36 (1930): 654.

GRAYSON: These were obviously positive ions?

MEYERSON: Yes. And back in Europe in 1931, Stewart and Olson published a paper in which they reported spectra of propane and butane where they also arrived at the conclusion that the ionizing electrons decomposed the hydrocarbons on impact.[8]

GRAYSON: Were these people in industry or academia, Stewart and Olson?

MEYERSON: Let's see. I have Stewart and Olson elsewhere. I find that when Hogness and [H. M.] Kvalnes published that work, that was at the University of California. Hogness must have come to the University of Chicago later. The work by Stewart and Olson lists as the only address here, Berkeley, California. So I would guess that they were at the University also. So there were academic people in the game very early, but not very many.

GRAYSON: Yes. And their interest was more in the organic chemistry side as opposed to the physics side?

MEYERSON: Well, it was more than that. These various little efforts were first trying to get some qualitative notion of what happened in a discharge and then later under electron impact. They were really struggling with instrument design and construction. They had some systems that were terribly complicated by virtue of the pressures generally being a good deal higher than what we ultimately learned is necessary for good reproducible work. So that there was a lot of chemical ionization that was going on right then, except that's not the term that was being used for it.

GRAYSON: So it was one of those technologies that was happening, but nobody knew about it until thirty or forty years later.

MEYERSON: That's right. Now in the mid 1930s Nier measured the mass spectrum of benzene, as I think you know. I don't think he ever published that.

GRAYSON: That's my understanding. He showed it to me from his original notes. I've got a xerox copy of the thing plotted out by hand that he made in the end of the thirties. But I think

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[8] H. R. Stewart and A. R. Olson, "The Decomposition Of Hydrocarbons In The Positive Ray Tube," *Journal of the American Chemical Society* 53 (1931): 1236-44

he also admitted to the fact that he just did it as a curiosity, and he didn't really do anything more with it.

MEYERSON: Although back here, there is a paper published in 1934 in the *Transactions of the Faraday Society* by Conrad, entitled "Decomposition of Hexane, Cyclohexane, and Benzene in the Positive Ray Tube."[9] He beat Nier on that. And then in about 1936 or thereabouts, Dwight Taylor was working at Caltech [California Institute of Technology] doing his graduate work on attempting to develop a mass spectrometer that the folks there thought might possibly have some actual application in petroleum refineries. They gave up on that, but he's published at least one paper. And I have that somewhere in my files, although it's not right in front of me at the moment.[10]

When the people at Consolidated decided to take a stab at trying to develop a mass spectrometer, they learned of this work at Caltech. And they went back and talked to the people there and, in fact, ended up borrowing the equipment, the instrumentation that had been built there and disassembled, to start with. And Dwight Taylor showed up one day in their office looking for a job. They hired him. So that he played a part. I don't know how long he was there and how much of a part he played. But he was involved in the very early work there at Consolidated also.

There was an academic contribution, but it was small. It was very small. And the great bulk of what developed for many years in mass spectrometry came out of industry and predominantly the petroleum industry. I remember that in the late fifties and early sixties, when for the first time a lot of academic people were moving into the field, many of my friends and cohorts working for petroleum laboratories and for chemical companies were quite upset with our annual mass spectrometry conferences. They said that these conferences were becoming more and more theoretical, and they don't see how they're going to be able to continue to persuade their management that there is anything useful that they're going to learn there.

I started, at that time, to find myself on a kind of a fence with one foot in the theoretical or fundamental end of things and one foot in the applied kind of thing. I didn't have a lot of company trying to build bridges and persuade these guys that they're not going to be able to afford to ignore what's going on at the other end of things. And if they don't find enough analytical content to keep them satisfied, I urged them to just prepare more papers themselves, because the program committee will schedule virtually anything that anybody in the field offers. So, if you want a symposium or just individual papers on analytical problems, prepare them and submit them. And they'll get on the program and you'll have more analytical.

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[9] R. Conrad, "Decomposition of Hexane, Cyclohexane, and Benzene in the Positive Ray Tube," *Transactions of the Faraday Society* 30 (1934): 215-20.

[10] D. D. Taylor, "A Modified Aston-Type Mass Spectrometer and Some Preliminary Results," *Phys. Rev.* 47 (1935): 666.

GRAYSON: This almost comes back to the issue that we started this discussion on, with Frank Field and the difference in opinions about the utility of being involved in real problems vs. just doing unconnected or distant basic research. So I guess, as I mentioned earlier, this appears to be a dichotomy that's there, and there are few people who'd like to bridge that area. But it seems like many people would prefer to be in either one or the other and not be in the middle.

MEYERSON: [laughter] Yes.

GRAYSON: Whereas the middle is kind of a most interesting place. I would think it could be quite challenging.

So we're up into the area of about the late forties, you have started with Standard Oil of Ohio [Standard Oil Company (Ohio)]...

MEYERSON: Indiana.

GRAYSON: ...Standard Oil of Indiana [Standard Oil Company (Indiana)]. And you found a home with a mass spectrometer, finally, as an instrument that you feel good with, makes use of all your background. What did you do when you got on board there and started to get into it? What kinds of things were you... I mean, we know the applications that were being performed. But what did you start out in?

MEYERSON: First my group leader, as I mentioned before, was E. B. Tucker. The senior man, senior person, in the team working on the mass spectrometer was Henry Grubb.

GRAYSON: That's Grub...

MEYERSON: B. Two b's. Henry was a product of Purdue University. He'd worked for the company about ten years then. He and Tucker had worked together in the grease plant, I think, until somebody decided that the company needed an analytical group. And when Tuck was tapped for that, he insisted on taking Grubb along with him.

Henry had done his undergraduate work in physics and his graduate work in physical chemistry and he was a very, very sharp guy with a great deal of insight and understanding of technical matters and of people. Both. Quite apart from anything technical, he had read widely and extensively in biblical history. And he loved to get into discussions of things he found there. I still recall his describing some human society—and I don't know where or when it existed—in which the established pattern was that the guy who occupied the seat way at the top of the heap, the king or emperor, whatever he was called, had enormous power. But when he ascended to that seat, he had already agreed that he would be allowed to maintain that position of power for one year, and at the end of that time he would be beheaded. And Henry felt that our politics in this country might be improved considerably [laughter] if we were to adopt a practice like that. [laughter]

GRAYSON: Yes, it's an interesting way to go.

MEYERSON: I recall that, not only Tucker but also Henry was very generous, very kind, very understanding in helping me to get started. As I put it to them, I felt I had a decent background in both chemistry and electronics. I was well aware that both were rusty, and I was going to need more time. Henry turned me loose there, introduced me to the equipment, introduced me to the manuals and the other people. And he told me not to feel under any pressure, just take all the time I want. And he said, “By the end of the year, we expect that you'll be functioning pretty smoothly, but don't feel it necessary to push yourself or to rush things before then.” That really was very helpful to me in getting started.

GRAYSON: Yes, it sounds like an ideal situation. Particularly for, as you say, someone who has the background, but is rusty and hasn't used it. It's fair. So then you just essentially...did you just run samples or just run the machines?

MEYERSON: I ran samples. From time to time when work was being done on some of the circuitry, if I indicated I'd like to have a hand in it, I was welcomed.

GRAYSON: Was this in repair?

MEYERSON: Well, it was more than that. First the instrument was still really pretty primitive. For example, if you sat down to run a gas sample—and that was what most of the work was on the machine—running it would take maybe fifteen, twenty minutes. During that time you kept your hand on the potentiometer that controlled the filament current. And you kept looking at the meter that measured the ionizing current. And you kept jiggling the filament current in order to try to hold the ionizing current as steady as you could.

Now actually Henry and L. J. Schmauch had built a feedback control circuit about a year earlier that would do that automatically. But at the time I started to work there, that feedback circuit was on the blink, was being rebuilt, and I think perhaps redesigned. So that this is the way in which we had to operate.

There was a lot of instrument development that was still needed at that point. And as I think you may be aware, as I've stated elsewhere, when that instrument was sold to Indiana Standard in 1943, the sales contract included a provision that any instrumental developments or improvements that might occur there within the company's laboratories would be made available without any cost, without royalty, to Consolidated. So that there were a good many instrumental developments that took place there in our laboratory, that took place in users' laboratories in lots of other places that were fed back to Consolidated and many of them were incorporated into the design of subsequent instruments.

GRAYSON: Speaking of the sales contract, do you have any idea what that instrument cost?

MEYERSON: Yes. That first instrument was 19,500 dollars plus some additional costs, but I'm not sure. The first ones they made, the first few, were sold for 19,500 dollars, plus additional charges for training. And I think, subsequently the additional charges were made a fixed amount of 2,000 dollars, and they operated at that level for some period of time.

GRAYSON: There's the issue Consolidated had a number of names. There was Consolidated Engineering and Consolidated Electrodynamics...

MEYERSON: Yes. I don't remember just when the name was changed, but they kept the same initials of CEC. But that's right. It started out, I believe, as Consolidated Electronics.

GRAYSON: Electrodynamics or Electronics?

MEYERSON: Electrodynamics. Yes.

GRAYSON: I was just curious, because I'm familiar with that name problem but I don't have any... anybody knows exactly when was what. I'm not sure that it really matters, but basically we know we're talking about CEC.

MEYERSON: That's correct.

GRAYSON: So then you were encouraged to use the machine and still you had the oscillographic recording?

MEYERSON: Oh yes, that's all we had. We took the photographic record out in a black sleeve, took in into a darkroom and processed it. At first we recorded both the trace and the scale. There was a device in the camera, which was referred to as an oscillograph, which put a scale on the paper. It was optional. You could make it as heavy or as light as you liked. Only later did somebody establish—and we confirmed for ourselves—that if we follow the photographic processing and drying schedule very carefully, that the paper would dry to the same size every time. So that you didn't have to have a scale recorded on the paper. You could use a ruler; slide it along to measure the peak heights. But it took a while before we realized that.

GRAYSON: So you probably used a lot of oscillographic recording paper in this business?

MEYERSON: That's correct. That was Haloid. The first couple of batches or so that we used of Eastman paper, when they tried to break into this field, were really not very satisfactory. I don't remember for sure what the problem was. I seem to recall that the paper was thinner than the Haloid paper, and it dried very brittle so that it tended to crack.

[TAPE PAUSED]

MEYERSON: And we stuck with Haloid for years. Now ultimately that problem was corrected, but it took a long time.

GRAYSON: In running the instrument you were really kind of running a little bit blind. You were setting parameters looking at needles, but you aren't actually seeing ion current in real time. Is that correct? In other words, is there any monitor on the instrument that would tell you that you're actually getting ions into the detector. Many modern instruments have the nice feature that you can see a small portion of the spectrum or a couple of peaks. You can adjust the ion source parameters and optimize the tuning and so on.

MEYERSON: Yes.

GRAYSON: But, as I recall, on this instrument you basically kind of set it up and...

MEYERSON: You know, I don't remember clearly, but I think that there was a meter on one of the panels that responded to ion current, so that when we were focused on a beam, the needle would deflect. We would have had to have something; I seem to recall that.

GRAYSON: Did you ever run into a situation where you thought you had a spectrum and when it developed you found out that you didn't have anything?

MEYERSON: Not that I recall.

GRAYSON: So it was a pretty dependable operation once you got into the hang of it?

MEYERSON: Oh yes. Over the years, even while a great many of these Consolidated instruments were still in widespread use, a good many people tended to be critical of the procedures that Consolidated had set down and established that became more or less standard throughout the field. They felt that they just didn't measure up. That does not jibe with my perception. As far as I can see, the Consolidated people did extraordinarily well in selecting good operating conditions to get highly reproducible, quantitative results.

I have a recollection of having heard or read somewhere that they originally started by recommending using fifty-volt electrons, but by the time I got started that had been changed to seventy. I think seventy is still pretty generally in use in all kinds of instruments. And the point was, of course, that they wanted to find an electron energy, such that if it varied a few volts one way or another it wasn't going to have any measurable effect on the ion beams. And they did very well. They did a remarkable job of putting together apparatus that could be well evacuated and in selecting conditions that were essentially free of any bimolecular processes, which minimized any effect of a potential well in the source.

I suspect that electron impact, even today, continues to be probably the most satisfactory general-purpose ionization method for a vast assortment of problems. So they did very well by us. I feel that really all of us who have been part of this game have a huge debt to the people at Consolidated.

GRAYSON: Because they did their homework and they did it right?

MEYERSON: That's correct.

GRAYSON: Do you have any idea what size their operation was in terms of people at that time? Were they targeted by a relatively small group of people? I would assume maybe fifty?

MEYERSON: I don't really know. It was a small outfit certainly. Consolidated was organized as a corporation in 1937 with Herbert Hoover, Jr. as the president and Harold Washburn as Vice

President, Director of Research. During the first several years at least, I don't think I was ever conscious of more than maybe a dozen technical people in their organization. Now my contacts were primarily with people in their research department. But then they overlapped research into sales. And I'm certain that we must have had contact with some of the people in their engineering department.

Again, I think you know that my closest contact, who was a very valuable source of information to me when I was putting together the reminiscences paper two years ago, was Harold Wiley, who died a year or two ago, incidentally. I believe that Charlie Judson finally, through me, did get together with Harold and did go through his files and historical material while he was still alive, on behalf of the history subcommittee of ASMS [American Society for Mass Spectrometry]. But he certainly played a key role there.

Cliff [Clifford] Berry played an important role in the design, both of the mass spectrometers and of the early computing equipment. And that early computing equipment really was early. The analog computer for calculating gas analyses was introduced at the third Consolidated group meeting in late 1945. And that was the first of those meetings that was set up formally in the format of a technical conference. So that they started putting together computing equipment really very early in the game.[11]

I was quite annoyed when I was in the course of completing that reminiscences paper for publication. I added two aspects of the work that had not been in the original presentation at San Antonio [Texas]. One of them had to do with computing equipment, and the other had to do with type analysis and qualitative analysis. At San Antonio the computing end of things was taken care of pretty much by a paper by Klaus Biemann, but he made no attempt to cover anything prior to his entry into the field, which came in the late fifties. When I did start hunting up some of the earlier computing work, one of my sources led me to a text, a history of electronic computing, in which the author describes some work that Cliff Berry did when he was a graduate student at Iowa State [University] working for John V. Atanasoff.[12]

GRAYSON: How do you spell that?

MEYERSON: Atanasoff. The man who is generally credited with having built the first fully electronic computer is John [W.] Mauchly. And when Mauchly was still at a fairly early stage of his thinking in these terms, he heard a paper at a conference by Atanasoff and wangled an invitation to visit Atanasoff's laboratory in Iowa. There he was given a demonstration of what they called the ABC, the Atanasoff Berry Computer. Berry was the graduate student working for Atanasoff who had done most of the actual shop and laboratory work putting the thing

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[11] Seymour Meyerson, "Reminiscences of the early days of mass spectrometry in the petroleum industry," *Organic Mass Spectrometry* 21 (1986): 197-208.
[12] Ibid.

together. As I understand it, their computer was only partly electronic; it was largely electrical.[13]

In this published history of computing, the author acknowledges the work that was done by Berry there at Iowa State and says that when he left Iowa State, he went on to a rather undistinguished career. When I found that, I was shocked! And I wrote him a letter and pointed out that for mass spectrometrists for a lot of years the name Cliff Berry was one that we revered highly. He made enormous contributions both to developments in mass spectrometry and to computing equipment for use with mass spectrometry. I felt that the author owed somebody an apology for that. I never got an answer.

GRAYSON: Well, I guess in terms of the big picture of computing as it's evolved today, Berry's contributions may not have seemed like later developments didn't really fit into that picture. But as you point out, to mass spectrometry, indeed, the work was a very important work. Obviously, the author had a single focus in what was important.

MEYERSON: The author of this book apparently is described somewhere as a historian of science, and one would think that he would recognize the existence of fields of science other than pure computing.

GRAYSON: Yes. It's a difficult problem area. I suppose that's why I'm interested in doing the kind of thing we're doing here, is to get people's input who were really there and who knew these individuals and have access to information that is not in books because they're primary sources. Maybe we can do a little bit of righting of Berry's slighting as a result of this. It would certainly be quite a historical resource that people can access in the future in this bit of the past, and his work will be remembered.

So we are pretty much to the point now where you've gotten familiar with the instrument. Obviously, you've done quite a bit of research in mass spectrometry. When did you really start to do research work and tackle problems that were fundamental in nature?

MEYERSON: I think that must have been about 1951 or thereabouts. In 1948 we moved from the original mass spec laboratory. It was a large room with a small office adjacent to it in a relatively small three-story red brick building in the middle of the refinery with a railroad siding right outside our window. And every time a train moved on that siding, the whole building shook.

GRAYSON: So did the mass spectrometers.

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[13] Ibid.

MEYERSON: So did the mass spectrometers. Also the mass spec lab was the only room in the entire building that had air conditioning. And that was because the instrument demanded it. I also recall vividly that there were times when (it wasn't suppose to happen) somebody would turn off the air conditioning at night in order to save electricity. And when that happened, the big mercury vapor rectifier tubes that were used in a bridge rectifier circuit for high voltage supply would all arc across and in the morning everything was dead. And when we started up again, it took us maybe two or three hours for everything to warm up again so we could operate and do useful work. And that was a source of real trouble. And somewhere along the line, somebody came along with a high vacuum rectifier tube that was far more compact than these mercury vapor tubes but had the same basic arrangement; it carried far less current but it carried all the current we needed. That was an enormous improvement, when we went to those high vacuum rectifier tubes.

GRAYSON: Do you recall what the accelerating potential was for the instrument?

MEYERSON: Well the supply provided 4,000 volts. And I think that we actually started our scan at something like 3,800, if I remember rightly.

GRAYSON: This was a voltage scanning machine?

MEYERSON: Yes, a voltage scanning machine. Very definitely.

In 1948 we moved into our new research laboratories on New York Avenue, about a mile from the refinery. We left part of the crew with the old machine in the old laboratory. They became the tech service laboratory, and research was now in the new laboratory. And we got ourselves a 21-102 instrument, which we later modified, converted it to a 21-103.

GRAYSON: The first machine then, was that a 21-101?

MEYERSON: That's correct.

GRAYSON: Does anybody know the history of that numbering? I know there's a 21 series of instruments. We have 21-101, -102, etc. I was just curious if you had any concept of where it came from. Obviously, somebody came up with the numbers.

MEYERSON: No. Not really. I can't claim that. I don't know. I suspect it's pretty arbitrary.

One of the nicest stories about that original machine (I hope this isn't a waste of time, because I included the story in my reminiscences also.) was that we were very conscious that the homogeneity of the magnetic field was a lot less than one would have liked to have had. And the explanation for that was that it was very hard to get a supply of the necessary iron during the war. And with all the priorities that they might have had, the iron that went into that magnet came over the fence from a Navy shipyard in the dead of night. That was for real. I remember that vividly. That was a favorite story around our laboratory for years. [laughter] And when I was putting together my reminiscences paper, I checked that with Harold Wiley, and he confirmed it as absolutely correct, and he thought it was perfectly safe to say so at this stage.

GRAYSON: Oh, you don't just go and throw a hunk of iron over the fence, though. I mean there had to be a little bit of collaborative effort somehow to get that dude . . .

MEYERSON: Yes, that's correct. In any case that was stolen from a Navy shipyard. It simply was the only way that they could get the iron they needed. And you just did what you had to do. There wasn't any other way. It was exactly like a supply sergeant in the army. How well your army outfit functioned, how well you ate, what sort of equipment you had, was, in very large part, a function of how effectively your supply sergeant was as a scrounger. And the same thing happened during the war here in the civilian economy.

GRAYSON: Al [Nier] mentioned a similar thing where he was on the other side of that fence. He was evidently in charge of doling out or approving the use of a particular, I guess, electrometer tube or some kind of electronic device that there were very few of, and they needed all of them in his work, and he had been put in charge of being a policeman for these things. So if anybody in the world wanted one of these, they had to get his approval for it, because there were so few and they were needed so critically. So that was something that we don't think of today as an issue because everything is there. But at that time it must have been very difficult to do a lot of things, simply because you couldn't get what you needed. It was available but you weren't able to get it because you were in the wrong end of the business.

MEYERSON: That's right. In the new lab, gradually the range of our work expanded considerably. And I should say, even before then, even in the old lab, I remember that we started to introduce the analog computer. I don't know at exactly what stage the first digital computer came in. That was a card programmed calculator, IBM's CPC. Whether we were still in the old lab or we moved by then. But I remember that we had a crew of, I think, originally four or five clerks who carried out all of our calculations. Subsequently, that clerical staff grew to a maximum of seven. These were people, generally high school graduates, who were reasonably comfortable with numbers. Some of them had taken a few courses at college level, but not very much. We began to realize that they were slowing down.

[END OF TAPE 1, SIDE 2]

GRAYSON: We finished a part of our discussions just before this break about the calculations that were performed by essentially high school clerks in performing all the necessary number crunching for analyzing mass spec data. I was going to ask you, did they take the information off of the oscillographic record or were they provided with numbers?

MEYERSON: They took it off the oscillographic record. They measured it, and they did all the calculations on a Marchant Desk Calculator.

GRAYSON: This is exactly what I did when I first became involved in this business, working for O. P. Tanner, at Monsanto. They sat me down with a...

MEYERSON: What year was that?

GRAYSON: I started working there in the early sixties while I was still attending St Louis University. Undergraduate. So this would have had to have been about 1961, I guess. I remember, clearly, being introduced to all these elements that we still had the oscillographic recorder with a dark sleeve; the darkroom sleeve; which you had to develop and then essentially that was a lot of my job there is just to do a lot of the transcribing the data from the oscillographic record onto the calculating pad, going through all the calculations, and stripping out the various components and that kind of thing. So I kind of identify with those high school clerks. [laughter]

It's an interesting fundamental way to start into the field. At the time, though, I'm not sure if it was there when I started or it came shortly after. They introduced a device called the Mascot.

MEYERSON: Yes, it came later.

GRAYSON: With regard to your story.

MEYERSON: At your end, I don't know at what stage.

GRAYSON: We're talking about your story here. So basically, the high school clerks, that was seen just to be a bit of a bottleneck? Was it just because you could produce so much more data than they could handle, or were they just not functioning?

MEYERSON: No. They were just slowing down because they saw this analog computer—which is all we had at that time—as something that was going to replace some of them. And they saw it as a real threat. When I realized what was going on and what was motivating them, my task was to sit down with them and try to make them understand that the simplest and easiest, least complex of these calculations were going to be increasingly done by a computer. But that, in the meantime, we were beginning to get some insight as to how to approach more complex problems, qualitative analysis, molecular structure work, that was going to require a great deal more understanding and experience than anything that we could picture building into a computer.

And so my hope, at that time even—that was very early, that was in late 1947, let's say—my hope at that time was that a good deal of that kind of interpretation would in time be turned over to these guys. In any case, we got over that hump. We moved into the new laboratory.

I think I started seriously concerning myself with correlations between structures and spectra, sometime in 1951 or thereabouts. It was stimulated by a lot of work that was coming to us from one particular organic team that was doing exploratory organic chemistry. And quite a number of the samples that came to us from them gave spectra that looked so clean and so simple that they almost had to be something approaching single pure compounds, but we just didn't have the background to be able to identify what they were. [I. W.] Kinny and [G. L.] Cook's paper on the mass spectra of alkylbenzenes and alkylthiophenes came out somewhere about that period of time.[14]

GRAYSON: So almost all of the applications of the instruments had been directed toward the analysis of mixtures?

MEYERSON: Strictly quantitative analysis of mixtures.

GRAYSON: And so now you're beginning to see for the first time pure compounds.

MEYERSON: That's correct.

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[14] I. W. Kinny and G. L. Cook, "Identification of thiophene and benzene homologs. Mass spectral correlations," *Analytical Chemistry* 24 (1952): 1391-6.

GRAYSON: That's an interesting point.

MEYERSON: That were not known; that were heavier than anything we had dealt with and with unknown structures. In about 1951 was when this got started. And I put together a correlation paper on mass spectra of alkylbenzenes that in effect started where Kinny and Cook left off. In the middle of the range of alkylbenzenes with five carbon alkyl groups they covered the lower ones, they covered some of the five-carbon ones. Then I carried it from there on for some stretch. And published that correlation paper in the *Journal of Applied Spectroscopy*.[15]

That opened up a real can of worms. It called our attention to all kinds of chemistry that was going on there, that we did not understand anything about. For example, the work on cationization of cyclopropanes goes back there to that, where at that point we found that if you start with tertiary amylbenzene, tertiary pentylbenzene, the first step is the loss of the ethyl group, and that's perfectly clear. You break a bond once removed from the aromatic ring and you lose an ethyl. But then the next step is to lose ethylene, and it was the metastable peaks we had at that time that told us that that's a second step. It doesn't happen directly from the molecular ion, but it was not at all clear where it came from.[16]

GRAYSON: Did you know the interpretation of the metastable peaks? I mean the metastable peaks were there.

MEYERSON: Yes, at that time we knew that. Well the original work with the metastable peaks, the original picture was really pretty well established by [John] Hipple and his collaborators at Westinghouse [Electric Company] who published on this matter back in 1945 to 1947. It was a while before I picked it up. But we were conscious of metastable peaks from the moment we started looking at mass spectra.[17]

GRAYSON: They show up in the single focusing machine.

MEYERSON: Yes. And at a very early stage, we understood that when we find the metastable peak for any reaction step, that's got to be a relatively low energy process. That we learned early in the game. I don't think that we could have filled in enough of the background at that

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[15] Seymour Meyerson, "Correlations of alkylbenzene structures with mass spectra," *Applied Spectroscopy* 9 (1955): 120-30.

[16] Seymour Meyerson, "Reminiscences of the early days of mass spectrometry in the petroleum industry," *Organic Mass Spectrometry* 21 (1986): 197-208.

[17] Ibid.

point to justify that conclusion. But we had gathered that, and we thought of metastable peaks in those terms.[18]

GRAYSON: You know you're saying “we”?

MEYERSON: Well, I'm particularly talking about Henry Grubb and me. We compared notes a lot. During my whole break-in period there, I don't think anybody ever just sat me down and said, “All right now. Here is what we do. Here is what you're going to do. This is what we think is the chemistry and the physics in this system.” But any time that I took the initiative and asked a question, they tried to answer it. Henry and Red Schmauch in particular. They gave me all the help in the world, but I was not pushed, I was not prompted. It was just made available to me.

In any case, once we started to work with...after the correlation paper, that paper defined a whole long string of problems. Like just where does that ethylene come from? It's the second step in the breakdown of tertiary pentylbenzene. And I think it was in the summer of 1954 that I laid hands on my first labeled compounds. These came from Harold Hart of Michigan State University who spent the summer working in our laboratories.

And sometime during that same period of time, I started to work with Paul Rylander who was a superb physical organic chemist. I think you know some of the history there; I sketched that in the reminiscences. We had an opportunity to start looking at ^{13}C labeling and deuterium labeling in various contexts at that time. And when we did the ^{13}C labeling of tertiary amylbenzene with the ^{13}C in the α position, we learned that ^{13}C was retained quantitatively when the first ethyl was lost and that was reassuring. We felt much better about it.

But when the second C_2 chunk of the ethylene was lost, we found that there were about two chances in three that the label, which was in the α position, was going to be lost. Which said to us that the remaining three side chain carbons, after you kicked off the first ethyl, lose identity—become equivalent — indistinguishable from the point of view of the phenyl ring. And so out of that came a picture of a phenylated cyclopropane ring, which I think was the first proposal anywhere of an ion neutral complex. In any case, with the correlation paper completed and published, and with a first-rate physical organic chemist collaborating with me, both in the interpretation and in preparing compounds to work with, we started our exploratory program. We very quickly began to make some sort of chemical sense out of the systems we were working with.

GRAYSON: These were all under electron impact ionization?

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[18] Ibid.

MEYERSON: All electron impact. There wasn't anything else at that time. Although I think that field ionization first appeared about 1954. We didn't have it for a long time after that. And although we were fascinated by it, and that was one case in which a new development came along and we recognized immediately that this is something that's going to be important. And we tried our best to follow what was going on in that area.

By about 1956 we had our first three papers put together.[19] We had distributed them, reported them within the company. We had talked with a couple of consultants that our company had retained. But in particular we had talked with Herb [Herbert C.] Brown of Purdue. We reviewed much of this material with him, and we reviewed it also with Bob [Robert] Burwell of Northwestern [University]. And after all these years, our sessions with each of those two guys are still pretty vivid in my memory.

In our session with Herb Brown, Paul Rylander got up to the board and he laid out the entire story. This cationized cyclopropane ring, which was both the phenylated one and other labeling of methylated and protonated cyclopropane rings. And all through his presentation, Herb sat there and kept shaking his head "No, No, No." And at the end he said he didn't like it, which didn't surprise us. But he didn't have anything else to offer. We asked him, "How else might we approach the thing?" And he didn't have anything to offer...curiously, a number of the labeled compounds that we had used in that work came from him.

I have a number of work folders, of problem folders, that I brought back home when I retired seven years ago that, if the fates so ordain, I may still get around to going through and trying to organize it and write up. One of those has to do with norbornane. We had a lot of labeled norbornanes. Some of the first ones, I think, came from Herb Brown. Whatever the specific mechanistic details may be, that first hydrogen atom that's lost from norbornane comes at random from the entire molecule. I don't think that that's been published by anybody else. And I hope that I'm able to get to it sometime. But he was unhappy with it, but really didn't have any alternative to offer.

We had a very different kind of response from Bob Burwell. After we reviewed tropylium and cationized cyclopropane with him one late morning, he called the session to a halt and he said that he had been looking for holes or things that he can question and complain about in our presentation all morning. He said he's been playing the devil's advocate because he thought that was the most useful thing he could do. And he felt that we had dealt very effectively with all his objections. He urged us to go ahead and publish, submit it for publication, submit it for presentation at appropriate meetings. And he added that these ideas of ours, our notions that we had presented , he said, may very well have a life of ten years. And for theories of organic reaction mechanisms, ten years is a long life. Moreover, they may even be right. [laughter]

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[19] Paul N. Rylander and Seymour Meyerson, "Organic ions in the gas phase. I. The cationated cyclopropane ring," *Journal of the American Chemical Society* 78 (1956): 5799-802; Paul N. Rylander, Seymour Meyerson, and Henry M. Grubb, "Organic ions in the gas phase. II. The tropylium ion," *Journal of the American Chemical Society* 79 (1957): 842-6; and Seymour Meyerson and Paul N. Rylander, "Organic ions in the gas phase. III. $C_6H_5^+$ ions from benzene derivatives by electron impact," *Journal of the American Chemical Society* 79 (1957): 1058-61.

GRAYSON: That was nice of him.

MEYERSON: So we took that really as a very high compliment, and we ran with it. So the first two papers, I think tropylium and the [phenylated] cyclopropane ring, Paul Rylander presented at an ACS national meeting early in 1956. And just shortly afterwards, I presented the cation-ionized cyclopropane ring and the $C_6H_5^+$ ion, but not tropylium, at a mass spectrometry conference.

At that mass spectrometry conference in 1956 I met Fred [Fredrick P.] Lossing for the first time. He came up and introduced himself during a break. And we met afterwards and had a meal together and spent quite a long time getting acquainted and exchanging ideas. I remember vividly that during that time he was aware of what we had been doing with tropylium. I don't remember just how. We hadn't published it yet. But he raised a question. He said, "What do you think the most probable structure or the most stable structure of $C_3H_3^+$ is?"   He had already published work reporting that $C_3H_3^+$ from deuterium labeled propyne showed that the first hydrogen atom lost from propyne was taken at random from the four positions. And that together with whatever else we may have known or suspected at that time had brought us to the point where Henry and I had been talking about $C_3H_3^+$ for quite some time as a three-membered ring. And this is the answer that I gave Fred, and he said that's exactly the same conclusion they had come to. Protonated cyclopropenium—I'm not sure that was the proper name for it at that time, but in any case he said, "You know we should call it tryllium, which I thought was lovely, but we didn't do it. Years later he published his work on that ion and proposed as the most stable structure of $C_3H_4^+$ a protonated cyclopropene.

GRAYSON: So the reason, I guess, from a scientific viewpoint is that the equivalence of the hydrogen suggests the ion basically is formed into a cyclic arrangement, thereby making all the hydrogens equivalent.

MEYERSON: Yes.

GRAYSON: So the loss of any one is not preferred over the others. So whatever structure the molecule has prior to ionization, once the ion is formed, it really has a different structure than the original neutral molecule.

MEYERSON: That's correct.

GRAYSON: So any time you see a non-preferential loss of a hydrogen you're suspecting that the structure of the ion is ringed up?

MEYERSON: Somehow an element of…we take that as a sign of some measure of symmetry that has been established, and it...

GRAYSON: …which may not be present in the neutral molecule.

MEYERSON: That's right. And it may or may not involve the entire molecule. For example, in the cationized cyclopropane ring, it involved only the side chain carbons. It did not involve any of the hydrogens from the phenyl. They did not get mixed with the side chain carbon hydrogens. So in any case, that's what grew out of it.

Well, once we got started in working our way up to those first three papers, I should say that Paul and Henry Grubb and I, all of us, really had to work hard to try to get a grip on what the literature had to say up to that point, of what went on under electron impact. And I suspect that some of that reading was much more difficult for me than it was for either Paul or Henry because I simply did not have the theoretical background for it. So I did a lot of poring over articles that appeared very largely in *Physical Review* and the *Journal of Chemical Physics* and journals that people working with organic compounds generally didn't read.

Now, Paul Rylander had a strong background, and he kept on top of the literature, and he was a fine laboratory man. He brought a lot of strength to this whole operation. I learned a huge amount from him. I learned a great deal from both of those fellows. And the moment that we realized, for example, that all hydrogens in toluene had become indistinguishable before the first one was lost, Paul came up with tropylium. Only very shortly before then had the first tropylium salts been recorded by a group from Harvard [University], I think. [L. H.] Knox and [William von Eggers] Doering.[20] And he was confident that this is what the thing must have become.

Now, there were some curious aspects of that. The first mass spectra of, I think, all of the singly deuterated toluenes had been published in the API/NBS collection of spectra quite some time before that. They had come out of the National Bureau of Standards. But as far as we were aware, nobody seems to have paid any attention to the fact that the hydrogens had lost their identity. Whether anybody noticed or not, I don't know. There was nothing that we were aware of that told us that anybody had recognized what those data said.

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[20] W. Von E. Doering and L. H. Knox, "Reactions of the Cycloheptatrienylium (Tropylium) Ion," *Journal of the American Chemical Society* 79 (1957) 352-6.

GRAYSON: Let's interrupt a little bit here. We're getting into some pretty fundamental research. How did the company view this work? I mean, you were obviously encouraged to do it and to publish it, but it certainly is not...

MEYERSON: Well it was a mixed reception. On the one hand, the man who at that time... Two or three people, who over a period of years during that period were at the top of the heap in our research organizations, were all superb research men. And they really never raised any problems, any questions, about doing this work.

When we got the first three papers reported within the company, it took several months before we were able to get approval to report them outside, because there were people in our hierarchy who understood enough of what was going on. And they recognized that there is some very fundamental chemistry going on here, and there may very well be implications that will have practical applications. So let's take some time to digest it, to talk about it within the company and to see if there's something we may have been missing.

And several months went by, and ultimately they gave us approval. They couldn't see how to use it, except that it's of scientific interest. They recognized that. They recognized that it's of potential importance. But beyond that, they couldn't see how anything of industrial application was going to come out of it. So ultimately, we were given approval.

GRAYSON: Did you get any feedback which would amount to, well, why are we doing this?

MEYERSON: Yes, various ways. I remember once, I presented some of that material at a conference of people from company locations all over the country—chemists, engineers, all sorts of people; management people. And Tuck told me of one man who was in our downtown office and at a very high level, although I don't remember what his title was, and he said that fellow was listening to my paper, and Tuck said his face was getting redder and redder. For a while he thought the guy was going to explode. And Tuck took him aside and tried to explain to him why it made sense for us to be doing this sort of work.

On a couple of occasions, I was taken aside by people at very high management levels and asked what are we doing in the mass spectrometry laboratories these days and why. I had come to appreciate fairly early in the game that I've got to be able to give reasonable answers to such questions. And I gave them my answers. I could see two lines of reasoning to justify it. The first was that there was enormous potential for analytical work—for qualitative work, for structural work, and quantitative applications. And that beyond what had already been published elsewhere, unless we did our own exploratory work and began to develop clear concepts of the underlying chemistry, we were going to miss out on the basis for the claims of analytical results that could reasonably be expected to be useful in all sorts of problems that are going to arise over the years.

Now it took a long time for that to sink in and somewhere along the line I remember learning through the grapevine that the question of the justification of much of my work outside applied service work was raised regularly once or twice a year at very high management levels. But the same grapevine reports told me that every time that happened there was always somebody there at that meeting who was prepared to get up and answer the questions, to state why it made good sense for the company to be supportive of this kind of exploratory work.

I know that in some cases I was surprised to find out from whom that support came. But it came in particular from one guy, whom I knew only very slightly, and I had not known previously that this guy was more than vaguely aware of anything I was doing. But obviously he was, and he understood it, and it made sense to him. And every time the question arose he got up and he told them why.

GRAYSON: And this was...? The name of the person?

MEYERSON: That was Edmond Field. He had come to our company as a senior scientist from duPont [E. I. du Pont de Nemours and Company]. And it may be that his experience at duPont helped him to understand much better than he might have otherwise the sense of what I was doing. I don't really know. I've never had occasion to discuss that with him.

GRAYSON: But you do know that he championed the work and responded to questions in ways that could have been damaging to him?

MEYERSON: Yes, that I learned through the grapevine on a number of occasions. Now, the second justification that I offered was that we have a system here in which there is chemistry that takes place that we have developed some tools to explore. We could find out what's going on there, at least in part. And much of that chemistry is quite unorthodox. It's very different from what organic chemists conventionally expect or guess at or write. And we are convinced that organic chemists can use this kind of insight as a source of ideas of unconventional chemistry to look for. So that, this was the second justification.

Now, that second one was one that we had tried to sell. Henry and I had tried very early in the game to get that notion across to organic chemists around our place; but with very little response. Generally, the organic chemists that we talked to would listen carefully and found the chemistry interesting, but then said, “Now, what are you suggesting I do to follow up on it?” And really neither Henry nor I was enough of an organic chemist to be able to answer that question adequately.

I should add that Rylander had left our company in 1956, so he wasn't around. He might have been able to convince other organic chemists much earlier than we could. So nothing much happened in that line until some time in the early sixties when Ellis Fields—Ellis is an organic chemist—picked up some of these notions. And he evidently took our suggestion seriously.

And for quite a period of time, he come down to my laboratory and we would just get out collections of mass spectra, the API/NBS series, and we paged through the material and from time to time found what looked like an interesting molecule. And I would proceed to speculate about what I thought might be going on there. Ellis would write little notes to himself, and he'd go back upstairs to his laboratory and dig out the compounds that we had spotted and just pass them through a hot tube and collect the black gunk that came out the other end and send it to me for analysis. And in a remarkable number of cases, we were able to find evidence that said that something was going on with simple heat treatment that looks to be very, very close to the things that seemed to be going on under electron impact. And that started my collaboration with Ellis Fields.

As you know, he and I have published a huge number of papers jointly. We learned very quickly that for any system that we looked at, we looked at the behavior in the mass spectrometer under electron impact and behavior in a conventional chemical system, whether it's driven by heat or as a photochemical reaction, we looked on those as two sides of a coin. We said they've got to be related somehow. There's some connecting links, generally, and let's see what we can find. And in any number of cases, we started with what we knew in one place. We were able to find evidence of similar or related processes occurring in the other system as well. And we published a great deal of material on one or both sides of that system—growing out of it.

GRAYSON: So this work then, once it got underway in your new location, really led to more work along the same lines. More fundamental studies of a variety of different compounds in a pure form?

MEYERSON: Yes. Very early in the game after the first few papers, I started receiving requests from academic people for help with mass spectral analysis in connection with their problems. And usually these were cases where people were using some sort of stable tracer labeling and they wanted help with isotopic analyses. The first several of such requests, I went to Tuck and told him what the request was and asked for approval to do the work, and he never questioned it. And after the first several times, he just gave me a green light and said I should just use my own judgment. If it looks at all reasonable, go ahead. If it looks like it's going to take an undue amount of time and effort, then feel free to turn it down. But otherwise feel free to take it on. And of course that gave me access to a lot of labeled compounds.

GRAYSON: So these would be instances where people would actually send you samples to analyze? Or they saw the utility of mass spectrometry but didn't have access to the equipment?

MEYERSON: No. There were not very many mass spectrometers in academic laboratories at that time. And for all practical purposes, it was the only way to follow label retention in a chemical sequence to visualize the chemical reactions that were going on. Sometimes I could get more information; sometimes not very much. It varied. I never made any commitment in advance. But in any case, it gave me access to materials and, in addition, it also gave me materials even without labels that I didn't have ready access to otherwise.

GRAYSON: So in many ways there was a benefit to both sides?

MEYERSON: Yes.

GRAYSON: You got a chance to become acquainted with a much broader range of compounds, both labeled and unlabeled, than you would have just in your work environment, and they got the benefit of getting information that they had no other way of getting.

MEYERSON: Absolutely.

GRAYSON: So it was a very good collaboration…that type of thing?

MEYERSON: Yes. And in some cases, these typically started out as very informal kinds of arrangements, where something useful might come out of it, something useful to one or both of us, or it might not. We didn't know in advance. But in a number of cases it developed into really full scale collaboration, so that I have several joint publications, for example, with Harold Hart. He worked at our laboratory one summer and furnished me a number of labeled compounds. I have quite a number of publications with [Gerasimos J.] Mike Karabatsos, also from Michigan State, including work on protonated cyclopropane in solution.

I think my first contact with him came through Harold Hart. One summer Mike spent part of the summer working in our laboratory with our mass spec group. He actually divided his time; he spent part of the time upstairs in the organic area doing the synthesis and then came down and worked with me on the mass spectra.

Ernest [L.] Eliel spent one summer at our laboratory when he was at Notre Dame [University]. He had already a pretty well established reputation in stereochemistry. He was well enough established, when he came to spend the summer working in our laboratories that the people, the supervisors, the administrators in the organic area where he was assigned told him to take the first week or whatever he needed to wander around and talk to people in their area and see what they were doing and pick the project that he would like to work on. And he found the collaborative work that Paul Rylander and I were doing, decided that's what he'd like to work on. And he spent that summer working with us.

Now what that meant was that he spent most of his summer synthesizing organic compounds, and particularly labeled organic compounds, for this work. He was tremendously productive. He loaded us up with so many promising and useful compounds that we must have spent a lot of time over the next maybe three or four years collecting the mass spectral data, working through it, and interpreting it.

I made any number of trips down to Notre Dame [University] to talk face-to-face, spent time with Ernest working over the data and getting the manuscripts ready for publication so that we have quite a number of joint publications that came out of that.

My work with Leonard [C.] Leitch at the National Research Council in Ottawa [Canada] is in the same category. A couple of things that I published with a guy at McGill University Jack [John T.] Edward came in the same category.

Somewhere along the line, I think twice, when Consolidated organized a three-day school of mass spectrometry, independently of any technical meeting, somewhere on the East Coast, in the New Jersey area, they asked me to give one of the lectures in this three-day course. And at one of those, I became acquainted for the first time with Catherine Fenselau and found that she had been doing some work on, I believe it was a seven carbon heptaldehyde, heptanal. I had already gotten started on some of the work trying to make some chemical sense out of the mass spectra of aldehydes in work done jointly with Leonard Leitch at the National Research Council in which we worked with pentanal and hexanal. I'm not sure, we may have worked with butanal as well..

And then about the same period of time, I learned of some people that the Department of Agriculture at their experimental laboratory up in Peoria [Illinois] who had been working with nonanal had some mass spectral data on a labeled nonanal, so I proposed and these other folks all agreed, let's put it all together. And the one preliminary communication and one full paper that we ultimately published on the mass spectra of aliphatic aldehydes came with a list of six or seven coauthors, about four or five different laboratories, but it was a far more comprehensive treatment than any one of us could have done alone.

GRAYSON: It must have been fun getting it organized.

MEYERSON: It was.

GRAYSON: Who was the lead person on that?

MEYERSON: Well, I really sat in the center of it and I did the first draft of both the preliminary communication[21] and the full paper[22] following it. Now, I think, for the preliminary communication Catherine expressed interest in being listed as a senior author and nobody else had any problem with that and we published it that way. I think that I appeared as a senior author in the full paper subsequently, but it really wasn't important.

[TAPE PAUSED]

MEYERSON: …Wiley has been kind enough to continue my complimentary subscriptions to the various mass spec journals that they publish.

GRAYSON: Yes, that's a nice favor for them to do that for you.

MEYERSON: Yes. I landed on those lists by virtue of being a member of two editorial advisory boards for lots of years. And they have seen fit to leave me on those subscriptions. And the journals keep coming, and it's very helpful in maintaining at least a moderate level of continuing contact with the field.

GRAYSON: When we broke prior to lunch, we were discussing the fact that you had published a number of papers with a variety of coauthors. If we could pick up on that. You pointed out that the majority of those coauthors are outside of your industrial organization.

MEYERSON: Yes, about sixty of a total of eighty. And one that I failed to mention before—and I certainly wouldn't want to leave him out; he was a very important one—was Fausto Ramirez. Fausto was, as you probably know, an organic chemist whom I first met when he spent a summer at our laboratories also. The same summer Harold Hart was there, I think. After that, it was quite a long time before we first collaborated on anything. But in the last several years before I retired, we did a lot of collaboration; particularly on organophosphorous chemistry and problems in that area. And some of the work of which I'm proudest came out of that.

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[21] Catherine Fenselau, John L. Young, Seymour Meyerson, William R. Landis, Edward Selke, and Leonard C. Leitch, "Cyclobutanol intermediate in the decomposition of aldehydes under electron impact. A further photochemical analogy," *Journal of the American Chemical Society* 91 (1969): 6847-8.

[22] Seymour Meyerson, Catherine Fenselau, J. L. Young, W. R. Landis, EdwardSelke, and Leonard C. Leitch, "Organic ions in the gas phase. XXIV. Reactions of aliphatic aldehydes under electron-impact," *Organic Mass Spectrometry* 3 (1970): 689-707.

GRAYSON: The summer employment of these people, was that something that was done by your organization or did that evolve from the contacts that you had made as a result of your work?

MEYERSON: No. As far back as I can recall, at least, maybe not the first few years, but certainly since the early fifties, the company every summer has hired a number of university and academic people to work in the laboratories for the summer. They felt that this was a useful two-way kind of thing; that it would help to build good rapport with academic people; and that these folks were likely to bring ideas and knowledge and experience into our organization that could be helpful to the professional goals of our own people. And that's been just part of the pattern for lots of years.

GRAYSON: Given that it has been the pattern since the fifties, do you think that those goals were actually met?

MEYERSON: I think so. I have already mentioned that I established working relationships with Fausto Ramirez and Harold Hart, when I first met both of them there in 1954, and those continued all through the years. So they were certainly valuable to me. I know that I've learned from every one of these other people that I collaborated with, whether they were in our company or anywhere else. And I'd like to think that they all in turn learned something from the experience in working with me as well. But certainly, from where I sit, it's been very, very valuable and very personally rewarding. So that over the years I really built up a large network of friends and acquaintances in laboratories in this country and much of the world, and it's been delightful.

GRAYSON: Do you perceive that a similar thing has happened with colleagues of yours in the research laboratories at Standard Oil [of Indiana]. Are there other individuals that you're aware of that were your contemporaries and coequals who did the same thing or achieved the same kinds of rewards from this exchange?

MEYERSON: I can't really speak for any other organizations. I worked for just the one for so many years.

[END OF TAPE 2, SIDE 1]

GRAYSON: ...that there are other people who were your coworkers who had the same positive experience.

[TAPE PAUSED]

MEYERSON: Yes. I've known quite a number of people in the organization there who have had the same kind of experience to a greater or lesser extent and have also found it highly rewarding and certainly conducive to professional growth.

GRAYSON: It seems like an extremely liberal attitude that the company had, kind of a very open-minded approach. It sounds like it was a good approach and a good way to do things. It's just that it doesn't have or wouldn't have what you would call concrete, real specific hard payoffs for the company. So it would be more of a very soft payoff in terms of interactions and networking and that kind of thing. Obviously, industrial organizations do have a focus on making money. I find it kind of interesting that they would support this type of a thing, which cost them some money over such a long period of time, and then continue to support it through thick and thin. Obviously, there were good years and bad years for Amoco [Corporation].

MEYERSON: Well I think what we're talking about here is closely akin to the questions that, from time to time, came up in connection with the kind of work that I was doing. To answer the question of why I was doing what I was doing. And how's what I'm doing going to help the company make a buck. Some of my answers there also tied in with this kind of consideration.

I came to know personally a great many people in industry and academia and in government laboratories in this country and abroad who were darned good. And there have been a number of times over the years when I had some problem that I was struggling with. It might have been in my own exploratory work or it might have been in connection with some very practical, down-to-earth applied problem that I wasn't quite sure how to cope with, but where I could make a pretty good guess of who was likely to be able to help me. And I could get on the phone and call somebody anywhere, wherever he might be, and reach him and get the benefit of that kind of acquaintanceship.

In addition, the personal relationships that grew up there paid off in connection with recruiting in many ways. For example, at McGill University and at the National Research Council in Ottawa, both of those places, I had quite a number of very good friends in both of them. And they were awfully helpful to me. First, when I made a visit to either of these, they helped make the visit very pleasant. At the National Research Council, every time that I was up there, Fred Lossing would always have a bunch of people to his house for a party for me every time. At McGill one evening during the two or three days stay that I had there, I always took George Just and Jack Edward and their wives out to dinner. And the other evenings I would be a dinner guest in their homes.

But in addition to that, they were more than helpful in helping me size up candidates. I can recall vividly either George Just or Jack Edward asking to see the lists of their doctoral candidates who had signed up for me to interview and telling me, “That guy is a deadbeat. Forget about him,” or telling me, “You know I have a student here whom I don't find on your list and who ought to be here. He's one of the best we've had in years.” And he'd chase down to the student's laboratory and chase him up to see me, even though he had not signed up in advance for an interview.

In one instance, we had a candidate who had come in cold, had approached the company. He was working in an academic institution in Chicago and apparently there was somebody high in our management who was an old family friend of his and he worked through this guy and a request came in such a way that people in research didn't feel they had any choice. We had to essentially drop everything and schedule an interview with this fellow, fast. That interview took place on a day when I happened to be off sick or something. I wasn't there. And the fellow made a remarkably good impression on almost everybody he talked to. It was clear when I got back, they were talking about making him an offer.

As it turned out, the fellow had been on the staff of the chemistry department of the University of Houston some years earlier. And had left there, gone somewhere else, and then ultimately to some school in Chicago. But at the time I was scheduled to pay a visit to the University of Houston and to talk before a seminar there with my host and long-time friend, Jerry Meisels.

And so the way we left it was that I would question Jerry about this fellow when I was down there and our management would not do anything about following up the interview with this fellow until I got back. When I mentioned this guy's name to Jerry, first he said excuse me, he went and closed the door to his office, and then he came back and told me the story. And there was a long, terrible history.

He said during the time that this guy had been there, he had all but wrecked the department. He did so much damage to students, to support staff, he had alienated almost everybody on the professional staff of the department. After he had been given his notice, he had maybe two years in which to find a job elsewhere. He had, with no authorization, spent huge amounts of departmental money. It was a very bad experience. He had one or two graduate students in the mill at the time that he left. They were left hanging. For one of them, there was no way that anything could be salvaged from his research.

GRAYSON: So the guy had the credentials and he interviewed well, but basically you didn't want him?

MEYERSON: That's right. And when I brought back that report, believe me, it played a large role in the ultimate decision. Things of this kind are... it's hard to put a dollar value on them, but they make a difference. And the kind of professional reputation and professional contacts that I had built up were valuable to the company in many such situations.

GRAYSON: Earlier, in the very beginning, you talked about the fact that there was this dichotomy that some people sought between the applied work and the research work. During your career in the fifties when you were doing all fundamental things, did you have to deal with applied problems as well or problems with more mundane...?

MEYERSON: Always.

GRAYSON: What kinds of things were you doing in this environment?

MEYERSON: After we moved into the new laboratories in 1948, very gradually I worked my way more and more into qualitative analysis and molecular structure work. In large part, by virtue of the conceptual connections, let's say, with the exploratory things that I was doing... Ultimately after we moved into the new location in Naperville [Illinois], that was 1970, from that time on, well I was assured by my division director, not too long after we moved in there, that the kinds of challenges questioning my activities that had used to happen at high management levels years before had not happened for a long time. And I could be confident that they would never happen again. That the management hierarchy all the way up and down the line understood by then that a great deal of the kinds of things that I was doing to assist people with their own programs at various times, I was able to do because I'd been doing my homework for the previous twenty-five years. And they were confident that there would never again be any questioning.

By the time we'd gotten to Naperville, we were a much larger mass spectrometry laboratory. And at various times we had four and five and six and seven instruments and several technical people in the department, quite a number of support staff, and a division of labor that was unavoidable so that the quantitative analysis that we continued to do, the qualitative analysis, more or less at a simpler level, and a lot of the other things, type analysis, these were chiefly in the hands of other people. And a large part of the calculations and interpretations were computerized by that time.

I found myself on the receiving end only of problems that people had already tried to attack by other easier and faster and cheaper means and had not been able to cope with effectively. So that I was frequently working with molecular structure problems, sometimes awfully simple, sometimes very complex. I can think of one case in which the material that came to me was an additive that our company had been purchasing from some other firm and putting into some of the products. The vendor was not willing to reveal the chemical composition. My problem was to define the chemical composition and while I was at it, I found that from the impurities, the contaminants that were mixed in with it, I could state with a great deal of confidence what the entire synthetic procedure had been that the supplier had used to make the compound. Now, this is kind of an extra dividend. But it was not uncommon to find things of that kind.

A good many of my problems had to do with additives, both in petroleum products and in polymers. And there was a whole category of additives that were used in polymers that were put there in order to minimize or eliminate color development in the product. In one category of problem situations that came to me over a period of quite a number of years in which our product, our polymer, had been used to make a commercial product, and after the consumer product had been manufactured and had sat on the shelves in a warehouse somewhere, surfaces that had been treated with a white pigment were turning yellow, and people were unhappy about that. And what I established there was that the additive that was supposed to prevent color formation was the source of the yellow color. It was photosensitive, and if you got it out in the sunlight long enough the color would go away again. But then you stored it somewhere away from the sunlight and the color would come back again.

GRAYSON: So it wasn't good at preventing the development of coloring in a bright environment?

MEYERSON: And it was not too hard really to find some other additive that could perform the function just as well and would not produce the coloring. Furthermore, after we had been through that the first time, over a period of several years, the same problem developed again and again, kept coming back to me. In each case I had to do enough work to confirm that it was actually the same problem. It was.

GRAYSON: When I talked to O. P. Tanner, and it was also an industrial environment, we did address the issue of a kind of a dollar payoff in mass spectrometry. A lot of the time the instrument can be used to find out information along the lines we're discussing. In. O. P.'s case it was a very costly plant operation that was being developed along certain lines. And given the mass spec data, they were able to change the development along the lines that ended up making it successful rather than if they had gone along with the original plan in which the plant production would not have performed as desired. So clearly in that case, the cost of the instrument was a small fraction of the money it saved the company in the real world situation. Can you think of any similar incident that had that kind of a big kick payoff for your organization?

MEYERSON: It's a little hard to answer that question adequately because it's such a long time since I dealt with the kinds of problems that were involved there. I do remember some years ago when one of the in-house publications of Amoco's carried a story about the development years previous of... I'm trying to remember what the process was, and I can't even think of the proper name of it, right now. But in any case, a major petroleum refining procedure in which credit was extended to the people who had played a role in all sorts of brilliant ways to make that process feasible. I think this was platformate. I don't recall anything specific that I did there, except for what were then perfectly routine mass spec analyses of samples coming from the plant during that time. But somebody evidently felt that the kind of analytical support that I was able to furnish merited my being listed among the people who played crucial roles in the development of the process. This kind of thing has happened on a few occasions at least. Now that's not a very specific kind of answer, but it's the best I can do.

GRAYSON: Well I think it addresses the issue. Frequently, particularly as this equipment becomes more expensive, it seems that it's harder to justify in a lot of industrial, or in any environment, and myself I see that after the fact, it seems as though many organizations want a piece of equipment. But when it's purchased, there's a lot of assumptions made that basically it's doing whatever it was that it was bought for. And management seems to ignore or forget it, which I guess in many cases is good and perhaps in others isn't. But I mean in other words, frequently no one ever bothers to find out well…Meyerson told us if we bought this piece of equipment then we'd be able to handle these problems. So they buy it but nobody ever comes around to find out, to kind of check up and see whether or not that was successfully done.

MEYERSON: Perhaps a kind of indirect answer to the question might help. Over all the years that I've worked for the company, I do not recall any time that I or Henry Grubb or anybody else working with me in there in mass spec had to go to our management and sell them on instruments that we wanted to buy. We had to let them know that we wanted something and give them some notion of what it was for and beyond that essentially our management was convinced very easily every time that the investment was going to pay off.

The most vivid recollections I have in connection with the purchase of new equipment are that, when I was having trouble and taking more time than had been anticipated to decide precisely what equipment I wanted, I found myself being needled to get on the ball and let us know what you want us to buy. This happened repeatedly all through the years I was with the company. I felt that whoever the individuals may have been, there were people in our management who had to make decisions of this kind, who had developed and who continued to maintain over the years a lot of confidence of our whole mass spec team. So that they did not give us a hard time. They really just wanted the benefit of our best thinking about what equipment we ought to have and that would be useful.

GRAYSON: Was that a fairly short turnaround time? Typically would a year be sufficient to recommend the purchase of a piece of equipment then to have it issued a purchase order?

MEYERSON: Well sometimes more and sometimes less. But that's a pretty good ballpark estimate. Often once management had agreed that some sort of instrument was to be acquired and they put it in the capital budget, they really would have liked us to move as quickly as possible. And I found myself trying to hold them off.

GRAYSON: Then I take it a good assessment of what the vendors had to offer at that time and which would be the best piece of equipment for your particular applications, does that require that you would want to hold off a little bit?

MEYERSON: Yes, it varied. And there certainly were times when we submitted samples or measurements to two or three or four manufacturers and there were times we wanted to get results. Sometimes it took a long time to get those results. And there were times when we had a pretty good notion of what equipment we wanted. But if we were not satisfied with it as it stood, and we insisted that the manufacturer needed to do some modifications, some further development, and that took time. So that it varied from one situation to another.

We took a long time, for example, before we got high resolution. We didn't buy our first high resolution instrument until 1966, I think it was. And certainly there were a number of laboratories that had it earlier than we did. And we had spent a lot of time and effort before reaching that decision.

GRAYSON: There are obviously a number of vendors who were into the business of providing instrumentation. Would you care to comment on promises and delivery? Frequently vendors are pretty liberal about what they say their equipment can do, and then not so infrequently when it's on the floor, it's not so easy to meet some of those specifications. Do you have any anecdotes or stories that you'd like to share with us about those events or perhaps a piece of equipment that really turned out to be maybe a lemon?

MEYERSON: By and large, all through the years while the CEC instruments were being made and marketed, by and large our experience with them was pretty doggone good. Occasionally there were at least one or two people that they had in the field that we tried very hard to avoid. There were certainly times when we felt very much more comfortable doing our own maintenance than having the vendor do the maintenance work. Partly because we didn't trust the field people that they had out in our area at that time. We would prefer that they keep their hands off our machines.

I recall about the time very early in the game, about the time that we moved out of the original laboratory and refinery to New York Avenue that, for the new machine that we bought there in New York Avenue, we turned down the offer of a service contract. We did most of our own servicing. Occasionally we had to call in somebody from the vendor. But at that time I remember that Grace Marsh who remained with the machine in the tech service laboratory and was responsible for its operation decided to buy the service contract.

For some years the service contract included, I think, one or two regularly scheduled visits a year for preventive maintenance, aside from anything else. And I recall vividly that every time there was a call for preventive maintenance at Tech Service, they shut the thing down. The field man did whatever he was going to do on it, and he tried to start up again, and the machine was on the blink. This happened constantly. It got to be a real long-time standing joke. And the moral of the story, which is an old moral to anybody working with equipment is: “If it ain't broke, don't fix it.” [laughter] That you shut down the equipment and you change the temperatures of parts to all of the circuitry and something is thrown out of kilter and you may be down for a couple of weeks. This just happened repeatedly.

I recall also one instance in which — this must again go back to somewhere in the ballpark of 1948 or thereabouts—we were having trouble with the analog computer that we bought from Consolidated. The guts of this computer consisted of a great big drum with twelve rows of twelve helipots each. So it's 144 potentiometers in a network. And if any one of those helipots was thrown off, it affected all the other 143. They did not operate independently of each other. And so realigning these got to be a very hairy experience.

Well we developed a whole slew of rules of thumb for coping with it. And for the most part we managed pretty well. But on at least one occasion we got into a terrible bind and we just didn't know where to start anymore. We wrote a letter to Consolidated describing the symptoms and asked them for advice. And we got back a letter with a lot of long words and a lot of what we interpreted as pure double talk that said in effect if the thing is out of alignment, you just have to start working at it and realign it. [laughter] And one of my coworkers wrote a little note in the margin of the letter: “Very sage advice.” [laughter] So this kind of thing happened.

The time came when we were in the market for other instruments. We were checking out other manufacturers and we made a firm decision to go the way of VG rather than Kratos for some of our newer equipment. At present I think we have both Kratos and VG equipment. I shouldn't say we; my successors, the other folks who are still in the laboratory have both. But we visited a great many laboratories and talked to people, and got the benefits of their experience with these firms. And VG, as nearly as we could size up, seemed to be very much more responsive to their users' problems and needs than Kratos, at least at that time. I don't think that we ever regretted that choice.

GRAYSON: You mentioned purchasing a high resolution instrument at that time. Obviously the choice would have been between electrical detection and photoplate detection, as one of the things you had to deal with. How did that come out when you essentially addressed that issue? How did you end up?

MEYERSON: We got a CEC 21-110 which offered us both photographic and electrical detection. We were not able to clearly rule out the need for photographic detection at the time we were trying to size up what was available and how we were going to use it. We made a moderate amount of use of it over the years. Not as much as we thought we might. But we felt that... Now Chuck [Charles F.] Robinson and I shared in that decision and we were both convinced that there was an advantage to having both available.

GRAYSON: There's always been an issue—at least there was an issue for a long time—which would be preferable? I suppose that with the 110, you could have both detection schemes gave you more flexibility. Earlier when we had a little discussion after lunch, you mentioned that the company made names of employees available to local schools and universities as speakers. I'd like to go over that material again. What was the company's interest or purpose in this type of thing?

MEYERSON: Our management was pretty well convinced that even if for nothing else, purely for recruiting purposes, there was a great deal to be gained by our people knowing personally and having good personal professional relationships with academic people. The company was very careful to separate visits to give seminars from any recruiting visits. Within this country they never combined them. Our visits to give seminars and to just become acquainted with the departments were set up entirely independently. And generally there was no talk at all about recruiting during that time. We felt that if we and the academic people in departments knew and respected each other, then whether it's recruiting or other things, there would be occasions when we could call on each other and trust each other. In addition there were many times when spending a day in a department and talking to maybe a dozen people in the department during the day I would learn enough about what the kinds of research that people were doing there that I could bring back some suggestions or ideas that could be helpful to other people in our organization as well. So there were all kinds of advantages.

In addition there were times that our company has for many years contributed through its foundation, made financial contributions for the support of academic institutions and, particularly for chemistry and chemical engineering departments, has funded fellowships. And there have been a number of occasions when... And of course when I made visits to an academic department, we would learn of some specific kind of need or problem within the department. As a result of which our company might make a gift of some piece of equipment to the department. We helped each other in various ways.

There were a number of times when personal contacts that were built up during these visits led to expression on a part of academic people of their interest in spending the summer working in our laboratories. And that was worked out and they came and spent a summer working with us.

GRAYSON: So basically it sounds like the company had decided at fairly high level to be a good scientific citizen and pursued that very sincerely and not just as a facade or window dressing. I mean they really were interested in being a good scientific citizen in interacting with the community.

MEYERSON: Absolutely. I think that my visits to McGill, University of Montreal, and National Research Council in Canada were the only instances in which we combined seminars, professional, and personal visits of that kind with recruiting. That was kind of a historical accident because the contacts with these institutions occurred first when I was invited to speak before seminar groups up there. And I found myself sufficiently impressed by a lot of the people up there that I urged our company to institute recruiting, to extend our recruiting activities to them. And a number of years we did that, but I handled both and combined the two in a single visit. I spent three days in Montreal [Canada], and two or three days in Ottawa [Canada].

After the first several years I found that the number of people who signed up for interviews was large enough that it would have been just hard for me to cope with them all. And Ellis Fields started to go along with me on those visits, and we would divide up the interviews. And then somewhere along the line, I dropped out of that entirely and Ellis continued for a number of years. So that we maintained the contact and the pattern of relationships with those institutions.

I can recall, incidentally, a fascinating situation. The first time that Ellis made that trip with me, at the National Research Council there was an explicit ground rule that they would not accept any professional interviewers who are not themselves technical people. And furthermore, if they were technical people who had moved into administration, they were not acceptable. They needed to be technical people involved in technical work. Now, we were in a perfectly good position there because all the professional interviewing for our research and development was done by technical people doing technical work wherever they went. And there was never any question.

But the first year that Ellis made that trip with me, he was not well acquainted up there, and people didn't know what to expect of him. He was asked explicitly what his background and work consisted of and he spelled it out. And even so, some of the people that we went around to talk to were a little suspicious.

I remember Ellis telling me of one instance where, after we had interviewed the postdoctorals finishing their work there, we were both individually making the rounds of staff people under whose supervision these postdoctorals were working, to talk to them about the interviewees. And there was one laboratory that Ellis walked into where the chemist whose laboratory it was had been discussing some organic chemical problem with his postdoctorals and they had the blackboard covered with chemical symbols, and when Ellis came in, the fellow immediately called him over and asked him, “What do you think about this chemistry?” And Ellis said that there wasn't a moment's question in his mind that this fellow was testing him.

GRAYSON: Why do you think the school was so particular about...?

MEYERSON: Well, this is not a school. This is the National Research Council.

GRAYSON: Okay.

MEYERSON: Their training is all at a postdoctoral level. I think that's a good policy for all institutions where they train people for professional work.

GRAYSON: That they should be hired by people doing the technical work.

MEYERSON: Absolutely. I mean it is a terribly tough task trying to size up people in a short while, with a half hour, forty-five minute, or hour's interview. Under the best of circumstances, these interviews always left me terribly uneasy. I always leaned heavily on the subsequent information I got by talking to the staff people under whom these candidates had been doing their work. And many of these, of course, were people whom I'd come to know well over the years and whom I trusted.

And even then, there were many times when I brought back recommendations to our laboratory, that we should, or should not, invite this candidate to come to Whiting or to Naperville for an interview. But I made that recommendation with a great many misgivings. I felt very unsure. I mean you know that what comes out of those interviews can make a world of difference in the entire life of the individual and can make an enormous difference to the organization that's considering hiring him. And I don't have that much confidence in my ability to size people up in short order. After I've known them for several years and I've had a lot of dealings with them, it's another matter.

GRAYSON: Yes. Sure.

MEYERSON: And I'm confident that the ground rule under which the National Research Council there operates is definitely sensible. And, while it doesn't solve the problem, it at least helps.

GRAYSON: Yes. It addresses the issue and then tries to keep it from getting worse than it is. I mean, the problem from being even worse.

When we had a little walk after lunch, you'd mentioned that in the laboratory there had been a particularly depressing period when there were some layoffs. I'd like to review some of the things that you talked about then. Although you were still employed after this initial layoff period that you had been counseled by people who respected you that perhaps it would be a good idea to look around.

MEYERSON: Yes. This was in 1961. The company had cut back costs, expenses throughout the entire organization, all aspects of the work. And R&D was no exception. One Friday afternoon the entire professional staff of R&D at Whiting was called in to one big meeting. We were given the word of what was happening. About twelve to fifteen percent of the professional staff was being laid off. A great many research areas that had been part of our program for years were being simply eliminated. It was the start of a long period of great depression around the organization.

Many people who had not been laid off decided that this was not the kind of thing they wanted to live with, and they left of their own free will and went elsewhere. And nobody around the place could avoid thinking in terms of possibly making a change during that time.

It was somewhere during that period that Tucker, who was then my division director, called me in to his office and offered a bit of fatherly advice to me. He expressed his great discouragement. He could see no hope that things were going to get better there in our organization. He pointed out to me that I had built a good professional reputation, and that I should have no difficulty in breaking into the academic world, and he urged me to do that.

GRAYSON: You had about fifteen years' experience then? From 1946 to 1961?

MEYERSON: Yes. It may have been 1962 or 1963, but it was in that time frame.

He also reminded me that if I did decide to follow his recommendation he would further advise me not to agree to anything without having a very clear and firm understanding first as to how I was going to get that union card, the doctorate. And he pointed out that while there in our laboratory as in many industrial laboratories the doctorate may play a large part in determining the starting salary of an individual, after a few years it really doesn't make too much difference any more, that what one has done on the job is far more important than what degree one may have brought before coming. But that in the academic world, it doesn't work that way. And he urged me strongly, and as I mentioned to you also while we were talking, over a few-year period there on a number of occasions I was approached by academic institutions with offers.

And, in particular, I remember vividly the first such offer. It came from a fine, large chemistry department in a major state university. There was an offer of a full professorship, with tenure. And the man who approached me on behalf of the department, whom I had come to know pretty well for some years by that time, told me without my raising the question, that when the department chairman asked him to talk to me, he had reminded the chairman that I did not have that higher degree, and he wanted to be sure that it was clearly understood so that there would be no embarrassment later. And the chairman's response was that he was well aware of it, that he was confident that we could work out a program, so that if I did come I could be confident of getting an earned doctorate within about a year.

Maybe it would have worked out; maybe it wouldn't have. But I found that offer very flattering, I appreciated it a great deal as I did other offers by other institutions later. But in each case, I ultimately decided to stay put. That even though things were bad in many respects there in our laboratory, the atmosphere was very gloomy much of the time, it seemed to me that the half dozen or dozen people in the organization whom I regarded as our outstanding professional people were still there. I felt that as long as that nucleus was still there, there was hope. And if you make a change, no matter what the circumstances may be, there is also a large gamble. There's always inevitably a large gamble involved. And I decided to stay put.

[END OF TAPE 2, SIDE 2]

GRAYSON: At this particular point, we had just finished discussing this depressing period, as you mentioned it. I was just curious, obviously there was a money problem with the company at the time. This was in the early 1960s?

[TAPE PAUSED]

MEYERSON: The early 1960s.

GRAYSON: I'm trying to think, what would have made the oil industry...?

MEYERSON: I don't really remember specifically, but over the years there had been times when it's been very easy to get money within the company and there have been times when it's been very hard. There have been periodic sharp cutbacks that often took place in all departments. R&D as well as manufacturing and everything else. I would say that this was one of them and this one hit R&D particularly heavily.

GRAYSON: So it was just part of the normal ebb and flow of the industrial environment, and you were really hard hit by this one?

MEYERSON: I think that's right.

GRAYSON: Well I think we've covered a lot of territory. I would like basically to maybe hit some of these questions that are areas that we have outlined here.

You've worked in this area for many years and have done quite a bit of important fundamental work. What would you characterize as the single most...? What do you think of as the highlight of your career or research in this area?

MEYERSON: I don't really know any one answer to that. There were quite a number of pieces of work that I had a hand in that I think back to with real pleasure. And from time to time in looking for some reprint, I will page through a number of them and I find quite a number that I'm really very pleased with even years afterward. Certainly, the one publication we have that attracted the most attention was the tropylium paper. I don't think there's any question about that. And, in connection with that, I might add just one little item.

Our original tropylium work was done with strictly deuterium tracing. But subsequently we took our first stab at using a ^{13}C tracer, in which we labeled toluene with a ^{13}C atom in the α position. Now if our picture was right, if after the loss of the first hydrogen, the $C_7H_7^+$ ion that's left attains complete symmetry so the various carbons can't be told apart, and if then two carbons are broken out at random from the seven carbon ion, then you're left with five. Then, if you have one tagged atom in the $C_7H_7^+$, the $C_5H_5^+$ ion should be five-sevenths or 71 percent labeled.

I remember when I got that spectrum out and calculated the label retention in $C_5H_5^+$, I got on the telephone and told Paul Rylander that the experimental figure was 69 percent. Paul's response was, “Ain't it grand when nature's on your side”.

GRAYSON: Yes, yes.

MEYERSON: There is another item that comes to mind that really hasn't much to do with organic chemistry—and I don't think that very many people even who have followed a lot of my work are specially conscious of it — but I think it's also in about 1961; a lot of things happened in 1961 I think;—that I was doing some work with deuterium label. This was work with Ernest Eliel, as I recall. And working with deuterium labeled chlorine compounds. And when you collect the data on the fragment ions and go to calculate the label retention, you have to first calculate out the natural abundance, of course, of all the atoms that are there, and that included the chlorine. And I was getting consistently inconsistent results.

And after this had happened with some three or four different compounds, I began to suspect that maybe there's...well, there had to be something wrong with either what the machine was doing or with the data in all the established tables of isotopic distributions. And so I took down about maybe a dozen different chlorine-containing organic compounds from our shelf, measured their spectra on our 21-103, and then calculated out the contributions of heavy carbon and calculated what the isotopic distribution of the chlorine atoms would have to be if they were going to be consistent with our data. There was a difference between the numbers that came out of that, which did not have a lot of spread. I had a lot of data on several different compounds and over a wide mass range of several different ions and the average figure there differed by several tenths of a percent from the then accepted value from most of the published tables of the chlorine atoms.

I wrote up my finding and I drafted something that I considered as some kind of a letter to the editor and I sent it to several people for comment first, including Al Nier. This was particularly appropriate, first because he has had such a large part in establishing the isotopic distributions of isotopes in nature, and secondly because the then accepted distribution of the chlorine isotopes had been determined by him years earlier. The value that I had come up with differed, as I say, by several tenths of a percent from his, and it agreed with another value that had been determined by some people at, I think, a nuclear research laboratory in Canada several years ago and had appeared in some of the older tables but apparently had been discarded in favor of Nier's newer results which were considered better. I had some fascinating responses. Nier said, "This sounds great. Publish it." The fellow from Canada... Chalk River [Laboratories], I think. Is that the name of the lab?

GRAYSON: That sounds familiar. I believe there is a Chalk River facility.

MEYERSON: I think there's somebody up there...he wrote me a long letter. It was longer than my proposed letter to the editor, in which he filled me in on all the possible sources of error. And it was a very discouraging thing to read, believe me. But he was being very cautious. He was certainly not arguing in favor of my data, even though they were in accord with his published data of some years earlier. In any case...

GRAYSON: Are you saying that Nier, whose numbers you were disputing, who said, "Yeah that looks like...it looks good."

MEYERSON: He said, "It looks like good careful work. Publish it."

GRAYSON: But the fellow whose numbers you were supporting said, "Well, here's all these problems that you could be having." [laughter]

MEYERSON: That's correct. Well I submitted the letter to *Analytical Chemistry*, and it was published early in 1961.[23] Later that same year, maybe six months later, the National Bureau of Standards published a redetermination of the natural abundance of chlorine isotopes, which had been a very demanding piece of work in which they had first separated the isotopes and

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[23] Seymour Meyerson, "Natural abundance of chlorine isotopes," *Analytical Chemistry* 33 (1961): 964.

obtained each one in something close to 100 percent isotopic purity.[24] And then after that proceeded to try to take into account the instrumental discrimination effects on both of them. And the numbers that they came up with agreed with mine to about two decimal places. Which was fantastic. They cited my letter, and I appreciated that.

Well at least one fellow in our laboratories—this guy is a chemical engineer—years later reminded me of that piece of work, and he has done so repeatedly. And I've heard him refer to it on a number of occasions in open meetings of one kind or another at our laboratories, in which there was some question about some discrepancy or some piece of data and in which essentially what he said was this. He said, "We have at least one guy on our staff who, after making measurements—the best measurements he had—finds that they are not in accord with the measurements in the accepted tables of data, has enough confidence in his own work to challenge the published data." And then he describes what I did there. I remember the first time that I heard him say that in an open meeting. I'm not sure whether I'd ever even met him personally at that time. But, believe me, it gave me a lift! [laughter]

GRAYSON: Sure. Well, I mean, it has to because it shows that you are definitely in command of the equipment and the interpretation of the results. You're also minding your scientific views 'P's and 'Q's and not running off with a piece of half-cooked information.

MEYERSON: Yes. Now I think you asked me for sources and what kinds of things I've done that have given me a great deal of satisfaction. I think I was fortunate in that I learned a lesson very, very early. I learned not ever to be the least bit concerned if someone wants to challenge our interpretations of the data, but to do my darnedest to be sure that the experimental data that I published are trustworthy. I don't know of any instance of any experimental data we've ever published that were demonstrated to be wrong by somebody later.

A great many of the measurements that we published have been repeated by other people, and often with very much more sophisticated and complex and elaborate equipment. To the best of my knowledge, our published data has always held up. And I think I have a bias in favor of trying to do the work by the simplest procedure and the simplest equipment possible. Now I take a lot of satisfaction, taken a lot of encouragement from the good agreement between actually both experimental data and interpretations that we have published over the years. And the experimental data and interpretations have been followed over many years by many measurements by very competent people with much more elaborate and sophisticated equipment than anything that we used. I find that highly satisfying.

GRAYSON: Well, that's a sign of a good, thorough, and professional scientific career, and that's definitely what we're in this business for, is to do the best we can. And, when it can stand

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[24] W.R. Shields, T.J. Murphy, E.L. Garner, and V.H. Dibeler, "Absolute Isotopic Abundance Ratio and the Atomic Weight of Chlorine," *Journal of the American Chemical Society* 84 (9) (1962): 1519-22.

the test of time and the rest of the published work that comes after it, then it's got to be a gratifying thing.

I'd like to switch over to talking about instrumentation for a while, in terms of development. Clearly, as you've already indicated before, when you first got involved in mass spectrometry, you had to be the feedback circuit for regulating the trap current in the beginning. There'd been a lot of developments in mass spectrometry over these years. Which ones would you single out, or a handful of them, do you think are some of the most important developments?

MEYERSON: Well, I'm not sure that I can pick out one or a few. I learned early that even though we were nominally carrying out chemistry triggered by electron impact, in fact, we learned early that there are other things that can get involved also. That with all the care in the world, we could never completely rule out contributions of simply thermal reactions. Temperature always has an effect. Makes a difference. At the very beginning, in particular, there were enormous contributions of bimolecular reactions that took years of engineering development to minimize...

GRAYSON: Is this by better vacuum systems?

MEYERSON: Of course, of course. Now since then bi-molecular processes have been developed in a somewhat different way and become an important part of mass spectrometry, but in a system in which they are under some control. Field ionization and field desorption have opened up techniques, have given us access to materials that we simply had no way of working with otherwise. We learned early in the game that in a field ionization experiment, there is always a chance, there's always likely to be some contribution from field desorption. And, on the contrary, in field desorption, there is often, I am convinced, a contribution of field ionization—of materials that we have historically not regarded as volatile, but that have enough vapor pressure under the conditions in which we operate, there is a field ionization component. There is simply no such thing as anything that is absolutely, unequivocally pure unadulterated ion chemistry.

The first many years of mass spectrometry, we tried very hard to interpret our mass spectra on the assumption that all the fragments we observed arose by single steps, breaking of one or two bonds in the original molecular ion. It was a long time before we came to recognize that there may be much more going on. That there are reorganizations, rearrangements that may interfere, that there are stepwise processes.

Quite a number of years ago in one piece of work that we published, I think on trinitrobenzene, using traditional, conventional metastable peaks, we demonstrated a long string of processes that went through as many as four, five successive events. This is typical, not atypical, so that we have tried to interpret our data on the assumption of a level of simplicity greater than anything existing anywhere in chemistry. Maybe in part this is naiveté that we inherited from the history of our instruments, which, after all, were primarily originally developed by physicists. Not very much input from chemists.

And we accepted what they told us and went on from there. A number of the early publications point to the appearance of a $C_2H_5^+$ion in the mass spectrum of isobutane as an enormous mystery. And looking back on it, it's hard to imagine how a chemist with or without any direct experience of mass spectrometry could regard that as that complicated and as that mysterious. You could dream up several possible scenarios leading to it. And whatever you decide to accept or not accept, it's not really terribly important.

I can recall being asked in all seriousness by a physical organic chemist of world stature if it's safe to assume that all ions having the same elemental composition have the same structure. [laughter] Now today that seems wild, ridiculous. Quite early in the game, a number of people—I guess David Stevenson and his associates were particularly prominent in this area—started to make appearance potential measurements and to develop numerical thermochemical data from such measurements; on the basis of some assumptions as to what was involved.

And somewhere along the line, it very gradually—believe me, it was gradual—began to realize that there is something that is less than obvious in this whole picture where people found products that could not be produced simply by breaking one or two bonds in the original structure. They had to be the results of a rearrangement of some kind. The common, usually implicit, assumption was that there is some extra energy involved in these rearrangements. And somewhere along the line, if we can figure out how to measure it, we'll find that extra energy.

As time went on, we began to realize that many of these rearrangements at least occur because they allow the saving of energy. They occur because they take less energy than simply breaking a bond. In one of our early papers, we were led to point this out, and say that we are forced to conclude at this point that the process leading to a product involving molecular rearrangement is likely to have required either more energy or less energy or the same energy as a product that does not involve rearrangement. And that's about all you can say if you insist on generalizing.

Again at an early stage—that must have been sometime in the late 1950s—I remember speaking before a seminar at Purdue University, where Frank Field had lectured fairly recently, maybe a month or so before me, and where people were quite shocked to hear me express any reservations about the values of bond dissociation energies derived from appearance potential measurements. This kind of shock was widespread in the literature at that time too.

I think that mass spectrometrists, whatever their own reservations might have been, by and large had succeeded in snowing people concerned with thermochemistry, who felt that they knew so little of what was involved in mass spectrometry they were not prepared to question or challenge the numbers. And they simply were taken aback to hear anybody express any hesitation about taking those numbers at face value.

I guess one other piece of work that maybe is worth mentioning now is some work that I did with Fausto Ramirez not too many years ago on the enzymatic hydrolysis or non-enzymatic hydrolysis of phosphate esters, in which we tried to clarify as well as we could the role of PO_3^-, the monomeric metaphosphate anion. I found that a very satisfying piece of work. We did ^{18}O labeling on that. And I think that we came out of it with about as clear-cut a picture as one can reasonably hope for, at least for the foreseeable future.

GRAYSON: So this is getting a little bit away from the petroleum or petrochemical aspects of the applications. As you pointed out earlier organophosphorous chemistry, which is quite a bit different, I'm sure, than…

MEYERSON: Yes, but...the saying is trite, but the kinds of things that I'd gotten into, have reminded me innumerable times that everything is related to everything. [laughter] And a very large number of the exploratory pieces of work that I had gotten into over the years often just because some compounds were available to me that made it possible. A great many of those gave me insights and led me to write mechanisms and draw conclusions that turned out to be helpful in attacking analytical problems and mechanism problems that came to me from people in our research and development, from people out in the field, and manufacturing, marketing throughout the company over the years since then.

In addition to that there was a whole family of organophosphorous compounds that were utilized as additives in both polymers and petroleum products. So that I never felt any hesitation in putting time and effort into trying to understand phosphates, whatever I could learn of phosphorous chemistry. I didn't feel that I had to explain it or apologize to anybody in the company, and nobody ever challenged it.

GRAYSON: The computers had a big impact on mass spectrometry. Would you care to comment on how you perceived the impact? At one time earlier you discussed the fact that computations were originally done essentially by hand for gas analysis. What do you think about computers in mass spectrometry?

MEYERSON: Years ago when computers were still fairly new in our organization, over a period of a few years, I took two or three courses in computing techniques. There wasn't anything terribly difficult or complicated about them, but there was nothing to ever grab me either. And I never felt any great need for using these techniques in the kinds of things that I got into. So that I have never really established rapport with computers.

On the other hand, I have learned well that computers hooked up to mass spectrometers can give us a huge amount of first-rate data in very little time. And much of those data would simply not be accessible by any means without computers. And I have been perfectly happy to take advantage of all of that. Fortunately, there have been several people in our mass spec group in Naperville who have had good rapport with computers, and they were prepared to make the measurements that I wanted. And so I had the benefit of their efforts and I've taken advantage of the data with no hesitation at all. But I have not talked to the computers myself, and I do not own a personal computer.

My wife [Lotte] recently bought a word processor, and her intent was that it was for both of us to use. But I've stayed as far away from it as I can. I use my little portable Hermes non-electric typewriter for any correspondence, or any typing, or anything that I need to get done. But Lotte uses her word processor, and it just has made a world of difference to her.

GRAYSON: So basically you feel that you are a beneficiary of computers in mass spectrometry, but that's as far as you want to go with it? [laughter] You didn't really use it to apply any of your fundamental studies or anything like that.

MEYERSON: No. I use the data. But for years for the most part in quantitative analysis done at the mass spec lab in Naperville, and in a lot of the qualitative analysis of unknown mixtures of various kinds has been computerized to the point where the data are fed from mass spectrometers directly into the computer and it carries out essentially all the operations and types up a report. Somebody has to look it over to be sure that nothing has gone drastically wrong. But that's all right.

On the other hand, the kinds of problems that I'd been engaged in, we don't understand the chemistry well enough to be able to tell a computer how to do it. So these were things that I puzzled over and scratched my head over. But I did make use of lots and lots of data that came out of computers.

GRAYSON: ASMS has been around for a while in various forms, at least at meetings of mass spectrometrists, starting out with the early vendors and ASTM [American Society for Testing Materials] and then ASMS. Can you single out some things that you've noticed in the evolution of the meeting from its inception other than clearly essentially sponsored by different people and it's gotten larger. But the flavor of the meeting, do you have any feelings or observations over those years of how it's evolved?

MEYERSON: First remember that seven years have passed since the last ASMS conference I attended, and I don't know what's happened since then. I receive the proceedings and I appreciate having them, but I haven't been to any of the conferences. Certainly the number of people attending, the size of the meeting has increased tremendously. The number of simultaneous sessions has increased a great deal.

There was a huge struggle before we went from just one single session to two simultaneous sessions, and another big struggle when we introduced a third simultaneous session, and a fourth, and I don't know whether it's gone beyond that or not since then. The moment simultaneous sessions came on the scene, I started to find myself with conflicts. And I've always found myself with conflicts of that kind at ASMS conferences. Somehow the papers, if I go to a meeting and I have a dozen papers that I want to hear, the chances are very good that they will be given at only four or five or six different times, and there would be two or three different papers given simultaneously in different places, that I would like to get in. And it can't be done. The same thing has happened at ACS [American Chemical Society] meetings, of course. I don't know any answer to that.

Well, we started out as an organization of analytical chemists. Some of us were heavily into instrumentation and others were heavily into trying to get and interpret the data, and some of us had a hand in both fields. But as academic people came in, a larger part of these conferences had started to be devoted to considerations that seemed more and more arcane to analytical chemists, who were concerned with the day-to-day problems, very mundane problems.

And over the last several years, a large part of the emphasis in these conferences, as a large part of the emphasis in the journals, has been on biological problems, in which the mass spectrometry is almost incidental. And I don't know any way around it either, because biological problems are important. And I'm pleased to see the field in which I was able to make some contributions over a number of years itself in turn able to contribute to the solution of biological problems.

At the same time, I look at the proceedings of the ASMS conference these days, and I find a great many papers that are written in a language that I simply don't understand. And I have to hunt for the ones that I want. Furthermore, as generations have come and gone, I find that a great many of the people doing mass spectrometry today share characteristics that I have encountered over the years among many younger chemists. I've had the privilege and the pleasure of refereeing lots and lots of papers for the journals over many years. And particularly in papers that have come to me for refereeing, I have often found younger people sometimes, often while they're still doing their graduate work, sometimes just after they've completed their graduate work, or they've just started their professional careers, write papers that in effect said that “This field is a huge, chaotic area with no order to it, but now I have arrived and I'm going to bring order out of it. So pay close attention.” And with that in mind, they themselves ignore the great bulk of the work that was done before they arrived. And they ignore almost completely anything that's more than five years old. It just can't be worthwhile. Now, particularly in work with Ellis Fields, because he and I have looked at these things in much the same light, we have taken great pleasure in our papers that we have published in chasing down pertinent work and drawing attention to it that was sometimes more than a century old.

GRAYSON: Oh my.

MEYERSON: And I think both of us have felt that that's just great. We can't always do that and they're not always easy to find. Now even when a number of years ago at the Washington meeting of ASMS when a couple of people put together an exhibit on the development of mass spectrometry. In that exhibit I recall it described this unpublished spectrum of Al Nier's of benzene as the first intentional mass spectrum of an organic compound. They weren't too far off, but they were off because that came about 1935 or thereabouts. And I've chased down a fair number of such spectra going back to 1928. I'm not prepared to swear that there were none before 1928.

GRAYSON: Yes. Most of the ones that you mentioned earlier.

MEYERSON: Yes.

GRAYSON: That was news to me because I obviously haven't dug back that far.

MEYERSON: Of course.

GRAYSON: But it's good that someone's looking back there.

MEYERSON: Yes. It's kind of fun to find what people were thinking about and doing back then and to find on the one hand in the first many years of mass spectrometry—and I suppose the same thing could be said about a lot of other fields of chemistry and of other sciences—on the one hand, we tried awfully hard to interpret our data in terms of some tremendously over-simplified models, and at the same time right along with that, people working in our areas exhibited some marvelous intuitions. These were all happening together. And sometimes the same people.

GRAYSON: Yes. The point that you mentioned about—I guess if you want a better word or phrase, you'd call it ignorance of the literature—came home to me yesterday when I was attending a Pittsburg Conference [PittCon] and a session on pyrolysis. The speaker essentially discussed the fundamental mechanism of pyrolysis and polymethylmethacrylate, and one of the people in the audience took him to task in the question and answer session indicating that this work had been done before, and it was in the literature. And the mechanism had already been published.

Although he was very kind about it; he wasn't nasty about it, but he really was saying the guy doesn't really have to discuss that in his presentation. He could have discussed the other parts of it, and then referred to the fact that prior work had already been done. I don't know if there is any way that the issue can ever be dealt with—the fact that people don't take enough care in looking at the literature. I guess it's a measure of professionalism...

MEYERSON: No, it comes out in various ways. On the one hand, over the last year or two years, three years, I have caught several instances in the literature in which people have dealt with things that my associates and I have published on, and obviously, totally unaware, in one case the guy found a breakdown pattern that struck him as quite extraordinary and unprecedented. And he proceeded to not only report it and to use these adjectives to describe it, but to offer a possible mechanism to account for it. And it's a system that we first published on in 1956 and have dealt with repeatedly since then and elaborated on and in which I did a review paper on it in 1989, bringing it all up to date. And this guy's speculation is just simply wrong; and we knew it.

I was annoyed when I wrote to him, and he acknowledged that "Yes," he missed it all. I felt that he should have published a correction to his paper. But obviously he didn't feel that was called for. In a different sense, I find that more often than not, for the last umpteen years when there is a reference in the mass spectral literature to tropylium, nobody bothers to identify the source anymore because this is something that everybody knows. It's simply become part of the established knowledge that's taken for granted. And so it doesn't make a difference any more. No one is concerned with where it came from. In a way, it's a great compliment. I don't know, I guess I've got some kind of mixed feelings.

GRAYSON: Well, I can understand. It's a fundamental contribution, and I guess it's like the kind of thing that happens when a brand name becomes a household word, like Kleenex, that type of thing.

MEYERSON: Yes, this is true.

GRAYSON: People just accept that as being definitive and that's enough and it's all understood by everyone. I can see it's kind of a problem in a way. Certainly what it shows definitely is the overall acceptance of what at the time, when it was originally proposed, was a very difficult concept in which you obviously, as you pointed out before, Brown had difficulty with it, but he couldn't come up with anything better. [laughter] And so, the fact that it has become accepted is somewhat satisfying in its own right.

MEYERSON: I remember the first time that we talked about tropylium to a group outside our company; was in our little Midwest Spectroscopy Group that was centered around the Chicago area. And one day, meeting in our laboratory, I presented that work. It was the first time I talked to anybody outside of the company, and it was two consultants. And John [P.] Kinder who worked for Sinclair—perhaps you remember him.

GRAYSON: The name sounds familiar. Yes.

MEYERSON: Well he was the lead man in their mass spectrometry operation for years. He was quite upset by it, and he said, “Do you realize what you're saying?” He said if what you're saying is right, we can't believe anything the mass spectrometer tells us any more. [laughter]

GRAYSON: Well, not anything. But the issue really goes back to the fact that an ion is different than a neutral molecule. Some of the chemistry may be the same, but it's not all guaranteed to be the same.

MEYERSON: And even behind that, we're dealing with chemistry. Chemistry is often much more complex than simply breaking one or two bonds in an original structure.

GRAYSON: Do you have any other areas that you want to cover? I kind of covered pretty much the things I was interested in, in our outline.

I don't know if you'd care to comment on any of the individuals that you've known all those years in mass spectrometry and different parts of the country or the world. Obviously, you had a lot of interactions, and we've talked about a number of those individuals. I am always reminded about Frank Field, and I guess the fellow at [University of] Delaware, Burnaby Munson. When they published their original work on chemical ionization, the reviewer really took them to task and essentially...I guess it was a very similar thing to the incredulousness with which the tropylium ion was met.

MEYERSON: Similar to what?

GRAYSON: The incredulous aspect of the tropylium ion. This guy, the reviewer, recommended that the work not be published because it just didn't make any sense; it was all wrong, everything. And when they first started in chemical ionization, the first publication that detailed the concept of chemical ionization. You've known Frank. Did you meet him at the mass spec conferences? Plus he worked for Humble, was it?

MEYERSON: He worked for Humble. I met him at the group meetings hosted by Consolidated very early in the game. I don't think Frank was especially happy with tropylium either, at first. I can't remember very clearly, but I have a vague recollection of that.

[END OF TAPE 3, SIDE 1]

GRAYSON: Do you know a fellow in mass spectrometry by the name of Stevenson?

MEYERSON: Dave Stevenson. He worked a while for Westinghouse, and he and John Hipple worked together. From there he went to Shell Development Company in Emeryville, and then back when Consolidated was busy developing its first commercial mass spectrometer and trying to sell it, they were demonstrating it to people—and again part of this is in one of my publications —the first order that came in was from Esso—[Standard Oil of New Jersey]. They had submitted, as a number of other oil companies had, a gas sample for analysis. And in short order, the CEC people had not only given them a first-rate analysis, but they had found one or more components that had not intentionally been there. Although it was clear where they'd come from, the Esso people were impressed. And they wired an order to CEC for three mass spectrometers. Harold Wiley's response to that was that really made his day.

That order was later canceled when CEC presented Esso with their sales contract including the provision that any instrumental developments they made were to be given free of royalty to CEC. They really went through the roof and canceled the whole thing. And they got together at Westinghouse where Hipple had been working on the development of a commercial instrument. And they pulled together a number of academic and industrial laboratories to place orders for instruments with Westinghouse and to lend people to spend a period of time at Westinghouse working with Hipple on the instrument development project to get it ready for commercialization.

Stevenson went back to Westinghouse from Shell and worked on that. He was involved in the instrumental developments. He did a great deal in the way of early labeling of hydrocarbons, measurements and interpretations of appearance potentials, developing means for estimating thermochemical data. He made enormous contributions.

Dave was a big man, a big, heavyset fellow. When he got up at a Consolidated group meeting to give a paper, I always had the feeling that he was looking around that audience and thinking, “Gee, what a bunch of dumbbells! There isn't anybody here that's going to be able to follow what I have done.” And rather than try to explain and make sense of what he had done, he would tell us what he had accomplished in such a way that it sounded to me, at least, as though he was trying to impress us with his accomplishments without any pretense of trying to explain anything.

On the other hand, if you got him off by himself to talk to him, raise a question with him, nobody could have been kinder, more generous, more helpful in trying to explain anything that he could, as well as he could. He's really a very, very nice guy. But he made it all vanish when he got up to give a talk to in front of an audience.

GRAYSON: This fellow's name was Stevenson?

MEYERSON: David P. Stevenson.

GRAYSON: Yes. That's a name I haven't run across.

MEYERSON: You'll find him in two or three of the things I have published.

GRAYSON: So, this concept that CEC had started out with getting proprietary rights to developments really caused a problem with some of their potential customers?

MEYERSON: Oh, yes. Now, curiously, Standard Oil of Louisiana, which was a subsidiary of Standard Oil of New Jersey, did order a 21-101 from Consolidated, but they paid an extra 7,500 dollarsin order to have them delete that provision. Somewhere along the line—I don't know exactly when—Consolidated thought better of it; they deleted the provision altogether, and they gave Louisiana back their 7,500 dollars.

GRAYSON: Well, I guess they were having trouble with their customers. I mean it doesn't do them any good to have this wonderful technology if nobody is going to buy it because of this clause. So, I guess they saw that it was necessary.

MEYERSON: The capacitance micromanometer that has been standard in, I guess, most analytical mass spectrometers and lots of others for several decades now; the basic patent on that belongs to Henry Grubb and John Strange of Standard Oil Company (Indiana).[25] And Consolidated did get royalty-free use of that development. However, my understanding is that on any of those micromanometers that they sold as free-standing instruments, other than as part of a mass spectrometer, they paid royalties to Indiana Standard. And a curiosity there is that Henry and John never built a model of that thing. It was strictly a conceptual write-up that got them that patent.

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[25] U.S. Patent No. 2,567,263, applied for May 9, 1945, granted September 11, 1951.

GRAYSON: As a pressure measuring device, it seems to have a lot of technology to it. Did any of them have training in electronics?

MEYERSON: Yes, both of these guys were good electronics men. John Strange was a physicist formally. And as I told you, Henry did his undergraduate work in physics and his graduate work in physical chemistry. And even going back to all the work that was being done on the circuitry during the first many years after I joined the company— Red Schmauch in particular, I think, was allergic to any unused panel space. [laughter] He was always coming up with something to build or modify. And after Henry became a supervisor, first a group leader, then a section leader, his heart was in the laboratory and in the equipment. And he continued, as long as he lived, to steal a great many hours a week from any supervisory or administrative responsibilities to spend in the laboratory working with the equipment.

GRAYSON: This guy's name was Red Schmauch?

MEYERSON: Red Schmauch. It was L. J. Schmauch, S-C-H-M-A-U-C-H.

GRAYSON: Ok.

MEYERSON: He is retired, long retired. He is, I think, still living in Whiting, Indiana. Henry Grubb died about 1973 of colon cancer. A bad, bad time. Now we should add various items that maybe will be pertinent. After Henry died, his job—at least the supervision of mass spectrometry and some other spectroscopies—was taken over by Ron Martin, M-A-R-T-I-N.

Ron had been an analytical chemist for a long time, working particularly with separations in conjunction with various chromatographies. And then he'd gone on to other assignments. Then they brought him back to analytical, made him an analytical supervisor. Ron's heart also remained in the laboratory. After he had been in his new office for about a week or ten days, he came around to see me one day, and he said he'd been looking around, talking to people in his group, trying to get some feel for what they do, how they do it. And he had concluded that I was having more fun than anybody else in the group. He said, “ Break me in on mass spectrometry.” He said he thought that he could probably get all of his pencil-pushing work done in maybe twenty hours a week, and he'd like to spend the rest of the time doing something that was more fun.

I first turned him over to one of my technicians. By this time, clerks and technicians were all in one job category, and this was a guy who'd worked with us as a clerk for a long time and was then reclassified as a technician, and who was pretty good, and he got Ron started. After a short while, he turned Ron over to me, and I took him just a short distance beyond that.

Then Ron started picking up problems as they came in, that he thought he'd like to get a hand in. And he was not bashful, not shy in any sense. He took some enormously complex, very difficult problems. He would collect all the data he could, get all the paperwork, all the background, lay them out on a big table surface, call me in to look over it with him, talk about how to approach it, what kinds of measurements might be useful, and then he would carry on and get the work done, interpret it as far as he could, and spread them all out again and call me in again. And we'd go over it again, decide whether it was done or we weren't getting anywhere, or were unlikely to get anywhere, or whether there was more to done, and how to approach that, and how to carry on next. And he'd go on from there.

Now we had a marvelous working relationship. You see, administratively, I reported to Ron Martin. Technically, professionally, he was reporting to me. [laughter] And this didn't show up on a table of organization, obviously.[26]

GRAYSON: Sure. You bet. [laughter]

MEYERSON: This was the way in which our personal working relationships developed. And it was just damn good fun. It was highly satisfying, and I know from things that Ron's told me that he found it, I think, as satisfying as I did. And it was just great. I have had occasion to describe that from time to time to people from other organizations, and I don't know of anybody in any organization anywhere that has had that kind of working relationship with somebody else.

GRAYSON: Yes, it's kind of unusual. But, I guess to a large degree, it revolved around his interest in not leaving the technical area behind.

MEYERSON: Also, I think it was possible, in large part, because neither of us felt the need to prove anything.

GRAYSON: Yes. You both had established yourselves in your disciplines and felt comfortable. You didn't have to establish a dominance, I think.

MEYERSON: Yes. Now, it was curious in another way. You see, as I said, administratively, I reported to him. And he knew and I knew that on the T.O., his job classification was here and

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[26] Louisa Wray Dalton, "C&EN Employment: How Best Firms Retain Chemists," *Chemical & Engineering News*, 81(43) (October 27, 2003): 61-7.

mine was here, and I was getting paid substantially more than he, and I didn't want the administrative responsibilities.

Again, in the very early sixties, I was called in one time by the man at the very top of the heap of our organization, asking a number of questions. In part, how what I was doing today was going to make the company a buck tomorrow. But in addition he asked me where I would like to see myself within the company organization five years in the future or ten years in the future. And my answer was that I'd like to stay in the laboratory and go wherever the laboratory work led. And he and his successors, in turn, all honored that request. Occasionally, over the years, they would check with me to see if I had any second thoughts. They never pressured me to do anything different. It was great.

GRAYSON: Yes. It seems as if the company has been pretty much of an ideal employer. I mean obviously there were rough times, but in terms of trying to maximize professional development of an individual, they seemed to be clearly dedicated to that.

MEYERSON: Yes. The company introduced a dual ladder system fairly early in the game. And it certainly worked very well for me. I know that didn't hurt financially. The last part of our research organization moved from Whiting to Naperville in 1974. At that time the guy who had been our division director in Naperville went to another assignment. He preferred to work in the laboratory. He became a research associate in another area. And the guy who had been division director in Whiting became division director of the recombined analytical division.

Our company had a practice that goes back a long way in the past. It was kind of a haphazard matter at first, but as the years went by, they put more and more emphasis on it, that everybody in the company organization all over the world gets formal counseling from his or her supervisor at least once a year. There was a lot of emphasis put on that. There was a great deal of training of people—how to carry out this counseling, what to look for, and how to deal with it.

And our new division director, after the move in 1974, was Frank [J.] Piehl, P-I-E-H-L, who also was an old research man in analytical. But once he became a supervisor, he knew that his responsibilities as supervisor were going to take all he had to give it and then some. I think he might have enjoyed continuing some technical work, but he knew that he couldn't do it.

When counseling time came, the first time after he moved out there, I had to go to him for counseling. He was a supervisor, in the administrative ladder; I was in the technical ladder. But our job categories were parallel, approximately the same. They overlapped very largely. He offered me a chair, a cup of coffee, and asked how am I doing—about himself, that's what I mean. And so the upshot of it was that we counseled each other. And again, it was an extraordinarily productive working relationship that I had with this guy.

There really were a great many extraordinary people in that organization. I assume—I am not in close touch any more now; I just get out there once in a while—but I assume, and I certainly hope, that there are still a great many extraordinary people in that organization. And over the thirty-seven and a half years or so that I worked there, it was just darned good fun, challenging, and exciting practically every day of the week, every week of the year.

I know a lot of people in professional work who can't make that kind of statement. I know people in professional jobs who as they approached the time that they could first take retirement without a penalty couldn't wait for it to come. This is something I'm just incapable of understanding. I would like to think that at least people who are professionals for the most part would have the kind of experience that I had, and that at least some other people that I have known well there have also had.

And I've come to realize that in part what I'm saying here right now reflects on the organization. It obviously has got to reflect on the attitudes that were part of the organization and those that are part of the individual. They've got to somehow fit each other. I consider myself very, very fortunate.

GRAYSON: Well, I would agree with that. I think clearly the organization does reflect a part of each; the organization and the individual. The thing that I perceive is perhaps this approach may have also reflected a particular period in the history of science and technology in the country; because the things that I hear you describing about the organization during the time frame in which you worked for it don't seem to be the kinds of things I'm seeing in organizations today; at least the ones that I'm familiar with.

There seems to be a much more bottom-line oriented attitude in—I don't know what the exact word would be, but I feel it to be less of a caring for the individual and more of a cutthroat type of a thing. And I've seen a number of un-nice things happening in large organizations in the St. Louis [Missouri] area that don't reflect the same experience that you're describing in your career. And I think in prior years and earlier years, perhaps their attitude was much more like the one that you experienced. But I think in today's time, there may be a change in the management philosophy. I don't know what it's like in your lab, your old lab now. I hope it stays the same as it has been.

MEYERSON: I hope so, too. But I'm not close enough to it to know.

GRAYSON: Yes. It's instructive, I guess, perhaps for anyone that's going to listen to this reel or these notes of the past. But there seemed to be a real, honest and mutual trust between the employee and the organization. And there seemed to be an attempt to make the job transcend just the bottom-line oriented results. In other words, clearly there was some concern earlier in your career. “What are you doing?” The work that you are doing, what good is it? Clearly you were able to address that to their satisfaction. After a while it was kind of an example of us all.

And that issue didn't need to be brought up year after year after year. It's hopefully this attitude towards work and technical work in the future will be along the lines that we've discussed here. Unfortunately, I’m afraid a lot of what I see today, it's not at all that way. It's very much more cutthroat; and loyalties and people trying to establish the interactions that you speak of, they don't seem to consider that to be valuable.

MEYERSON: Well, I think aside from the time period and some things that may have been different in some one period than in another, I think in large part it has to do with the people involved too. Now, I've spoken quite a bit here about E. B. Tucker and about Henry Grubb. And from where I sit, both of those guys were extraordinary people. And there's no way, I've known for a long time that there's simply no way in which I can ever repay my indebtedness to them. The only way in which I could even attempt to do that is to help in any way that I could the professional goals of younger people in the organization with whom I was in touch. But that's all. And I think the fact that these two were extraordinary people was recognized not only by a great many people in our organization, but by people outside the organization as well.

I remember my surprise on one occasion when I had accepted an invitation to talk before a seminar at a competing oil company in their research department. It was Conoco—Continental Oil. And talking afterwards to the man in that organization, who was their counterpart of Tucker in ours, and he assured me that Tucker (he'd come to know Tucker quite well in working together in ASTM committees at one time or another) was way ahead of nearly everybody in this kind of work, in technical, analytical work anywhere. I was pleased to hear that and kind of surprised to hear it from somebody entirely outside of our organization.

GRAYSON: So he had really established a good reputation, and he was exceptional, and well recognized outside.

MEYERSON: Yes. You take people like Tucker, Grubb, and technical people, somebody like Ellis Fields, an organic chemist with whom I collaborated for maybe twenty-five years, and quite a number of people outside of our company with whom I had collaborations that were very close, very effective, went on for many years. And again, I consider myself extraordinarily fortunate to have had this kind of personal contacts.

Even now, it's been over seven years now since I retired, and over the last several years when we have traveled abroad, if there is anything going on in mass spectrometry where we've traveled, I found that I was an honored guest. In 1988 we traveled in Australia and New Zealand, and I gave a paper at the annual conference of the Australian/New Zealand Society for Mass Spectrometry. And when we arrived there and got our conference programs, I found that they had put me down to chair a plenary session at which there was going to be just one paper given by my long-time good friend, Jim [James D.] Morrison, on forty years of mass spectrometry in Australia. They didn't even bother to check with me in advance before putting me down on the program. And as we traveled from city to city, there were a good many people, mass spectrometrists, in several cities who really put themselves out to host us and make us feel at home, welcomed.

In 1989, as part of another big, major trip abroad that we made, we spent a week in Israel, chiefly in Jerusalem [Israel], and there they scheduled me to talk before a seminar at the Hebrew University. Ilia [V.] Goldenfeld and his wife, who had come to Israel from Kiev [Ukraine], from the Soviet Union, in the early seventies, and whom I've come to know quite well over the years, arranged a very fancy Ukrainian dinner at which we were the guests of honor. We really have come to feel that we have good friends in lots of places around the world. And we have continued over the last several years to have people from lots of different countries come here. They come here and they may spend a few days with us and it’s delightful.

GRAYSON: Well, these are the kind of associations that are worth more... you can't measure them.

MEYERSON: Fringe benefits of working in science. This is the way I look at it.

GRAYSON: Yes. Excellent fringe benefits. I really don't have any further issues that I'd like to pursue. I don't know if there is any particular item or something we haven't touched on that you feel you want to comment on or add. I have this rather catch-all category at the end about serendipitous events and synergistic happenings, but I think we've covered a lot of those.

MEYERSON: Let me add one item that just comes to mind now.

GRAYSON: Sure.

MEYERSON: I mentioned to you that in working with Fausto Ramirez, we did some work on the monomeric metaphosphate anion, PO_3^-.[27] We actually published about two things dealing with monomeric metaphosphates. The first one we made a search of the mass spectral literature, all of it that I could find, on organophosphorous compounds for anything that I could pick out that referred to monomeric metaphosphates. And I found a lot of citations, but none of it was explicit where metaphosphates were formed as neutrals or as protonated species in conventional positive ion mass spectra.

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[27] Seymour Meyerson, E. S. Kuhn, Imre Puskas, and E. K. Fields, “The nominal butyl ester ion in the mass spectra of long-chain n-alkyl esters,” *Journal of Physical Chemistry* 80 (1976): 2855-60; and Seymour Meyerson, “Mass spectrometry in the news, 1949,” *Organic Mass Spectrometry* 28 (1993): 1373-4.

Later we went to the negative ion mass spectra and we reported the first direct observation of monomeric metaphosphate anion in negative ion mass spectrometry in a preliminary communication and followed it with a full paper. But in chasing down the background, what we struggled with and pointed out there was that for about a quarter of a century organic chemists working with phosphates had been speculating about monomeric metaphosphate anion as a key intermediate in a great deal of phosphate chemistry for years, but there had never been any clear unequivocal evidence that such species existed.

There was some evidence that suggested there was a great deal of chemistry that was rationalized in terms of this kind of intermediate, but there were organic chemists working with phosphates who continued to refer to it as an imaginary intermediate. During the same twenty-five year period, quite a number of mass spectrometrists had put down and published work on phosphate esters and related species in which such species also were present; they were all over the place.

But there wasn't the slightest hint in the organic chemists' papers that any of them was aware that a mass spectrometer existed. There was not the slightest hint in any of the mass spectrometry literature that any of the authors were aware that the organic chemists working with phosphates had a problem. Now these were two groups working in areas that overlap like that. For twenty-five years each one totally unaware that the other existed. Utterly amazing! And I suspect that that kind of thing must have happened any number of times in science.

GRAYSON: Yes. Basically it was because of the partnership that you'd established with Ramirez...

MEYERSON: Yes. We pulled all those pieces together. Because he knew the organic chemistry end of things and I knew the mass spectrometry end. And so we pulled them together. Here is the first thing that we published in that area. Well this is essentially what I have just told you here that really...

GRAYSON: The two areas of study went along parallel independent lines.

MEYERSON: That's correct. But each of them...

GRAYSON: Each of them ignoring the other.

MEYERSON: That's correct.

GRAYSON: Two of you guys got together.

MEYERSON: Let me give you this. You should find something of interest in it. And let me give you also these other two things on monomeric metaphosphate. This is the original direct observation and this is the full paper on monomeric metaphosphate. I don't want to load you with any more than that...

GRAYSON: Well, no.

MEYERSON: ...but I think that you might find those things of some interest, and they're in the area that is somewhat removed from some of the things that you normally follow.

GRAYSON: Yes, that's true. That's true.

Well then, if you don't have any other inputs that you want to make, we've got ourselves a good interview.

MEYERSON: Thank you, it's my pleasure.

GRAYSON: Thank you.

[END OF TAPE 3, SIDE 2]

[END OF INTERVIEW]

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