SCIENCE HISTORY INSTITUTE

ALEX G. HARRISON

Transcript of an Interview
Conducted by

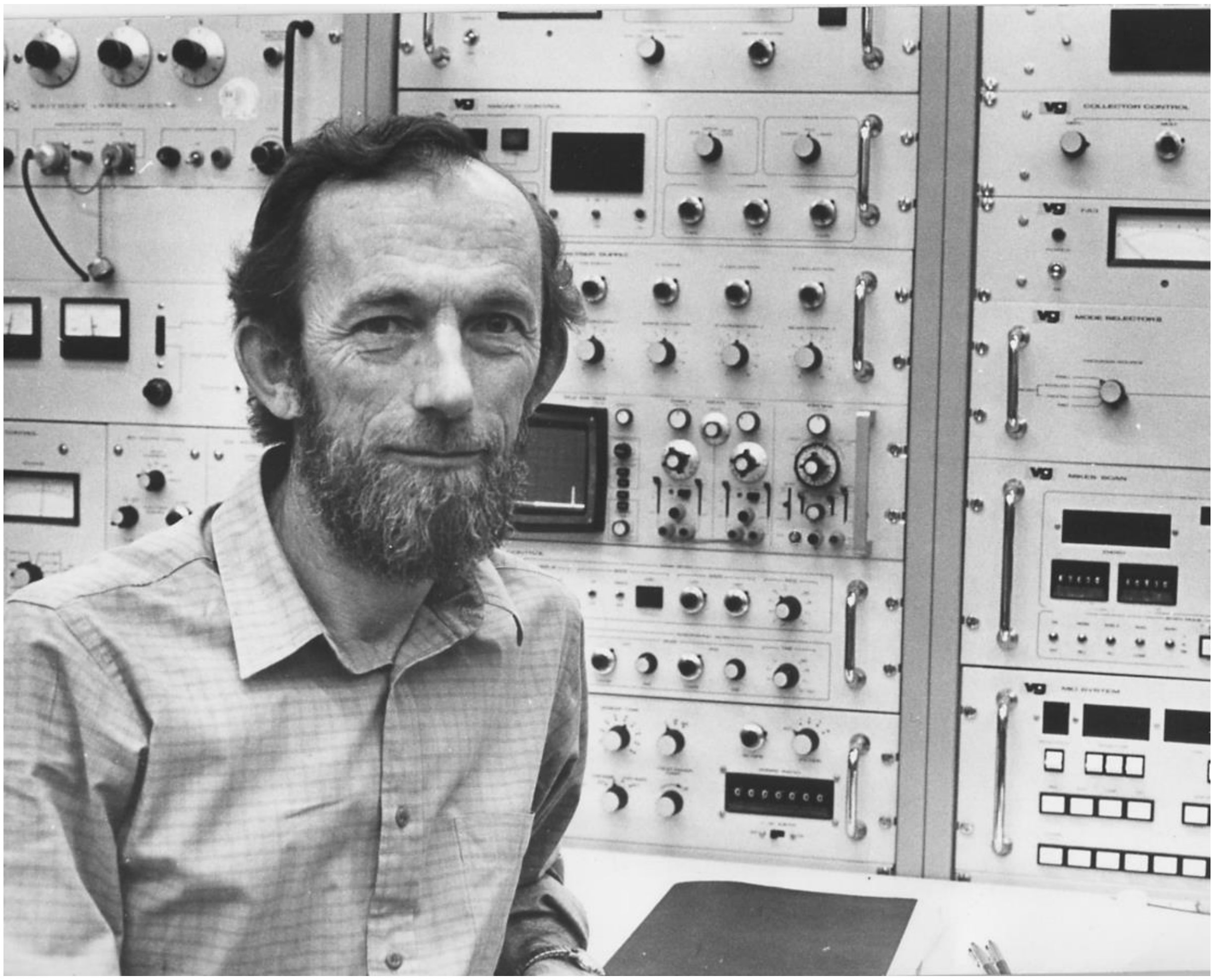
Michael A. Grayson

at

University of Toronto
Toronto, Ontario, Canada
on

13 November 2013

(With Subsequent Corrections and Additions)



Alex G. Harrison

ACKNOWLEDGMENT

This oral history is one in a series initiated by the Science History Institute 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.

This project is made possible through the generous support of the
American Society for Mass Spectrometry.

CHEMICAL HERITAGE FOUNDATION
Center for Oral History
FINAL RELEASE FORM

This document contains my understanding and agreement with the Chemical Heritage Foundation with respect to my participation in the audio- and/or video-recorded interview conducted by Michael A. Grayson on 13 November 2013. I have read the transcript supplied by the Chemical Heritage Foundation.

1. The recordings, transcripts, photographs, research materials, and memorabilia (collectively called the "Work") will be maintained by the Chemical Heritage Foundation and made available in accordance with general policies for research and other scholarly purposes.

2. I hereby grant, assign, and transfer to the Chemical Heritage Foundation all right, title, and interest in the Work, including the literary rights and the copyright, except that I shall retain the right to copy, use, and publish the Work in part or in full until my death.

3. The manuscript may be read and the recording(s) heard/viewed by scholars approved by the Chemical Heritage Foundation unless restrictions are placed on the transcript as listed below.

This constitutes my entire and complete understanding.

(Signature) ______________________
ALEX HARRISON

(Date) 3 January 2016

OPTIONAL: I wish to place the following restrictions on the use of this interview:

I understand that regardless of any restrictions that may be placed on the transcript of the interview, the Chemical Heritage Foundation retains the rights to all materials generated about my oral history interview and will make the title page, abstract, table of contents, chronology, index, et cetera (collectively called the "Front Matter and Index") available on the Chemical Heritage Foundation's website. Should the Chemical Heritage Foundation wish to post to the Internet the content of the oral history interview, that is, direct quotations, audio clips, video clips, or other material from the oral history recordings or the transcription of the recordings, the Chemical Heritage Foundation will be bound by the restrictions for use placed on the Work as detailed above. Should the Chemical Heritage Foundation wish to post to the Internet the entire oral history interview during my lifetime, I will have the opportunity to permit or deny this posting.

I understand that the Chemical Heritage Foundation will enforce my wishes until the time of my death, when any restrictions will be removed.

Revised 12/18/2012

This oral history is designated **Free Access**.

***Please note*:** This oral history is protected by U.S. copyright law and shall not be reproduced or disseminated in any way without the express permission of the Science History Institute. Users citing this interview for purposes of publication are obliged under the terms of the Center for Oral History, Science History Institute, to credit the Science History Institute using the format below:

> Alex G. Harrison, interview by Michael A. Grayson, University of Toronto, Toronto, Canada, 13 November 2013 (Philadelphia: Science History Institute, Oral History Transcript #0905).

Science History Institute

Chemistry · Engineering · Life Sciences

Formed by the merger of the Chemical Heritage Foundation and the Life Sciences Foundation, the Science History Institute collects and shares the stories of innovators and of discoveries that shape our lives. We preserve and interpret the history of chemistry, chemical engineering, and the life sciences. Headquartered in Philadelphia, with offices in California and Europe, the Institute houses an archive and a library for historians and researchers, a fellowship program for visiting scholars from around the globe, a community of researchers who examine historical and contemporary issues, and an acclaimed museum that is free and open to the public. For more information visit sciencehistory.org.

ALEX G. HARRISON

1931	Born in Peterborough, Ontario, on 1 April

Education

1952	BSc, University of Western Ontario (London, Ontario), Chemistry
1953	MSc, University of Western Ontario, Chemistry
1956	PhD, McMaster University (Hamilton, Ontario), Chemistry

Professional Experience

	McMaster University, Department of Chemistry
1956-57	Postdoctoral Fellow
	National Research Council, Ottawa
1957-1959	Postdoctoral Fellow, Chemistry
	University of Toronto, Department of Chemistry
1959-1960	Lecturer
1960-1962	Assistant Professor
1962-1967	Associate Professor
1967-1993	Professor
1971-1974	Associate Chair
1993-present	Professor Emeritus
	University of Warwick, Department of Molecular Sciences
1975	Visiting Professor
	École Polytechnique Fédérale de Lausanne, Institut de Chimie Physique
1982	Professeur Invité
	University of Colorado, Boulder, Department of Chemistry and Biochemistry
1989	Visiting Professor

Honors

1962-1964	Alfred P. Sloan Fellow Award
1971	Noranda Lecture Award, Chemical Institute of Canada
1971	Fellow, Chemical Institute of Canada
1980	Visiting Distinguished Alumni Lecturer, McMaster University
1985-1987	Killam Research Fellow
1986	Maccoll Lecturer, British Mass Spectrometry Society
1995	Canadian Society for Mass Spectrometry Award of Excellence
2005-present	Alex Harrison Graduate Fellowship in Analytical Mass Spectrometry, University of Toronto

ABSTRACT

Alex G. Harrison was born in Peterborough, Ontario, Canada, one of two sons. His parents were farmers but had the Scottish appreciation for education. Harrison attended a one-room school, where his aunt was teacher. He won a two-year scholarship to the University of Western Ontario and decided to study chemistry. Harrison completed both his bachelor's and master's degrees there. Next, he went to McMaster University for a PhD. He worked on thyroid function and thyroxine in Harry Thode's lab, getting a much-cited publication. The sulfur cycle introduced him to mass spectrometry. His postdoctoral applications of his PhD work, still in Thode's lab, garnered him two more publications. He did a second postdoc on free radical mass spectrometry with Fred Lossing at National Research Council (NRC). He married during this time, and he took up skiing at Paul Kebarle's urging.

Harrison's first academic position was as lecturer at the University of Toronto, where he began research into ion molecule reactions. He earned tenure, taught, and became associate chair of the department. With funding from the NRC, Harrison was able to purchase a double-focusing mass spectrometer and set up a service lab. A chemical ionization (CI) mass spectrometer enabled him to analyze a broader array of compounds. Harrison became active in the American Society for Mass Spectrometry (ASMS), serving on the board of directors. He organized a regional lab at McMaster. When he received the Izaak Walton Killam Research Fellowship he was able to dedicate two years solely to research; he began working on negative ion chemistry and produced another much-cited publication. Reactive collisions and fast atom bombardment (FAB) and peptides and b ions have occupied him since. Taking early retirement, Harrison was able to keep his lab and continue to work on b ions. He still maintains collaborations with Talat Yalcin, Bela Paizs, and Benjamin Bythell, and is still publishing.

Harrison discusses international contributions to the field of mass spectrometry. He feels that current mass spec work is perhaps too much focused on development, rather than research. He believes that having trained many good mass spectrometrists is one of his major contributions. He credits his mentors for giving him encouragement and the freedom to explore; and he also praises his wife. He describes his own mentoring style. He celebrates that there are more women in science, especially environmental science. He considers mass spectrometry less competitive than other fields, and more collegial and cooperative. Though the field is radically changed from his early days, he believes that mass spectrometry has much still to provide to science, that its future is neither predictable nor stagnant.

INTERVIEWER

Michael A. Grayson is a member of the Mass Spectrometry Research Resource at Washington University in St. Louis. He received his BS degree in physics from St. Louis University in 1963 and his MS in physics from the University of Missouri at Rolla in 1965. He is the author of over 45 papers in the scientific literature. Before joining the Research Resource, he was a staff scientist at McDonnell Douglas Research Laboratory. While completing his undergraduate and graduate education, he worked at Monsanto Company in St. Louis, where he learned the art and science of mass spectrometry. Grayson is a member of the American Society for Mass Spectrometry (ASMS), and has served many different positions within that organization. He has served on the Board of Trustees of CHF and is currently a member of CHF's Heritage Council. He currently pursues his interest in the history of mass spectrometry by recording oral histories, assisting in the collection of papers, and researching the early history of the field.

ABOUT THIS TRANSCRIPT

This interview was conducted as part of the Mass Spectrometry Oral History project, a collaboration between the Science History Institute and the American Society for Mass Spectrometry. The Mass Spectrometry Oral History project records the human dimensions of the growth of mass spectrometry in academic, industrial, and governmental laboratories during the twentieth century.

The Center for Oral History, Science History Institute, is committed both to preserving the recording of each oral history interview in our collection and to enhancing research use of the interviews by preparing carefully edited transcripts of those recordings. The preparation of interview transcripts begins with the creation of a verbatim typescript of the recording and proceeds through review and editing by staff of the Center; interviewees also review the typescript and can request additions, deletions, or that sections be sealed for specified periods of time. We have established guidelines to help us maintain fidelity to the language and meaning of each recorded interview while making minor editorial adjustments for clarity and readability. Wherever possible, we supply the full names of people, organizations, or geographical locations mentioned during the interview. We add footnotes to the transcript to provide full citations for any publications that are discussed, to point to extant oral history interviews, and to clear up misstatements or provide context for ambiguous references in the transcript. We use brackets to indicate the addition of material that was not in the audio, and bracketed ellipses to indicate the deletion of recorded material. The transcript also includes time stamps at five-minute intervals. We omit without noting most instances of verbal crutches and all instances of nonlexical utterances. We also make small grammatical corrections where necessary to communicate interview participants' meaning. Finally, staff of the Center create the abstract, chronology, table of contents and index.

TABLE OF CONTENTS

Early Years 1

Born Peterborough, Ontario, Canada. Parents farmers. One-room schoolhouse; Scottish emphasis on education's value. Won two-year scholarship to University of Western Ontario; finished both BSc and MSc there. Christian Sivertz's influence. Paying tuition; raised calf that won $900 prize.

McMaster University and Early Career 7

Entered PhD program at McMaster University. Worked in Harry Thode's lab on thyroid and thyroxine; also sulfur cycle, requiring mass spectrometry; using homemade spectrometer. Wrote much-cited publication. Continued as postdoc in Thode's lab. Isotope geochemistry. Postdoc at National Research Council (NRC), working for Fred Lossing on free radical mass spectrometry. Married. Paul Kebarle. Building or adapting instruments.

University of Toronto 21

Accepted lecturer position at University of Toronto. Given mass spectrometer by Donald LeRoy. Ion molecule reactions. Frank Field, Burnaby Munson, Victor Talrose, Jean Futrell. Alfred P. Sloan Foundation Fellowship gave him two years of funding. Earned tenure. Teaching duties; becoming associate chair of department, bringing back analytical chemistry, and being responsible for undergraduate program. When Sloan Fellowship ended, NRC increased funding. Funding from NRC to purchase double-focusing mass spectrometer; set up service lab.

Conferences, Sabbaticals, and Mass Spec Research 35

Attendance at conferences. American Society for Testing and Materials (ASTM). Origins of American Society for Mass Spectrometry (ASMS). Chemical ionization (CI) mass spectrometer. Seymour Meyerson. Attending international conferences of ASMS in Oxford, England, and Paris, France. Sabbatical with Keith Jennings at University of Warwick. Tino Gäumann in Lausanne, Switzerland. Organized regional lab at McMaster, with gas-phase mass spec at University of Toronto; funding from Natural Science and Engineering Research Council (NSERC). Adding quadrupole instrument. Recycling used mass spectrometers. Izaak Walton Killam Research Fellowship: no teaching for two years; began working on negative ion chemistry. Another much-cited publication. Reactive collisions. Fast atom bombardment (FAB) and peptides and b ions. Talat Yalcin from Turkey.

Early Retirement 60

Able to keep lab and office, continue to work on b ions. Funding decreasing after emeritus status granted. Supervising only fourth-year student research. Continues collaborations with Yalcin, Bela Paizs, Benjamin Bythell; publishes. Gas-phase basicities and proton affinities. Sequencing and scrambling. Descriptions of his instruments. Patents. New project with trapped ions. General CI not used so much anymore; now atmospheric pressure CI.

General Observations 75

Most significant paper ‘Bond Strengths of Ethylene and Acetylene.’ Writing papers vs. writing book on chemical ionization. Editor, board of directors of ASMS. Hundredth anniversary of mass spectrometry’s beginnings; Paper on early days of ion molecule reactions. Citation indices. International contributions to field. Current mass spec work more applied. Having trained many good mass spectrometrists a major contribution. Many foreign students. Canadian scientists ‘muzzled’ by government. Encouragement from mentors and wife. Mentoring and advising More women in science, especially environmental science. Changes to the field; future not predictable but not stagnant.

FIGURES

INTERVIEWEE: **Alex G. Harrison**

INTERVIEWER: **Michael A. Grayson**

LOCATION: **University of Toronto**
Toronto, Ontario, Canada

DATE: **13 November 2013**

GRAYSON: [. . .] This is the 13th of November, Wednesday, 2013. I'm at the office of Professor Alex Harrison at the University of Toronto, and my name is Mike Grayson and we're going to do an oral history interview of Professor Harrison and his career. So now that we have the formality out of the way with regard to that, I'll go ahead and ask Professor Harrison to tell us something about his parents, his early childhood, his upbringing, and those kinds of things.

HARRISON: Hmm, okay. I'm an April Fool. I was born April the 1st, 1931, in Peterborough, Ontario. Peterborough is a city about one hundred kilometers northeast of Toronto, [Ontario]. My parents were farmers, and so I'm a farm boy. We lived for the first six years of my life on a farm north of Peterborough, at which time we moved to southwestern Ontario and ended up on a farm almost halfway between the cities of London [Ontario] and Sarnia [Ontario]. For American people Sarnia will be known as being across the river from Port Huron, Michigan. So that was the area. It wasn't too far from Detroit, [Michigan] either. So from the age of six on I grew up on [this] farm. And the local town was called Watford [Ontario]. My primary school was a one-room rural schoolhouse, at which my aunt actually happened to be the teacher, so there was grade one to eight all in one room. That went fairly smoothly. Well, my birthday was essentially at Easter time and I was allowed to start school right after that. There weren't as many formalities. So my grade one was from Easter till the end of June. Then came back in the fall I was in grade two.

GRAYSON: So you kind of took a rapid advancement in your—

HARRISON: I finished primary school, grade eight, at the age of twelve and went to high school in the local town, Watford. I think the total population of Watford is about one thousand, and there were one hundred to one hundred fifty students in the school.

GRAYSON: Okay. In just a minute we'll go and talk a little bit about the farm upbringing. During your childhood, were you involved in the farming activities or—

HARRISON: Oh, yes. Yes. Once I got into high school in 1943? Yes. They had this game by the time I was in grade nine or ten—during the war—of course, that you could be excused from school early [. . .] to help the war effort by going home and working on the farm. So I did that a couple of summers after getting out, say the first of May or so—getting out of school. My grades were considered good enough.

GRAYSON: And so what kind of farming was this?

HARRISON: It was mixed. [. . .] Dairy cattle, some beef cattle, sheep, pigs.

GRAYSON: Okay. [. . .] You didn't grow crops as much as you did breeding?

HARRISON: Not to sell, no. It was a 100-acre farm, [. . .] so you couldn't do a lot.

GRAYSON: You did that during high school for a couple years and then—

HARRISON: Well, I finished high school in 1948.

GRAYSON: So, somewhere along the line this development of your career—by the way, let's get the names of your parents down because I think that's important. Your father's name was?

HARRISON: It was Leslie Harrison.

GRAYSON: Leslie.

HARRISON: Yes, and my mother's name was Grace. My mother died in early 1945, [. . .] and so my father and I were bachelors on the farm from then on.

GRAYSON: Oh, boy. So you're the only child?

HARRISON: I had a brother. He was six years older than me and he was in the Navy in the later part of the Second World War and was not at the farm. [. . .] He was allergic to every animal and plant that existed on the farm, so **<T: 05 min>** as soon as he was sixteen and could legitimately quit school, he moved to London and got a job. [. . .] He was much better off in the urban atmosphere than the rural atmosphere.

GRAYSON: [Yes] And his name was?

HARRISON: His name was Donald.

GRAYSON: Is he still alive or—

HARRISON: No, he died about seventeen years ago. [. . .] He was six years older than me. [. . .]

GRAYSON: And your mother's maiden name was?

HARRISON: Edwards.

GRAYSON: Edwards. Grace Edwards, okay. Now you grew up on a farm, but was there an emphasis on education in the family?

HARRISON: Yes. My mother had been a teacher before she married, in rural one-room schoolhouses, so she certainly encouraged [it]. And it was a largely Scottish community, and they placed a great emphasis on education.

GRAYSON: Okay. So that's important to understand the impact of early influences on your career and your decision to go into education—well, highly educated—

HARRISON: At that time farming was very physical and demanding, and I didn't think I had the physique to really carry on as a farmer. [. . .] I didn't know quite what I was going to do when I finished high school.

GRAYSON: Okay. Was there any teacher in high school that inspired you in any way?

HARRISON: Well, it was clear that if I was going to go on for any further education it was science or mathematics. It certainly wasn't the arts. I sort of got into chemistry by default. A local man, a young chap, had just done well at the Ontario Agricultural College studying soil chemistry, and this got a lot of publicity, so I thought, "Well, maybe I'll go to Ontario Agricultural College and study soil chemistry." [. . .] So I applied there, and they wrote back and said, "Well, you have to be eighteen before you can go," and I was only seventeen, so I thought, "Well, maybe I'll wait a year." So I went to Sarnia, which was the chemical valley. [. . .] And I applied for a job in all the companies there: Dow Chemical, various other ones, and got nothing.

GRAYSON: So you were interested in trying to start your career, and they weren't interested enough. [laughter]

HARRISON: So this was getting us on into the summer of 1948, and I should note that at that time there was a grade thirteen. High school now ends at grade twelve, [. . .] but then it didn't. It was grade thirteen. The final exams in grade thirteen were province-wide. They were set by the Ontario Ministry of Education. So, everybody wrote the same final exam in each subject. [. . .] The University of Western Ontario, which is in London, had the habit of giving a scholarship to the top student at each high school in its catchment area. And so I happened to be first in Watford High School in those exams, so I got a scholarship from Western, two years' tuition.

GRAYSON: Now, this London that you're speaking of is the London –

HARRISON: London, Ontario, [. . .] which is two or three hours west of here.

GRAYSON: Okay, okay. And yet you say the catch—

HARRISON: In the high schools in their area they offered a scholarship of two years' free tuition to the top student at each grade thirteen.

GRAYSON: And that area is what you referred to as a [catchment]?

HARRISON: What I call a catchment area—the high schools they would get their students [from]—so I decided to go to Western Ontario University. [. . .] I thought, "Gee if I'm going to a real university, I'll study mathematics and become an accountant or an actuary." I like playing with numbers. The first year was completely common for science and mathematics at that time, and it was my first introduction to calculus. [. . .] And somewhere or other I didn't get it, so I decided that **<T: 10 min>** after the first year if I went on in mathematics I was going to see more calculus, so I became a chemist. [laughter]

GRAYSON: So you had enough exposure to chemistry to feel that this was something that you could manage.

HARRISON: I thought that was a very good choice, yes, and I've never regretted it.

GRAYSON: Well, you know, it's too bad you didn't go into mathematics then. Maybe you would've been a great mathematician.

HARRISON: I'll leave that to my grandson.

GRAYSON: Okay. Is he studying mathematics?

HARRISON: He has a bachelor's degree in mathematics and he's a graduate student at the University of California, Berkeley. [. . .]

So, I graduated in 1952 in honors chemistry from the University of Western Ontario.

GRAYSON: Okay, but you refer to this as, maybe, not a real university or—

HARRISON: Oh, it was a real university, yes. Yes, it's a major university. It was one of the big four around at that time. And I stayed on there for a master's degree where I worked on free radical addition of mercaptans to olefins [photochemically] initiated. And I got my master's degree in 1953 [. . .] and then went on to McMaster University, which is [. . .] in Hamilton [Ontario], [. . .] which is sort of at the end of Lake Ontario—the west end of Lake Ontario. So it's fairly close from here and I started my PhD studies there—worked with a man by the name of Harry Thode.

GRAYSON: Now, were there any inspirational chemistry teachers in this route from undergraduate through master's degree to—

HARRISON: Oh, certainly at Western, the man I did my master's degree with—Professor [Christian] Sivertz—was quite a significant influence on my life.

GRAYSON: And that was because?

HARRISON: He encouraged me in many ways, and so I enjoyed working with him. Professor Thode at McMaster gave me a lot of free rein and encouraged me to do things on my own, so it worked out very well.

GRAYSON: [Yes], so Sivertz was more of a mentor when you were a graduate student?

HARRISON: Yes. Well, I did a fourth-year research project with him as well, so it was both fourth year, and first year of graduate school.

GRAYSON: Do you recall the circumstances of that period with regard to, you know, cost of college? How did you pay for your—did you say you got, was it a two-year—

HARRISON: Two years' free tuition.

GRAYSON: Free tuition and then you had to pick up your—

HARRISON: My father essentially picked up the [rest]—well, there was one thing. In 1948, about the time I got the offer of free tuition at the University of Western Ontario the local Rotary Club operated a calf club. [. . .] And so what they did is, they brought in a bunch of calves from the west in the fall, and the farmers' sons and daughters picked out one, and fed it and trained it and showed them in the following August. And I think that was the only time I ever went in the calf club, was that year. My father picked out the calf. It ended up being the champion calf when it was shown in August of '48 [. . .] and sold for nine hundred dollars. That was a lot of money in those days. [. . .] So that helped get me through university. But my father helped, of course. [. . .] He said, "I'll help you as long as you come home and work on the farm in the summertime."

GRAYSON: So you had this farm influence well after you started going to college.

HARRISON: Oh, yes. Yes.

GRAYSON: So, do you recall, then, what the expenses were for tuition in those days or any—

HARRISON: Oh, it was probably less than five hundred dollars.

GRAYSON: For a semester?

HARRISON: For the year.

GRAYSON: For the year? Wow, okay. And you also had living expenses, I assume. So you had to take care of that.

HARRISON: Yes. So, I was determined once I got my bachelor's degree that I wouldn't be dependent on my father again. [. . .] So I managed to get scholarships and I did have to **<T: 15 min>** borrow the money from the university's loan office to pay my tuition for the master's degree. Unfortunately, a year after I graduated they said I had to pay it back. I told them I was still a graduate student at McMaster, but that didn't cut any ice. [laughter]

So I went to my supervisor, Professor Thode. He, at that time, was involved with a thyroid clinic, [. . .] and that was on the campus. [At] the thyroid clinic they gave people a trace level of radioactive iodine and then, after twenty-four hours, found out how much was protein bound, which is the measure of thyroid activity. That's where the iodination of [proteins primarily occurs]. My job was to handle the radioactive iodine and do the measurements of percent [incorporation] into protein. If the thyroid was too active then they got a larger dose of radioactive iodine to kill off some of the thyroid. [. . .] So I worked several years—a couple years—

GRAYSON: So this was a paying job.

HARRISON: This was a paying job and the University of Western Ontario agreed I could pay so much per month to repay my debt.

GRAYSON: So when the iodine is—it causes the iodination— iodination of percent of the—

HARRISON: Thyroxine. Primarily, I think, it's thyroxine. And this occurs in the thyroid. [. . .] It was interesting.

GRAYSON: It was an experimental program or a treatment program?

HARRISON: It was both. [. . .] I think they were still feeling their way a bit, but they certainly were treating people.

GRAYSON: So, basically checking out the activity of the iodination process represented in the in the information about the activity of the thyroid gland, and whether it was overactive and so on, so they could—

HARRISON: Yes. I forget the numbers, but if you had more than a certain percentage incorporation of the iodine to protein, then your thyroid was overactive. [. . .] If you didn't have very much protein iodination, iodine attachment, then you had another problem. You had a thyroid that wasn't active enough. And then that had to be treated in a different way.

GRAYSON: I see, yes, okay. So you kind of got a little bit exposure to the biological side of the world at an early period, even though you didn't know if you'd end up in a biological business. [laughter]

HARRISON: Right.

GRAYSON: Didn't know it'd be a career.

HARRISON: Exactly. So I got my PhD in '56.

GRAYSON: That was work on sulfur?

HARRISON: Professor Thode had two main lines of research. One was nuclear chemistry—nuclear phenomena. I'd hoped to get into that, but he was very persuasive and got me into sulfur isotope geochemistry. And what I was supposed to do was set up the sulfur cycle in the lab.

GRAYSON: Sulfur cycle?

HARRISON: There's bacteria which will reduce sulfate to sulfide. Then other bacteria, which would oxidize the sulfide to sulfate. The idea was to make up a mix of these two, both anaerobic, and see how much fractionation there was of the sulfur or the isotope is between sulfate and sulfide. If you do the calculations, it should be about six or seven percent enrichment of the sulfate and the heavy isotope sulfur-34. So the first thing I was told—people had looked at the bacteria that reduced sulfate to hydrogen sulfide and reported that occurred with a 1.1 percent fractionation, sulfur-34 reacting more [slowly]. So Professor Thode said, "Well, set up the thing and do this again just to get experience." I got no isotope effect. So he said, "You've done something wrong. Do it again." [laughter]

So I took very great pains and did it for quite a long time. And it turned out that when the bacteria were first young and growing there was no isotope fractionation. But as they got old there was isotope fractionation, like to about two percent. So my project now became trying to understand this.

GRAYSON: Well, [yes], that's what I was going to say. Why was that interesting?

HARRISON: It turned out when the bacteria are very, very active the **<T: 20 min>** rate-determining step in the reduction of the sulfate is the transport across the cell wall, and that isn't involving breaking bonds or anything, so there's no isotope effect. When they get slow—old and slow like us—then the actual reduction of sulfate in the cell [. . .] is rate determining. And that occurs with an isotope effect. [laughter]

GRAYSON: Okay. So one of them is like a transfer or a diffusion situation.

HARRISON: Yes, one is the—well it's an active transport across the cell wall, but—

GRAYSON: Okay, and then later on it's actually some—well it's a reaction.

HARRISON: It's actually breaking bonds, and that occurs with an isotope effect.

GRAYSON: Interesting. So then this, I think, probably represented a very nice contribution to the understanding of that process in the literature.

HARRISON: I must say it's one of my most cited papers, [. . .] the paper we published on that, which was in the *Transactions of the Faraday Society,* so it wasn't actually a biological journal.[1]

GRAYSON: No, but it is surprising. To me, it is a little bit surprising that the change in a heavier isotope would be that a common biological response to the heavier isotope is more—there's a difference in the reactivity of the—

HARRISON: If you look at systems in nature where you have [both] sulfide and sulfate you find that the sulfate is enriched in ^{34}S compared to the sulfide. That's about 6 or 7 percent. [. . .] And of course that involved me with mass spectrometry too—

GRAYSON: So—

HARRISON: To analyze the sulfur isotope ratios.

GRAYSON: So you were using a mass spectrometry in these experiments that you did for Thode.

HARRISON: Yes.

GRAYSON: Okay. So what kind of instrument was that?

HARRISON: It was a homemade. Magnetic deflection, ninety degree.

GRAYSON: Ninety degree?

HARRISON: Electron impact source.

GRAYSON: Sure. So was that—

---

[1] A.G. Harrison and H.G. Thode, "Kinetic Isotope Effect in the Chemical Reduction of Sulphate," *Transactions of the Faraday Society* 53 (1957): 1648-1651; A.G. Harrison and H.G. Thode, "Mechanism of the Bacterial Reduction of Sulphate from Isotope Fractionation Studies," *Transactions of the Faraday Society* 54 (1958): 84-92.

HARRISON: The sulfur compounds were all converted to sulfur dioxide. And then the isotope ratio measured.

GRAYSON: So you got, like, the range of—what is that, 64 and 66 sulfur dioxide?

HARRISON: [Yes], it was—that yes, yes.

GRAYSON: Probably was a low mass.

HARRISON: [Yes].

GRAYSON: And was this ninety-degree-sector homemade instrument based on something that was in the literature? Do you know? Who put that together? You didn't do that, did you?

HARRISON: No, no, that had been there for some time. McMaster was a center for mass spectrometry. When I finished at McMaster, I went on to the National Research Council [NRC] in Ottawa. And I worked with one Fred Lossing. [. . .] And he had several instruments, which again were all [home] made and I think were largely made at McMaster. So they were, again, ninety-degree magnetic sector instruments.

GRAYSON: So, this is pretty much a standard type of instrument that was—

HARRISON: It was very common in Ontario at that time.

GRAYSON: [Yes], okay, and does this fellow work pretty well? Is it a pretty well-performing instrument for your sulfur studies?

HARRISON: Yes, yes. It worked quite well.

GRAYSON: It didn't have any idiosyncrasies or cause you any headaches?

HARRISON: Oh, yes, it did, yes. A mass spectrometer was not terribly reliable in those days, and so we had several periods when it wasn't really functioning terribly well.

GRAYSON: When you recorded spectra, was this on a strip chart recorder or did you do this—

HARRISON: You didn't actually record spectra. The instrument had dual collector slits.

GRAYSON: So it was an isotope ratio kind of thing.

HARRISON: And so you measured the ratio of that, and it had a switch that you could switch back and forth between your sample and a standard, and see how far that position of equilibrium of the ratio changed. And that's how you got your isotope ratio.

GRAYSON: So this was pretty much dedicated to this type of measurement.

HARRISON: Yes.

GRAYSON: It wouldn't be the kind of instrument that you'd walk in and say, "Take a look at this."

HARRISON: Somebody did look at a mass spectrum in the instrument at some stage [on] another instrument and couldn't understand what this big peak was at the mass-[to]-charge ratio of seventy-eight. Of course, it was benzene. [laughter] But we'd never seen a mass spectrum before.

GRAYSON: [Yes]. They're only looking at those two ions in the ratio. So if it works for that then . . . . . Now did you have to tune it up for that process or was it pretty much all set up? Or you probably had to do a little—

HARRISON: It had its difficult days.

GRAYSON: In **<T: 25 min>** terms of making sure that you were—

HARRISON: Everything was working right, yes.

GRAYSON: Those two isotopes that you wanted to look at.

HARRISON: Of course it was a metal flight tube through the magnet, but the ends were glass. When you had to change the filament you had to break open the glass envelope, put a new filament in and then glass blow the envelope back on. So you learned how to become a glassblower.

GRAYSON: So you were actually doing this glassblowing work yourself?

HARRISON: Oh, yes.

GRAYSON: Oh, okay. But then, of course, after that you had to thump it out to get rid of the waste.

HARRISON: Yes, oh, yes.

GRAYSON: So this was pretty much traditional. Do you have a recollection of what the radius of this instrument would be?

HARRISON: Six inch.

GRAYSON: Six inch. Okay, it's probably about standard.

HARRISON: [Yes].

GRAYSON: So, your first exposure to mass spec was that it kind of really just limited the isotope ratio type in the segment—

HARRISON: Yes, yes.

GRAYSON: That was probably—I don't know how far isotope ratio mass spec goes. I guess it goes back into the thirties?

HARRISON: I would think so, yes. I think most of the work on isotope geochemistry probably started more in the forties.

GRAYSON: Okay. Although you're looking at bacteria, it was for a geochemistry purpose? How did that connect?

HARRISON: Well, I finished my PhD in '56. And it wasn't clear I was going to make the fall convocation in 1956, so Professor Thode said, "Well, you can either carry on as a graduate student, or if you do make convocation, I'll hire you as a postdoc." So I made the convocation.

GRAYSON: What were the limitations that meant that you may not have been able to—

HARRISON: Well, if I didn't get enough results and didn't get it written up in the thesis.

GRAYSON: Oh, it was the matter of conclusions.

HARRISON: The usual problems. So, in the time as a postdoc with Thode, I actually looked at fractionation of natural samples from various—the one paper was from samples of Venezuela. One was sent to a basin in Utah and looking at the sulfur isotopes in various forms of sulfur in the petroleum deposits. We had two papers in *Bulletin of the American Association of Petroleum Geologists* describing that work.[2]

GRAYSON: That kind of used the information that you had determined in your PhD research.

HARRISON: Yes, it was more the applications of this. In 1957, I thought, "Oh, a mass spectrometer is not bad, but I don't want to stay in isotope geochemistry," and I had a background for my master's degree with free radicals.

---

[2] A.G. Harrison and H.G. Thode, "Sulphur Isotope Abundances in Hydrocarbons and Source Rocks of Uinta Basin, Utah." *Bulletin of the American Association of Petroleum Geologists* 42 (1958): 2642-2649; H.G. Thode, A.G. Harrison and J. Monster, "Sulphur Isotope Fractionation in the Early Diagenesis of Recent Sediments of Northeast Venezuela, " *Bulletin of the American Association of Petroleum Geologists* 44 (1960): 1809-1817.

GRAYSON: Now, tell me about the free radical stuff and your master's background.

HARRISON: It was the photochemical addition of mercaptan to olefins, which was a free radical chain reaction. And actually at that time mercaptans were sometimes used to affect the chain length. You could stop a chain of polymer growing by extracting a hydrogen from, say, butyl mercaptan. So, I had some background in free radical chemistry, background in mass spectrometry, and Fred Lossing at the National Research Council in Ottawa was doing free radical mass spectrometry, so I thought, "That sounds like a good idea." So I got a position as a postdoc with him starting in the fall of 1957. And we had measured ionization potentials of free radicals, measured photochemical decompositions of compounds, what free radicals were formed.

GRAYSON: So this was all really kind of very fundamental—

HARRISON: Yes.

GRAYSON: —research. And I guess this would be appropriate for a National Research Council activity.

HARRISON: Yes.

GRAYSON: Did the National Research Council have a—were they funding this or were they the organization that he was in sight of doing his work? **<T: 30 min>**

HARRISON: He was an employee of the National Research Council. They had a division of chemistry, which he was involved in. There was an analytical aspect to the work as well. One of the instruments had a full-time technician and was analyzing gas samples from the large photochemistry group that was in the division of chemistry.

GRAYSON: Okay, so there was—

HARRISON: And then there were two instruments devoted to research.

GRAYSON: Okay so you were actually doing, like I said, some analytical work with photochemistry—

HARRISON: Well, the lab was. I didn't get involved in it except once or twice maybe.

GRAYSON: Okay. But they did have some analytical application and stuff going on.

HARRISON: Oh, yes, yes, yes.

GRAYSON: Okay. These were all with homemade instruments that Lossing was—

HARRISON: Yes, yes. [. . .] He had spent some time when he was first appointed to National Research Council. He'd been sent down to McMaster to learn mass spectrometry and get some instruments built.

GRAYSON: Okay. Who were the big guns at McMaster that were leading the mass spec charge there?

HARRISON: Professor Thode was the main man.

GRAYSON: So, he basically had a mass spec interest or background or—

HARRISON: Yes.

GRAYSON: Okay, so he was not really analytical chemistry. He had a lot of interest in mass spectrometry. He saw that as a powerful tool for what needed to be done.

HARRISON: [Yes]. And they were doing fission. Some people in the department were doing fission yields. Nuclear chemistry was big just after the Second World War.

GRAYSON: Sure, sure, sure. And so you were—as you mentioned before, he had these two different areas of research and you wanted to get into one, but he pushed you into the other. So, the other research was this nuclear chemistry research. [laughter]

HARRISON: Well, actually, it turned out he mostly had physics students doing that work.

GRAYSON: Ah, okay. And so that was—

HARRISON: Which I hadn't realized before I signed up. [laughter]

GRAYSON: Okay. And, well, that would make sense for the physics students. But still it's got a chemical kind of a flavor to it.

HARRISON: Yes.

GRAYSON: I would assume that mass spec would've been more analytical in nature on the nuclear chemistry side.

HARRISON: Yes.

GRAYSON: Okay, so Thode was a big driving force in the development of mass spec in Canada.

HARRISON: [Yes].

GRAYSON: Okay. I wasn't really aware of that. And you know anything about his background or how he got into that?

HARRISON: He was originally from western Canada. He studied in the United States. I think he studied with Harold Urey and then was hired back into Canada and McMaster.

GRAYSON: Okay, well that would make sense since Urey was using mass spec in the thirties.

HARRISON: [Yes], I'm pretty sure that Thode worked with Urey.

GRAYSON: Okay. I could check on that. That would be interesting to find out.

HARRISON: And he was heavily involved in administration at the university in the past, and in the sixties served some time as president.

GRAYSON: I see. McMaster then—is that where [Ernest] Rutherford was—Rutherford came over to Canada and—

HARRISON: McGill [University], I think.

GRAYSON: McGill. Okay, [yes], okay. All right. I didn't mean to get too far distracted there, but it was interesting. I'm interested about mass spectrometry and the history, and I know quite a bit about what happened in the early part of the United States, but I wasn't aware of the importance of this fellow, Thode, and the incidence of mass spectrometry in Canada, and the fact that he was promoting the development and building instruments.

HARRISON: Yes, yes.

Lossing, at the National Research Council, usually had two postdoctoral students working with him. On a two-year basis, one appointed [each year], and so they sort of stepped. The first one—when I first went there, his other postdoc was Paul Kebarle.[3]

GRAYSON: Oh, I know Paul. [laughter]

HARRISON: Everybody knows Paul. And so I worked for a year with Paul.

GRAYSON: Oh, that's great. And so what kind of work were you doing then? This was—

HARRISON: Free radical ionization energies. It was the first attempts to get bond energies. If you measured the appearance energy of the methyl cations from methane and then measured the ionization energy of the methyl radical, you could use a [thermochemical] cycle to get a bond

---

[3] Paul Kebarle, interview by Michael A. Grayson, at University of Alberta, Edmonton, Canada, 22 May 2013 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0899).

energy. So that was a lot of the activity in terms of measuring ionization energies was to try to get bond energies. And then he used **<T: 35 min>** another area was mercury-photosensitized decomposition of organic molecules and tried to detect the first products, the free radicals that were formed.

GRAYSON: Mercury-sensitized decomposition. What's going on there?

HARRISON: Because the long-lived state or states of mercury-excited states, which when you put photons in you excite the mercury, which then transfers the energy to the organic molecule. And so we published several papers [in] that area as well.[4]

GRAYSON: So, this would be another way of getting into the energetics of—

HARRISON: Of understanding the primary processes and when you photolyse this organic molecule.

GRAYSON: So, the photochemistry part was looking at appearance potentials and ionization potentials.

HARRISON: That was the electron impact for ionization energies and appearance potentials.

GRAYSON: And so they were doing photoionization as well in the photo—

HARRISON: Well, it was photo excitation of the mercury. It wasn't ionization directly. Then there was a reaction chamber that fed into an electron impact source to actually ionize the free radicals.

GRAYSON: Ah, okay. So a little bit more of a struggle than an ionization.

---

[4] A.G. Harrison and F.P. Lossing, "Free Radicals by Mass Spectrometry. XVI The Hg-Photosensitized Decomposition of Biacetyl, Acetylacetone and Acetonylacetone," *Canadian Journal of Chemistry* 37(1959): 1748-1786; A.G. Harrison and F.P. Lossing, "The Hg-Photosensitized Decomposition of Benzaldehyde, Acrolein and Crotonaldehyde," *Canadian Journal of Chemistry* 37 (1959) 1696-1702; A.G. Harrison and F.P. Lossing, "Free Radicals by Mass Spectrometry. XIX Primary Steps in the Hg-Photosensitized Decomposition of Formaldehyde," *Canadian Journal of Chemistry* 38 (1960) 544-548.

HARRISON: Yes.

GRAYSON: [Yes], okay, okay. This was a job or a postdoctoral appointment at this point?

HARRISON: It was a postdoc.

GRAYSON: A postdoc, okay. This was like a two-year—

HARRISON: I had a two-year postdoc at the National Research Council, yes, with Fred Lossing.

GRAYSON: Yeah, all right. And so you were really exposed to mass spec even some more there?

HARRISON: Yes, oh, of course.

GRAYSON: [. . .] And kind of felt this might be a home; or did you have any ideas about going to some other areas of chemistry or—

HARRISON: No, I was looking for a job which involved mass spectrometry.

GRAYSON: So you decided that you really wanted to stay with it.

HARRISON: That was a decision I made to go to Lossing, was I thought that would be the field that I would fit best in. I thought of doing a postdoc in radiation chemistry, but it looked awfully complicated. [laughter]

GRAYSON: I think it is. You pretty much committed when you graduated with your PhD that you'd pursue mass spec for the time being anyway—and went.

HARRISON: Yes.

GRAYSON: Okay. So [were] there any other interesting things that you did during your two-year postdoc with Lossing at the National Research Council?

HARRISON: We had a lot of fun. I remember meeting Paul Kebarle for the first time, and Paul said, “Well, nice to meet you.”[5] And then within thirty seconds he said, “Do you ski?” I said, “No.” Paul said, “You will learn.” [laughter] We did. By this time I was married.

GRAYSON: That’s pretty interesting, yeah, Paul is quite a character.

HARRISON: Yes, so we did a lot of skiing the first winter when he was still there.

GRAYSON: Okay, was he a good teacher in skiing?

HARRISON: Yes.

GRAYSON: So let’s see, is this good ski country—McMaster is it, or no?

HARRISON: This was in Ottawa. And there was good skiing just about an hour’s drive north of Ottawa in the Québec Province.

GRAYSON: So you’d go up there and you’d learn the—

HARRISON: We’d go up there and spend Saturdays skiing if the weather wasn’t too cold.

So by 1958, ’59, I was looking for hopefully a permanent position. I heard in the spring of ’58 that one D.J. LeRoy [Donald J. LeRoy] at the University of Toronto. Well, he was Donald LeRoy, but he was always called D.J. He was one of the senior physical chemists in the department. And he, again, did gas-phase photochemistry. By the middle of the fifties he was having problems analyzing his samples, so he got money to buy a mass spectrometer, and somehow or other there was a long delay between them getting the money and the mass spectrometer being delivered, and gas-liquid chromatography developed to the extent during

---

[5] Paul Kebarle, Oral History Transcript #0899.

that period that solved **<T: 40 min>** his analytical problems. I heard about this, and so LeRoy was having a mass spec, but he didn't have any use for it.

So I was in Toronto in the spring of '58 for a conference, and I made a point of visiting the department and introducing myself to LeRoy. And to make a long story short I was hired here in '59. I came on duty I think the first of August. The mass spectrometer finally arrived about October, November, in '59. And it was mine. It was a Metropolitan-Vickers MS2.

GRAYSON: Wow. Was there any reason that he gave for the delay between—

HARRISON: I never did get that clearly explained to me.

GRAYSON: [Yes]. And LeRoy—did he have any problems with his leadership because he had ordered an instrument that he now didn't need or—

HARRISON: I never heard of any problem. The department was expanding at that time so they—

GRAYSON: You actually talked to LeRoy personally about this?

HARRISON: I visited him in the spring of '58 and said I was available, so in the late fall of '58 I was invited to visit the department, give a talk, and meet people, and sometime in the winter of '59 I was offered a position as a lecturer.

GRAYSON: So lecturer position is kind of like a starting point for—

HARRISON: At that point, the department of chemistry—and I'm not sure what other parts of the university—made their initial tenure-stream appointments at the rank of lecturer. I was told that if my performance was satisfactory I'd be promoted to assistant professor after one year. And they didn't actually say it, but I presumed if my performance wasn't satisfactory I was out on my ear. [laughter] So I was promoted to assistant professor in 1960. And at that stage because the competition for faculty was getting—there was quite an expansion—initial appointments began to be made at the assistant professor level.

GRAYSON: Okay. So in this lecturer position that you had for a year did you actually do what that says or did you—

HARRISON: Oh, yes.

GRAYSON: So you were really teaching.

HARRISON: I was teaching, yes, yes, yes.

GRAYSON: And you didn't have any research activities going on at that time.

HARRISON: Well, the Metropolitan-Vickers MS2 arrived about October, November. I'd been assigned the fourth-year student. The fourth-year student did a research project.

GRAYSON: This is undergraduate.

HARRISON: Undergraduate, yes. And so he was assigned to work with me—a man by the name of Tait. And so he was there to get the machine running and then did research, and he actually stayed on then after he graduated and did a master's degree with me, so he was my first graduate student.

GRAYSON: Oh, and his first name was?

HARRISON: James.

GRAYSON: James Tait. Tell me about the performance characteristics of this MS2 instrument.

HARRISON: It was, again, a magnetic sector instrument. No differential pumping. I mean, it went in and went into the whole thing. Again, I think it was a six-inch radius of curvature.

GRAYSON: And was it a sixty-degree, ninety-degree?

HARRISON: Ninety degree. [Yes]. Metropolitan-Vickers [became] AEI [Associated Electrical Industries] of course.

GRAYSON: Right. Do you know when that happened?

HARRISON: Sometime in the early sixties.

GRAYSON: Okay. Sometimes they call it Metro-Vick and then it became Associated Electrical Industries, part of Associated Electrical Industries.

HARRISON: But it had a mass range. One could just see the mercury isotopes. Resolution of, maybe, 150.

GRAYSON: But it was sufficient for what you needed to do at the time.

HARRISON: We started off doing some appearance energies. We got into ion molecule reactions with that instrument. It was not the best instrument for it, but we—

GRAYSON: Yeah, how did you do ion molecule with—this is a single-focusing ninety-degree magnetic sector instrument.

HARRISON: We jacked up the pressure in the source and looked to see what was happening.

GRAYSON: There was no differential pumping so that **<T: 45 min>** increased the pressure everywhere.

HARRISON: We couldn't go terribly high, no, but it did work and we got some results out and—

GRAYSON: So this would be working. This almost would be like what we call chemical ionization. It's kind of headed in that direction then.

HARRISON: Well, it got there eventually when [Frank H.] Field and [Burnaby] Munson—[6]

GRAYSON: Pumped up the pressure.

HARRISON: —got a better instrument. And, actually, we made another homemade instrument in about the mid-sixties, which had differential pumping, and we could measure the source pressure directly. That was largely made, again, at McMaster, and it was the same idea—a six-inch radius of curvature to ninety-degree mass spectrometer.

GRAYSON: But the reason for this instrument was to do ion molecule reactions.

HARRISON: Yes.

GRAYSON: It was the reason why you went in and decided you wanted to build your own instrument—because you were interested in this ion molecule reaction area of the research.

HARRISON: [Yes], I got into that. The second year I was at the National Research Council the postdoc with Lossing was a chap by the name of Pottie—Ross Pottie. He's worked with Bill Hamill at [University of] Notre Dame on ion molecule reactions. And also Joe [Joseph] Franklin had visited the lab and gave a talk on his work on ion molecule reaction, so it looked interesting to me.

GRAYSON: This is Franklin from —

HARRISON: Joe Franklin from down in Humble Oil [and Refining Company].

GRAYSON: Humble Oil, okay. I always thought that was a strange business, where a small industrial petrochemical would have, you know, people like Franklin and Field and Munson working in their lab doing—

---

[6] Frank H. Field, interview by Michael A. Grayson at Durham, North Carolina, 9 and 10 December 2009 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0636). Burnaby Munson, interview by Michael A. Grayson at University of Delaware, Newark, Delaware, 9 April 2010 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0688).

HARRISON: They obviously got a lot of freedom to do what they wanted.

GRAYSON: I guess. That was pretty amazing. I did interview Munson. I believe that they would actually bring high-powered chemists to talk about their science on a regular basis this little oil company.[7]

HARRISON: I think once Esso took over Humble Oil—I think that, or Exxon or whatever it's called in the States.

GRAYSON: So we have all this emphasis from different areas that ion molecule reactions looks like something that you want to continue to get into. And so you—

HARRISON: One of the things we did was a single-source instrument. You have a repeller voltage to push the ions out, so your ion molecule reactions [occur] at different [ion energies] depending where in the path they collide and react. [Victor] Talrose, in Russia, had developed a pulsing technique in which he put a short pulse of electrons through to ionize. It didn't have a voltage on the repeller. And then sometime later pushed the ions out by putting a pulse on the repeller. So that's what was called the zero field system. And we took that up in '62, '63.

GRAYSON: Okay. So this gave you a better control over the energy.

HARRISON: We got [rate constants] with essentially thermal energy ions.

GRAYSON: Okay, which is what you were really interested in.

HARRISON: Yes. I think the other person that took that up in North America was Jean Futrell.[8]

I should probably note that in 1962 I was awarded a Sloan Fellowship.

GRAYSON: Sloan Fellowship. That means—Sloan sounds familiar but—

---

[7] Burnaby Munson, Oral History Transcript #0668.

[8] Jean H. Futrell, interview by Michael Grayson at Richland Washington, 28 and 29 October 2012 (Philadelphia: Chemical Heritage Foundations, Oral History Transcript #0706).

HARRISON: Alfred P. Sloan was head honcho at General Motors for some time in the twenties and thirties. And, I guess, even then the head honchos made a lot of money. So he set up the Sloan Foundation—the Alfred Sloan Foundation—in the mid-thirties. And in ’55, **<T: 50 min>** I think it was, they started this Alfred P. Sloan Fellowships. The idea was to find people in universities in the early stages of their career who showed promise and give them a two-year unrestricted grant. And I was fortunate enough to be awarded one of these in 1962, [. . .] which also led to my getting a promotion and tenure. [laughter]

GRAYSON: Okay. So you’re on the faculty and this obviously is a feather in your cap, and it helps promote your career here.

HARRISON: Yes. It also gave me money to expand our activities.

GRAYSON: How much money did Sloan give you?

HARRISON: Well, just to put it in perspective, my research grant in the ’61-’62 grant year was four thousand dollars Canadian, which sounds terribly small, but of course my salary was eight thousand. The Sloan Foundation gave me nine thousand dollars a year for two years.

GRAYSON: That was a lot of money.

HARRISON: That was a lot of money in those days.

GRAYSON: Oh, yeah. Before we get into that, though, I want to back up a little bit and talk about your lectureship here. We kind of got off into your research career, but when you were teaching, what— basic organic chemistry or chemistry to freshmen or . . . ?

HARRISON: I did a lot of teaching of chemistry to first-year students.

GRAYSON: First years, okay.

HARRISON: Well, my first lecturing assignment was what I called “Chemistry for Poets.” [laughter] There was a general arts program in which you had to take one or two science courses. And one of them of chemistry was available. So I taught that for a number of years.

GRAYSON: Oh, so even after you had completed your duties as a lecturer.

HARRISON: Oh, no I was—

GRAYSON: You were—

HARRISON: Assistant professors and associate professors have their normal teaching load.

GRAYSON: Oh, teacher—okay.

HARRISON: It didn't change.

GRAYSON: Okay. But basically your first year you had more of a teaching load than any other?

HARRISON: No. It was about the same every year.

GRAYSON: Okay, and in addition to "Chemistry for Poets" you taught—

HARRISON: Chemistry for first-year life science, chemistry for first-year physical science.

GRAYSON: Did you have any of those big three-hundred-student sections, where, you know, you had to help with—

HARRISON: [Yes], we got some, especially later on, but then also I got involved in lab supervision. At that time there was a pre-dental program; they took a chemistry lab. And I was involved in that.

GRAYSON: So how many people would end up in this "Chemistry for Poets" class?

HARRISON: It wasn’t called that of course. [laughter]

GRAYSON: I know.

HARRISON: Thirty or forty.

GRAYSON: Oh, okay, so it was relatively small compared with some of the large sections.

HARRISON: Yes. There was actually a lab [that] ran with that too, [. . .] so they got their lab coats dirty too.

GRAYSON: Ah, did any of these people ever go into chemistry with this exposure, do you know?

HARRISON: Not that I’m aware of. [laughter]

GRAYSON: But they did learn some chemistry to help fulfill that requirement?

HARRISON: [Yes].

GRAYSON: That’s good. Okay. But then also during the rest of your career here teaching was part of the— I mean you had a regular teaching role that you had to attend to.

HARRISON: Yes. [. . .] We also had at the early days a general science program, and there I did do some teaching in chemistry at a more advanced level, and then things changed around about 1970. In the early seventies, I spent some time as associate chair of the department. [. . .] During the sixties, there was an argument that one didn’t need to teach analytical chemistry, so the analytical chemistry program had gotten really shut down here at U of T, and it happened at other places too. One of the things I managed to achieve as the associate chair was to reintroduce analytical chemistry and convince the department to hire an analytical chemist. So we introduced first a second-year course in analytical chemistry, and since I had promoted it, I got the job of teaching it. So from then on much of my teaching was in introductory analytical chemistry, and I got involved in some fourth-year courses, which were more advanced analytical chemistry **<T: 55 min>.** So I sort of switched from being a physical chemist to an analytical chemist.

GRAYSON: So, in this coursework you were actually talking about all forms of—I guess I would call it instrumental-based analytical chemistry? Or did you teach any wet analytics?

HARRISON: The introductory course was mostly wet chemistry.

GRAYSON: Oh, mostly wet analytical, okay. But then later on there—

HARRISON: But then we introduced a couple of years later a third-year course which was instrumental analysis.

GRAYSON: Okay, okay. Yeah, analytical chemistry seems to be kind of like the—what would you call it?—the stray godchild or stray orphan of chemistry in a lot of ways.

HARRISON: Well, it's come back to a considerable extent here.

GRAYSON: Well, that's good, I think. It seems to be something that people like to pick on and say, "Well, we don't really need that anymore." I don't know what the rationale is, because it depends on what you've made, and you know—

HARRISON: I'll just go back briefly to that Sloan Fellowship. Nine thousand dollars was enough—more than enough—to hire a postdoc. We paid the postdoc about five thousand a year at that stage. So I managed to get a postdoc, [Feye Meyer from the University of Amsterdam], to come and work with me.

GRAYSON: This was an unfettered grant. You could do what you want with it.

HARRISON: Yes. It's now twenty-five thousand dollars a year for two years. But it certainly did give a good boost to my research activities. The point I think I want to make is that when it ended, fortunately the NRC grants and scholarship area division upped my grant, to recognize that I no longer had the Sloan. So I was away.

GRAYSON: Did they do that without any—

HARRISON: Well, I applied for more.

GRAYSON: Okay, you applied for more.

HARRISON: And then made it known. Yes, obviously, but they recognized that this man had got a kickstart. Let’s keep him going. And I think that’s required even in the United States; of course, too, most of the awardees of Sloan grants are in the US. I’ve got a list of those who received awards in 1962, and there are a number of people who are now well-known chemists, who got a starting kick.

GRAYSON: I’ve always wondered if anyone has ever done a follow up, like you mentioned here, on these things, where grants are, you know, given to a year, a group. And does anyone ever look at that ten, twenty, thirty years downstream to see how those grantees did?

HARRISON: I would assume that the Sloan Foundation makes some [. . .] assessments of the utility of their method. Certainly, I would say that half the chemists that got [an] award in 1962 are people that became well known. Now presumably this Sloan Fellowship helped them in that. John Baldeschwieler for example.[9] [. . .] And Charles [Chuck] DePuy, also got an award that year. I forget how many Nobel Prize winners have received Sloan Fellowships but there certainly are a number.

GRAYSON: Yeah. I’m just curious. Now this is an American foundation?

HARRISON: Yes.

GRAYSON: But it’s awarding grants to—

HARRISON: United States and Canada.

GRAYSON: United States and Canada. So, I mean, it’s definitely—I mean, it’s specified to include Canada.

---

[9] John D. Baldeschwieler, interview by David C. Brock and Arthur Daemmrich at Philadelphia, Pennsylvania, 13 June 2003 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0280).

HARRISON: Yes.

GRAYSON: So North American. Okay. Do they normally award these each year to someone in Canada?

HARRISON: As far as I know, yes. And actually in 1962 I was the only person working in Canada who got a Sloan Fellowship. I think there were more, and certainly a number of my colleagues here have had Sloan scholarships. Our Nobel Laureate, John Polanyi, got one a bit earlier than I did I think, ’57 or ’58.

GRAYSON: [Yes]. Now, he went into kind of philosophy eventually didn’t he—Polanyi?

HARRISON: No, no. He’s still an active chemist.

GRAYSON: Oh, okay, okay. Well, maybe, I’m getting him confused with some other fellow.

HARRISON: You may be confusing him with his father Michael Polanyi. **<T: 60 min>** This is John Polanyi.

GRAYSON: Yeah, okay, very good. Thank you. Did you have travel funds that came with this or you could use this money any which way you wanted to—the money?

HARRISON: Yes, it was unrestricted.

GRAYSON: So, you could go to a meeting if you wanted to, or conference meeting and so on.

HARRISON: Yes.

GRAYSON: Okay, so now mass spec is, in the fifties and sixties, late fifties, it’s beginning to kind of expand—

HARRISON: Yes.

GRAYSON: —territory, so to speak.

HARRISON: In fact, in the late fifties and sixties, of course, John Beynon in particular and others—Klaus Biemann and Fred McLafferty—showed that you could do accurate mass measurements on smaller ions anyway, and work out the elemental composition.[10] And the journals began to accept this mass measurement in place of elemental analysis. So in the mid-sixties some of the organic chemists in the department here said we should get into this. So we applied in ’66, I think it was, for a double-focusing mass spectrometer, to be used for service work, and with hopefully some time for research. We were successful and got the instrument in 1967. By now Metropolitan-Vickers was AEI and it was an AEI 902, MS-902.

GRAYSON: Okay. And you got money for this instrument from?

HARRISON: The National Research Council.

GRAYSON: Okay, so they must have been supporting you?

HARRISON: That was the funding [agency] through up until ’75. Then they set up what’s called the Natural Science and Engineering Research Council [NSERC], which is separate from NRC. But until then they had a scholarships and grants division of the National Research Council.

GRAYSON: So the National Research Council exists still, but it funds other things?

HARRISON: It’s not a funding agency. [. . .] So in 1967, I’m pretty sure it was, we got the MS-902, which actually was in the room next door here, and hired a technician to operate it. And that was the beginning of the service laboratory, which has now grown enormously. Now, this was service for the university here. For the department. [. . .] I guess we did a little bit of stuff outside, but not a great deal.

---

[10] John H. Beynon, interview by Michael A. Grayson at Swansea, Wales, United Kingdom, 22 April 2008 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0420); Klaus Biemann, interview by Michael A. Grayson at Alton Bay, New Hampshire, 29 August 2006 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0279); Fred W. McLafferty, interview by Michael A. Grayson at Ithaca New York, 22 and 23 January 2007 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0352).

GRAYSON: Oh, okay. And I'm just trying to think now. Early in '62 you would've been thirty years old.

HARRISON: Thirty-one. I was born in '31.

GRAYSON: So then, this would be about a normal point in your career for advancing to bigger and more important things. Was this Sloan Fellowship and then getting instruments for— and adding large instruments. That's a fairly large instrument, the MS-902, so I don't know what they cost back then, but they—

HARRISON: I forget too.

GRAYSON: Well over a hundred thousand, I would imagine.

HARRISON: [. . .] Yes. It was what the NRC at that time called a major installation. And so they set [up] a visiting committee to test you out and see if you really knew what you were doing, and so forth. [It] was a major grant.

GRAYSON: They didn't want somebody buying this thing just because they got the money to buy it.

HARRISON: That's right. [laughter] Though they really meant that my colleagues— I was responsible for putting the grant in. My colleagues were called before the visiting committee to explain why and how they would make use of this facility, so it was a full-blown effort.

GRAYSON: Ah, so it was pretty serious that they wanted to make sure the money was well spent.

HARRISON: Yes.

GRAYSON: Ah, okay, good. And obviously it worked. So that was, you say, in the mid-sixties that this happened? Mid-sixties that you got the MS-902?

HARRISON: Sixty-seven.

GRAYSON: In ’67. Now by this time you’d probably gone to some mass spec conferences.

HARRISON: Of course.

GRAYSON: Would you recall the first one you went to?

HARRISON: The first one I went to was in 1959. The **<T: 65 min>** ASTM [American Society for Testing and Materials] Committee E-14 met in Los Angeles [California]. And Fred Lossing said—many of his postdocs came from Europe, and he paid their travel expenses—so he said to me, “I didn’t have to pay your travel expenses so I’ll pay your way to Los Angeles.” So it was great. I mean, I met people like Fred McLafferty, Sy [Seymour] Meyerson.[11] All of these people I knew about. Frank Field, Joe Franklin.[12] Well, I’d met Joe, I guess. So that was my first meeting.

GRAYSON: Yeah, that was a pretty intimate group in ’59 right? They were one oral session and— [laughter]

HARRISON: The big argument in Los Angeles during that time was, “We may have to go to two concurrent sessions. Oh, that’d be terrible.” [laughter]

GRAYSON: Yeah. So you continued, I assume, to attend the [ASTM Committee E-14] conferences after that for a pretty regular basis.

HARRISON: Yes.

GRAYSON: Okay. There’s a little bit of mass spec ASTM history that I’m trying to research and maybe you can tell me, since you went to those early meetings. And I think the last meeting they had in New Orleans was in 1962. I can find out for sure, but from what I’ve heard the reason they haven’t been to New Orleans since this distant time was because one of the

---

[11] Fred McLafferty, Oral History Transcript #0352; Seymour Meyerson, interview by Michael A. Grayson at Gary, Indiana, 7 March 1991 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript, #0398).

[12] Frank H. Field, Oral History Transcript #0636.

gentlemen who attended the conference was black and he was not treated very kindly in New Orleans, in 1962. And I don't know if—

HARRISON: Well, when did ASMS [American Society for Mass Spectrometry] start? Not until the late-sixties.

GRAYSON: Yeah, it was in the late sixties, later because the first one I went to was in '66, I think, and they were still ASTM, and they were ASTM for a couple years. I think it was '69 when they went to ASMS.

HARRISON: Certainly there were meetings of the Committee E-14 through the sixties.

GRAYSON: Right, and they also went alongside of ASMS meetings for a few years until –

HARRISON: That I don't know.

GRAYSON: But I was wondering if you knew anything about this, as I say, in New Orleans in 1962.

HARRISON: No, I wasn't there.

GRAYSON: Okay, okay. Because that's what I've heard is that they didn't go—they haven't gone back since because of the fact that this black fellow was discriminated against.

HARRISON: Well, that might well have been.

GRAYSON: And I'm trying to find out as much as I can about that. So now you have a technician who's doing the service work for you.

HARRISON: Yes, we did some research on it, but it wasn't used full time.

GRAYSON: Oh, okay, and as research what would you do, now you've got a double-focusing, high-resolving [power] instrument?

HARRISON: So, we could look at metastable ions and various things like that and understand fragmentations a bit better.

GRAYSON: Okay. But you still had your own research instruments that you used for activities. Other more researching-type things. [. . .]

HARRISON: [Yes] And in about 1973, I managed to get money to buy a DuPont 21-490 instrument, for chemical ionization.

GRAYSON: Ah, 21-490. So this would've been getting towards the end of—the 490—that was a single-focus then?

HARRISON: Yes. Again ninety degree.

GRAYSON: And this was for chemical ionization that you bought it.

HARRISON: Yes. We had also bought one—an electron impact—which went into the service laboratory at about the same time.

GRAYSON: All right.

HARRISON: So that got us really into chemical ionization.

GRAYSON: Ah, that would've been in the early seventies.

HARRISON: It's around '72, '73. I don't remember an exact date. Of course, Munson and Field had published their papers, and it looked all very interesting.[13] And we were using chemical ionization on our homemade instrument, essentially to probe structures of ions using different reagents, to protonate the molecule to see how it fragmented.

---

[13] M.S.B. Munson and F.H. Field, "Chemical Ionization Mass Spectrometry. I. General Introduction;" *Journal of the American Chemical Society* 12 (1966): 2621–2630; Burnaby Munson, Oral History Transcript #0668; Frank H. Field, Oral History Transcript #0636.

GRAYSON: [Yes]. The chemical ionization work was the natural follow-on to [ion]-molecule studies you would say. And that meant that you once again got back into the fundamental of—continuing to go into the fundamentals of—

HARRISON: Yes, I was more interested in the fundamental than the analytical applications.

GRAYSON: Of course, CI [chemical ionization] did have an impact on the analytical applications, because now you could, you know, analyze a whole bunch of compounds that were no longer even previously in EI [electron ionization].

HARRISON: Oh, yes, it was a very useful [addition].

GRAYSON: Yes, **<T: 70 min>** in my sense it was the first significant development in ionization that started to bring mass spec to a larger and larger array of compounds.

HARRISON: Yes.

GRAYSON: That's a fair assessment of the—

HARRISON: Indeed, right on.

GRAYSON: Because I think in the EI world it was considered mostly a niche technique—MS—for a long time, people considered that mass spectrometry was a little kind of a niche analytical tool—

HARRISON: It wasn't really a main purpose instrument.

GRAYSON: Well, that has changed. [laughter] Mostly because we can create ions out of things that we couldn't do before. I mean, that's my sense.

HARRISON: Well, then, of course, electrospray and so forth really changed the world.

GRAYSON: So, what prompted your ion molecule reaction studies? You had a certain area of ion molecules that you were into then or—

HARRISON: I had in the back of my mind, when I started doing ion molecule reactions, that I might be able to use it for getting the structures of ions.

GRAYSON: Structures.

HARRISON: It didn't work. [laughter] Not very well, until I got an instrument where I could put a reagent in a collision cell, and stick an ion in and see what the reactions were. And that didn't happen until the eighties.

GRAYSON: Okay. And why were you interested in structure?

HARRISON: Well, that was basically what we were doing with Lossing, my [students], and so forth. I got involved. I don't know how much you know about the [benzyl/tropylium question].

GRAYSON: A little bit.

HARRISON: Which was an area that Sy Meyerson worked in [extensively].[14] And we got involved in that too. What was the form? Was it a [benzyl] ion or a tropylium ion that was formed? We published some papers on that.[15]

GRAYSON: Sy is a really interesting fellow. I think he just moved into a retirement community a couple years ago if I'm not mistaken.

HARRISON: Well, he's no chicken.

But you said, did I attend conferences. Of course, I was a fairly regular attendee at [ASTM Committee] E-14 then ASMS. But there are also the international conferences. I guess

---

[14] Seymour Meyers, Oral History Transcript #0398.

[15] J.M.S. Tait, T.W. Shannon and A.G. Harrison, "The Structure of Substituted C_7 Ions at the Appearance Potential Threshold," *Journal of the American Chemical Society*, 84 (1962): 4-8; F. Meyer and A.G. Harrison, "A Mechanism for Tropylium Ion Formation by Electron Impact," *Journal of the American Chemical Society*, 86 (1964): 4757-2761.

the first one I went to was in 1961, which was held in Oxford [United Kingdom]. I mentioned to LeRoy, who by then was head of the department that there was this conference in Oxford. He said, “Would you like to go?” I said, “Well, of course, but I don’t have any money.” [He said], “Oh, I’ll pay for it.” [laughter] There was much more money at that time than there is now. So we went to England. My wife is English, and so it was a chance to visit her family, and I went up to Oxford to the international conference. Then ’64 the international conference was in Paris [France]. That’s where I gave my paper, at an international conference. And I’ve been at quite a number of other international meetings.

GRAYSON: Now do you recall—I know that the international conference started with your — it sounds like it might’ve been the third or fourth international conference, the one at Oxford?

HARRISON: I think the one at Paris was either the third or the fourth.

GRAYSON: Oh, okay. So it might’ve been the second or third, the document—

HARRISON: Yes.

GRAYSON: Yeah, okay. Did they have the triennial thing established by that time? Would they have been every three years?

HARRISON: Yes, and I just learned that they switched. They’re going to biennial now.

GRAYSON: Oh, really? Very interesting.

HARRISON: The international that was held in Japan in 2012 is going to be in Geneva [Switzerland] in 2014. And it’s going to be in Toronto in 2016.

GRAYSON: Oh, there you go. Well, they haven’t had it in North American have they?

HARRISON: No.

GRAYSON: The international conference? That’ll be the first one.

HARRISON: [Yes].

GRAYSON: Interesting. I don't know if I'll be around for that. It'd be interesting to be able to go to that. And so what was that conference like, the Oxford conference? They weren't running parallel sessions were they or—

HARRISON: There was very little parallel session. You could mostly hear it all.

GRAYSON: And my recollection is they also recorded the commentary and the question/answer session at the end and included that in the paper, which was kind of nice.

HARRISON: Yes. **<T: 75 min>**

GRAYSON: How did they do that? Did they have somebody that actually—

HARRISON: I don't know.

GRAYSON: [. . .] They had to have somebody either record the stuff or maybe they had a person who did scientific shorthand or whatever. But—

HARRISON: I don't know how they did it, but I do know that the bound volume that came out from the Paris conference had questions and answers and identified the questioner and the [speaker]—I'm not sure how it was done.

GRAYSON: Yeah, that's interesting. Yeah, it's a nice thing to have. You see what people were thinking who listened to those papers.

HARRISON: I think I have the Paris one here.

GRAYSON: Yeah, I'm sure. Yeah, I've got a couple in my collection of mass spec books at home. I think I've got the first four conferences altogether. Did you meet anyone interesting at that conference, do you recall, at Oxford?

HARRISON: Hmm, I can't recall meeting anything that changed my life. [laughter]

GRAYSON: That was the first international conference then—in Paris was the first time that you gave a paper at an international conference right?

HARRISON: Yes.

GRAYSON: And that paper was on?

HARRISON: Ion molecule reactions. We were doing the zero field pulsing technique to get thermal energy [rate constants]. And we had done some work on [fragmentation of] organic molecules— organic ions as a function of time by pulsing and changing the time span. So it was a combination of those two.[16]

GRAYSON: Okay. So you would get information about energy dissipation as a function of time and—

HARRISON: It was an interesting conference.

GRAYSON: You had graduate students working for you and all this [ion] molecule reaction stuff?

HARRISON: Yes, yes.

GRAYSON: And they were following through on the same kinds of studies, and basically [you] were guiding them through their research work. Okay. So the seventies. At this point you had actually probably gotten some responsibilities. You were saying you were an assistant chair?

HARRISON: I was associate chair of the department from '71 to '74.

---

[16] T.W. Shannon, F. Meyer, A.G. Harrison, "A Pulsed Ion Source for the Study of Unimolecular and Bimolecular Reactions of Gas-phase Ion," *Canadian Journal of Chemistry* 43, no. 1 (1965): 159-174.

GRAYSON: So what does that entail?

HARRISON: I was running the undergraduate program.

GRAYSON: Ah, okay. How big was that program?

HARRISON: We had some very big courses because a lot of the students who wanted to get into medicine. So the second-year organic course, first-year general chemistry course, were very large. You had several sections of lectures. I had to juggle people around and make sure people got reasonable lecture loads.

GRAYSON: Yeah, these big sections are a bit of a challenge. I assume that they had help with grading papers and that kind of thing, because, you know, it's a lot of work to grade a couple hundred papers. [laughter]

HARRISON: When it gets to be five hundred it's even more, because I remember we had to take over a lab for a while in the summer after the exams were written, and all the lecturers would be sitting there going through all these damn papers. [laughter] I don't know what's done now.

GRAYSON: Yeah, well that's a challenge, for sure, but that's part of the educational process. And so that was a primary, as you were associate chair, that was the primary challenge.

HARRISON: My primary responsibility was the undergraduate program.

GRAYSON: You had to make sure that everybody got a fair share of the teaching and nobody got bent out of shape too much about that.

HARRISON: Any ideas about changing the curriculum. This was also at the time that student activism was becoming significant, so one had to deal with the student population as well.

GRAYSON: So what were they being active about then?

HARRISON: Oh, they wanted to have a say in what was being taught, and what was going on, which was a dicey situation. [laughter]

GRAYSON: Yeah, so how did you handle those issues?

HARRISON: Probably poorly.

GRAYSON: Yeah, well that's got to be a little bit of a challenge, for sure. Well, now I know in some situations today, I talked to Keith Jennings in the UK a couple years ago and their chemistry is kind of falling on hard times.[17] It's kind of like they have to have enough students to maintain the faculty, and not enough people are doing chemistry anymore over there. So are you **<T: 80 min>** having any of those problems here?

HARRISON: No. I think things are going quite well in chemistry here. There has been a shift in emphasis in the department. And there's much more biological chemistry now. And that attracts people. And the other area which is very popular is environmental chemistry.

GRAYSON: So that's still popular here?

HARRISON: Oh, yes. We have four or five faculty who are basically environmental chemists. I go to some of the environmental chemistry seminars, and they're certainly very well attended in terms of numbers. I think it's going fairly well, I think. Of course, I'm out of the loop now.

GRAYSON: Sure. I find it a bit disconcerting that, you know, in the UK there should be a de-emphasis of this important scientific field because you don't have enough students, you know, to support the faculty. That's a little scary.

HARRISON: Well, not many students in Great Britain can now afford to attend university.

GRAYSON: Well, then, I guess that represents another—

---

[17] Keith R. Jennings, interview by Michael A. Grayson at Leamington Spa, Warkwickshire, United Kingdom, 24 and 25 April 2008 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0419).

HARRISON: The tuition fees have become rather enormous.

GRAYSON: Okay. I wasn't aware of what that situation is.

HARRISON: I think they're up to eight thousand pounds or so, so only the affluent really can attend. So, I think that's part of the reason why enrollment in universities in the UK is going down and they're relying more and more on bringing people in from overseas.

GRAYSON: Ah, okay. That's happening everywhere though.

HARRISON: Yes. Oh, yes, yes.

GRAYSON: I got the impression there may be a large Indian population in this area, in Toronto?

HARRISON: A large population of almost any type you want to talk about. We have a very large Chinese population, a large Indian/Pakistan population, not so much in the downtown area, but if you go up to a fairly small town or a fairly large town up a bit north of Toronto [. . .] called Brampton [Ontario] you'll find that it's almost entirely Indian.

GRAYSON: Hmm. So, but, I mean, these people are … that brings up, though, the point about a cost of education. How pricey is the University of Toronto these days?

HARRISON: I think it's around six thousand.

GRAYSON: Canadian?

HARRISON: Canadian funds, or Canadian dollars per year.

GRAYSON: Okay. And is that just for tuition or—

HARRISON: Yes.

GRAYSON: That's just for tuition.

HARRISON: Oh, yes.

GRAYSON: Okay, so in American that would probably be about seven thousand, maybe, American, I think. Wash U [Washington University in St. Louis] where I was at, I think they're in the twenty-five or thirty thousand range, so it's just getting extremely exorbitant in some of these private schools in the States. But apparently they're able to get people to attend. Okay, well.

HARRISON: I was just going to say would you like to take a break and maybe get a coffee?

GRAYSON: Sure. I think that would be a good idea. [. . .]

[recording paused]

GRAYSON: [. . .] So we had gotten into the mid-seventies with your career. So where do you want to go from there?

HARRISON: Well, I should note that in '75 the first six months I had a sabbatical leave, which I spent with Keith Jennings at Warwick [The University of Warwick].[18] So, I did that at that time, to collaborate with him. We had done some collaboration before that. It was fairly quiet. One of the problems that came up in the seventies, at least at Toronto, was a lack of graduate students in physical chemistry. People weren't interested, and those that were wanted to work with lasers, and I didn't have lasers, so I had problems with supply of graduate students in the seventies. Fortunately, I was able to get some postdocs and visiting scientists, and so forth, and keep things going.

GRAYSON: How about funding? You were still getting funded then?

HARRISON: I was getting funded reasonably well, yes.

---

[18] Keith R. Jennings, Oral History Transcript #0419.

GRAYSON: So, it wasn't the problem **<T: 85 min>** that many people are experiencing today who are just starting out and funding was pretty—

HARRISON: No. Of course, by that time I was a full professor and had a fairly reasonable publication list. So nothing much of interest happened. I spent the first six months of 1982, at they call it [École] Polytechnique Fédérale, in Lausanne [Switzerland], working with one Tino Gäumann, who had good instrumentations and so forth, and some of his colleagues.

GRAYSON: What was the motivation of going there? Was that a sabbatical?

HARRISON: Tino had been twisting my arm for some time to come and spend time with him.

GRAYSON: Okay, you knew him through—

HARRISON: I knew him. I'd met him at various times.

GRAYSON: Conferences, okay. And so he said, "Why don't you come visit?"

HARRISON: He was the director of the Institut de Chimie Physique, had lots of money. And they paid me fairly handsomely to be a visiting professor—Professeur Invité. [laughter]

[Two papers resulted from that time].[19]

GRAYSON: Invited, yes. So that was probably a fun thing.

HARRISON: Yes. But of course I'd mentioned earlier that Paul Kebarle had taught us how to ski[20]. So, for six months in Switzerland why not ski?

---

[19] A.G. Harrison, T. Gäumann and D. Stahl, "Structure and Fragmentation of $C_3H_7O^+$ Ions Formed by Chemical Ionization," *Organic Mass Spectrometry* 18 (1983): 517-524. A.G. Harrison, R. Houriet and T.T. Tidwell, "Gas Phase Basicities of Substituted Styrenes. Comparison of Gas Phase and Solution Reactivities," *Journal of Organic Chemistry* 49 (1984): 1302-1304.

[20] Paul Kebarle, Oral History Transcript #0899.

GRAYSON: Oh, definitely, definitely. So where did you go skiing up there?

HARRISON: There were several places. We were near Lausanne down in the valley, and you'd just go up any mountain, pretty well, and you'd find a ski resort.

Then in'84, a group of us in Ontario here got together. We knew we weren't each going to get a mass spectrometer—a big one—so we needed to set up regional centers. And so the one was a regional center for analytical mass spectrometry [McMaster Regional Centre for Mass Spectrometry], which would be located at McMaster. The other was [a] regional center for gas-phase ion chemistry [Ontario Regional Ion Chemistry Laboratory (ORICL)], which would be located here at U of T. And my co-applicants were Bob Boyd [Robert Boyd] and Ray March [Raymond March], whom you may know.[21]

GRAYSON: Sure.

HARRISON: With support from others such as [Terry] McMahon, [Diethard] Böhme and [John] Stone.

GRAYSON: Now this was in Ontario?

HARRISON: Yes. [. . .] But the funding came from—by this time NSERC was established, the Natural Science and Engineering Research Council. So, the money came from them.

GRAYSON: [. . .] But you had kind of decided to form a cooperative program amongst the people in the scientific community who were using mass spectrometry in Ontario—

HARRISON: Because at that time Boyd was at University of Guelph, which wasn't too far away. March was at Trent University, again, which wasn't too far away. So, we were going to be the three main users of the instrument.

GRAYSON: For the gas-phase ion chemistry studies.

---

[21] Raymond E. March, interview by Michael A. Grayson at Peterborough, Ontario, 27 October 2014 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0921).

HARRISON: Yes. And we got funded in 1984, and I ended up buying a ZAB-2FQ from VG Analytical.

GRAYSON: ZAB-2F—that sounds vaguely familiar. Q—so, they had a quadrupole at the end.

HARRISON: It was a BEQQ instrument.

GRAYSON: BEQQ. Alphabet soup. [laughter]

HARRISON: And so we got that money in '84, and the ASMS [met] somewhere on the West Coast at that time. And we had a lot of chats with Chris Porter at VG Analytical about what we could do. In fact, Chris **<T: 90 min>** was very helpful. He was the VG man we dealt with. So that machine was delivered in '85, the summer of '85. Here's the picture of the gang at the VG plant in England on the final tests of the instrument.

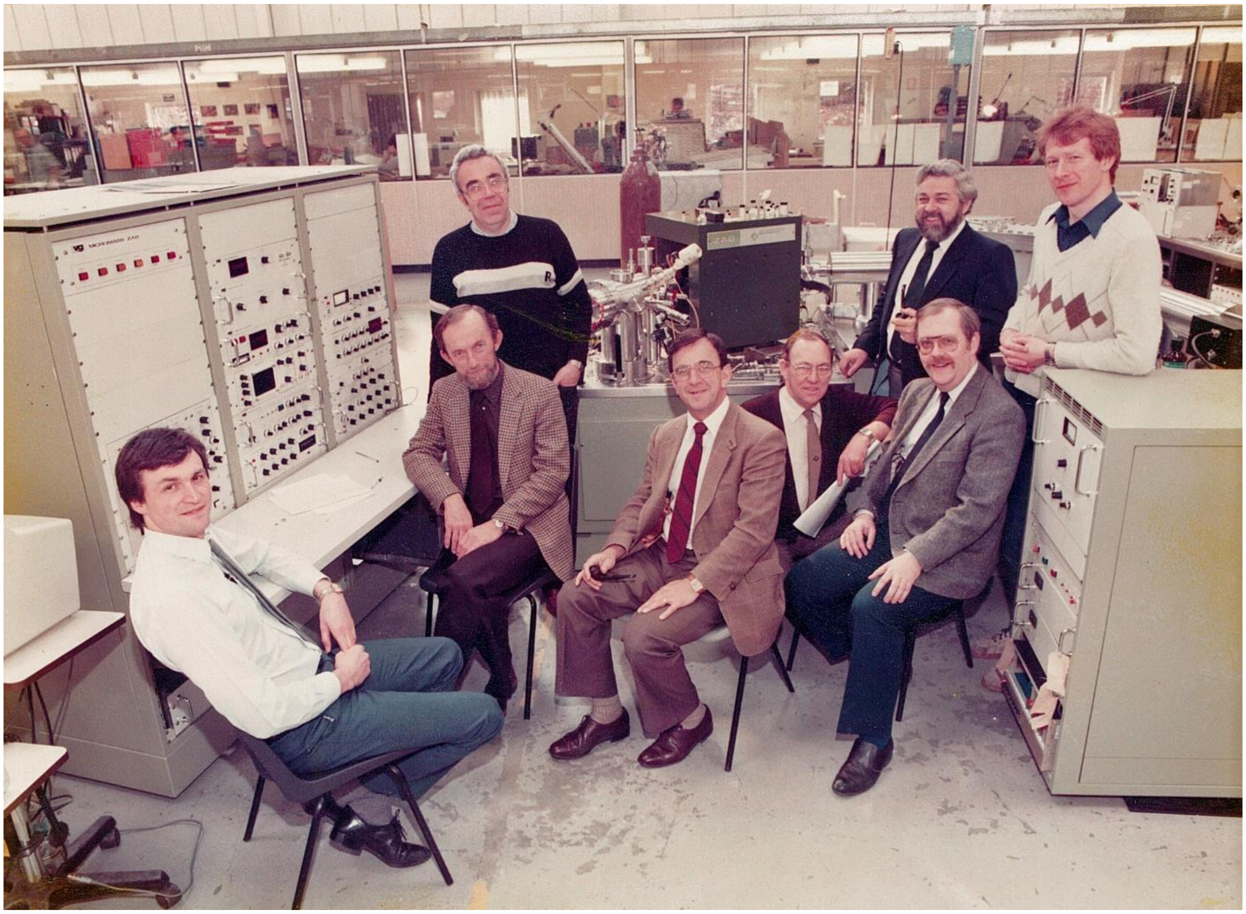


Figure 1: Harrison (second from left) and colleagues with mass spectrometer, 1985

GRAYSON: Yeah. So with this instrument you have to have a retardation of the ions before you get into your Q, right?

HARRISON: That's right.

GRAYSON: Now was that a significant problem?

HARRISON: Oh, it seemed to work very well. This was the first one that had been produced commercially of that configuration. That's Chris Porter, by the way, from VG. I guess it was Graham Cooks that probably built one himself. But this was the first commercial one.

GRAYSON: I guess the first two sectors gave you high resolving [power] so you could be a bit more selective in your ion?

HARRISON: [. . .] You could mass select with the magnet and look at the CID—the high energy CID—with analyzing by the kinetic energy with the electrostatic analyzer. You could also get into this game, which we got into, a bit of neutralization re-ionization of ions, which McLafferty and Chrys Wesdemiotis, and others had started.[22] Or you could take your ions through, as you say, with double-focusing resolution, and decelerate them into the quadrupole cell, and do CID as a function of, say, collision energy and so forth. So, it was a very versatile instrument.

GRAYSON: My sense is that the QQ part was probably the technology for doing those experiments was pretty well developed by that time.

HARRISON: Yes.

GRAYSON: So it'd make a lot of sense to—

HARRISON: [Yes]. Triple quads were around by then. We thought of a triple quad, but decided this was much more versatile, and managed to persuade the site visiting committee to—

---

[22] Fred McLafferty, Oral History Transcript #0352.

GRAYSON: Once again?

HARRISON: Well, they didn't actually want to fund the quadrupole part, but we worked out a deal with VG that the BE part was actually used. It had been used for a year or so, I think, in Switzerland, so we got a reduced price on that, so we'd have enough money to tack the QQ onto the end.

GRAYSON: So you got a used BE and a new QQ?

HARRISON: Right. [laughter]

GRAYSON: Well, I mean they're always willing to work with people to—

HARRISON: [Yes], they were very helpful.

GRAYSON: The funding committee was not interested in supporting the addition of your QQ at the end.

HARRISON: [Yes], they didn't think that was really necessary.

GRAYSON: They thought it was like icing on the cake or something, [. . .] when in fact if you're going to do ion molecule work, it seems to me it would be highly necessary.

HARRISON: We did more work in the Q part of the instrument than anywhere else.

GRAYSON: I would think so.

HARRISON: That instrument worked, I guess, until about 1998, at which time I had no students, and we had enough instruments in the service laboratory, I could do my research there, so we got rid of it.

GRAYSON: What happens to these old instruments like that? Do they just get converted into scrap metal or—

HARRISON: [The instrument went to Sander Mommers, an associate of John Holmes at the University of Ottawa who rescued old mass spectrometers.[23] I think the BE part eventually went to Dimitri Zagorevski—a former associate with Holmes—at Rensselaer Polytechnic, but I am not certain]. But it was removed without charge for us. So they could just play around with it.

GRAYSON: Okay, interesting. Yeah, well, I mean, it probably still worked, I imagine. [. . .] It was just a no more use of it for here, so you had to find some other place for it.

HARRISON: I might also mention that in ’85 I was awarded a Killam Fellowship. This was a Canadian award. **<T: 95 min>.** Izaak Walton Killam was in the financial business in the first half of the twentieth century. And they didn’t have any family, so when he died—he died first. When his wife died, she set up the Killam Foundation. And the Killam Fellowship gives the university enough money to hire a replacement for lecturing for the staff member who gets the award. And the staff member is supposed to be excused from all teaching and administrative duties. So, I had two years of that, which coincided with the arrival of this instrument. So, it worked very well. Of course, my application emphasized all the things we could do with this [new] instrument. [laughter] So that worked out very nicely.

GRAYSON: So you never had a situation that John Beynon had, where when he got his Royal Society Fellowship they gave the money to him.[24]

HARRISON: Yes.

GRAYSON: They didn’t give it to the school, which caused the school a great deal of consternation, as you can imagine, but basically they put it in a bank account that only he could draw from. And, of course, the bank was happy to have it because they used it to earn interest. But it was kind of interesting that it was his money.

HARRISON: No, that’s not the way the Killam works. It gives the money to the university. And I think it’s now about seventy thousand dollars a year. So that the university has seventy thousand to hire a replacement lecturer.

---

[23] John L. Holmes, interview by Michael Grayson at the University of Ottawa, Ottawa, Ontario, 12 December 2013 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0906).
[24] John H. Beynon, Oral History Transcript #0420

GRAYSON: They're directed to what they can do with it, but it's their money–

HARRISON: They're free to do, I think, whatever they want with the money, but the understanding is that the recipient is to be relieved of teaching and administrative duties.

GRAYSON: Now did you apply for this or—

HARRISON: Yes.

GRAYSON: Okay, so it was something that you knew was available and—

HARRISON: Yes, yes. Some of my colleagues had had them.

GRAYSON: Ah, so you knew that there was a good thing.

HARRISON: Yes.

GRAYSON: This is a two-year program, and it gave you enough money to do what you wanted for—

HARRISON: Well, it didn't give me any money directly. It gave me the free time.

GRAYSON: [. . .] Yeah, to do some science. And you didn't have to bother with teaching then.

HARRISON: [. . .] For two years, yes.

GRAYSON: Really get in there and do some work. [laughter]

HARRISON: Yes.

GRAYSON: I guess, you didn't have any administrative—

HARRISON: No, I didn't have any administrative duties for those two years either. I was relieved of those.

GRAYSON: So, that was an opportunity to make some headway—serious headway—in ion molecule reaction studies.

HARRISON: We got into negative ions, which we hadn't been able to do before.

GRAYSON: Okay, now negative ion work—

HARRISON: Negative ion chemistry.

GRAYSON: Why? What was the interest in doing negative ion?

HARRISON: You get different reactions—negative ion molecule reactions, different things happening.

GRAYSON: So, you're starting out with negative ions coming out of the end source.

HARRISON: Yes. Well, you're doing chemical ionization in the negative mode in the ion source.

GRAYSON: Okay. It's not really feasible to any—much in the way of negative ion work with the EI source, is it? Did you do any or is there—

HARRISON: Oh, we did some, yes. We did a whole pile of stuff.

GRAYSON: Oh, and negative ions in EI mode.

HARRISON: Yes. So we could take the negative ions into the quadrupole and do CID. We always—

GRAYSON: This was in the eighties?

HARRISON: Yes, this was in the last half of the eighties.

GRAYSON: But prior to that you weren't doing any—

HARRISON: We weren't doing it except for the time I spent with Keith Jennings, where we did do some negative ion work.[25]

GRAYSON: And so the whole field of negative ion is probably under ––

HARRISON: It hadn't had a lot of activity.

GRAYSON: Yeah, underexplored.

HARRISON: So it was nice to explore.

GRAYSON: Yeah. And you say the reactions that occur—the ion molecule reactions that occur in negative ions are completely different than what you would observe in the positive ions?

HARRISON: [Yes]. [. . .] It's an alternative way of—a negative chemical ionization is altered to the normal positive proton transfer. Don Hunt had started a lot of the negative ion work.

GRAYSON: But this is still not very much done with negative ions.

---

[25] Keith R. Jennings, Oral History Transcript #0419.

HARRISON: No. It hasn't really caught on. Many instruments are not capable of detecting negative ions **<T: 100 min>** very efficiently.

GRAYSON: Yes. It's an interesting problem. They're still ions, but it's just, like, I guess somehow they behave differently. [laughter] You can use the same optics and whatnot to move them around and do that kind of stuff.

Well, that was a good experience, then. I guess you were able to get some extra publications out during that period when you didn't have any other responsibilities? Okay, so what other areas—this is in the mid-eighties.

HARRISON: Yes, this is the last half of the eighties that we got the ZAB instrument. It proved so popular that the National Research Council Atlantic Laboratories in Halifax, Nova Scotia, bought one, and they enticed Bob Boyd to move from the University of Guelph to the National Research Council in Halifax. They used their ZAB Q. [laughter] So we lost Boyd.

GRAYSON: Okay, but in the meantime the instrument got him another job, a different job and it proved its worth—

HARRISON: Yes.

GRAYSON: The addition of the Q was—

HARRISON: It became quite a popular instrument, I guess, our first publication describing the instrument was well-received.[26]

GRAYSON: I've seen it's among your more heavily cited papers, at least from the citations that I can find out about. So, that really was an instrument that was a commercial instrument. That was not something that you guys built here.

HARRISON: Oh, no. No, no. We called the center the [Ontario] Regional Ion Chemistry Laboratory. [. . .] Or ORICL for short. [laughter] And we had money for some time to have a full-time manager, who kept the thing going, and scheduled people on and so forth.

---

[26] A.G. Harrison, R.S. Mercer, E.J. Reiner, A.B. Young, R.K. Boyd, R.E. March and C.J. Porter, "A Hybrid BEQQ Mass Spectrometer for Studies in Gaseous Ion Chemistry," *International Journal of Mass Spectrometry and Ion Processes*, 74 (1986): 13-32.

GRAYSON: So, you actually went and set it up so other people could use it from around Ontario.

HARRISON: Anybody who wanted to could come in and do their experiments with the help of the manager.

GRAYSON: Did you have some kind of a vetting process, or submission of proposal to use the instrument?

HARRISON: No, if Ray March decided that one of his students should come down and do some experiments that was fine.[27] We'd book them in as much as we could. I think it worked out fairly smoothly. It turned out that Ray was the one who did the most work on it from outside because, as I say, Bob Boyd moved to Halifax.

GRAYSON: Wow, that was a bit of a loss for you guys. But—

HARRISON: Well. he had a good career there.

GRAYSON: Now he's not there anymore, is he?

HARRISON: He's retired and had moved to Vancouver [British Columbia].

GRAYSON: Aha, yes. Okay, what next, then, as far as this negative ion chemistry world?

HARRISON: [. . .] By putting reagent gases in the quadrupole collision cell we were able to do some work on identifying ion structures by ion molecule reactions. It's just something we'd been trying to do. It was my original idea back in the fifties and sixties, but it wasn't until the eighties when we could, say, put methylamine in the collision cell, and bombard it with different ions to see what the reactions were, and there were other people doing some of the same sort of work. John Tedder for example in the UK.

---

[27] Raymond March, Oral History Transcript #0921.

GRAYSON: So this—you might call this—

HARRISON: Reactive collisions.

GRAYSON: Reactive collisions, yeah. [. . .] As opposed to ion molecule collisions.

HARRISON: Right, yes.

GRAYSON: You're **<T: 105 min>** actually promoting some kind of chemistry going on, and that's what's really interesting.

HARRISON: That was quite a bit of work done in the late eighties.

GRAYSON: So the reactions that you got, the mass spec data from that would give you some clues to the structure of the ion?

HARRISON: [Yes]. We did put deuterated methanol in. CH_3OD. Put it in ions then, and looked for the exchange—how many exchanges occurred and so forth. And so that was another— it opened up a whole general area. There were some other people doing this sort of thing, John Tedder in St. Andrews in Scotland was doing some of this work. [. . .] It was Lord Tedder actually. His father was one of the big wheels in England during the Second World War. And he had a student from Finland, Jorma Jalonen, who did a lot of that work too. [. . .] He had worked with Tedder.

GRAYSON: And this was in the reaction molecule . . .

HARRISON: This was in the reactive collisions, yes.

GRAYSON: Reactive collision, okay. This is kind of a small field right?

HARRISON: Yes, yes.

GRAYSON: So you guys were doing it and the people here in—

HARRISON: Yes. So there were a few other people who were incidentally doing it. I was asked to give a keynote lecture when the international mass spectrometer conference was held in Finland, because they were interested in this sort of thing. So I gave a keynote lecture on reactive collisions.

GRAYSON: [Yes], and this primarily was why you got this kind of an instrument or—

HARRISON: Well, there were many reasons because it was a very versatile instrument, which allowed you to do all sorts of things. The one thing I wanted to do when we got on into the end of the eighties and the beginning of the nineties was to start looking at peptides, but we didn't have a FAB source. It looked like it was going to be fast atom bombardment on this instrument was what we needed if we were going to get into biological molecules. The reason I wanted to get into that was that back in the seventies one of my students, Chun Tsang, had done quite a bit in chemical ionization of carboxylic acids. And he came to me and said, "Well, I'd like to do amino acids." And I said, "Well, why? We know that carboxylic acids lose—when they're [protonated], lose water and form the acylium ion, and so what's going to happen with amino acids?" Well, it turned out he was right and I was wrong. They don't form amino-acylium ions, which are unstable with respect to kicking off CO and forming the [immonium] ion. So I knew that what they were called in the peptide world in the eighties—b ions that b1 ions weren't stable, but b2 and larger ions were stable. I didn't see why putting an amino acid residue on the end of one should make them suddenly stable, unless something was happening. This was something I wanted to try.

GRAYSON: By this time your graduate student was long gone though.

HARRISON: Oh, yes, he was having a good career at the Hong Kong Polytechnic University.

GRAYSON: But he had a good idea.

HARRISON: He had a good idea. We published a paper.[28] We published some back-of-the-envelope calculations that, in fact, this was an exothermic reaction [for] the acylium ion to lose

---

[28] C.W. Tsang and A.G. Harrison, "The Chemical Ionization of Amino Acids," *Journal of the American Chemical Society*, 98 (1976) 1301-08.

CO, and there have been better calculations since. So I applied to NSERC for a number of years to get a FAB source and fast atom gun and didn't have success.

GRAYSON: No. At least that would not have been a very expensive proposition, would it?

HARRISON: I didn't think so, but they didn't want to give it to me. Finally I convinced VG, or I think by that time it was Fisons, that they could support my research. So they gave me a long-term loan of a fast atom source atom gun and a source. We already had on an instrument in this lab, the power supplies to run the gun so **<T: 110 min>** we got into FAB on this instrument, which again was very useful.

GRAYSON: And when would that have been?

HARRISON: That was '93 that they gave the loan.

GRAYSON: Because FAB had been used pretty heavily in the eighties, right, or when it came out, so you were kind of behind the curve.

HARRISON: We had it for the analytical service, but we didn't have it on the ZAB, which was where I wanted it. And so we got that. That was the year I retired actually—1993. I took early retirement.

GRAYSON: Was that for cause or because you wanted to or—

HARRISON: The university was in very difficult financial condition, and they were trying to get the high-priced help off the payroll, so they made it attractive to take early retirement.

GRAYSON: I see, so sweeten the kitty.

HARRISON: And I knew my pension wasn't going to be that great, so in effect they basically paid me one hundred and fifty thousand dollars to avoid three hundred and fifty thousand dollars of salary. And so I retired, but I'd confirmed that I could carry on.

GRAYSON: You wouldn't be 62 then. So that wasn't—

HARRISON: I was going to have to retire at 65.

GRAYSON: I mean that was the law—the rule?

HARRISON: Pardon? That was the law at that time, so I knew I only had three more years before I had to retire so why not get some money out of it? [laughter]

GRAYSON: Yeah, and then, well, particularly I have the understanding that you could continue to work.

HARRISON: Oh, yes.

GRAYSON: So that meant that gave you what—an office?

HARRISON: My office. I still had my lab.

GRAYSON: So you still had the lab?

HARRISON: Over the years, of course, the lab has disappeared. My office has been downsized to here, but I still have an office. I still can use the machines that are in the service laboratory, which we can visit sometime. And that's when we got into that peptide mass spectrometry. [. . .] I thought it would take a couple of years to sort out these about b ions. But it got me so involved I'm still carrying on. [laughter]

GRAYSON: So b ions were not as simple as they thought.

HARRISON: No, not at all.

GRAYSON: Not at all. So that's another area where I think you've been getting a fair amount of press, so to speak, in the—

HARRISON: Yes.

GRAYSON: "Why are B Ions Stable Species in Peptide Spectra"— I've got 241 cites for that paper.[29]

HARRISON: [Yes], that would be about right.

GRAYSON: That was published in '95 after you retired.

HARRISON: Yes. We got the fast atom FAB [. . .] going in the summer of '93, and I put a fourth-year student research project to look at that problem. She got enough results that it looked as though we had something. And in the spring of '94 I had a postdoc come in, by the name of Talat Yalcin, from Turkey. And he tackled the problem with a great enthusiasm. We showed that the b2 ion was a cyclic [protonated oxazolone].

GRAYSON: Cyclic oxazolone?

HARRISON: Essentially, as the bond was breaking, the amide bond breaking, the next carbonyl group came over and cyclized. And that did a lot.

GRAYSON: So the original fourth-year student was investigating these b ions and then you used your FAB to produce—

HARRISON: Looking at the fragmentation.

GRAYSON: Of peptides, small peptides?

HARRISON: Yes. And particularly looking at the metastable ion loss of fragmentation because they lose carbon monoxide, the b2 ions. And this occurred with considerable kinetic energy release, so we began to have some idea of the potential energy surface. And Talat carried

---

[29] T. Yalcin, C. Khouw, I.G. Csizmadia, M.R. Peterson and A.G. Harrison, "Why are B Ions Stable Species in Peptide Spectra?" *Journal of the American Society for Mass Spectrometry*, 6 (1995): 1165-1174.

this on, and we got involved with one of my colleagues, a theoretician did theory on it. And it all worked out very well.

GRAYSON: Okay. So that’s part of the paper about b ions that came out in—

HARRISON: ’95 **<T: 115 min>.** We published. That was our first paper in that field.

GRAYSON: Yeah, and of course by this time the whole business of proteins and peptides is starting to take off.

HARRISON: Oh, yes.

GRAYSON: Because now ways of seeing these things—ionizing these biological —in the species are becoming in vogue. As I say, a dirty little secret about mass spectrometry; if you can’t make an ion out of it, then … [laughter] you know.

HARRISON: Now these days, yes.

GRAYSON: But nowadays, I think you can make an ion out of just about everything I can think of. So that’s your first foray into the peptide world. A lot of people are interested, why? I mean, it’s kind of very fundamental, but apparently people see that as an important—

HARRISON: It’s a field that has the fundamental studies. One hopes the more you understand about how a protonated peptide fragments, the more it will help in sequencing. That’s the basic—the fundamental idea is that this is going to help if you know such and such, which bond breaks, why, and things like that. I thought for sure that we had solved all the problems of peptide sequencing. [laughter]

GRAYSON: Well, but I mean you’ve put in some effort to it over the last twenty years, and that’s work that’s appreciated, I think, by people who are using it as a regular tool. I’m just kind of curious. [. . .] There’s this paper on “Gas-Phase Basicities and Proton Affinities of Amino Acids and Peptides” that’s gotten a lot of attention.[30] And why is that so important?

---

[30] A.G. Harrison, “Gas-Phase Basicities and Proton Affinities of Amino Acids and Peptides,” *Mass Spectrometry Reviews*, 16 (1997): 201-217.

HARRISON: People seemed [to be] interested in wanting to know what were the proton affinities of amino acids and small peptides. It turns out that was a—tried to be a critical analysis of the data that was available, and it seems that I did fairly well in establishing the proton affinities.

GRAYSON: These are measured?

HARRISON: These were measured, calculated, mostly measured. I used mostly experimental work. Quite recently one [Guy] Bouchoux from France published another review of amino acids, basicities and proton affinities, based mostly on calculations.[31] He's primarily a theoretician. His numbers came out in the most part to be in good agreement with what I had recommended in '97. Was it '97? Or '99, '00, I think.

GRAYSON: It's '97.

HARRISON: Ninety-seven okay.

GRAYSON: [. . .] So he's trying to get at it from a first principles calculation?

HARRISON: He does experimental work, but he's very good at the calculations. There was some possibility that I might collaborate with him on this newer summary, but I told him to go ahead and do it on his own.

GRAYSON: Oh, okay. This is a recent publication in—

HARRISON: Yes, it's in 2012 in the *Mass Spectrometry Reviews*.

---

[31] G. Bouchoux "ChemInform Abstract: Gas Phase Basicities of Polyfunctional Molecules. Part 3. Amino Acids," *Mass Spectrometry Reviews* 31 (2012): 391-435.

GRAYSON: Oh, okay, very good. So this work has gotten a lot of attention, the "Amide Bond Dissociation in Protonated Peptides. Structures of N-Terminal Ionic and Neutral Fragments."[32] That was also published in '97 with over a hundred cites. That seems to be this really fundamental work, and is very attractive. You've got the scrambling of sequence information and collision-induced dissociation of peptides.

HARRISON: That creates problems. We had pretty well established that b ions were, well, oxazolones **<T: 120 min>.** That is, you had this five-membered ring at the C-terminal and then the chain terminating in an amine at the N-terminal, and so I thought, "Well, fine. Why don't I go ahead and use MS/MS techniques to do collision-induced dissociation of the b ions to get sequence information?" And I found when I'd looked at some b5 ions that I got the same spectrum, as if I'd taken the cyclic peptide with the same residues, protonated it, dissociated it. In other words, I got ions, which weren't indicative of the sequence. By this time I had established a collaboration with one Bela Paizs who at that time was at the German Cancer Research Center somewhere in [Heidelberg] Germany. [. . .] I'd established he was a theoretician.

GRAYSON: Hmm, another theoretician.

HARRISON: And by this time, [Imre Csizmadia], who I had worked with here had disappeared and went back to Hungary, so Bela did the calculations [on] all this, and we published a note in 2006 and the full paper in 2008.[33] And it turns out what's happening is this, when you get to b5 and you have the oxazolone [at the C-terminus], and the amine [at the N-terminus], the amine comes up around, and attacks the oxazolone, and you get a cyclic form, what then can reopen at different bonds, and fragment. And so you get sequence scrambling.

GRAYSON: So you shouldn't have bothered looking. [laughter]

HARRISON: That idea doesn't work, like many of my ideas, but it does create some problems, and so there's still controversy over how significant this is in terms of peptide sequencing. There are different views.

---

[32] M.J. Nold, C. Wesdemiotis, T. Yalcin and A.G. Harrison, "Amide Bond Dissociation in Protonated Peptides. Structures of the N-Terminal Ionic and Neutral Fragments," *International Journal of Mass Spectrometry and Ion Processes*, 164 (1997): 137-153.

[33] A.G. Harrison, A.B. Young, C. Bleiholder, S. Suhai and B. Paizs, "Scrambling of Sequence Information in Collision-induced Dissociation of Peptides," *Journal of the American Chemical Society* 128 (2006): 10364-10365. C. Bleiholder, S. Osburn, T.D. Williams, S. Suhai, M. Van Stipdonk, A.G. Harrison and B. Paizs, "Sequence Scrambling Pathways of Protonated Peptides," *Journal of the American Chemical Society* 130 (2008) 17774-17789.

GRAYSON: You know, they don't need to make things more complicated. They're complicated enough as it is. But that's been pretty much your total focus since—

HARRISON: Since about '93.

GRAYSON: Yes, you really got into the peptides, and I guess it's a fertile area as well, because it draws a lot of attention. Let me see. You have no really serious issues with trying to develop any financial support for your research projects. That's been something that's been—

HARRISON: Well, once I retired, NSERC started decreasing my grant, and I decided in 2010 that I wouldn't bother to apply for further renewal. That would've been for 2011. So I didn't apply for renewal. So my grant should've ended in the spring of 2011,but they gave me a year to use up what I still had left, so 2013 is really the time that I don't have funds. It's inevitable I'm going to retire sometime before too long. I've got to, or they'll put me six feet under.

GRAYSON: Yeah, the interest-granting situations, is that a year renewable thing, or a two-year renewable? Or once you get them to change—

HARRISON: Sometimes it's two. Sometimes it's three. I just forget what my last ones were.

GRAYSON: It's kind of once you get in the system you're there if you don't screw up. Is that kind of the way it works?

HARRISON: At the time I retired in '93 I was getting about fifty-five thousand dollars a year in grant.

GRAYSON: You're not getting any salary from the university once you retire, right?

HARRISON: No.

GRAYSON: Right, okay. So this is your—

HARRISON: This is my money for research. It's not going into my pocket. By 2001 to 2005 they had me down to twenty thousand dollars, but that was a five-year grant. Twenty thousand a year. And again from 2005 to 2011, essentially twenty thousand a year. **<T: 125 min>** It was my first step towards really retiring.

GRAYSON: So when did you stop taking graduate students? Or have you stopped or what?

HARRISON: Yes. And since I was made a professor emeritus it was possible for me to supervise MSc [Master of Science] students, but I couldn't supervise PhD students. I decided I wouldn't do either. So for a few years after I retired I had fourth-year students working with me on their research projects.

GRAYSON: Is that pretty much standard to have a fourth-year student do a research project?

HARRISON: Yes. [. . .] It's an option. They don't have to, but many of them do; and I kept postdocs until about '98 or '99.

GRAYSON: Okay. So it's just you're really kind of shutting things down, but I mean you're still getting stuff in the literature and publications through 2013.

HARRISON: A little bit. A little bit.

GRAYSON: Yeah, so it's obviously work that people are interested in so, you know, why not?

HARRISON: Why not? Oh, it's fun. What I really like is getting into the lab and doing experiments. I find that very satisfying.

GRAYSON: Well, that's good. It's good you have the opportunity to do it, but, I mean, like you say, you probably are not going to be doing it a ton of a lot longer but ––

HARRISON: Not a lot, no.

GRAYSON: Have you ever had any issues, other than with the department, or with the university, other than the fact that they wanted to kind of save money by getting you in an early retirement situation? But, I mean, you've had pretty good support from the university throughout your career here in terms of—

HARRISON: I have no complaints.

GRAYSON: No complaints, good, good.

HARRISON: In fact, the money for the DuPont 21-490 came from the university.

GRAYSON: Oh, okay. Now is that the one that—

HARRISON: The chemical ionization instrument.

GRAYSON: Okay, so that was your research instrument.

HARRISON: Yes.

GRAYSON: And the—but [the NRC—National Research Council]—paid for the service instrument.

HARRISON: Well, the 902. The MS-902.

GRAYSON: Oh, no, I'm sorry, that 21-490.

HARRISON: There was a 21-490—

GRAYSON: There were two of them, right?

HARRISON: Yes, one was in my research group and that was funded by the university.

GRAYSON: For chemical ionization.

HARRISON: And the one that was in the service lab was also funded by the university, as I recall.

GRAYSON: Okay, they picked up both of them, great.

HARRISON: There seemed to be some money around at that time.

GRAYSON: Yeah. What did you think of that instrument as an instrument?

HARRISON: We did some changes to it, increased the differential pumping, and it worked quite well.

GRAYSON: Okay. Well, I think it was essentially that was derivative of—I mean DuPont ended up inheriting CEC [Consolidated Engineering Corporation] through a fairly complicated route, and so it was kind of like the swan song I think for the CEC.

HARRISON: And it turned out to be the swan song for DuPont, too, in terms of mass spec.

GRAYSON: Yeah. Speaking of which, did you ever have any CEC instruments, from Consolidated Engineering Corporation or Consolidated Electrodynamics in your experience? No?

HARRISON: No.

GRAYSON: Obviously, they had a very large impact on the field in the beginning.

HARRISON: Oh, yes, yes, yes. I mean, it was a choice when we wanted to get into double-focusing whether we went with the CEC instrument or the AEI instrument. We went to the AEI.

GRAYSON: So, I guess at that time the CEC instrument was photoplate detection?

HARRISON: Yes.

GRAYSON: And did that affect it?

HARRISON: That was one of the considerations, that it would look better to maybe go the other way.

GRAYSON: [Yes]. That was a seriously contentious issue at the time.

HARRISON: Yes.

GRAYSON: There was a distinction of—

HARRISON: Which way to go—oh, yes.

GRAYSON: Yeah, and people spent a lot of time arguing about that. Computers kind of made that trivial, since they can process the data right up to the [inaudible].

HARRISON: Well, that's been one of the main advances [. . .] in the last fifty to sixty years, are computers.

GRAYSON: Oh, yeah, definitely, definitely.

Did you ever get any negative feedback when you went to publish your work? Did you have any, what you might say, hostile reviews or reviewers that more or less said, "This isn't really worth publishing," or anything—

HARRISON: Oh, yes, there have been a few withdrawn.

GRAYSON: How did these things get resolved? Did you go to the editor?

HARRISON: I think if the criticism got too much I withdrew the paper. I didn't need publications. It wasn't essential.

GRAYSON: Ah, okay. But did you think the criticism was fair or was it just **<T: 130 min>**—

HARRISON: Well, one of the amusing ones was the first paper we published after I came to U of T, which we submitted to a journal. It was a question of tropylium versus benzyl [ion] structures. With Kebarle and Lossing we had measured the ionization energies of meta and para substituted benzyl free radicals to form substituted benzyl ions and had seen considerable differences for meta and para substituents.[34] So at Toronto we measured the appearance energies of C7 ions from meta and para substituted benzyl derivatives. If these reflected the differences observed in the radical ionization energies presumably we had a benzyl structure, but if they were the same the meta/para orientation had been lost in forming the tropylium ion. We saw both behaviors. And one of the criticisms was, "If you believe this work…" [laughter]

GRAYSON: If you believe this work. [laughter]

HARRISON: It's interesting, but… [laughter]

GRAYSON: It's a nice way to start the review.

HARRISON: Yes. [laughter]

GRAYSON: What do you think I'm doing here, you know? And, obviously, you've had good work funding, so, I mean, you haven't really had any serious rejections in the funding department, I would assume, over the years.

HARRISON: No, the only problem I had was getting this FAB source. That was the only time I had any troubles.

GRAYSON: Yeah, and it seems like it's kind of interesting. It wouldn't be an expensive request, I wouldn't think.

HARRISON: No, it wasn't terribly expensive.

---

[34] Paul Kebarle, Oral History Transcript #899.

GRAYSON: Maybe they didn’t like the idea of FAB.

HARRISON: Well, I guess not, or they didn’t think I should be doing it.

GRAYSON: Hmm, interesting. Throughout all of this have you done any patent work, or any work that would be patentable—

HARRISON: No.

GRAYSON: —or considered patentable or so on? Because I think there’s a push in American universities, since the nineties, to try and patent everything that comes out of the—

HARRISON: Oh, you’d better believe it, especially there is, certainly in Canada.

GRAYSON: There is?

HARRISON: Oh, yes. The grants for basic fundamental research are not increasing, but any funding that is increasing is going for things which hopefully will produce something useful within the next few years.

GRAYSON: Something patentable?

HARRISON: Yes, and makeable, and make money for the economy.

GRAYSON: Well, I mean, the theory is that research is a source of creating wealth, eventually, but I don’t know if it be so A, B connected.

HARRISON: That is something I have some problems with, that we can dictate what result we’re going to get, and it’s going to be useful, and it’s going to sell billions, and solve the country’s economy, but that is certainly the attitude that is being taken these days. I think there’s a lot of basic research, which may lead to something or may not, but at least it’s advancing knowledge, and not just using it.

GRAYSON: Okay, well, I had one point I was going to touch on, and it slipped through my mind while I was talking about some other stuff, so let me use some of the other items I've got in here. So, you really haven't done any instrumentation development, but you've selected instrumentation that is well suited for the type of research that you wanted to do, and you've done some instrument modification, I would assume.

HARRISON: Yes, and of course the zero field pulsing was adapting work of Talrose, which we used extensively [from] about '68 or '69, **<T: 135 min>** that required setting up sorts of pulse generators, and so forth, and feeding that through. And '68 or '69 we started doing what we called trapped ions. The idea was you had a low-energy electron beam—the energy low enough that [it can't] cause ionization. And every once in a while you pulse that up to ionize for a short time, and the positive ions got trapped in the negative electron space charge of the electron beam.

GRAYSON: The ones that you had created?

HARRISON: The one we created with the pulse of high-energy electrons. And then you could let them sit there and stew, and eventually push them out with the repeller. And this was first brought to our attention when a chap by the name of Danby. [John Danby], at Oxford, published a paper on this.[35] He didn't do any ion molecule reactions. He was C.J. Danby. I don't know what his first name was. So I wrote to him and said, "Are you going to do ion molecule reactions, because if you are I'll leave it to you. If not, I'd like to do it." He said, "Go ahead." So I had a good postdoc at the time by the name of Alan Herod, and we set up this trapped ions technique, and we found we could trap ions for up to four milliseconds. So, you could do all sorts of sequential ion molecule reactions. So, we used that quite a bit for a while.

GRAYSON: So you've got these ions that you formed inside the ion source for a period of time. And they are—

HARRISON: They are colliding with the neutral molecules and there's a negative well, of course, from the electron beam which was eight or ten microamps.

GRAYSON: That's kind of keeping of the guys.

---

[35] A.J. Bourne and C.J. Danby, "A Technique for Ion Trapping in Pulsed-Source Mass Spectrometry," *Journal of Scientific Instruments (Journal of Physics E) Series 2,* 1(1968): 155-155.

HARRISON: Keeping them there, yeah.

GRAYSON: And it gives them—

HARRISON: They're alternating back and forth. It turned out that our best estimate was that their kinetic energy was about a third of electron volt.

GRAYSON: Oh, okay. So they were self-reacting, the ions and the neutrals.

HARRISON: Oh, yes. [. . .] For example, if you took methane in a mixture, say, with something of a higher proton affinity and mostly methane, a little bit of [the other], you could see the methane ion molecule products come up and then go down, as they ionized the next one, so you could follow the sequential reactions. That was a lot of fun.

GRAYSON: Yeah, it sounds like it'd be an interesting experiment. When was that done?

HARRISON: That was done in the late sixties. I think our first publication was 1970.[36] Alan Herod, who did the work, spent most of his [working] career with the British Coal Board and became an expert on the chemistry of coal and coal-derived product.

GRAYSON: [Yes]. An important source of energy with all kinds of problems associated with it.

HARRISON: [Yes].

GRAYSON: Let me see here. I'm going to go through and check further. Are there any things that we haven't covered that you wanted to talk about while I'm looking up—

HARRISON: No, I think I've covered most of it.

---

[36] A.A. Herod and A.G. Harrison, "Bimolecular Reactions of Ions Trapped in an Electron Space Charge," *International Journal of Mass Spectrometry and Ion Physics.* 4 (1970): 415-431.

GRAYSON: I have a question about your most significant publications. Didn't you mention that you were interested in the ones that were the most fun, or am I getting—

HARRISON: Sorry, what?

GRAYSON: Did you mention that you were most interested in the publications that were the most fun as to the most significant?

HARRISON: I have no problems with the ones that are well referred to. The one at the top of the list is a paper that I got involved in when I was on sabbatical at the University of Colorado.

GRAYSON: Oh, what was that about?

HARRISON: That was the bond energies in ethylene and acetylene.

GRAYSON: Ah, yes. Now, that's kind of an esoteric—

HARRISON: And I worked with Chuck DePuy and Veronica Bierbaum for six months in 1989. And another staff member, whose name I forget now, came in one day and said, "This is an interesting problem—bond energy" in something or other, **<T: 140 min>** ethylene I think it was. "It'll only take you a couple of days to solve this." Six months later we had half the department, including Carl Lineberger, involved in this, and it was a very good paper.[37] We got all the bond energies in ethylene and acetylene by negative ion mass spectrometry and the photo detachment of electrons by Lineberger. Got all the information we needed to get good values. So it was a fun project.

GRAYSON: So this guy waltzed into the lab and wanted this information.

HARRISON: Oh, he thought it would be an interesting project that we could solve in a couple of days. It took six months.

---

[37] K.M. Ervin, S. Gronert, S.E. Barlow, M.K. Gilles, A.G. Harrison, V.M. Bierbaum, C.H. DePuy, W.C. Lineberger and G.B. Ellison, "Bond Strengths of Ethylene and Acetylene," *Journal of the American Chemical Society* 112 (1990): 5750-5759.

GRAYSON: And it took, half the department to—

HARRISON: Well, it was [. . .] a couple of postdocs. It was DePuy, some postdocs, students with Carl Lineberger, Carl himself. Well, Barney Ellison was the chap's name who had this idea. I think his name was on the paper too. It looked like a firm of lawyers, the list of authors. [laughter]

GRAYSON: That's interesting. Now how is this information useful as you got all this ion detailed information about acetylene and ethylene?

HARRISON: Again, people are doing reactions of ethylene and acetylene. It's useful to have this type of bond energy information.

GRAYSON: I wonder how many people are in that area.

HARRISON: Well, it's had over three hundred [. . .] citations.

GRAYSON: Yeah, wow, interesting. Yeah, "Bond Strengths of Ethylene and Acetylene," 1990. Fun. So any other fun papers that you recall with fondness?

HARRISON: Oh, they were all—most of them are pretty good fun. There was one in '67, which has been popular—it was popular at the time anyway—in which we were concerned with ion molecule reactions involving polar molecules. What role did the dipole play of the polar molecule? And we worked out the theory for the case where the dipole locked in with the approaching ion in the most favorable way, and worked out what the rate of collision would be, and the rate of reaction, and then showed experimentally that that didn't apply. But that, of course, created some interest. And it, I think, basically, led to Mike Bowers' theoretical calculations that average dipoled orientation and things like that to explain the effect of dipoles on ion molecule reactions. I suppose the one thing that wasn't actually fun was writing the book on chemical ionization of mass spectrometer.

GRAYSON: Oh, yes. Apparently book writing is not so much excitement. What prompted you to do that?

HARRISON: I thought it might be [an interesting] exercise and CRC Press approached me. Would I write this? We had a monograph on that. So I decided to try it. Then they wanted a second edition, so that was easier because then I was computer [literate] more or less and able to use computers. [laughter]

GRAYSON: Yeah, the first time was pretty challenging, I would imagine, about the—

HARRISON: I wrote it out in handwriting and had a secretary here in the department type it up for me. But by the nineties when we did the second one, it was computer generated. My wife tells me if I ever do it again it'll be divorce. In fact, I was approached to write a third edition of the book [. . .], and I said, "No way." [laughter] Well, general chemical ionization isn't used a lot these days. What is used is atmospheric pressure chemical ionization.

GRAYSON: When did that begin to be such a popular—that was kind of a Canadian development, wasn't it?

HARRISON: Yeah, the SCIEX company, up in the north end of the city, or just outside the city, got into this area in the seventies. The person who was responsible for it was a professor in the Institute for **<T: 145 min>** Aeronautics at U of T, name of Barry French. [. . .] He developed these methods, and that was where SCIEX started.

GRAYSON: Okay. And that's been pretty much taken over, the chemical ionization field these days.

HARRISON: Yes.

GRAYSON: It's probably a little bit simpler, and it's become much more of an analytical tool than a research.

HARRISON: And I'm trying to think of his name. There was a chap in Vienna who did a lot of work—proton transfer mass spectrometry. I just can't think of his name at the moment [Werner Lindinger]. I met him a few times, and we corresponded quite a bit, but he accidently died while on holiday in Hawaii.

GRAYSON: That's not good. Do you have any other things for now? We could break for lunch, I guess.

HARRISON: Maybe we should go to lunch. The Faculty Club is fairly close.

[END OF AUDIO, FILE 1.1]

GRAYSON: Okay, we are continuing after a pleasant lunch at the Faculty Club here at the University of Toronto, courtesy of Professor Harrison. That was very nice. Just before we started this session you mentioned ASMS and membership, or working with the board. You served a tour on the board of ASMS, if I'm not mistaken.

HARRISON: That's correct, yes.

GRAYSON: Okay. What was exciting about that that, you know, you were talking about?

HARRISON: Well, it was interesting to be involved with the organization. It was much smaller, of course, then.

GRAYSON: This would've been—

HARRISON: Seventy-six to '79. I was on sabbatical with Keith Jennings in Warwick in the first half of '75, and I [got a] call.[38] People tracked me down—I think it was Fred McLafferty—and said, "Would you let your name stand for board of directors of ASMS?"[39] I said, "Why not?" So I ended up being the editor. And at that time, of course, there was a bound volume produced of shortened abstracts or the extended abstracts, and it was my job to get that produced and mail it out. It took up time, and a lot of time in the summer and fall.

GRAYSON: Yeah, it's almost as bad as writing a book. [laughter]

HARRISON: [. . .] About as time consuming, yes. But I had a very good secretary part time who handled a lot of the gut work.

---

[38] Keith R. Jennings, Oral History Transcript #419.

[39] Fred McLafferty, Oral History Transcript #0352.

GRAYSON: Eventually they hired out the work to Judith Watson Sjoberg. Was she on board at that time?

HARRISON: Who?

GRAYSON: Judith Watson?

HARRISON: Oh, I don't think so.

GRAYSON: Yeah, if you were having your secretary do things that was part of my understanding of why ASMS wanted to hire someone to do the job full time, because they were—

HARRISON: It was growing. [. . .] So, the first two years I was responsible for assembling it and mailing it out, and so we took over a lab here on a weekend and brought in all my friends and my daughters and their friends, and we packaged these things up and mailed them out. And the third year I assembled them, there was somebody in Pittsburgh [Pennsylvania], I think, who had some way of sorting them out and mailing them, so they got shipped down there.

GRAYSON: Well, if you mailed them from here wasn't that an issue with—I mean, you had to pay Canadian postage to –

HARRISON: Yeah, that's right, yes.

GRAYSON: So that was—

HARRISON: Overseas postage. Well, not overseas, but it's more expensive to send things to the United States, yes. But they didn't complain.

GRAYSON: So at that time the board also—were they vetting the papers that were submitted for presentation at the conference? I mean, abstracts had to be submitted.

HARRISON: Yes.

GRAYSON: But at one time they just pretty much accepted everything that came in, but eventually they started, you know, looking at—

HARRISON: Yes, I've been on abstract review [committees].

GRAYSON: Okay, but they weren't doing that during your tenure on the board.

HARRISON: I don't think they were, no. I think everything was accepted, which was not necessarily a good thing. [laughter]

GRAYSON: Well, and I think they wanted to also try and think it as much on the science side as possible or people would come in with papers that were from the vendors that were more like sales pitches. I think they were trying to control that and assure that papers were a good quality as best they could. But then that is a lot of work to review those papers and decide, first off, whether to accept it, and then which section it goes in.

HARRISON: Yes, I don't envy the people who have this job now; it's gotten so big.

GRAYSON: Yeah, we're looking at, what, three thousand submissions I think it is, at a minimum, or close to that for sure. So that was an interesting experience for you to be involved in the management of.

HARRISON: Yes.

GRAYSON: It was ASMS by that time.

HARRISON: Oh, yes, yes, yes.

GRAYSON: Yeah, ASTM was no longer in the picture. What was your feeling when, I think it was Graham Cooks was one of the instrumental persons in, getting ASMS to evolve out of ASTM at that time. Did you have any feelings?

HARRISON: I wasn't involved with it particularly at that time, but I think it turned out to be a very good idea. I'm happy with it.

GRAYSON: Well, ASTM **<T: 05 min>** E-14 didn't really, I think, represent the subject well. I mean, they weren't in the business of developing testing methods anymore, really.

HARRISON: No.

GRAYSON: I mean, that's what ASTM is for, is to develop testing methods, which was what was interesting or important to the petroleum chemistry community.

HARRISON: Yes.

GRAYSON: Well, the petroleum chemistry community was just a small fraction of the society.

HARRISON: ASTM, the committee E-14 couldn't cover effectively the whole range of activities that developed in mass spectrometry through the sixties and seventies particularly.

GRAYSON: Well, and in addition this is a time when the academic side of the field was kind of outweighed, or overtook the applied or the industrial side of the field.

HARRISON: Oh, yes, yes, yes.

GRAYSON: I mean, I think it's probably not well appreciated that mass spec got its start in an industry.

HARRISON: Oh, it got a start in the oil industry in the Second World War. Testing gasoline and since the oils—

GRAYSON: So, it took a while for that to evolve into an academic [field]. And I think the ASMS represents that—

HARRISON: That evolution.

GRAYSON: Yeah, much better. We were talking before lunch about fun papers that you were involved in. Did you recall any other? We talked about two or three of them, and I don't know if there were some others that you had in mind that I can bring up.

HARRISON: No, I can't think of any other particular papers.

GRAYSON: Okay. There's a sense of accomplishment to see that other people are citing your work in large quantity, and I always think it's interesting when older papers are cited because I think the technology for keeping tabs has become much better recently. But I guess a citation index has been around for some time.

HARRISON: Web of Science, yes, is what I use.

GRAYSON: Oh, okay. And that's pretty accurate, you think, in terms of keeping tabs on what's out there?

HARRISON: I think it misses out some of the early papers that one might've published but—

GRAYSON: I think they all do. [. . .] A lot of them are made available in PDF format, but still, you've got to have the software to search through all these and find the references and the work.

HARRISON: Well, I'm involved now—2013 is the hundredth anniversary of the beginning of mass spectrometry. The first paper of J.J. Thomson [Joseph John Thomson] was 1913.[40]

GRAYSON: Yeah, that was the year he published also the rays of positive electricity. I've been active in promoting that this last year, so we had a session. I was able to get the ASMS program chair to give us one oral session for the history of mass spec in the meeting in Minneapolis [Minnesota]. So that was a bit of a challenge because there's so much going on. But I was gratified to see—it was Thursday afternoon, competing against all the other normal sessions that are running simultaneously—and we had, I would say, at least three hundred plus people attend our history session at the last day of the conference, in the last session of the

---

[40] Joseph John Thomson, "Bakerian Lecture: Rays of Positive Electricity," *Proceedings of the Royal Society of London. Series A, Containing Papers of a Mathematical and Physical Character* (1913): 1-20.

conference. So that was well received. We had a session at the American Chemical Society Conference in Indianapolis [Indiana] on the history of mass spec, a symposium. We had actually Ken Standing come from Canada and give a talk on time-of-flight mass spec.[41] So we've been promoting the idea that this is really the hundredth anniversary of mass spectrometry.

HARRISON: Well, the *International Journal of Mass Spectrometry* has decided to publish one or more issues on the developments in mass spectrometry, and I've been asked to write a [retrospective] paper on the early days of ion molecule reactions kinetics, so I know what you mean by trying to find some of these old papers. [laughter]

GRAYSON: They can be had, but it's some—

HARRISON: It takes a lot of searching.

GRAYSON: It can be a challenge. Well, one of the interesting things looking at J.J. Thompson's earlier publications, he actually, I think, was doing a little bit of —unknowingly— was doing a little bit of **<T: 10 min>** either ion molecule or CI because he had this H^3 or this ion was mass 3. They didn't know where it was coming from.

HARRISON: Yes, yes. Yes, that was the early work. There were several people who got involved in that and showed that it really was H^{3+}. I found those papers. They took some doing to track them down. But the one I'm having trouble with is the early work of Victor Talrose in Russia. I'm sure some of it has been translated into English versions of the journal, but I have to go and tackle our librarian and get this sorted out.

GRAYSON: And that's going to be challenging there, but it's very important. That was one of the facets of the ASMS session we had at the ASMS meeting, was to get the international feel. So we had a woman from Russia. We had Simone Keating from Germany. We had Yoshi Yawakawa from Japan. And Bob Boyd. And so we had an international representation of the developments in mass spectrometry outside—well, in the United States, but outside of the United States, as well, because we believe there is so much out there. And so I think that was kind of very revelatory, particularly to the younger people in the crowd, you know, that there's this huge contribution to the field from all over the world. And it was interesting to have these people from different parts of the world talk at the conference. That may be why we got such a strong attendance, I don't know, but it was very well received, and I think a lot of people

---

[41] Kenneth G. Standing, interview by Michael A. Grayson at University of Manitoba, Winnipeg, Manitoba, 29 October 2014 (Philadelphia: Science History Institute, Oral History Transcript #0922), in process.

enjoyed finding out a little bit about the history of the field. And this is what keeps me interested in exploring and talking to people like yourself who've been there and done a lot, too. So right now it seems to me that the Canadian emphasis, I mean, you take Kebarle, and you, and Standing.[42] It seems like those are all kind of looking at fundamental aspects of the field, as opposed to something very applied or whatnot. Does that seem to be a—

HARRISON: That would be an honest assessment, yes. I would include John Holmes in Ottawa in that group as well.[43]

GRAYSON: So, I wonder if that represents the funding priorities that came from the agency. I guess the NRC is the primary funding organization.

HARRISON: Well, most of the ones that you've mentioned are either retired or close to retirement. I think there are a number of younger ones, people who are doing more—particularly in proteomics—quite a number of young people spread across the country who are doing more applied work.

GRAYSON: Well, I think that that's getting to be the case now with the proteomics is becoming the big emphasis.

HARRISON: It becomes big emphasis yes, yes.

GRAYSON: And metabolomics and glycomics and all these other—

HARRISON: Yes, all the -omics. [laughter]

GRAYSON: Yeah, it's kind of crazy all this -omics stuff. So it looks like you've had a very successful career in networking with your peers inside and outside of the University of Toronto, and you've been invited to go abroad to spend six months at a time doing research in [England] and Switzerland. That seems like a nice duty.

---

[42] Kenneth G. Standing, Oral History Transcript #922; Paul Kebarle, Oral History Transcript #0899.

[43] John. L. Holmes, Oral History Transcript #0906.

HARRISON: It was very satisfying, very interesting. One of the first I had was in 1974. I'd been invited by the Hungarian Academy of Sciences to spend a week in Budapest [Hungary]. And so we went to Budapest. Barbara [Harrison] only stayed three or four days. She had to pick up our daughters and get them back in time for school. But I was there for a week and that was very interesting. I visited all the labs that had mass spectrometers, and finally on the last Friday they had a one-day symposium on modern trends in chemistry, which I gave a talk at.

GRAYSON: This is '74?

HARRISON: [In] 1974. It was certainly still in the communist regime.

GRAYSON: Were there any issues with you getting in and out of the country?

HARRISON: No. No, we had no trouble at that time. We arrived on a Friday evening. We were given a supply of Hungarian money and told **<T: 15 min>** —well, the Academy had a guest house at that time, so we were put up at the guest house. We were provided with money and [they] said, "A car will pick you up on Monday morning." So we had Saturday and Sunday to roam around. We may have been followed, I don't know, but I didn't notice it. [laughter]

GRAYSON: Yes, apparently that was something that did happen from time to time. And people would be followed around particularly from the—

HARRISON: It may well have been. I don't know. It didn't bother me.

GRAYSON: But in terms of interaction with the scientific community there –

HARRISON: That was interesting, yes.

GRAYSON: That was wide open? There wasn't any attempt at trying to curtail who you saw?

HARRISON: No, no. The Danube River comes down through Budapest and there's a big bend. So on the Tuesday the lab that I was mainly visiting declared a holiday. We drove up the Danube seeing sights and so forth and came back the straight path down through the hills, and that's where you saw the Russian Army, up in the hills, above Budapest. I think it was Russian Army.

GRAYSON: Do you know when Hungary actually was freed from the Russian occupation?[44]

HARRISON: I'm not exactly sure.

GRAYSON: Was it Kebarle that escaped from Hungary before—[45]

HARRISON: No, he was Romania, I think, or one of the other countries. Bulgaria. He was Bulgaria.

GRAYSON: Okay, and he just managed to get out—

HARRISON: I think so.

GRAYSON: It's interesting how people wanted to get away from—whenever the Russians started to move in. They wanted to move out.

So right now you still are publishing. Your current work interests are probably pretty much done, I think, at this point, right? I mean, in terms of what you're doing—the kind of science you're doing.

HARRISON: Yes.

GRAYSON: So and that's all been mostly in the protein end of things.

HARRISON: In the last [. . .] twenty years, yes.

GRAYSON: Yeah. And you're doing this work with fourth-year students?

---

[44] Withdrawal of Soviet troops from Hungary began after the Revolutions of 1989. The last Soviet troops left Hungary on 19 June 1991.
[45] Paul Kebarle, Oral History Transcript #0899.

HARRISON: I'm doing it largely by myself now, [. . .] but I have some collaborations. I've had quite a bit of collaboration with Bela Paizs. That has now sort of ended. He's got too involved in being a professor, so he's not doing much research. What's taken over is probably Ben [Benjamin J.] Bythell, who is [at] Missouri University, St. Louis, who was a postdoc with Paizs.

GRAYSON: There's a local mass spec discussion group in St. Louis that I was involved in starting many, many, many years ago.

HARRISON: Yes. We've all been there.

GRAYSON: I just got a notice, an e-mail notice, not too long ago that they were inviting in someone who had just taken a position at University of Missouri in St. Louis so—

HARRISON: That probably is Ben Bythell.

GRAYSON: Yeah, must be.

HARRISON: He's quite a good, mostly theoretician, but also experimentalist. So, he's tackling some theoretical aspects of a problem I have. I have the results and he's trying to interpret them in terms of structures and energies of ions. One of the papers that you mentioned that I published this year was in collaboration with my former postdoc, Yalcin, who's a professor in Turkey.[46] And we've kept in touch.

GRAYSON: So how does that work in terms—he was from Turkey?

HARRISON: He was from Turkey and applied for a postdoc position with me. He seemed good. He turned out to be good. He bounced around with several other groups in North America and tried to get an academic position in North America, but eventually went back to Turkey and is doing quite well. I think he has the mass spectrometry lab in Turkey.

---

[46] Alex G. Harrison, Cagdas Tasoglu, and Talat Yalcin. "Non-direct Sequence Ions in the Tandem Mass Spectrometry of Protonated Peptide Amides—An Energy-resolved Study." *Journal of the American Society for Mass Spectrometry* 24, no. 10 (2013): 1565-1572.

GRAYSON: Does he have a MALDI [matrix-assisted laser desorption/ionization] do you know?

HARRISON: He certainly has electrospray. I'm not sure about MALDI. I think he may have.

GRAYSON: Oh, that's interesting, okay. But **<T: 20 min>** you're still connected with him and cooperating on things.

HARRISON: Yes, yes, we correspond on a fairly regular basis.

GRAYSON: I guess Turkey is the most western of the eastern countries.

HARRISON: Yes. You see, Yalcin seems to have a lot of money because he attends most of the [ASMS] meetings and brings two or three students with him, and that's not cheap coming from Istanbul [Turkey] or near there.

GRAYSON: That's interesting. My son visited somebody, a mass spec lab in Turkey so I'll have to find out if there's a connection that way; I think that would be kind of interesting if there was a connection that way.

All right, I don't know if you have any other topics that we haven't discussed that maybe you want to talk about? We've covered, I think, your most significant publications and the most fun publications. [. . .] So what do you think—the impact of your work is on the field of ion molecule reactions?

HARRISON: I don't think we should neglect the aspect of training. Several of my students, graduate students or postdocs or visiting, have done quite well in the academic world. One of my students who graduated in '86 went straight to the Ontario Ministry of the Environment and is now one of the world's experts on dioxin and other chlorinated compounds, mass spectrometry thereof.

GRAYSON: Ah, and his name would be?

HARRISON: His name is Reiner. Eric.

GRAYSON: He's kind of in a high-level position in the Ontario Ministry here.

HARRISON: Ontario Ministry of the Environment, yes. But he's done very well, but he did his graduate work with me. Another one who was about the same time is now running one of the, proteomics labs at one of the universities in Florida, so there was more than just the impact of the results, but the training of the students, and I don't want that to be downplayed. I looked at this as a joint effort, where we were solving problems, and they were learning and I was learning.

GRAYSON: And so as important as the body of work [. . .] is, the people who have benefitted from their association with you—

HARRISON: Yes. [. . .] You didn't meet Michelle in the mass spec lab, but you saw probably the young woman running around?

GRAYSON: [Yes]

HARRISON: She did a master's degree with me. She went to work for Eli Lilly who had a research and development lab in Toronto. They closed that a number of years ago, so we brought her back to work here. I'm not quite sure, but I think, maybe, the triple quadrupole—the instrument that's there came from Eli Lilly. They were going to throw out all their mass spectrometers because there might be secrets or capacity things or so forth. I heard about this through Michelle and another student who'd worked there and got the chairman to intervene, and I think we ended up with two or three triple quads, one of which came here. One or two went to the suburban campuses. I think we got everything except for the hard drive. [. . .] Yeah, you can see the reason.

GRAYSON: Yeah, I can understand why they wouldn't want to give you the hard drive.

HARRISON: No. But a couple of my students have I mentioned earlier, Tsang, who had a good career at Hong Kong Polytechnic University. He now lives in Vancouver. Another one [Hei-Wun Leung], was a research associate with me for some time in the seventies. He ended up at the Hong Kong Baptist University.

GRAYSON: Hmm. It sounds like you've had a fair number of students from abroad.

HARRISON: Oh, yes.

GRAYSON: Primarily **<T: 25 min>** Chinese, Asia?

HARRISON: I had an Indian postdoc at one stage, early in the career. Postdocs and a number from the UK, the Netherlands, Israel, United States, China, Japan—

GRAYSON: Pretty much broad spectrum.

HARRISON: Yes. I'm still getting letters or e-mails from Chinese students particularly, applying for a postdoctoral position. No way, Jose. [laughter]

GRAYSON: Thanks, but no thanks, right? [laughter]

HARRISON: Right.

GRAYSON: This is kind of a broad-spectrum question. What do you think is the importance of the vitality of research and development chemistry for the country, for progress. I mean, really kind of a broad, wide-open question. Do you have any feelings one way or the other?

HARRISON: Well, obviously, there's a lot of work going on in proteomics, cancer, searching for a cure for cancer of various types. There's also a bunch of other stuff going on, microfluidics and things like that. One of my colleagues and his associate just won some award for developing an anti-fouling compound to apply to boats and fishing nets and so forth, which doesn't have the heavy metals, that bit of a hazard to the fish in the ocean. So, there's a lot of surface chemistry going on and nanochemistry, a big field, quite a bit of that going on here.

GRAYSON: So, you think that's probably where we're going to be getting with chemical research and development.

HARRISON: [Yes], I'm afraid it's going to be too much development and not enough research. That's the way the [present] government wants to run. Funding picture is too—

GRAYSON: I read an article in the *C&E News* [*Chemical & Engineering News*] this last week or two about the government here is kind of waiting on people who are doing work in the environmental chemistry area. Are you aware of this? They're kind of trying to keep them from—

HARRISON: There are several things happening in Canada at the moment. One is that scientists who work for the government are pretty well muzzled. They can't speak clearly about what they are doing or what they want about the results they're getting.

GRAYSON: Is this irrespective of the field that they're in?

HARRISON: Pretty well, yes.

GRAYSON: The impression I got from this article is primarily, or certainly, focused on people working in the environmental—

HARRISON: Well, that is the field that they're really clamping down on. Canada is not a good neighbor in terms of environmental record. We haven't had any effective measures to control CO_2 levels. [. . .] We have a burgeoning oil industry in Alberta, which is very dirty oil. It's more almost asphalt. It's very energy consuming to get it out. It creates a lot of CO_2 emissions. But the government seems to have taken this as the economic future is to produce oil, and there are many arguments over building a pipeline from Alberta down to Texas, which is waiting for [President Barack] Obama's approval.

GRAYSON: That's the Keystone Pipeline?

HARRISON: Yeah, that's the Keystone. So they don't like to be criticized in terms of their environmental records. And so anybody, at least who's under control of the government, is pretty well muzzled. Now another thing there was an experimental lakes area in **<T: 30 min>** northern Ontario, which was a gem in terms of having a whole lake that you could carry out experiments on. This was funded in part by the federal government. They've closed it down. It played an important role at looking at acid rain and things like that.

GRAYSON: The problem with these activities is you don't want to make an investment that's thrown away and you don't go back and decide you want to start it up again and get back to where you were. [. . .] Unfortunately, our non-scientific friends can't comprehend the idea that you just don't turn a switch and things, you know, go back to where they were before you clobbered them. [laughter] Well, I've noticed in the United States at least over my career—I've worked in industry most of my career—is that the sympathy, or the support of science in the industry has become considerably less and less with time. I don't know if you have a feel for what's going on in Canada in the industrial laboratories, how that's playing out?

HARRISON: There is a significant problem that Canadian industry doesn't do a lot of research, especially if they're a subsidiary of an American company. I mentioned Eli Lilly. They had research development labs here in Toronto for quite a long time. They closed them down, moved their stuff all down, back to the States. I forget where Eli Lilly is headquartered.

GRAYSON: Somewhere in Ohio I think—Cincinnati [Ohio] or Indianapolis [Indiana] or whatever—

HARRISON: Somewhere in there. Indianapolis, I think maybe. So that's part of the problem. But I think in general the Canadians tend to be too complacent. "Business is working fine now. Why should we try to upgrade it and update it and do research? We're making enough money now, what the hell?"

GRAYSON: Well, you know there are all kinds of attitudes and approaches, and that's one.

HARRISON: We have a problem at the moment with BlackBerry, which has been a big company for much of the time up in Waterloo [Ontario]. But they, I think, didn't keep up with the times, didn't put enough money into their research and development. "We're selling cellular phones, why should we continue to do research?" And Apple overtook them.

GRAYSON: Oh, yeah. Well, that's an area that's almost as bad as mass spec in terms of the technological development, because, I mean, mass spec is an area that is hard to keep up with in terms of the technology. It's just crazy in my mind. You know, every year you go to the ASMS meetings. You go to the Pittsburgh conference [Pittcon] and just have a tremendous amount of new developments. To me, it's very amazing. J.J. Thompson would be proud. [laughter]

HARRISON: Well, I recall when I first started going to these meetings in '59 and on, there was a lot of talk and development of time-of-flight instruments, because they were going to be simpler to put up with the rockets in space, but they were low resolution, low mass range machines at that time. Look where they've gotten to now. A major development.

GRAYSON: All the old sector machines that held sway have kind of been put out to pasture.

HARRISON: I don't think they must sell many sector instruments now.

GRAYSON: That would be an interesting thing to find out. I have no idea.

HARRISON: No, I don't either.

GRAYSON: I'm sure you're right. The whole thing has changed. Unless you have any other comments that you want to make, then I think maybe we've had the audio portion of the interview pretty much done. I'm not sure if you want to sit for a video recording or not. That's up to you.

HARRISON: I'd just as soon not.

GRAYSON: Okay. Can I just then talk about the more philosophical topics that are **<T: 35 min>** in the video interview, one of which relates to mentoring and being mentored. You had a number of mentors in your career as you were growing up and coming into chemistry, and developing your own career, so what was it that these mentors did that worked, that helped you, or maybe inspired you or motivated you? Do you remember?

HARRISON: Well, I think I was lucky in my MSc and PhD work to have mentors—my supervisors—who gave me quite a bit of freedom to do my thing and encouraged me. That gave me more enthusiasm, to see that they appreciated—they encouraged me to carry on. I made mistakes, and they pointed them out to me—nicely. [laughter] Fred Lossing [at] NRC in Ottawa was a great person. I enjoyed working there not only for the chemistry, but for the interaction with a very intelligent man. So, all of those three helped a lot. When I came here D.J. LeRoy had taken over as—well, in 1960 took over as head of the department. He was very encouraging. Sometimes I didn't take his advice. I remember one day him telling me, "You know, you really should wear a tie when you're lecturing." I didn't take that advice. [laughter] I've met a lot of encouraging people, and the greatest encouraging person has been my wife.

GRAYSON: Oh, okay. She encouraged you not to write another book. [laughter]

HARRISON: She helped me write the second one.

GRAYSON: Oh, really?

HARRISON: She's been a wonderful companion.

GRAYSON: So, you now have done a lot of mentoring of others.

HARRISON: Yes.

GRAYSON: So, what is it that worked best with these people coming up, and how did you connect with them and get them to kind of follow your advice and—

HARRISON: There is not a standard method, I don't think. Each student is different, and you have to adjust your mentoring to that student. Some of them want to be left alone; give them the grand outline and they want to get ahead with it. Some of them will want to have discussions frequently. I tried to recognize these differences, but I also wanted to make it clear to them that we were working on this together. It wasn't me way up there telling you, "Go and do this." We jointly had a problem to solve. And I think that was very important, too. [. . .] Try to make it as a collaboration if we can, and as a result several of those students I still am in contact with, we became friends.

I've mentioned where this trapping of ions in an electron beam. The person that did that, Alan Herod, was English. We've kept in touch. He came back this past summer and stayed three days with us. We got an e-mail this morning. Our granddaughter is in England at the moment. She's finished high school, and she's taking a year off to explore, and she's working in a town called Broadway in England. She had the day off either yesterday or the day before, and Alan Herod and his wife picked up Kathryn and took her around exploring the countryside. This is association dating back to the sixties.

GRAYSON: [Yes], nice. So if you saw someone—a young person today—who hadn't gotten involved in science yet, but seemed to show that they had an interest or aptitude for it, how would you advise them to go forward, to encourage them to pursue a scientific career?

HARRISON: How would I? I would encourage them to really think about what would make them happy and satisfied, not **<T: 40 min>** to become an inorganic chemist because that's trendy, but are you going to be happy there? Or maybe even to become a physicist instead of a chemist. Whatever is going to make you happy, I think, that's the main thing.

GRAYSON: Try and own the process and not be driven by someone else's idea of what you should be.

In your career in science there's been a fairly large number, or increase in the number of women who have come to pursue science. How do you see this development?

HARRISON: I think it's a very good one. I find here it's particularly in environmental chemistry. I think they're more socially conscious.

GRAYSON: Hmm. You think perhaps they may be better suited for some kinds of investigations than men may be?

HARRISON: I think their interests probably are slightly different. As I say, I think they're more socially concerned.

GRAYSON: Obviously, science is a field that has a lot of competition for funding and competition to be the first person to publish. Have you been involved in any of that type of competition and what do you think about the whole idea of competition in science?

HARRISON: I suppose I've been involved by attending meetings, I've been pretty well aware of what is going on in the field, but I find I don't think mass spectrometry is as cutthroat in that respect as some areas of science. And I've found for the most part that people I've met and the people I know are always very helpful, encouraging, so I haven't felt competition was a major problem.

GRAYSON: Okay. You sense that it isn't a problem or it is more competitive in other fields of science, but you feel that mass spec is a more of a collegial—

HARRISON: Yeah. I think in the development of medications or new drugs, there may be more cutthroat. There's more money involved in the long run. And one wonders if the biological mass spectrometers may be more cutthroat then the old-fashioned ones, which I am one. [laughter]

GRAYSON: Yes. Me too. [. . .] Do you have any comments to make about the changes you've seen in the field of mass spectrometry over your career? It's gone from a kind of a niche technique to—

HARRISON: One never would've dreamt, say in the fifties, that you'd be doing peptides and proteins. It's just changed the whole field has changed. ASMS. But the developments have been major, major, major, major, and the instrumentation to take these two techniques alone have been major as well, so it's just nothing like I saw it in 1950s.

GRAYSON: But you're able to appreciate where it is today, so where do you think it's going to head from here? [laughter]

HARRISON: I couldn't predict. I think there are still lots of developments that can take place in proteomics, lipidomics, a lot of these areas. I think the DART ionization, as Matt [Forbes] was saying, is a good step forward, which one might not have thought of originally. It's why you turn people loose, is they're going to come up with some new ideas.

GRAYSON: Yeah, that's a crazy ionization technique. I started out using a 110 to analyze gas samples. [laughter] Now it's just like you couldn't—it's hard to comprehend what's going on with the field anymore, and I don't think it's good to try and predict, really, because it's so unpredictable. **<T: 45 min>** I think the only thing you can say, I think, there were people in different times who thought that the field had hit the wall, you know that it—

HARRISON: There's nothing more that could be built.

GRAYSON: Right. And I think that if you have that position, I think you're in trouble, because every time people said it hit the wall, then somebody would come along and blow it all away, and off you are into a whole new field, so it's definitely got a future. I'm sure of that. Whatever it is I don't think any of us could predict, but it's not going to—

HARRISON: It's not going to be stagnant.

GRAYSON: That's for sure. There are too many people doing too many good things and having a lot of fun doing it. That's what counts—having fun. Well, unless you have any other comments or thoughts that you want to preserve for posterity, so to speak, we can probably wrap things up here.

HARRISON: I don't think I do, no. No.

GRAYSON: Very good.

[END OF AUDIO, FILE 1.2]

[END OF INTERVIEW]

INDEX