CHEMICAL HERITAGE FOUNDATION

RICHARD E. HONIG

Transcript of an Interview
Conducted by

Michael A. Grayson

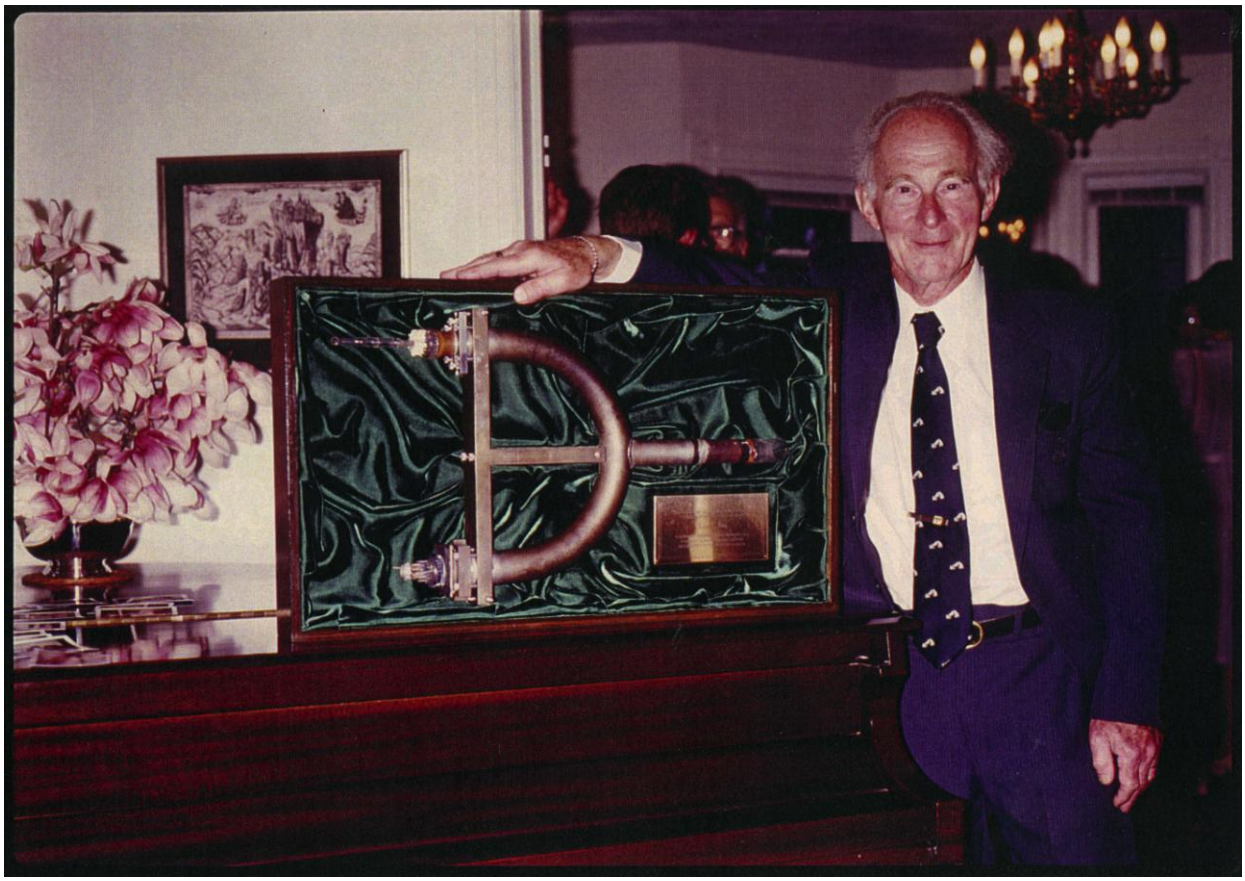
at

The Quadrangle
Haverford, Pennsylvania

on

27 April 1996

(With Subsequent Corrections and Additions)



The spectrometer tube from the Secondary Ion Mass Spectrometer constructed at RCA Laboratories, Princeton NJ, in 1954 by Richard E. Honig. Photograph taken at his retirement party, April 1987.

ACKNOWLEDGMENT

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

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RICHARD E. HONIG

1917 Born in Göttingen, Germany

Education

1938 B.S.E.E., Robert College, Istanbul, Turkey
1939 M.S., Physics, Massachusetts Institute of Technology
1944 Ph.D., Physics, Massachusetts Institute of Technology

Professional Experience

Massachusetts Institute of Technology, Cambridge, Massachusetts
1939-1940 Lecturer, Physics
1941-1946 Researcher, Radiation Laboratory

Bluffton College, Bluffton, Ohio
1940-1941 Lecturer, Mathematics & Physics

Socony-Vacuum Research Laboratories, Paulsboro, New Jersey
1946-1950 Researcher, Mass Spectrometry

RCA Laboratories, Princeton, New Jersey
1950-1966 Researcher
1966-1982 Head, Materials Characterization Group
1982-1987 Staff Scientist

Brussels University, Brussels, Belgium
1955-1956 Visiting Researcher

Honors

1964-1968 Chairman of Subcommittee VII on Solids Studies of ASTM E-14 Committee on Mass Spectrometry
1968-1970 Vice President, American Society for Mass Spectrometry
1970-1972 President, American Society for Mass Spectrometry
1972-1974 Past President, American Society for Mass Spectrometry
Fellow, American Physical Society
Adjunct Research Professor, Rensselaer Polytechnic Institute (while still at RCA Laboratories)
1986 Awarded The Science Medal from the Vrije Universiteit of Brussels

Member, Böhmische Physical Society

ABSTRACT

Richard E. Honig was born in Göttingen, Germany, the eldest of three boys. His father, a professor of law at the University of Göttingen, was among the first group of professors dismissed from the university by the Nazi regime in 1933. The family subsequently relocated to Istanbul, Turkey, where Honig's father had been asked to help westernize the Turkish educational system. Honig spent his last two years of high school at a German school in Istanbul, where he augmented the classical education he received in Germany with a math and science curriculum. He went on to attend Robert College, an American college in Istanbul, from which he was graduated with a bachelor of science degree in electrical engineering.

In 1938, Honig moved to the United States to pursue a Ph.D. in Physics at the Massachusetts Institute of Technology (MIT). Through a course in nuclear physics, he became interested in the nature of atoms, molecules and particularly isotopes, and eventually built his own mass spectrometer to study the effects of deuterium and cyclotron radiation on methane. Because there was little activity in mass spectrometry at MIT at the time, Honig immersed himself in the literature and visited several commercial laboratories involved in mass spectrometry, notably John Hipple's lab at Westinghouse Corporation and a commercial lab in New England that owned a Consolidated Engineering Company (CEC) mass spectrometer. His thesis on the nature of gas flow in that mass spectrometer, which was written under the direction of Clark Goodman, an MIT geologist with good knowledge of nuclear physics, grew out of observations he made on the gas inlet system of the CEC instrument. While still a student at MIT, Honig taught for a year at Bluffton College in Ohio and then, following the completion of his Ph.D., taught for several years at MIT. He became a U.S. citizen in the early 1940's.

In 1946, Honig accepted a position at Socony-Vacuum Labs in Paulsboro, New Jersey, where he was able to continue the pursuit of his interest in the study of small hydrocarbon molecules with mass spectrometry. Honig joined the research staff at the Radio Corporation of America Laboratories in Princeton, New Jersey, in 1950, where he remained for the rest of his long career. His work began in Don North's group, studying materials used in hot cathodes. He designed and built a two-stage mass spectrometer, which led a few years later to the development of a secondary ion mass spectrometer (SIMS). He spent a year during the mid-1950's at the University of Brussels helping to start a mass spectrometry laboratory with Jean Drowart. He traveled extensively in Germany and England, observing the post-War recoveries of the two countries while participating in mass spectrometry conferences that were beginning to spring up in the late 1950's and early 1960's.

Honig's career at RCA focused on materials characterization, particularly impurities in semiconductor materials, first with mass spectrometry and then later with a variety of surface analysis techniques when he became head of the newly formed Materials Characterization Research Group there in the mid-1960's. He reported coupling a laser to a mass spectrometer, demonstrating that the chemical nature of metal, semiconductor, and insulator surfaces could be probed by laser desorption followed by mass analysis. He and his group built a number of mass spectrometers, including several within ultrahigh vacuum systems to facilitate surface analysis. His long-time interest in cluster formation led to his measurement of elemental vapor pressures as a function of temperature and the evaluation of previously reported values for these quantities. The so-called vapor pressure curves he generated, initially hand-drawn in the days before computer-aided graphics, were first published in 1957 and updated in 1962 and 1969.

Honig stepped down from his managerial position in 1982 and spent the next several years back in the laboratory helping to design and build a new mass spectrometer to study the organic materials on surfaces. When RCA was purchased by General Electric in the mid-1980's, the nature of research in the laboratories changed, and Honig elected to retire in 1987, just short of his seventieth birthday.

During the interview Honig describes some of his collaborations with colleagues and his papers, of which there are many. He talks about the growth of mass spectrometry technology and its organizations, the American Society for Testing and Materials and the American Society for Mass Spectrometry, of which he was the second president. He suggests that his work in the development of SIMS started in the "Stone Age" of mass spectrometry, where available electronics limited progress, and finished with the flowering of the technology which was made possible in part by the advent of solid-state devices.

INTERVIEWER

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

TRANSCRIPT REVIEW

Special thanks to **Bryan L. Bentz** of Waters Corporation who contributed his time to reviewing this transcript for the family of Richard E. Honig.

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INTERVIEWER: **Michael A. Grayson**

LOCATION: **The Quadrangle**
Haverford, Pennsylvania

DATE: **27 April 1996**

GRAYSON: This is 27 April 1996 and my name is Mike Grayson. I'm sitting here with Dr. Rick Honig, and we're going to do an oral history about Rick's contributions to mass spectrometry. And traditionally, we start an interview of this type with digging into a little bit about the how you got involved in the technical or scientific area at an early age, high school, your early education. Was there any particular point in time, when you saw science, a technical career as being interesting, challenging, rewarding?

HONIG: Yes. The interest was greatly furthered when I was in the last two years of high school. Of all places in Istanbul, Turkey at the German high school. And we had excellent teachers in math, chemistry, and physics.

GRAYSON: Could we back up a minute and find out why you were in Istanbul, Turkey, in a German high school?

HONIG: Certainly. I was born in Göttingen, Germany, in 1917 at the worst possible time. We lived there until 1933, when my good father, who was a professor of law at Göttingen University, was kicked out [of the university] by the Nazis on the very first shift. He was not only [very well known to] the Nazi world, […but] he also had a bad heritage as far as the Nazis were concerned.

GRAYSON: When you say "the very first shift?"

HONIG: There was the very first [group] of six professors on the 27 April 1933 [who] were dismissed. And my father was one of them, along with [Richard] Courant, later on [Max] Born…James Franck was one who retired on his own volition. So, that was the beginning of the end for the scope and fame of Göttingen University. We went then, in the fall of 1933, that is my family and I—two younger brothers, parents, myself—to Istanbul, Turkey, where my father had been engaged to help in making education in Turkey change into a European style education, as opposed to the Islamic. That's why we were in Turkey. That's why it was, I spent

my last two years of high school in Turkey at the German school. I then went to Robert College [of] Istanbul—the American college—existing then, no longer exists as such.

GRAYSON: Was this a small school, Robert College?

HONIG: It was a smaller school. It was not by any means first rate. Since physics was not available as such, I took the electrical engineering course, and graduated after three years with a BSEE. in 1938. Then, I went together with my father at that time, to the United States, and I enrolled at MIT [Massachusetts Institute of Technology] for graduate work.

GRAYSON: At MIT at that time, was it…I mean, it's always been a prestigious institution. Was it difficult for you to enroll? Because as you indicated, your undergraduate training was perhaps, not as good as it could have been?

HONIG: It was not difficult, because I happened to be pretty much at the top of our class at Robert College. But I found many, many open areas that needed to be patched over <**T: 05 min**>. So, that's where I got interested in electronics, physical electronics, and a course in nuclear physics. It was that course of nuclear physics then that got me very interested in such things as atoms, molecules, and particularly, isotopes. I learned whatever there was to be learned about mass spectrometry at the time.

GRAYSON: The interest in mass spectrometry, was that because you had an analytical interest in…

HONIG: That analytical interest was due to a part-time job that I had with what was known as the…let me think, the origin of petroleum. And that research project was based on the assumption that maybe lower, smaller molecules of carbon, hydrogen combinations could be polymerized due to natural radioactivity. So, we carried out some projects taking methane and subjecting it to deuterium radiation [and] subjecting it to alpha rays. Therefore, I decided to try to see what came out of it. It seemed that mass spectrometry was the way to go.

GRAYSON: Who was the sponsor of this activity?

HONIG: It was sponsored jointly by the physics department and by people in…I'm trying to think.

GRAYSON: That's quite okay, no rush.

HONIG: That's why my gaps come in. We had a Professor [Walter L.] Whitehead in a different department, who was a geologist…was really in the geology department. So, at the time, I was completely ignorant about how to build a mass spectrometer and I had practically no guidance at all. In fact, we tried to approach Al [Alfred O. C.] Nier at the time, but Al Nier was not available, because he was working on the ultra secret project which of course, we couldn't know.

GRAYSON: So, this would have been, what year? 19…

HONIG: That was 1942.

GRAYSON: 1942, okay. So, let's see. You had graduated from Robert College in 1938, and enrolled at MIT. Did you enroll immediately the following fall?

HONIG: Right away.

GRAYSON: Okay. So, you would have been doing graduate work for about three years or so by then.

HONIG: Well, actually, there was a gap of one year, when funds ran out for me, and I taught physics and mathematics at a very small college in Ohio—Bluffton, Ohio—Bluffton College [now Bluffton University].

GRAYSON: So, you actually departed from the Boston area?

HONIG: Departed from the Boston area for one year. Came back in…now, I was away 1940, 1941, and I came back in 1941. […So] in about 1942, I was ready to do something about mass spectrometry.

GRAYSON: And as you say, by then, Nier was pretty much involved [in other business].

HONIG: Was involved and not available for consultation at all. So, I was completely <**T: 10 min**> on my own. There was nobody at MIT who knew anything about mass spectrometry. In fact, the professor who taught nuclear physics tried to dissuade me. He said, "There had been people in California who had been working for many, many years on this, and it's a very difficult area." So, I proceeded to build a mass spectrometer. And let me say at the outset, in retrospect, I'm far from being proud about it, but it did work. It had the fabulous resolving power of one [part] in a hundred. [laughter]

GRAYSON: Wow, you know that's better than many first attempts. Then did you work primarily from the literature?

HONIG: I worked from the literature. I read all the literature. Eventually, I had a chance to visit one or two laboratories…three laboratories, I guess. One was [John A.] Hipple's laboratory in…

GRAYSON: Was he at Westinghouse?

HONIG: At Westinghouse [Research Laboratories, then in East Pittsburgh, Pennsylvania].

GRAYSON: At that time.

HONIG: Yes, at Westinghouse. Another laboratory, here in the neighborhood was a commercial laboratory that had bought a commercial spectrometer from what was known subsequently as CEC [(originally Consolidated Engineering Corporation, which was changed later to Consolidated Electrodynamics Corporation)]. At that laboratory, I noticed how they were introducing gases, but they wouldn't tell me what the trick was. They were not allowed to. So, I inspected the gas inlet system and drew my own conclusions.

And in fact, wrote my first paper which was a pretty good paper on gas flow in the mass spectrometer.[1] I realized that the gas flow had to be a molecular flow in order to give you true results. So, eventually, the mass spectrometer worked, and I was able to get some fairly preliminary results. In 1943, I was able to base my thesis on this and get my PhD.

GRAYSON: What was the design?

[1] Richard E. Honig, "Gas Flow in the Mass Spectrometer," *Journal of Applied Physics* 16 (1945): 646-54.

HONIG: It was a Nier type, 60° deflection […], based on the fact that I wanted to get the biggest possible radius for the best possible resolving power. And everything that I had at that time of course, was inherited—borrowed—from what was available at the labs at MIT, because our funds were exceedingly limited, and during [the] war time you couldn't get it anyhow.

GRAYSON: Okay. So, you were, kind of, at the opposite situation that Nier was in, where basically, he had everything at his disposal in terms of the equipment and the latest in tubes and electronic devices. Whereas, you […] didn't have anything of the latest and greatest and had to kind of patch it together.

HONIG: It was, yes, the bootstrap operation. And to record the ions, I used the elephantine FP-54 tube and an old style galvanometer.

GRAYSON: So, was that one of these measurements that you had to increase <**T: 15 min**>…

HONIG: Point-by-point…

GRAYSON: Point-by-point.

HONIG: Point-by-point, I had to manually obtain the spectrum.

GRAYSON: Were you varying the magnetic field or the [accelerating potential]?

HONIG: I was varying at that time with that instrument the accelerating potential [rather than] the magnetic field.

GRAYSON: What was the accelerating potential in the instrument? I mean what was its highest [capability]?

HONIG: Well, there I can't give you a hard [number], but it probably was no more than 2,000 volts, down to maybe 400. So, it had to be done in stages, and I had to vary the magnetic field. The magnet was actually fed by a system, a DC supply in the basement of MIT that had on it a huge switching arrangement with mercury contacts—mercury pools—very healthy.

GRAYSON: Yes. And so, this was like a 'house' [supply of direct current electricity]?

HONIG: A house supply and it was clearly not feasible to change the magnetic field very readily. But I could change it.

GRAYSON: And then, this was used to look at organic materials?

HONIG: This was used to look at the organic materials that were generated by taking methane, ethane gases and irradiating them in a cyclotron or [with] deuteron radiation or [with] alpha rays from the radioactive source.

GRAYSON: And what was the result of these experiments?

HONIG: The result of these experiments was that, yes, these gases will polymerize into heavier hydrocarbons. But, whether that really answered the question that was the original purpose, isn't clear to me yet, because these hydrocarbons were very complex. They were far from being straightforward.

GRAYSON: How did you deal with the issue of fragmentation in organic compounds? Did you use a low ionization potential for the ion source ?

HONIG: That problem I tackled much later, when I spent…not much later, while I was for four years working at what was known then, as Socony-Vacuum Laboratories in Paulsboro, New Jersey, now [Exxon]Mobil. At the time, [the] laboratory was known in the neighborhood as "the vacuum." And there I was from 1946 to 1950. […I also] stayed on for two [of those] years at MIT, […where] half of my time was spent teaching. Of course, these were basic courses, first year and second year physics.

GRAYSON: What was the class size that you had to deal with at that time?

HONIG: The class size, I would say about thirty or so.

GRAYSON: So, before we move forward, I just would like to maybe back up, and was there a particular teacher back in your high school days? Or, you indicated that it was in the latter two years of your high school education, I think that…

HONIG: Yes.

GRAYSON: You started to get an interest…

HONIG: Two teachers. One teacher's name was Günde […] <**T: 20 min**>, a very bright, very capable man who taught us physics and some math. The other teacher, who was equally capable, and very nice, was a Dr. Julius Stern, S-t-e-r-n. At that time, he was the only left over Jewish teacher at the German high school, and then, had to leave. Those two teachers were able to further my interest in math.

I might say that my education in Germany had been strictly the classical education that started with Latin, some rudimentary English, a lot of Greek. So, for seven years I learnt more about classical languages and next to nothing about science.

GRAYSON: What did Dr. Stern teach?

HONIG: He taught chemistry and some of our math courses. And in fact, I still, through a classmate of mine, only about two years ago, had contact with this Dr. Stern before he died. He was still in Istanbul.

GRAYSON: So, the move from Germany probably changed the whole progression of your life in many more ways than just physical relocation.

HONIG: Yes. Yes, although, I note looking back, I realize that I began to be interested in mathematics say at age thirteen or so. And there again, that school in Germany did not teach you much math, and less physics and less chemistry, at all.

GRAYSON: So, if you had stayed in Germany your education would have been very classical and you would have perhaps not even become involved in science.

HONIG: Well, I think I would have, but it would have taken a little longer, yes. Because I was not interested in following my father's career, who had taught law, as I mentioned earlier.

GRAYSON: Okay. Then to move forward now, you graduated from MIT in 1940…?

HONIG: 1943.

GRAYSON: 1943. And you were able to find…

HONIG: Excuse me, correction. It was 1944.

GRAYSON: Okay.

HONIG: It was 1944.

GRAYSON: And you were able to find employment readily.

HONIG: Then, I stayed at MIT for two years, from 1944 to 1946, continuing teaching, continuing on that project.

GRAYSON: Who was your, essentially, thesis advisor or…

HONIG: The thesis advisor, his name was [Clark] Goodman. But he was essentially, more interested in geology and knew next to nothing about the specific physics, except he knew quite a bit about nuclear physics. But nobody at MIT at that time had been even remotely aware of mass spectrometry.

GRAYSON: Do you have any idea of what happened to the mass spectrometer <**T: 25 min**> after you left MIT?

HONIG: Yes, I do. I do. A successor to my job on that project was later on attempting to run the mass spectrometer. But unfortunately, [he] connected the magnet in such a way that he could only see negative ions. And I was called in in 1946 on a consulting basis to find out what the trouble was. And checking on the magnetic field, I straightened him out very quickly. So, I won't tell you who it was, because he became quite well known in the field of making mass spectrometers later on. [laughter]

GRAYSON: Well, but it would be nice to know.

HONIG: No, I better not.

GRAYSON: Well, this mistake is somewhat common. It's a simple mistake.

HONIG: It's a simple mistake, yes.

GRAYSON: And easy to make, I…

HONIG: But you see the mass spectrometer was running when I left. And later on for some reason or other, had been disconnected, and had been reconnected, but unfortunately, the wrong way.

GRAYSON: Well, he was just doing negative ion work ahead of his time. [laughter]

HONIG: So then, jumping forward from 1946 to 1950, being at Socony-Vacuum, who had the idea of establishing a research laboratory after the war, and called in some very capable people. In fact, one of my colleagues was Gregory [H.] Wannier. I'll spell Wannier, W-a-n-n-i-e-r, originally Swiss, a high-powered mathematician who in fact, very kindly worked with me on one project. And one paper that I'm proud of, of that period was "A way of Measuring Ionization Potentials in Hydrocarbon Gases," which at that time, up to that point had been pretty much hit or miss.[2]

But I found out very soon that that laboratory was not really going anywhere. And after four years, I decided it was high time to leave, which I did and went to RCA Laboratories, where some of my former MIT colleagues were.

GRAYSON: Before you start in the RCA experience, I was curious, why was vacuum part of the name of the Socony-Vacuum enterprise?

[2] Richard E. Honig, "Ionization Potentials of Some Hydrocarbon Series," *Journal of Chemical Physics* 16 (1948): 105-12.

HONIG: They made gasoline. And why they called it Vacuum, I honestly don't know. And eventually, they apparently decided that there was a better way and they called it Mobil—the flying horse. That was after my time.

GRAYSON: Yeah. But, okay, so I don't know. Maybe it was just something in the war that they had perhaps tried to disguise the operation, because I know they [made] fuel, and the technology of fuel was a very hot topic during the war.

HONIG: But of course. But, whether there was any vacuum involved, I seriously doubt it.

GRAYSON: And also, I'd like to—without revealing the name of anyone. I was curious…we know that Professor [Klaus] Biemann <**T: 30 min**> made quite a name for mass spectrometry at MIT in recent years. I was just wondering if there was any connection between the mass spectrometry that you left at MIT and Professor Biemann's.

HONIG: Not to my knowledge. I think Klaus Biemann came to MIT much later. In fact, the first time I met him was at an international mass spectrometry meeting in Oxford. And it may have been in 1961, perhaps. I may be off a year. […]

[END OF AUDIO, FILE 1.1]

GRAYSON: Here we are recording on the second side. It's a half hour to a side.

HONIG: Half hour to a side, I see […].

GRAYSON: So, are there any other connections or items of interest during this period before you started your, essentially, real mass [spectrometry career] or really got into the many other things that you wanted [to do]? How did the war…was the war an issue in anything that you were doing at the time?

HONIG: Well, for quite a while it couldn't be an issue because it was only in 1943…or was it 1944? Probably 1943, that I became a citizen. Until then, I had been classified as an enemy alien.

GRAYSON: So, you took citizenship sometime after you came to the United States.

HONIG: I applied for it of course, immediately after I came to the United States. But at that time, the lag period was typically four years, except during the war it turned out to be longer.

GRAYSON: Surprise.

HONIG: And it was at times disheartening to be classified as an enemy alien, when I was as much of an enemy of the Nazis, as this country was.

GRAYSON: Yeah, if not more so.

HONIG: Yes. In fact, while I was at MIT, I was limited in how far I could move without permission from whoever was in charge. And it took a dispensation for me to go from Cambridge to north [New] Jersey, when I got married in 1943.

GRAYSON: So, the war did…

HONIG: The war did affect me in an indirect way. I wasn't allowed to use my camera. I turned it in to my supervisor, Professor Clark Goodman, who kept it for me, very kindly, until I was permitted to use it again as a citizen. Those are some of the highlights of that period.

GRAYSON: So, then you started at RCA Laboratories after you determined that the Socony-Vacuum operation wasn't really going to be what you wanted.

HONIG: [Yes]. Although, I did learn a lot about mass spectrometry [at Socony-Vacuum]. Because at that time, they had bought, at my insistence, at the beginning, a CEC instrument, 180° deflection instrument.

GRAYSON: Would that be the [21]-100 Series?

HONIG: Yes. It was a 101, I think.

GRAYSON: And what did you do with that instrument?

HONIG: Well, with that instrument, I did a number of things, including the low primary energy electron studies to obtain ionization potentials and applied that as an analytical tool too, by being able to eliminate certain components [that] had higher ionization potentials.

GRAYSON: Did this also, eliminate fragmentation in the spectrum…

HONIG: [It facilitated] fragmentation studies […]. Then, I got interested in deuterated materials <**T: 05 min**>, which was quite new.

GRAYSON: Were they easy to come by?

HONIG: We were able to get, at that time, which was after the war…yes, I was able to get deuterated water, which was reasonably pure. And then, we could make deuterization studies.

GRAYSON: What was the interest in the deuterium work at the industrial[setting].

HONIG: Well, I guess it was the fragmentation studies. And perhaps there was no direct connection or excuse for doing that. But initially, they were quite willing to go into studies that might be long-term studies for the first two years, and then that stopped.

GRAYSON: Was this directive from on high, a management change in position?

HONIG: There was a management change and eventually, they were trying in fact, to channel me into something else, and I refused.

GRAYSON: Into what other area, did they want you to…

HONIG: Well, they thought they were going to take on solid state physics. I realized that I was not really prepared to do solid state physics at that time…[hadn't] the necessary background. So, after a while, I decided that this wasn't going to get any better, and rather worse, and I left on my own. They tried to keep me, but I said, "No, thank you." But I only left after I evaluated various possibilities to work with Hipple at Westinghouse [or] to go to Oak

Ridge, Tennessee. I had an offer, but then, RCA came through, and [it] turned out to be a very good choice.

GRAYSON: So, you were contemplating going to work with Hipple then, at that time.

HONIG: Yes. Yes, I would have liked to very much. He was a very nice man, and very positive. But, I also realized that Westinghouse was not a particularly good company. [laughter]

GRAYSON: And Oak Ridge…what kind of work did you anticipate there, if you'd gone there?

HONIG: I would have done some mass spectrometry. But at this point, I must confess, I'm a little hazy. And at the time, when we visited Oak Ridge, my first wife said, "Well, I guess this would be all right for a couple of years." And that was not my intent.

GRAYSON: Well, Oak Ridge was at that time, pretty elementary place to live, I guess.

HONIG: Yes. But, it would have been a perfectly, a highly interesting place [scientifically]. And some of my former colleagues from MIT were there, too.

GRAYSON: So, you then effectively, decided to go to work at RCA Laboratories, is that located here in the Jersey area?

HONIG: Princeton [New Jersey].

GRAYSON: Princeton.

HONIG: Princeton, the Princeton Laboratories, which […] were very different from many of the other RCA locations. It was indeed a remarkable research laboratory with a very good spirit, and some <**T: 10 min**> top-notch people there.

GRAYSON: When you started, how large was the facility in terms of people?

HONIG: As I recall, as far as scientists and…they called all of us "engineers" from early on, but let's say scientists and some engineers, about two hundred. So, it was quite small compared to what it then turned out to be in later years. But I joined the physics research group, which was a very nice group with a man in charge at that time, by the name of Don North. Don was an abbreviation for his first and middle names. A very fine man with whom, I had immediate contact and understanding. And the problem that they were thinking about was to study electron emission from cathodes.

GRAYSON: And what, obviously, I guess RCA was in the [business of vacuum] tube production.

HONIG: Very much in the tube business, tube developments, new tubes, understanding the physical processes that occurred during electron emission from oxide coated cathodes. And that's where the idea was presented or was discussed rather, that one could determine the surface layer of these oxide coated cathodes by positive ion bombardment.

GRAYSON: So, this would be a way of sampling the surface for mass spec analysis.

HONIG: Exactly. The pre-runner to what you now call SIMS [secondary ion mass spectrometry]. And that problem appealed to me immediately as something very worthwhile to look into because it was something entirely new, as far as I knew. Nobody had ever done anything like that before. Although, later on I found that there had been a very short note by [Richard F. K.] Herzog in Vienna [Austria], right after the war, who used his…he had a mass spectrometer.[3] The Herzog of the Mattauch-Herzog combination. Some very, very rudimentary work had been done which, however, nobody was aware of, including myself, until mid 1950s, I found out. So, that's when it all started.

And I then just proceeded to design an instrument. First of all, I had big ideas that it would be possible to use a double-stage instrument. The first instrument, that first mass spectrometer to produce the primary ions and select them, and selectively bombard the surface sitting in the second mass spectrometer. I soon found out that <**T: 15 min**> there were problems in sensitivity. At that time, there were many things not yet available that we have today.

GRAYSON: So, let me clarify this. You would use a mass spectrometer to select the ionic species with which to bombard the surface from which other species would be emitted…

[3] R.F.K. Herzog and F. Viehböck, "Ion Source for Mass Spectrography," *Physical Review* 76 (1949): 855–6.

HONIG: Exactly.

GRAYSON: …and analyzed in the second mass spectrometer.

HONIG: Exactly.

GRAYSON: So, the first mass spectrometer was almost like an ion source of sorts?

HONIG: Yes. And these were now 180° deflection instruments. […] So, I had in fact, and I still have somewhere, the original plans for a double instrument that went in sequence like that, but soon realized that, I had to start less ambitiously with an ion source, which I then decided to put right into the so-called "second sector."

GRAYSON: So, this would be a design that you would have had built or you built. It wasn't a commercial instrument that you would buy…

HONIG: I had no…No. There was no way of [purchasing such an instrument. This] was all to be constructed right at RCA. Great RCA Corporation.

GRAYSON: They had facilities for doing…

HONIG: They had excellent facilities. They had an excellent, what they called…they called it a "model shop." It was really a group of people who could produce almost anything mechanical. So the first working instrument then, probably dates from 1951 [or] 1952. It took me about two years. And then there were various different things too. I needed a magnetic supply, power supply, magnetic power supply that would be highly stabilized. So, I was able to use a stabilization scheme that they had developed at RCA for their aboriginal computers.

GRAYSON: Aboriginal?

HONIG: Quite. Well, nearly aboriginal. These were very early…in the 1950s. 1950 at that time, they had a computer that was housed in a huge building. And so, I had a stabilization of roughly one part per million that I could achieve for the magnetic field stabilization.

GRAYSON: So, you used the same basic design…

HONIG: It was a beautiful design. It involved forty 2A3 [vacuum] tubes. 2A3 was the name of the tubes that were running in parallel, and produced a fantastic amount of heat, which was…now, in fact, I have some of this I have written up in the paper which I think I brought along for you, the early days of mass spectrometry there.[4]

Where it turned out that in summertime, the system just couldn't be operated, because there was no air conditioning. The RCA <**T: 20 min**> people were not quite sure that air-conditioning would really work. And so, it was only installed after quite a few years that I'd been there. And it was installed just as an experiment for the administrative wing, but for nobody else. And it turned out that my particular laboratory was one of the last that got air-conditioning. And I might also mention—I have this written up that—I called in at one time the man in charge of the buildings and grounds and power and showed him that it was impossible to work with high voltage under these conditions of high temperature, high humidity, because I had spark over. Well, he said, "The way to handle this is to reduce the relative humidity. And to do that, all you need to do is install some heaters."

GRAYSON: Make it worse? [laughter]

HONIG: That was true, believe it or not.

GRAYSON: Well, that changes the relative humidity, but.

HONIG: The relative humidity […].

GRAYSON: So you had support in this activity from, other than not getting air-conditioning, but you had financial support and interest…

HONIG: Financial support. Although they didn't [know] initially how much it would cost, either. I had no help, no assistants, or anything. I did everything single-handedly with my two left hands. But I had very good support all along and interest. So, then at one time, I was ready to do the experiment…just before I did, it turned out that there was a problem with germanium impurity. So, I was asked to see if I couldn't use my equipment to study germanium impurities, which I did. It was an approach that was slow, but it did work, and I was able to point out what

[4] Richard E. Honig, "The Development of Secondary Ion Mass Spectrometry (SIMS): A Retrospective," *International Journal of Mass Spectrometry and Ion Processes* 66 (1985): 31-54.

impurities were in the germanium samples. It was not by any means as fast or, eventually, as sensitive as the spark [ionization source] source mass spectrometer that was being developed at the same time at Bell Labs, in fact.

GRAYSON: This would help you, I guess, with the management being able to address an issue [of how funds were spent].

HONIG: Well, it helped management and it didn't help me, because it set us back on our original plans. But eventually, I was able to do—finally in around 1953—secondary ion mass spectrometry. But, in the process too, there were some sidetracks which were very interesting. When I was studying germanium, I found to my great surprise that germanium vaporizes not just as an atomic form, but also molecular form. And nobody had ever seen such a thing before, certainly not for germanium or the other Group IV elements. When I presented this at a science section, a science meeting, internal science meeting, there were some people who pooh-poohed and said, "Oh, this can't be right <**T: 25 min**>." And that then, led me to enlarge this and not just look at germanium, but also silicon, and carbon. Particularly, because at that time, the energy needed to vaporize carbon was not well known. There were, in fact, three different potential values that had been established by mass spectroscopy. And I was able to show that the carbon value was 170 […] calories per mole.

GRAYSON: 170?

HONIG: 170. Not 120, not 90. It was independently, and I didn't know about that either, also determined by Mark [G.] Inghram at the University of Chicago at the same time. When I heard about it, I acknowledged his work, and sent him mine. He never acknowledged mine. So, after these issues that were not secondary ion mass spectrometry, as we know it now, I finally got around to making the first real experiments with a special primary ion producing source right in the mass spectrometer—basic mass spectrometer source—and studied a dirty piece of silver and found the most amazing things on the silver surface.

GRAYSON: Why did you select the piece of silver?

HONIG: [laughter] Because it was there. And then, on to more sophisticated, further experiments.

GRAYSON: So, this was the beginning of using the particle desorption technique or, ionized particle desorption where you hit the surface and dislodge ions and material [characteristic of the surface].

HONIG: Producing primary ions in a primary source, hitting the target and looking at the secondary ions.

GRAYSON: What was the primary ion? Was it argon?

HONIG: I used all the rare gases eventually, but I started, I think, with argon. Argon, krypton, xenon. I think, I also tried helium.

GRAYSON: This was just exploratory to find which would work best, if any?

HONIG: It was exploratory to [determine] which would be most efficient.

GRAYSON: And the silver surface yielded information about…?

HONIG: Many impurities that happened to be on that particular silver. And that silver surface had not been degassed…was just put in raw, the way it was. And from then on, we went to many other surfaces.

GRAYSON: So, this was to follow up on the emission…sort of, essentially, materials for emission <**T: 30 min**> for tubes.

HONIG: Correct.

GRAYSON: But, by this time, [were vacuum tubes still of interest]?

HONIG: By this time, [the] problem [with vacuum tube cathodes] had been pretty well resolved and was no longer of interest. So, the SIMS work began to be free-floating.

GRAYSON: And then, you went ahead and started to look at other materials. What was the rationale then?

HONIG: I have to refresh my memory […].

[END OF AUDIO, FILE 1.2]

GRAYSON: Okay, we're kind of picking up from after having completed the study of [cathode] emission for vacuum tubes, and getting into doing other materials.

HONIG: And there's this article that I'm giving you that I called tongue-in-cheek, "Stone-Age of Mass Spectrometry, The Beginning of SIMS."[5] And there you will find the schemes that were initially planned, a two-stage spectrometer, which in fact, I built, but I found there was not enough sensitivity. So then, I put the primary ion source right in here. And these are all Nier type, and then, 180° mass spectrometers.

But you asked about what materials we used. I used and obtained for example, thin layers of ethyl—ethyl radical—on a clean germanium surface. I could detect those things…those monolayers of $C_2H_5^{\cdot}$ on clean germanium. Then later on, I got interested in carbon, in general, in the multi molecular character of carbon as it was produced by primary ions.

GRAYSON: So, this would be like carbon cluster formation…

HONIG: Carbon cluster formation. Yes. Where you see all these clusters up to, in the case of –about C12 I could see. And I even tried semiconducting diamond, that I got a piece of from South Africa. And negative ion patterns and positive ion patterns. This was a very flexible instrument that could be run. That work then, unfortunately got interrupted…in a way unfortunately, but also, fortunate. I took a year's leave of absence from RCA, and went to the University of Brussels to set them up in mass spectrometry.

GRAYSON: And what year was this?

HONIG: That was 1955, 1956.

GRAYSON: Why did they want to do this?

[5] Richard E. Honig, "Stone-age Mass Spectrometry: The Beginnings of 'SIMS' at RCA Laboratories, Princeton," *International Journal of Mass Spectrometry and Ion Processes* 143 (1995): 1-10.

HONIG: Well, it was…it came as a connection. It was still the carbon work. And at the University of Brussels, they had a man who was interested in the carbon [vaporization] value. He'd done a lot of work on it, and he was swearing by the lower value of 121 and didn't believe my 170 value.

GRAYSON: What method had he used to determine…?

HONIG: Well, that is the point. There were all sorts of indirect methods, but no mass spectrometry. And I thought maybe, it could be done with their equipment and a mass spectrometer. So, we came to terms and he invited me to come for a year. And I had a very interesting time there. But as it turned out, we couldn't really attach a mass spectrometer to their carbon equipment. And so, instead we did something else. A young, at that time, young man by the name of Jean Drowart, I'll spell it, Jean, J-e-a-n, as in Jean, Drowart, D-r-o-w-a-r-t. Who <**T: 05 min**> later on became a very, well-known mass spectrometrist, in his own right. He worked with me on this project and we put out a number of papers on the bond energy of diatomic molecules.[6]

GRAYSON: You were not able then, to do anything at that time to resolve the carbon issue between the 120… the difference…?

HONIG: Oh, that had already been resolved as far as I [was] concerned. [laughter]

GRAYSON: Okay.

HONIG: Had been definitely resolved. I could not prove to him what was wrong with his equipment. They did not realize the character of the carbon evaporation, which in part is molecular. And so, they were getting some value that included C-1, C-2, and so on, so forth—an average value which then misled them.

GRAYSON: So, this was [exactly the same] attitude that your friends had when you explained that germanium had a molecular component in its evaporation.

HONIG: Yes. It hadn't been seen before, so.

[6] J. Drowart and R.E. Honig, "A Mass Spectrometric Method for the Determination of Dissociation Energies of Diatomic Molecules," *Journal of Physical Chemistry* 61 (1957): 980-5.

GRAYSON: It's amazing how when we do research, we don't accept things that don't fit our idea of what it's supposed to be.

HONIG: There's a German saying, which I'll translate what the…sometimes my German gets in my way. "The person who does agriculture, what he doesn't know, he won't eat." Except it is in German, it's more descriptive because it used the "eat" form that is used for animals. *Was der Bauer nicht kennt, das frisst er nicht*. So, it was a very interesting interlude, interesting for my family, and for myself, living in Brussels, and we had a very interesting time.

GRAYSON: Was this clearly sanctioned by the laboratory, RCA Laboratory? Right.

HONIG: The RCA Laboratory was willing to let me go. They were not very happy that I did, but they acquiesced and say, "Okay, go ahead." But in the meantime, my instrument was not really being used properly at all. And, so some of my publications actually, were delayed because I was away for little over a year.

GRAYSON: How did you find that part of the country [Belgium]? It had been, what, about ten years after the war? Was this part of Europe touched much by the war?

HONIG: Well, Belgium had been touched in the war, because it had been overrun by the Germans. And there had been many, many people who had died—Belgians—as a result of it. And they have even the concentration camp there [Breendonk]. But on the whole, they had pulled themselves up by their bootstraps, and it was doing exceptionally well, considering that this was only ten years after the war [or] eleven years after the war.

It also gave us a chance to travel through Europe to see Germany, to look for [residual] Nazi [sentiment]. But you couldn't find any […]. <**T: 10 min**> At that time, I found exactly one German who claimed that [Nazism] was a good thing, [that] "Hitler was a great man." But the Germans were reconstructing like mad and very, very efficient. And their country was in far better shape at that time, than for example, England, [which] I also visited. It also gave me a chance to see various laboratories in Germany, and at a different time, in England. In fact, we stopped in Bonn [Germany] and I saw the first prototype mass spectrometer, well, quadrupole with four long wires strung in, from the first down to this—from the second floor down to the first floor.

GRAYSON: Oh, my, what a large instrument.

HONIG: It was a large instrument. It was a large instrument.

GRAYSON: This was being done by…I mean, was [Wolfgang] Paul, was Paul there?

HONIG: Yes. Paul was there. Yes. That's where he built it.

GRAYSON: Did he have any particular reason for the scale of the instrument?

HONIG: Well, I guess he wanted [resolving power…]. I […] remember the tremendous length of it. [But] the quadrupoles were not the right shape, because they were just wires.

GRAYSON: Did you get back to Istanbul at all, at that time?

HONIG: No. But I got back to Istanbul ten years later, in 1964, once, to see my old alma mater, which was in bad shape at that point. And that's another interesting story about how the foreign schools and universities in Turkey were being pushed out by the Turks. So, a few years afterwards, Robert College was simply written over to the Turkish government, became a Turkish university [Boğaziçi University].

GRAYSON: So, your father had been there to try and Europeanize the schools and in fact, they resisted that in the end.

HONIG: Well, yes and no. I mean, they did accept…many people at the time that we came in 1933. There were probably forty, fifty families of mainly Germans, but also, Austrians and some Swiss, some French, who were there. But there was always a sentiment of course, and strong feeling against them, because this was pushing out the old style religious professors who were there. And so there was a great deal of problems. So, eventually, my family…I didn't say that earlier, moved to this country in 1939, just before the war. And as far as, having help was concerned, they would accept it, but they were not happy with it. So, Robert College which was certainly several cuts above any Turkish university, initially, was taken over by the Turks. And the American influence was suppressed.

GRAYSON: So, let's see. We were back in the 1955, 1956 period when you were at Brussels. Was there any other work that you did there?

HONIG: Yes. Well, no. You see we had to build up a mass spectrometer from the bottom up. And we got it running <**T: 15 min**> very quickly. And did a number of studies, but mainly on diatomic gold, diatomic silver, diatomic copper was one publication.[7] It's in the publication list. And so, together with this very capable young man, who later became a well-known mass spectrometrist […].

GRAYSON: So, you were successful in establishing mass spectrometry there.

HONIG: Yes.

GRAYSON: And basically, that was the whole idea.

HONIG: That was the basic purpose. And they had some very capable people there. And this Jean Drowart later on came for a year or two to the University of Chicago to work with Mark Inghram, for example […].

GRAYSON: Then completing the Brussels, almost like sabbatical, you returned to RCA.

HONIG: Returned to RCA, and picked up again, where I left off, and did a few more studies that you see in that little write-up there. And eventually…oh, yes. What we did at the labs was in 1960 we got a spark source mass spectrometer. In fact, the first one in this country from what was then known as Metropolitan-Vickers [MetroVic] in Manchester [United Kingdom]…because there was much call for—study of—germanium impurities, except then it was silicon. And we had various contracts in fact, with the [U.S.] Air Force to study impurities in other elements.

GRAYSON: So you were funded actually…part of the work was funded by an outside agency?

HONIG: Later, yes. As time went on, we were encouraged to get outside funding. Things did change at RCA. The old spirit slowly disappeared and we had different people at the top, not all of them very successful, I would say. But there was still a fair amount of work we could do.

[7] Jean Drowart and Richard E. Honig, "Mass Spectrometric Study of Copper, Silver and Gold," *Journal of Chemical Physics* 25 (1956): 581-2.

And I think the next stepping stone that I see there was that, in 1963, I got interested in the possibility of using a laser to desorb surface materials. And I think ours was the first attempt to couple a laser into a mass spectrometer, which we did using the spark source instrument, which was all very rudimentary. But it worked.

GRAYSON: What kind of a laser did you use?

HONIG: There was a red laser—a ruby laser. In those days, there was still a great deal of magic and lack of knowledge in how to handle them. <**T: 20 min**> [laughter]

GRAYSON: Did you operate at a continuous mode, or pulse, or CW [continuous wave] mode…how?

HONIG: No, there was a pulse laser, clearly. So, we borrowed a laser. We borrowed equipment from other people. We got cussed out for it by the owner. [laughter]

GRAYSON: So, did you borrow it or did you take it?

HONIG: No, no. We borrowed it, with the help of the assistant, who was very helpful and said, "Here. Why don't you take this?" We said, "Fine." So, we borrowed the laser. And then, the owner found out. But those are the first two little papers about using a laser.[8]

GRAYSON: Did you couple it with…I mean, was that through a window of any particular [type]?

HONIG: It went through a window. Oh, yes, of course. It went through a window.

GRAYSON: I'd like to back up a second on the Air Force contracting.

HONIG: Contract.

[8] R.E. Honig and J.R. Woolston, "Laser-Induced Emission Of Electrons, Ions, And Neutral Atoms From Solid Surfaces," *Applied Physics Letters* 2 (1963): 138-9; and Richard E. Honig, "Laser-Induced Emission Of Electrons And Positive Ions From Metals And Semiconductors," *Applied Physics Letters* 3 (1963): 8-11.

GRAYSON: What laboratory was…

HONIG: There was an Air Force [laboratory…] in the Boston [Massachusetts] area, [AFCRL], Air Force [Cambridge] Research […] Lab at Hanscom [Field]. I remember going to the Boston area, [Bedford in fact…]. But, they actually came to us, as I recall. We had not approached them, but they came to us.

GRAYSON: Probably through literature reference or an individual reference.

HONIG: I couldn't really tell you. But, it was they, who came to us. Mr. [J. Paul] Cali, […] and we had a good working relationship for quite a few years.[9]

GRAYSON: And I suppose they just had a fundamental interest [in materials].

HONIG: They had a fundamental interest, and [if] I'm hard pressed to give you a hard and fast answer, I guess, at that time funds were floating around very freely.

GRAYSON: And the funding level, were you able to keep yourself busy or a couple of people or…

HONIG: It was something that kept probably one and a half people full-time, which was fine as far as, RCA was concerned at that moment.

GRAYSON: So, that really, probably helped, given the fact that the management attitude was becoming a little less conducive to doing pure research.

HONIG: Yes, of course.

GRAYSON: Because you were at least bringing in some kind of funding.

HONIG: We brought in some kind of funding at that point.

[9] R.E. Honig, "Mass Spectrometry as an Analytical Tool," *Annals of the New York Academy of Sciences* 137 (1966): 262-83.

GRAYSON: And they got to at least take overhead out of it, I suppose.

HONIG: Oh, yes. There was a very healthy overhead, I recall, very healthy.

GRAYSON: So, after you got back and started doing the laser work, was that just purely experimental, or exploratory…

HONIG: That was purely exploratory, and we didn't carry it much further than that. We didn't really have, I guess, the support.

GRAYSON: And the gentleman wanted his laser back.

HONIG: And the gentleman wanted the laser back. Well, what happened next? Then, eventually, a couple years later in 1966, I was approached; and I wasn't quite sure I was particularly interested, and whether I would take over <**T: 25 min**> a small group in what they called "characterization." And well, I agreed to do it on a year's trial basis, and then, decided well, this did give me something new to do too. So, I built up a very sizable group of people doing materials characterization of various types. And that included now, additional mass spectrometry. It included trying…let me refresh my memory for a moment. Embarrassing, but I have to. [papers shuffling] Oh, yes, somewhere along the line, I might mention this. Doing the mass spectrometry, I got interested in finding out, just what the vapor pressures were, for the various materials that we were working with at the temperatures involved.

That led to a very complete review of the literature on vapor pressures available at the time. And I first did that in 1957 on the side, because at that time, there was nothing at all, no collection at all. So, I published what is now, which was then became known as vapor pressure [curves] of the common elements.[10] And that I did twice again, once in 1962, and once in 1969.

GRAYSON: You scoured the literature as well as made your own measurements for this publication?

HONIG: Own measurements involved, yes. But I also tried to evaluate, of course, the literature. And that was the major purpose. And I always [say] that I wished I'd got a dollar for

[10] R.E. Honig and H.O. Hook, "Vapor Pressure Data for Some Common Gases." *RCA Review* 21 (1960): 360-8.

each reprint that was demanded of us over the years, thousands of them. And you'll find those curves in many laboratories still, I guess.

GRAYSON: Now, when you make this measurement, that's not an easy measurement to perform, right.

HONIG: No.

GRAYSON: How do you go about doing this basically?

HONIG: Well, one is, of course, you have to get a good temperature measurement. And you have to worry about whether the surface temperature is typical or not. You have to have somewhere a point of reference. Then you can develop a curve, which may have to be shifted up and down depending on that point of reference. So, I did that for germanium. And I did it for silicon. And then, you have to worry about the diatomic and monatomic molecules that are vaporized at the same time. But the great majority of the elements I evaluated from what was found in literature and there was a great deal of literature by that time, 1962, [and] in particular [when] 1969 rolled around. And I have a feeling that things are still pretty well, pretty accurate <**T: 30 min**>.

GRAYSON: Was the literature of pretty uniform quality or did you find that there was…

HONIG: No, of course not. I had to evaluate. I had to throw away quite a bit. And sometimes, fly by the seat of my pants.

GRAYSON: So, you had a basis for discarding literature. I mean, just the description of the experiment was not going to be able to get the information correctly. I suppose that measuring temperature was probably a serious problem.

HONIG: Measuring temperature is a serious problem unless you can measure it inside a cavity…

[END OF AUDIO, FILE 1.3]

HONIG: That's a very good type of tape incidentally.

GRAYSON: Yeah. Well, I recommend using a sixty-minute tape, because that works out…

HONIG: Oh, that breaks…yes. It makes it an hour.

GRAYSON: But I have some ninety-minute tapes, we're going to, if we use this one up. But, I don't know if you want to take a break at this point? We can always take a break and walk around and stretch our legs, and get a drink or do something different.

HONIG: Well, we can. And I also, I'd like to call my wife who will join us for lunch at…

GRAYSON: Why don't we go ahead and take a break.

[recording paused]

GRAYSON: So, we just finished discussing this issue with regard to the vapor pressures. And actually, the fact that, much of the work that was in literature. It just was a matter of evaluating it.

HONIG: Yes. And to use eventually a computer to help us draw the curves, which was quite an enterprise. And it's amusing that in the first version of it, I had approached the administration asking for computer help. And I checked it out and found that it would cost…I think it was seven hundred dollars. Well, the big boss we had at that time, said, "No, that is too much." [laughter] So, I drew everything by hand […] we had a very good drafting department [and they cleaned it up…]. .

I also got interested in between in something else, and that was to develop an ultra-high vacuum system, which I did using liquid helium which was at that time still quite novel and very cumbersome. So, I spent quite a bit of time on designing an ultra-high vacuum system with a small mass spectrometer built in.

GRAYSON: Why do you want to have such a system?

HONIG: Well, there was a great deal of call for ultra-high vacuum. And there was an interesting exercise to see how far you could push it and see what was left in such a system with a mass spectrometer. So, this was using mass spectrometry for ultra-high vacuum [studies].

GRAYSON: In what year was this?

HONIG: 1962, I see here.

GRAYSON: Because residual gas analyzers were available by then, I assume.

HONIG: Yes, but not to that level. […] And we were going down to 10^{-10} or 10^{-12}, something of that order.

GRAYSON: So you actually were able to obtain, in terms of vacuum, something that was 10^{-12} atmospheres, torr, millimeters of mercury?

HONIG: Torr.

GRAYSON: Torr.

HONIG: Yes.

GRAYSON: And what's left?

HONIG: And [what']s left [is] hydrogen, as I recall. That's what was left.

GRAYSON: Did the helium…was in a cryogenic [reservoir]?

HONIG: Was in a cryogenic system, yes, very cumbersome at the time too, filling it, keeping it full.

GRAYSON: Didn't this represent gasketing problems, I mean, in terms of getting effective seals and so on.

HONIG: Well, this was gold sealed. Sure, it was an all metal system with gold seals.

GRAYSON: Bakeable, I assume.

HONIG: Highly bakeable, oh, gosh. We baked the hell of them. [laughter] Pardon the expression <**T: 05 min**>.

GRAYSON: That being 100°, 200°, 300° C to…

HONIG: Oh, 400° C, easily, 450°, 300° was mild. But that was definitely a little side project that I did.

GRAYSON: So, in the materials' characterization area, you moved on to the semiconductor materials at this point.

HONIG: Well, yes. That's what was of interest. And then, as I said already…

GRAYSON: Of course, you had a group working for you by this time.

HONIG: Well, yes. As I prefer to say, they were working with me, not for me.

GRAYSON: Who were the people then…who were the first people that you collaborated with in some of these things?

HONIG: Well, I was able to bring in a number of people, Bill [William L.] Harrington. You may know his name, and later on, Charlie [Charles W.] Magee.

GRAYSON: Where did Harrington come from?

HONIG: He came from [Cornell University].

GRAYSON: Had he been working with Bendix, along the way […]?

HONIG: No, no. He was at Cornell […with George Morrison. Bill] was the first one that I was able to bring in. And then, later on, [P.] Jane Gale, but this was now in 1977, 1978, and Bryan [L.] Bentz still later, 1980s.

GRAYSON: Did Harrington…was he mostly a physicist […]?

HONIG: [No, he was an analytical chemist]. But he had some experience in mass spectrometry.

GRAYSON: Okay. And that [mass spectrometry] was the primary application?

HONIG: Well, there was a fair size mass spectrometry group. And in addition, we did ultraviolet, infrared [spectroscopy]. We then, went into a variety of other things—[ISS (Ion Scattering Spectroscopy) Auger, ESCA (X-ray Photoelectron Spectroscopy), and RBS (Rutherford Backscattering Spectrometry)].

GRAYSON: Did you do anything like emissions spectrograph?

HONIG: Well, there was an emission spectrograph that we'd had for a long time. It was used still quite extensively, yes.

GRAYSON: So, it had a general analytical flavor.

HONIG: It was a general analytical group…

GRAYSON: With a focus on semiconductors?

HONIG: Focus on semiconductors, yes.

GRAYSON: Was this type of a laboratory unique at that time?

HONIG: I would say we could be quite, at that time, we [were] quite proud of ourselves. We had about a total of twenty. As many as twenty-four people, some of them of course, assistants, and senior assistants. And a few other areas at this point, I'm searching, searching so it might come back to me. So, this then, became pretty much a full-time job with a group that <**T: 10 min**> size. You had to keep things going. And it was very well received at the laboratories.

GRAYSON: So, you were somewhat in a middle management position at that time.

HONIG: That's right. I was called a Group Head.

GRAYSON: Yeah. But it was not difficult for you to get funding from your management.

HONIG: No. I was quite successful and I had some intelligent people in the next layer that were very helpful. [During] the last phases, were able to get multimillion dollar funding for up-to-date equipment.

GRAYSON: Did you replace the Metropolitan-Vickers machine at some time?

HONIG: We still used it. Yes, we replaced it by a more modern one at one time. [laughter] The aboriginal 1960 model, we sold off for thirty thousand bucks to somebody in Denver [Colorado] and got another secondhand one that was more modern. And that was used a great deal, a great deal of the time. And then, Charles Magee built some very fine SIMS equipment that could be rastered. He was a very capable man. It was a fine group. It was, I would say a happy group.

GRAYSON: So, when would you characterize the period when this group of people was like in its heyday.

HONIG: The peak?

GRAYSON: When it started to come into its own.

HONIG: It would be…well, I retired from that job in 1982. It was still, at that time, at the peak of its effectiveness.

GRAYSON: And you would say it was making good contributions?

HONIG: It made excellent contributions to the laboratories. It was well received.

GRAYSON: Was it in the late 1960s when it started to come into its own? Or early 1970s?

HONIG: No. Well, I only took it over…there was a very rudimentary small group in 1966, when I took over. I would say it was in the mid 1970s that it began to flourish, because the buildup was very, very gradual, on purpose and by necessity.

GRAYSON: So, what did you look for in these people, mass spec experience, or physicists, or materials background?

HONIG: Well, some were interested in mass spectrometry and were interested in knowledge going beyond just the bread and butter stuff. So, that particularly, always emphasizing that they could and should have a project of their own for part-time—say somewhere between 10 and 20 percent of their time.

GRAYSON: Sounds idyllic.

HONIG: Kept them productive.

GRAYSON: Yes, sounds very nice. So, the emphasis at this point was an overall characterization of these materials not only by mass spectrometry, but any other analytical technique that would provide information that would be necessary in the construction of new devices, and so on.

HONIG: Yes.

GRAYSON: Does RCA… how large of a player were they in the semiconductor business at that time? Do you have any feeling? <**T: 15 min**>

HONIG: They were still quite successful, I would think. The overall laboratories had been built up to, at one time was at least four hundred technical, technically trained people, plus maybe another…oh, more than that. We were probably total population of thirteen [or] fourteen hundred people.

GRAYSON: So, the work continued then in mass spectrometry? What other large projects were attempted then in this phase, when you had the full fledged group working?

HONIG: Well, the last one was with Bryan Bentz that I actively worked with him on it. After I handed over [management of] the group to Bill Harrington…in fact, was to use a…and it's the last publication that we put out.[11] What we call an "organic SIMS instrument" with separate triple stage quadrupoles.

GRAYSON: So, there was a reasonable amount of instrument design that went on during [this later period].

HONIG: Oh, yes. We had some fun, indeed. We designed the instrument from the bottom up; completely bakeable…I have an ultra-high vacuum system. And I think it's still being used by Bryan Bentz at this point, who's one of the few people who's still there.

GRAYSON: To build a piece of equipment up from that stage would require quite a bit of time, I would assume.

HONIG: Oh, yes. Well, I was able to devote essentially full-time to it. And Bryan Bentz at the time, probably three-quarter time, half-time, three-quarter time.

GRAYSON: And you had enough experience from your previous work to be able to avoid a lot of the pitfalls.

[11] B.L. Bentz and R.E. Honig, "Design of an Organic SIMS Instrument with Separate Triple Stage Quadrupole (TSQ) And Time-Of-Flight (TOF) Spectrometers," *Springer Proceedings in Physics* 9 (1986): 192-7.

HONIG: Well, yes. And of course, In terms of equipment, there'd been tremendous progress with ultra-high vacuum pumps. And, so we were able to draw on experience we had built up over the years, if not decades.

GRAYSON: I noticed that this [last] instrument [used quadrupoles as an] analyzer. There was some consideration for doing it that way? I mean, you abandoned essentially, magnetic sector type approach?

HONIG: Yes. Well, for that purpose, we used quadrupoles. And of course, they were actually commercial quadrupole [instruments], but they were incorporated.

GRAYSON: So, you bought that technology off the shelf.

HONIG: We bought that technology off the shelf, and then modified it to suit our purposes. Yes. And of course, things took longer than expected, as usual.

GRAYSON: So, for this triple machine, could you just walk through <**T: 20 min**> from…you know, the machine from the beginning. If you start out on the first quadrupole, and what are you doing in these various sectors?

HONIG: I'd be better served, if I looked at that diagram, at this point. [laughter] Well, we also did quite a bit of design of the ion paths […] using computers, which was a major project to get the ion [optics] properly designed so things will focus at the right place onto the right surface.

GRAYSON: You indicated that this equipment may even be presently in use by Bryan Bentz, but that the laboratory is considerably reduced in size today.

HONIG: Well, if you want to go into that sad story, I'll outline it for you quickly. In 1985, to everybody's surprise, the top man of GE [General Electric], Jack Welch, and our top dog at the time, whose name I have mercifully forgotten, got together without anybody else knowing about it. And RCA was sold to GE for many billions of dollars. GE wasn't slow in demolishing the company. They really wanted NBC. That's all they wanted probably, and that's another story. RCA had never been willing to sell that, so they were bought, [lock, stock], and barrel.

So, then the question was, what do we do with the laboratory? After all, GE had a fine laboratory in Schenectady [New York]. No point in having two laboratories. So, they went searching around. They were willing to close it up at one time. But they were prevented

actually, by the Governor of New Jersey [Thomas Kean] at the time. So, eventually, the laboratory was handed over for free to the west coast research organization […], Stanford Research Institute [SRI International].

GRAYSON: Oh, okay.

HONIG: Stanford Research Institute, which is not really connected with Stanford anymore, took it over. And now the laboratory became what you might call a job shop. People, in order to be there, had to bring in some very substantial programs. And as a result, the laboratory that existed before 1985 doesn't exist anymore. There have been projects and most of the people who were there had left. Other people have come in. I have not <**T: 25 min**> kept track of it. Initially, I did. But, it's a rather painful experience. And the man who was selling us down the river took his twelve million dollars, bought himself a personal security man. The sad story is that a year later […] he died. Such is life.

GRAYSON: Well, corporate buyouts are part of the modern [business scene].

HONIG: And this is really one of the first—one of the early takeovers of that type. By now everything's like that. Right here in this area…the paper company's gone down the drain, everywhere. So, it's the end of an era, all I can say.

GRAYSON: I think you're right. When you first started working, did you feel that the management was interested in pursuing good science, as well as doing work that was relevant to the company?

HONIG: They were, because they were well provided for through the resources. The patents that were still active at that time […] paid very good royalties. And the laboratory, when I came in 1950, was fully funded by the royalties. Then of course, those royalties gradually dried up, and that's sometime around 1960 or so, when we were beginning to strike out and bring in projects or projects were brought to our attention.

GRAYSON: Such as the Air Force?

HONIG: Such as the Air Force.

GRAYSON: Did you do any more of these type of things in your materials' characterization? Were there other big customers that either, came to you or you solicited business, besides the Air Force?

HONIG: There might have been some minor ones. Surely, there were, but the Air Force contract was, in my area, the big item.

GRAYSON: Well, why don't we take a different tack here for a while, and back and up look at a different aspect of your career? You've been involved in mass spectrometry for a quite a number of years. And of course, as you're well aware, there's been a lot of mass spectrometry developed in the organic petroleum end of things. When did you go to your first, at that time, ASTM E-14 meeting? [This would have been a CEC Users Meeting. The ASTM E-14 meetings didn't start up until 1953.] Do you recall the occasion of that?

HONIG: That was in 1946.

GRAYSON: Which would have been one of the first meetings.

HONIG: Yes. Went to the meeting in…which was being sponsored by CEC, as I remember. Yes, there were yearly meetings.

GRAYSON: Was that the one that was held in association with the Pittsburgh Conference at that time, or was…?

HONIG: There were also Pittsburgh Conference meetings, but I think that was separate.

GRAYSON: Okay.

HONIG: I went to Pittsburgh a number of times.

GRAYSON: And you, to a certain degree, even though you're doing mass spectrometry, you were sort of outside of the norm for that group. ASTM E-14 was really a petroleum standards [operation].

HONIG: Yes.

GRAYSON: And you had done some work in [organics].

HONIG: But there were different committees, and I took over one of the committees early on, before we became an organization which was in 1969, I think. That's when ASMS [American Society for Mass Spectrometry] was first founded.

GRAYSON: But you found, even though most of the people that would have attended an early ASTM meeting were probably petroleum chemists, you found that they were willing to accept you into their fold as a mass spectrometrist.

HONIG: [There] were more [interests represented]…there were quite a few people who did basic work. There were people at the National Bureau of Standards, for example, Vernon [H.] Dibeler who worked very early on deuterium compounds.

[END OF AUDIO, FILE 1.4]

GRAYSON: Okay, we were talking about your [attendance at the] early […] ASTM meetings. Did you attend [ASTM meetings] regularly then, from the beginning?

HONIG: Well, yes, quite. [I first started] when I was at Socony-Vacuum. And then I still went to meetings when I first was at RCA. We went to most of the meetings every year.

GRAYSON: I guess, there was a fundamentals committee…

HONIG: There was a committee on fundamentals, and […] that was the one that I was involved in for several years, before 1969, 1968, whenever. [When] we turned it [the organization into] our own society.

GRAYSON: What was the impetus behind that? Do you recall why that was done?

HONIG: You ask a good question. I don't think I have a reasonable answer for it.

GRAYSON: Okay.

HONIG: But, I think really, that group went well beyond the description, being an ASTM sponsored committee. And of course, it became more and more clear as time went on, so this then led to the thought that we should be a separate, but equal society, who with ASTM was in parallel.

GRAYSON: And was it you…weren't you one of the first people involved as President?

HONIG: I was the second President, yes.

GRAYSON: You were the second President of ASMS [after Joe L. Franklin].

HONIG: Yes.

GRAYSON: Okay. And that would have been whenever, well in the late 1960s, early 1970s, probably.

HONIG: […] I was […] ASMS Vice President for Programs, 1968 to 1970. […Then I was] President 1970 to 1972, Past President 1972 to 1974.

GRAYSON: So, that was a pretty important period in the society as [it had] just started [to be independent of] ASTM.

HONIG: It was a very important period, because we didn't know whether we were going to fly. And I suspect we didn't have more than sixty or eighty people. And by now, seven thousand.

GRAYSON: Oh, yes. Yes.

HONIG: What is the present count?

GRAYSON: In terms of members, I'm thinking somewhere around, between two and three thousand…maybe twenty-four hundred or five hundred, something like that.

HONIG: Yes. In the meetings, we…

GRAYSON: Just continued to get bigger.

HONIG: Bigger and bigger.

GRAYSON: I, myself, am curious how long that will last, given the fact that so many companies are doing what RCA did, in reducing their investment in science. But, evidently, the meeting size continues to get larger. So, I guess people are finding other ways than working for companies to do science <**T: 05 min**>.

HONIG: Yes.

GRAYSON: Were there any particular issues that you had to deal with other than, this starting up kind of thing in your particular tenure as President or in those six years when you were involved? Was there any topic that was of concern or that we don't know about?

HONIG: Well, I guess the topic really was, let it fly.

GRAYSON: I mean, did the group actively do things to promote it flying or [did] they just, kind of, say, we're going to this and see if it works?

HONIG: Well, one of the issues that comes back to me now, is how do we divide up whatever financial support that existed between ASMS and ASTM E-14? And I guess it was divided evenly. Even though the number of people going into ASMS were far in excess of the people who stayed in this one group. But, what were the issues? Well, you had of course, personality problems. I'm sure. [laughter] But it was an active group, and we grew up together, mostly.

GRAYSON: Yeah. Well, obviously, it did fly. We got a very active group still, and I'm sure it's in no small part to the contributions that were made by those people who were having to deal with it, when it split off from the ASTM. And that was pretty successful. So, I think we all owe you a debt of gratitude for making sure that it worked…

HONIG: No. Well, one of the things we were trying to do was to get financial support from the companies, for example. So, we had a group of maybe, some twenty supporters initially, as I recall. Everything was on a small scale.

GRAYSON: Did ASTM provide any funds?

HONIG: Well, as we split, there were funds of ASTM and those funds were split, as I remember, one to one, even though ASTM E-14 had by far the smaller group of people staying there. It was quite a small…and initially we were agreeing they were coming to our meetings as part of it. So, there was essentially a group…partially independent group within. Well, not probably…not part of ASMS anymore.

GRAYSON: Well, as I guess you're aware, ASTM eventually was dissolved, I guess.

HONIG: I don't know what happened.

GRAYSON: Well, I was involved in a little bit of that, [R.] Graham Cooks, when he was President, floated an issue to decouple the ASTM E-14 with ASMS, and make them more independent; without whatever connections had been put in place.

HONIG: What year was that?

GRAYSON: Whenever he was president, which would have been probably about ten or twelve years ago. I'm guessing. But I guess what had happened was the ASTM-[E14] continued to be a smaller and smaller group, but it had equal weight with ASMS in board decisions and so on. And he felt that that was an inequity that should be rectified. So, basically at that time, there was a formal <**T: 10 min**> disconnection of the two societies of ASTM E-14 and ASMS.

And I don't even know if there is an E-14 group in existence anymore, because the original charter was to establish standards. [As] part of ASTM, [Committee E14] was to establish standards, and the mass spec meeting has obviously becoming a very critical part of science, but it had very little to do with establishing standards later on. Originally, it did have that type of a flavor. But [later on] it seemed to be that the annual conference was the primary function that ASTM actually discharged.

HONIG: And of course, those meetings initially, I recall, five days, possibly one or two sessions at most. And the talks were fairly long. They might have been, certainly twenty minutes at least. [As the meetings] got bigger and bigger, and time [for talks] got shorter and shorter. [laughter] And the number of concurrent sessions grew like Topsy.

GRAYSON: And when you were the Vice President for Programs, […] maybe this wasn't too serious a problem; where you didn't have to balance too many concurrent sessions.

HONIG: No. There were at most two concurrent sessions. Yes.

GRAYSON: Well, posters have become a big part of our conference.

HONIG: Absolutely, that's the only way to handle it.

GRAYSON: Are there any…this is, kind of, a big overview question. But, as we're becoming more and more aware; you [practically] lived through a period, technologically speaking, that is, almost like going from the Dark Ages…

HONIG: Stone Age, yes.

GRAYSON: Stone Ages. What do you see as being a major development in that period that—or one or two things—that maybe were driving forces for the blossoming of technology in the period of your lifetime? Clearly, there are a large number of developments. Have you reflected on any way on how maybe one particular or two particular technological developments have impacted things?

HONIG: I would guess the solid state devices, probably would be one of the outstanding ones. The death of the vacuum tube. [laughter]

GRAYSON: That probably was a good thing.

HONIG: Yes.

GRAYSON: It was a necessary technology until we got a better way of doing it.

HONIG: Yes. Well into the mid-1950s. As I mentioned earlier, I grew up with FP-54s that were about this big, that existed already in 1938.

GRAYSON: Was this like a triode?

HONIG: A pentode.

GRAYSON: A pentode?

HONIG: Yes. It was a pentode. And galvanometers. It was really the Stone Age. It was fun. There was a lot of excitement there, challenge.

GRAYSON: Did you ever feel discouraged because you didn't have the technical tools to perform a particular task? In development of equipment, you're always pushing technology <**T: 15 min**>, and it'd be nice if you had a better amplifier, or a better vacuum, or better gate valve.

HONIG: Well, not discouraged, but we were pleased to see the natural developments that occurred, whether it's in vacuum technology. After all, when I took the course in electronics at MIT, I was told a vacuum of 10^{-8} [torr]is about the best one you're ever to achieve. Of course, the reason was that you couldn't measure any better than that.

GRAYSON: What degrees did you take at MIT, were they engineering or physics?

HONIG: Physics. I guess, initially, I enrolled in engineering because I had my engineering degree. And then, took all physics courses, and so I ended up in physics.

GRAYSON: They had a PhD program in electrical engineering at that time?

HONIG: Oh, yes, they did. Yes. But, it was clearly not the area that I was interested in.

GRAYSON: Were you able to keep abreast of developments in electronics, in your instrument design at work? Were you up to speed on the latest in tubes and…

HONIG: Well, I would say yes. The development was slow in coming at the time. But, everything's, like, just making a magnetic field measurement, for example.

GRAYSON: Or measuring temperature.

HONIG: Well, measuring temperature…

GRAYSON: I meant to ask, did you like use [Knudsen] cells or for these gas, the vapor pressure of the elements […]?

HONIG: No, I did not. The equipment that I built wasn't big enough to accommodate those cells. So, I had to rely on some [device] that had to work in [a small space] when I read the temperatures.

GRAYSON: And so, you just simply heated the element in a small crucible?

HONIG: Small, yes, small crucibles. And there were problems like interaction, for example with silicon. What do you put silicon into so it doesn't interact?

GRAYSON: Did you have problems with species condensing out in places you didn't want them, in the instrument?

HONIG: Well, only when some well meaning person turned off, at one time, the fore pumps, and let the mercury vapor pumps keep on pumping. [laughter]

GRAYSON: Not a very smart thing to do.

HONIG: No. That happened at RCA.

GRAYSON: And why would [someone do that]?

HONIG: There was a [power] shutdown, which I wasn't aware of. When I came in the next morning, it left on the…of course, in those days we used mercury vapor pumps. And there was mercury vapor all over the place. Not good!

GRAYSON: [laughter] Must have taken some time to clean up that.

HONIG: I forget what I had to do, but it was from the bottom on up <**T: 20 min**>. Yes.

GRAYSON: Well, I don't know if there's any other comments, items that you feel that you'd like to cover…

HONIG: Well, there might be some that I can think of as we go along. I came poorly prepared, I'm afraid.

GRAYSON: No.

HONIG: In the sense that, I should have refreshed my memory and that begins to be a more, and more important thing to do.

GRAYSON: Well, why don't we take a break and…

HONIG: So, why don't we take a break, and chat for a moment […].

[recording paused]

HONIG: So, here's a brief biography.

GRAYSON: Okay.

HONIG: Here's a publications list.

GRAYSON: Excellent.

HONIG: And that goes up to 1982. Then, here was something that Jane Gale and Bryan Bentz did in 1989, I think. There is an [Honour Biography] issue [of the *International Journal of Mass Spectrometry and Ion Processes*, 103 (1990)]…

GRAYSON: I think I've got a copy.

HONIG: You have that?

GRAYSON: Jane sent a copy of this.

HONIG: Jane sent a copy. Okay, very good. So, you have that whole thing. You don't need this.

GRAYSON: The "Stone-Age" paper looks good.[12]

HONIG: The "Stone-Age" paper, I did in [1995], which tells you quite a bit of background everywhere. May be a little better too, than what I did today. And that then, was a part of this [copy of the *International Journal of Mass Spectrometry and Ion Processes* issue containing the article]. I just brought this along to show […]. Let's see. Bryan Bentz put together this issue. And you have that at home.

GRAYSON: Yes.

HONIG: You have a complete set of publications, of course. […]

GRAYSON: So, this was *International Journal* [*of Mass Spectrometry and Ion Processes*]?

HONIG: Volume 143, 1995, 1 to 288. That's where the "Stone-Age" paper came out. I did some other things at various times. Now, let me see.

[12] Honig, "Stone-age Mass Spectrometry."

GRAYSON: Did you ever meet Al Nier in your meetings?

HONIG: Yes, I did. I did several times. I met him at meetings. I once came over to their house…1970 or 1972. I met him at this [ASMS conference in San Antonio].

GRAYSON: Oh, the San Antonio…

HONIG: San Antonio Conference in 1984, where I put together [an invited Retrospective Lecture].

GRAYSON: And that was in 1984, San Antonio.

HONIG: [There] was a special [issue of the *International Journal of Mass Spectrometry and Ion Processes*].

GRAYSON: Let's see if that's the same thing as this, maybe.

HONIG: It's Volume 66, 1985.[13]

GRAYSON: Did you ever get to interact with Nier on any projects?

HONIG: No, not directly, because he then went into the space projects that he had and I did not. So, there was Harry Svec. Yes, that's of Nier.

GRAYSON: Oh, okay <**T: 25 min**>. Those were papers that came out of the ASMS meeting, I guess.

HONIG: Yes. They were…

GRAYSON: Published…

[13] Honig, "The Development of Secondary Ion Mass Spectrometry (SIMS): A Retrospective."

HONIG: …published. So, I put together pretty much a history of SIMS there.

GRAYSON: Okay, I've got the reference for that.

HONIG: You have the reference for that. Now, let's see what else, I brought along.

GRAYSON: Oh, do you have any of those early ASMS proceedings or ASTM E-14 conference proceedings?

HONIG: [laughter] No, I don't have those anymore. I left those behind at RCA. Yes. I had pretty much the complete set. There's one person who might still have them and that is [J.] Rogers Woolston, who went to most of the early meetings.

GRAYSON: Where is he now?

HONIG: Well, he left RCA labs already, fairly early. Didn't have much of a future there. He's a very interesting guy. He worked with me in fact, in 1957, he came, and very clever, very good instrumentalist. Far better than, I ever was. And so, he contributed quite a lot in fact, to my early instruments, like automatic controls for current controls. And he religiously went to all the meetings. So, he should still have them, and since he still lives in Princeton, he would be likely to have that information. He then went into computers early on. I would say he went into computers already in the 1970s. But he has a strong feeling of history. And I have his address at home, if that helps.

GRAYSON: Yes.

HONIG: I have to dig it out and I'll be glad to dig that one out. [papers shuffling] No, that's nothing. Nothing else. I had made two parallel copies. Okay. So, that's probably what I have in hand here.

GRAYSON: Well, this is good documentary material that will go with the interview.

HONIG: I still have my copies of my publication that's about all that I took along.

GRAYSON: When you left RCA, was it…I mean, you wanted to quit then, at that time? Or were you asked to leave?

HONIG: No, I wasn't asked to leave. I had [planned to leave in June of 1987 when I turned seventy…. But] the Laboratory [(RCA Labs)] pretty much folded in March of 1987, [so] by my retiring a few months early, I was able to [let] somebody else to stay [avoid being laid off] . So, I took [retirement] a few months earlier and left 31 March 1987.

GRAYSON: So, you left the laboratory more or less, when you wanted to.

HONIG: Yes, yes. But, I still worked with Bryan Bentz until [my wife and I] left [Princeton] in 1989. So, I still came in and worked with him [during] that time [(1987-1989)].

GRAYSON: But it was kind of pushed along a little bit, because of the management.

HONIG: It was – I didn't have to, but I certainly felt if I could help somebody else to stay, it'd be great. And so it was a matter of a few months. I would [have retired] in June, my birth date, birth month […].

GRAYSON: Okay. When you went to one of the mass spec meetings in Oxford [United Kingdom], you indicated to me that you had met Al Nier at that time. Was that the first time that you had met Al, personally or on one-to-one, face-to-face ?

HONIG: Probably, yes. In 1961…

GRAYSON: And was it [Josef] Mattauch or [Richard F.K.] Herzog was there…

HONIG: Mattauch…

GRAYSON: Mattauch.

HONIG: Yes. And they were obviously, old acquaintances. And found each other, so to speak, and decided that they wanted to commit this meeting to his camera, movie camera. So, they asked me to take the picture. And pretty soon, so I was shooting the picture straight ahead.

Pretty soon, one appeared from the left, and the other one appeared from the right, and came to the middle, greeted each other by doffing their hats, bowing deeply, and departing in the opposite direction. That was all.

GRAYSON: Was that camera Al Nier's camera or was that…

HONIG: No. That was Mattauch's.

GRAYSON: Mattauch's camera. So, probably that film exists somewhere.

HONIG: It might, but…

GRAYSON: Getting hold of it would be difficult, but it's an interesting thing that someone might want to pursue. Were there any other acquaintances? You met Al at a couple of other meetings, occasions, as I recall.

HONIG: I met him then a number of times. One of them was at invited conference in 1984 in Texas, I guess. This is it.

GRAYSON: Well, was that the San Antonio [meeting]?

HONIG: Yes, San Antonio meeting.

GRAYSON: […] Right. Did you have any technical interactions or were you just primarily [meeting as] friends and…

HONIG: Well, we met, had dinner together. But, there was no specific technical interaction. He talked about his fine mass spectrometers, as I remember. But he was always a very gracious, very warm, open person who was always willing to share his knowledge with other people.

GRAYSON: Are there any other prominent mass spectroscopists that you can recall off of the top that you might have met on the caliber of Nier, say Mattauch […]?

HONIG: […] Oh, yes, there was a German by the name of Hintenberger, who was very capable. A Dutchman <**T: 35 min**> […].

GRAYSON: Okay. Well, unless you have some other comments, or…

HONIG: No.

GRAYSON: Recollections you want to share with us, and I think we've got a pretty good…

HONIG: Yes.

GRAYSON: Recording here for references. And…

HONIG: Well, if there are any afterthoughts, I can always get in touch with you.

GRAYSON: Sure.

[END OF AUDIO, FILE 1.5]

[END OF INTERVIEW]

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