SCIENCE HISTORY INSTITUTE

KENNETH G. STANDING

Mass Spectrometry

Transcript of an Interview
Conducted by

Michael A. Grayson

at

University of Manitoba
Winnipeg, Manitoba, Canada

on

29 October 2014

(With Subsequent Corrections and Additions)

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.

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KENNETH G. STANDING

1925 Born in Winnipeg, Canada, on 3 April

Education

1948 BSc, University of Manitoba, Mathematics & Physics
1950 AM, Princeton University, Physics
1955 PhD, Princeton University, Physics

Professional Experience

University of Manitoba, Winnipeg, Canada
1953-1959 Assistant Professor
1959-1964 Associate Professor
1959-1967 Director, Cyclotron Laboratory
1964-1995 Professor
1995-present Professor Emeritus

Honors

1950-1951 Class of 1883 Fellow, Princeton University
1952-1953 Charlotte Elizabeth Procter Fellow, Princeton University
1958-1959 Nuffield Fellow, University of Bristol
1967-1968 NRC Senior Research Fellow (and Professeur Associé), Université de Grenoble
1985-1986 NSERC France-Canada Exchange Scientist, (and Professeur Associé), Université de Paris XI (Orsay)
1998 CMS Award for Distinguished Contributions to Mass Spectrometry, Canadian Society for Mass Spectrometry
2000 Meloche Lecturer, University of Wisconsin
2000 Synergy Award for University-Industry Cooperation, NSERC/Conference Board of Canada
2003 CAP Medal for Outstanding Achievement in Industrial & Applied Physics, Canadian Association of Physicists
2004 Elected to Fellowship, American Physical Society
2004 ACS Field/Franklin Award for Outstanding Achievement in Mass Spectrometry, American Chemical Society

2004	Elected to Fellowship, Royal Society of Canada
2006	Establishment of the “Ken Standing Award by the Enabling Technologies Symposium
2006	Brockhouse Award for Interdisciplinary Research, NSERC
2009	Honorary DSc, University of Manitoba
2009	Sir John William Dawson Medal, Royal Society of Canada
2010	Encana Award for Innovation, Manning Foundation

ABSTRACT

Kenneth Standing grew up in Winnipeg, Manitoba, Canada, the oldest of four children. His father was an accountant, his mother a primary school teacher and housewife. Standing says he ended up in science by process of elimination, by gradually ruling out subjects he did not love. He won a senior scholarship to the University of Manitoba. World War II intervened, and he joined the University Naval Training Division (UNTD), which had him stoking and cleaning boilers in Halifax, Nova Scotia, and Shelburne, Ontario, for a year. For his PhD, Standing followed a friend to Princeton University's physics department, where he worked on scintillation counting in Rubby Sherr's nuclear physics lab and then on a fast-cycling cloud chamber with Milton White. Both experiments failed, but Standing's two theses, one on double beta decay; and the other on proton-deuteron (p-d) reactions in nitrogen-14, got him his degree.

As a faculty member at the University of Manitoba, Standing was one of the first to study gamma-ray scattering. He spent five years building a cyclotron for Manitoba, tried to help fix the one in Grenoble, France, and then returned to Manitoba to become director of the cyclotron there. A project analyzing protein in wheat for the Grain Research Laboratory, and the arrival of Brian Chait from University of Oxford, pushed Standing toward mass spectrometry. When Chait went to Rockefeller University, Werner Ens and Ronald Beavis became Standing's first graduate students in mass spectrometry. All of his honors have been bestowed since he left nuclear physics, he says.

Standing discusses his many collaborations, pointing out that he needed chemists to provide the raw materials for his work. He explains his collaboration with SCIEX on a hybrid mass spectrometer. He talks about developing and perpetuating the field of time-of-flight mass spec, citing as his most important contribution his 1981 publication of the design of his original time-of-flight mass spectrometer. He also believes that his work on collisional damping was seminal. He talks about his publication record and his patents. When his funding from the National Institutes of Health and Natural Sciences and Engineering Research Council came to an end, Standing retired, but he continues to provide analysis for other faculty members and good public relations for the University.

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.

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INTERVIEWER: **Michael A. Grayson**

LOCATION: **University of Manitoba**
Winnipeg, Manitoba, Canada

DATE: **29 October 2014**

GRAYSON: [. . .] So, I'm going to start this by saying that this is the twenty-ninth of October 2014, and I am interviewing Professor Kenneth G. Standing at the University of Manitoba in his office, and my name is Mike Grayson, and without further ado, I'm going to go ahead and start the interview, and ask Ken a little bit about his background. Particularly, we're interested in the educational background [and] attitude of his parents towards education and science. So, I guess it'd probably be good to let us know when you were born.

STANDING: I was born on [the] third of April 1925.

GRAYSON: And you were born in what part of the country?

STANDING: In Winnipeg, [Canada].

GRAYSON: Winnipeg. [. . .]

STANDING: I'm a native.

GRAYSON: You're a serious native.

STANDING: [Yes].

GRAYSON: You've probably seen Winnipeg change quite a bit over those many years.

STANDING: That's right.

GRAYSON: [. . .] It is. And your parents' names?

STANDING: I've written all this down. My father was E. Graham Standing, and my mother was Elsie M. Standing.

GRAYSON: And so were you an only child, or were there other siblings?

STANDING: No, there were four of us. They're all dead except me. I was the eldest. But I'm still alive.

GRAYSON: You're the survivor.

STANDING: [Yes].

GRAYSON: That has sometimes—it's a good thing. Sometimes it may not be so good.

STANDING: [Yes].

GRAYSON: What did your father do for a living?

STANDING: He was basically an accountant. He ended up as vice president of an outfit called National Drugs Limited, which was a wholesale druggist in Winnipeg.

GRAYSON: I see. So, that was a fairly—I mean, a lot of people's background would have been in farming or cattle raising, you know, at this time, but even in Winnipeg at this particular time he had more of a professional job than probably most of the people in the city.

STANDING: He wasn't a professional, I'd say. He was an accountant.

GRAYSON: I guess the population of Winnipeg was quite a bit smaller in [1925].

STANDING: It was a fraction of what it is now.

GRAYSON: Which today, you're saying about eight hundred thousand probably?

STANDING: A few hundred thousand. [Yes].

GRAYSON: And your mother, did she have any education in her background, or—

STANDING: She was a primary teacher. She taught grade one before she was married, and then she became a housewife.

GRAYSON: So, that was a standard thing, then, if a woman teacher became married, she ceased teaching.

STANDING: That's right.

GRAYSON: We're not sure why they did that, but—

STANDING: Well, she had four children—

GRAYSON: Well, that's one reason.

STANDING: —very quickly.

GRAYSON: And so, did your father have any college education?

STANDING: No, he got an honorary degree from the other university here, the University of Winnipeg. He was very active in the United Church [of Canada], and he got something called—I think it was Doctor of Sacred Letters or something like that.

GRAYSON: I see. So this was in recognition for his many years—

STANDING: For his work for the church.

GRAYSON: So, his accounting background came from just his—I assume he went to high school?

STANDING: He went through school in Winnipeg, but—well, wait a minute. First of all, I had a maternal uncle who got an MD from Manitoba. And a paternal uncle who was a teacher and finally principal, and he received a BA. These were from the University of Manitoba.

GRAYSON: So this represented a fairly well-educated group of people in your immediate family circle?

STANDING: Reasonably well-educated group, yes. It wasn't . . . well, it wasn't highly **<T: 05 min>** educated but somewhat.

GRAYSON: Well, [. . .] I'm guessing that in general, though, it'd be more highly educated than the generally populace in Winnipeg at the time.

STANDING: Certainly so. [Yes].

GRAYSON: I think that's, you know, the kind of point that we would like to draw out at this point. So—

STANDING: But no scientific education particularly.

GRAYSON: [. . .] Well, then, let's start with your education.

STANDING: Okay—

GRAYSON: Obviously, you went to grade school somewhere.

STANDING: I was educated entirely in public schools in Winnipeg, to start with, and then as my siblings were, and—

GRAYSON: Were there any events in the early education in grade school that got you interested in science, or—

STANDING: Well, [. . .] I was educated in Winnipeg public schools, ending up in something called DMCI, which was Daniel McIntyre Collegiate Institute which was the high school, [. . .] and so I was educated in these public schools in Winnipeg, and I ended up in grade eleven being selected to write for a scholarship, which I was successful in receiving.

GRAYSON: All right. [. . .] Canada is a British . . . is related to Britain, okay?

STANDING: Originally, anyway.

GRAYSON: And, you know, in the British system, you get to a certain point in your education and you're given an exam that divides you into, as [Raymond E.] March said, divides you into the washed and the unwashed.[1]

STANDING: There's nothing like that in Canada, in Winnipeg, at least.

GRAYSON: So that educational—

STANDING: Well, there was certainly some segregation of students, and I was in the top class in high school.

GRAYSON: So they did recognize academic performance and try to enhance that by ensuring that you got a more rigorous education?

STANDING: Yes, that's right, and particularly since I was chosen to write for a scholarship. We had a special program in geometry that one gentleman gave which was very useful, actually.

[1] Raymond E. March, interview by Michael A. Grayson at Peterborough, Ontario, Canada, 27 October 2014 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0921), 6-7.

GRAYSON: So this was geometry. Why was geometry chosen for this?

STANDING: Well, because I had taken geometry in grade ten, and this was the end of grade eleven, so it was thought advisable to brush up people on geometry. And this was quite useful, actually.

GRAYSON: Interesting.

STANDING: I ended up getting a hundred [percent]. [laughter]

GRAYSON: You knew your geometry pretty well.

STANDING: That's right. That's right.

GRAYSON: Well, that's a very good course for logic, you know, logical thinking.

STANDING: Yes.

GRAYSON: And it's kind of better . . . different than algebra, for sure, and [trigonometry], but it is really—

STANDING: [Yes]. Well, I took algebra and trig as well.

GRAYSON: This was in high school?

STANDING: In high school. I still have the books.

GRAYSON: Well, some things don't change a lot. **<T: 10 min>**

STANDING: That's right.

GRAYSON: So, you wrote for the scholarship, which I assume you ended up winning.

STANDING: Sorry?

GRAYSON: You wrote for the scholarship, that you—

STANDING: Yes, for my so-called junior matriculation, at the end of grade eleven.

GRAYSON: And you won the scholarship?

STANDING: There were four senior scholarships available in Winnipeg, and four junior ones.

GRAYSON: I see.

STANDING: And I got one of the senior ones.

GRAYSON: And so what did that give you?

STANDING: That gave me a certain amount of money. It's laughable in present standards. I think it was one hundred and fifty dollars.

GRAYSON: Oh, wow.

STANDING: And it was expected that I would use it in university.

GRAYSON: Pretty amazing. So this also probably made it easier for you to get into the university?

STANDING: [Yes], although there was no problem then. [. . .] Anybody could get in. Whether you could pass or not was another question.

GRAYSON: So, this university would have been University of Manitoba?

STANDING: University of Manitoba. [Yes].

GRAYSON: [. . .] You said there was another university in town?

STANDING: [Yes]. There was originally a college of the University of Manitoba, and now it's a separate university, University of Winnipeg.

GRAYSON: So you did have a choice between those two schools when you graduated?

STANDING: I could have taken classes at the college. [Yes]. But I chose to take classes at the university proper.

GRAYSON: And [. . .] that would have been in [. . .] 1943?

STANDING: Let's see. I've written this down. That was 1943 to [1944]. I took what was then second year, and now is first year, University of Manitoba.

GRAYSON: And how large was the university at that time? Was it—

STANDING: How large was it? Oh, a couple thousand students, I think.

GRAYSON: And, I guess since—

STANDING: A few thousand.

GRAYSON: —since Winnipeg is the provincial capital, I guess that made it a town that would have more than one college, or, you know, school.

STANDING: Well, [yes], [. . .] originally, it was just one university, and the University of Winnipeg was then a college, and when we came in here, we passed various colleges that are still connected to the university.

GRAYSON: So, you had a regular four-year university—

STANDING: Well, no. I took one year to start with, and I was a member of the University Naval Training Division. UNTD, at that time.

GRAYSON: And [World War II] was going on.

STANDING: The war was going on [. . .], and so I went on active service at the end of that year.

GRAYSON: Which took you to where?

STANDING: Pardon?

GRAYSON: Where did you serve?

STANDING: I served in Halifax, [Nova Scotia] and Shelburne, [Ontario]. [I] never saw action.

GRAYSON: [. . .] So, what did you do there at Halifax and Shelburne?

STANDING: I was a stoker first-class and I cleaned boilers.

GRAYSON: Oh, boy.

STANDING: You climb into the boiler and you have a steel brush, and you brush off the **<T: 15 min>** rust, etc., that's in the boiler.

GRAYSON: So you want to get to—that would be an insulating coating, and you want to get that out of there so—

STANDING: That's right. That's right.

GRAYSON: So, that was your whole contribution to the war, was—

STANDING: I was originally cleaning boilers in Halifax. Then I was sent to Shelburne and again put on cleaning boilers. And finally, though, at Shelburne, I was sent out to a water pumping station that pumped the water to the base, and that was pleasant. There was a sign at the end of the little jetty that said, "Absolutely no swimming," and next to it was a diving board. [laughter] It was at a little lake called Rodney Lake, [. . .] which is still in existence, and it is now the source of the town of Shelburne's water supply, I believe. [. . .] I visited it much, much later, a few years ago.

GRAYSON: So, Halifax is in what province?

STANDING: In Nova Scotia.

GRAYSON: Nova Scotia. Okay. So that moved you to the East Coast, right?

STANDING: East Coast. [Yes]. I was moved first of all to Halifax, [. . .] which was the place where the convoys were assembled. Big harbor.

GRAYSON: So, were these convoys protecting shipping?

STANDING: [Yes]. They were convoys that were protected against the German submarines.

GRAYSON: There was an interesting article in a newspaper, or I heard it somewhere, that they just discovered a German submarine a couple hundred miles off the American coast in—

STANDING: Could be.

GRAYSON: —six hundred feet of water.

STANDING: [. . .] Well, there was, at the time of the surrender, which was in 1945, when I was still in Shelburne, there was a submarine that came in to Shelburne, and surrendered there.

GRAYSON: That's interesting. So that would have been a pretty dramatic event.

STANDING: Pardon?

GRAYSON: That would have been a very dramatic event, to have this German sub—

STANDING: It was fairly dramatic, although, of course, not as dramatic as it would have been a year or two earlier. It was after the cessation of hostilities in Europe.

GRAYSON: So, did you do anything else besides—what did you do at the water station? You just managed the water pumping?

STANDING: Well, managed the pumping. There were a number of us that managed the pumping there, I think four of us, and we had a little shack there that we lived in, and we were in charge of the pumping system.

GRAYSON: Was there some fear [. . .] that the pumping system could be damaged by . . .

STANDING: Enemy action?

GRAYSON: [Yes].

STANDING: Well, not that I was aware of. [laughter]

GRAYSON: [. . .] They wanted to make sure that there was potable water.

STANDING: [Yes]. There was a big tank halfway along between us and the base. We were several miles from the base. And there was this big tank, which was a storage tank that was up on stilts, and we were responsible for keeping that full, more or less. And we got food. We did our own cooking.

GRAYSON: So, you were essentially—

STANDING: At arm's length from the base.

GRAYSON: And everything else, and you stayed there?

STANDING: [Yes]. We stayed there.

GRAYSON: The group stayed.

STANDING: [Yes]. **<T: 20 min>** It was quite pleasant, actually. [laughter]

GRAYSON: And so once the war is over, then did that end your—

STANDING: Then I returned to Winnipeg, and very fortunately it turned out that I went on active service in '44, and I got out in '45 in time to go back to university, so I only missed one year.

GRAYSON: Oh, excellent. So, was it difficult to start back into an academic environment after kind of being—

STANDING: That's right. So, I returned to University of Manitoba, and took honors math and physics, which was, well, the highest-ranking physics that you could take at that time.

GRAYSON: So that was more of a challenge than just taking the normal math and physics?

STANDING: That was a challenge. [Yes]. At that time, it was a combination. You took [. . .] some honors courses, and you took the general program as well.

GRAYSON: So you had to work hard there? Harder than—

STANDING: Work hard, [yes]. Work hard. And at the end of it, I worked hard enough that I was accepted at Princeton [University].

GRAYSON: So, I'd like to back up a little bit. Obviously you had an interest in math and science.

STANDING: That's right.

GRAYSON: And when did [. . .] you realize that that was of interest to you? Was it like—

STANDING: Well, I guess [. . .] the idea is that you developed a tremendous interest in science early on, but I think I dropped courses until all I had left was math and physics.

GRAYSON: [laughter] So it was just that you had an inclination [. . .] towards the sciences?

STANDING: [Yes]. I certainly had an inclination towards science, but no tremendous impetus as to . . . . I guess, I had a Meccano set, which is the equivalent of . . . Erector, is it?

GRAYSON: [Yes]. Erector set.

STANDING: And [I] did that at home and I did, oh, the usual sort of thing, shorted out the electrical supply, and so on.

GRAYSON: Did you do any chemical experiments in that period?

STANDING: Not really. Not really. As part of this program in honors math and physics I took some chemistry. I took a couple of courses in chemistry, ending up with a course in organic chemistry. I still have the book up there.

GRAYSON: That was—

STANDING: [James Bryant] Conant.[2] So, I did take some chemistry but mainly math and physics.

GRAYSON: So, basically, as your own interest evolved into math and science, it wasn't some exterior mentor or some other person?

STANDING: Nothing very dramatic.

GRAYSON: Just an evolution?

STANDING: [Yes]. Evolution. I took math and physics, and then I remember the professor of mathematics, when I asked him for a reference, and said I was going to Princeton—or hoping to go to Princeton—he asked whether I was going in math or in physics. And I said, "In physics."

GRAYSON: He didn't care for that? He wanted you to go for math?

STANDING: Well, Princeton was pretty well endowed in both math and physics, and I guess I went to Princeton because my old friend **<T: 25 min>** Doug Milton, who had gone to school with me from grade three on, had gone to Princeton a year ahead of me.

GRAYSON: So he didn't have the year out for service?

STANDING: He didn't have the year off for service.

GRAYSON: So your grades at University of Manitoba were obviously pretty good.

STANDING: Were good enough.

GRAYSON: So, what did it cost to go to the university then? How much—

[2] James Bryant Conant, *The Chemistry of Organic Compounds*. New York: Macmillan, 1939.

STANDING: Well, I was fortunate that after my active service there was something set up by the Canadian government called the DVA, Department of Veterans Affairs, and they paid me so much a month and also paid my fees all the time I was at Manitoba and for a few years at Princeton.

GRAYSON: So this was a pretty sweet deal for your one year of service.

STANDING: That's right.

GRAYSON: [. . .] So they essentially paid for your college expenses and gave you a stipend to live on?

STANDING: And still they pay for . . . well, they give me a pension. Which is, oh, a couple hundred dollars a month, I think.

GRAYSON: Well, that's—

STANDING: Something like that now, on the assumption that my loss of hearing was due to something that happened in my—

GRAYSON: During the war?

STANDING: During the war. [laughter]. Which is really stretching it, of course, but anyway, they paid for the hearing aids, and they pay me a pension.

GRAYSON: And they paid—

STANDING: So it was a sweet deal, as you say.

GRAYSON: They paid for your education and gave you something to live on.

STANDING: That's right.

GRAYSON: So that worked well for your year of—

STANDING: It was very worthwhile.

GRAYSON: —cleaning boilers and [managing] the water supply.

STANDING: [Yes]. It paid for cleaning boilers.

GRAYSON: Well, you know, you did serve. I mean, you were enrolled in the Naval Corps before they actually called you for service.

STANDING: I was involved in the University Naval Training Division. [Yes].

GRAYSON: So, why did you . . . was that a choice that you made?

STANDING: It was a choice that I made. [Yes]. A lot of my friends were in the forces, and I guess I felt that I should contribute as well.

GRAYSON: So, they were in the hotter part of the war, you can say, and—

STANDING: [Yes]. I could have stayed at university.

GRAYSON: Sure. Well, I mean, your buddy Milton did, right?

STANDING: But I decided to enlist, go on active service.

GRAYSON: Very good. And so we never really got to how much it cost to attend University of Manitoba back then. It was—

STANDING: Well, it cost . . . I've forgotten. A very small amount. It cost one hundred and fifty dollars a year or something for fees.

GRAYSON: Oh, wow.

STANDING: And I was living at home. So I had lots of money.

GRAYSON: You were in good shape.

STANDING: I was in good shape.

GRAYSON: So you could save up, and you decided you want to go to graduate school. Was that your own personal decision, or was that something that came about through mentors, or through—

STANDING: That was my own personal decision, and I went to Princeton because this old friend of mine had gone the year before.

GRAYSON: And did you apply to any other schools?

STANDING: I think it ended up that I was only applying to Princeton, [. . .] which was in retrospect very foolish.

GRAYSON: Well, you know, at that—

STANDING: Since even then, something less than 10 percent were accepted.

GRAYSON: So you must have had pretty good credentials coming out of the university.

STANDING: Well, I <**T: 30 min**> think very largely it was because of Doug, who had gone there a year ahead, and he'd been successful, I think. And so I then followed in his footsteps.

GRAYSON: So, I'm kind of curious. Since it's an American university, I wonder if they had a program for accepting—particularly accepting students from outside of the United States? Because as a Canadian citizen . . . you are a Canadian citizen—

STANDING: Yes. They were very broad-minded.

GRAYSON: So, when did you land at Princeton?

STANDING: I landed in Princeton in the end of summer '48, I guess. No, wait a minute. [Yes], '48.

GRAYSON: And how did you find it—

STANDING: I've written these things down.

GRAYSON: Sure.

STANDING: Because I find it hard to remember.

GRAYSON: How did you find the campus then? This would have been immediately postwar.

STANDING: Well, I was offered a job for the summer, so I went down for the first summer before classes started, actually, and worked for a guy by the name of Rubby Sherr, [. . .] who was involved in the nuclear physics program there. They had a cyclotron, and Rubby was one of the active people in it. And the first summer I did . . . . Let's see. What did I do?

GRAYSON: It wasn't cleaning boilers.

STANDING: I worked on a possible use of radioactive uranium isotopes for scintillation counting. It was unsuccessful. I think at that time the summer options were just for ideas that somebody had, you know, to throw it out. So I worked for the summer for Rubby trying to develop this, which I think impressed him, because I could read French. From the Canadian background.

GRAYSON: So, now let's talk about that French business. So, Winnipeg I guess was primarily French-speaking, or—

STANDING: No. There's an area of Winnipeg that's primarily French-speaking.

GRAYSON: I see.

STANDING: Saint Boniface [. . .], but it was normal to take courses in French all the way through high school, and in fact I took French in second year, which would have been first year of university.

GRAYSON: So you were facile in the language.

STANDING: I was . . . well, I could read it. I wasn't really good at speaking.

GRAYSON: Well, that's an advantage, though, being able to read the—you know, particularly when you come into a scientific career, read a foreign language.

STANDING: Yes, yes. So, anyway, that I think impressed Rubby, because I had to read up about these uranium isotopes that most of the work had been done in France on them. **<T: 35 min>** So, I worked that summer for Rubby, and I worked the next summer for [Milton G.] White, who was head of the cyclotron lab, and that was on a fast-cycling cloud chamber.

GRAYSON: And what kind of cloud chamber?

STANDING: Fast-cycling. [. . .] Didn't work. [laughter]

GRAYSON: So, your scintillation experiment didn't work.

STANDING: That's right.

GRAYSON: And your fast-cycling cloud chamber didn't work.

STANDING: That's right. [Yes].

GRAYSON: You're doing pretty well.

STANDING: And then, at that time, and I think still, you had to write a so-called general exam, which was all of physics. They could ask you anything they wanted. [. . .] I wrote that in 1950 and passed it. [. . .]

GRAYSON: Was that a day-long affair, or multi-day, or—

STANDING: [. . .] It was multi-day.

GRAYSON: Multi-day?

STANDING: Several days. I can't remember exactly how many days, three days or something like that.

GRAYSON: So it was a fairly—

STANDING: Fairly rigorous exam.

GRAYSON: Pretty rigorous. Okay.

STANDING: I don't think I did very well with it, but I passed it, anyway.

GRAYSON: Probably it showed—

STANDING: And at that time, around that time, there was a big fire in the Princeton cyclotron.

GRAYSON: Oh, boy.

STANDING: It went up in flames.

GRAYSON: Oh, no.

STANDING: Because [. . .] it was cooled by oil tanks. The oils were cooled. And a friend of mine was one of the last people to use it. Anyway, we called him Burner Peelle. [laughter]

GRAYSON: Burner Peelle?

STANDING: Anyway, it was effectively destroyed, apart from the magnet, and so they had to rebuild it, which they did.

GRAYSON: So, I want to ask a little bit about the uranium isotope experiment. Was there at that time enough information to understand the dangers of the radioactivity from working with uranium?

STANDING: I don't think we ever worried about that.

GRAYSON: Oh, really?

STANDING: [Yes]. A few years later [. . .] they pricked your finger and tested your blood once a month or something or other, and I was tagged as somebody who was at risk. And so they sent me in to the outfit in New York, the Cancer Institute. And one thing I remember is carrying in a big jar which was a twenty-four-hour output of urine. Anyway, they couldn't find anything wrong with me. [laughter]

GRAYSON: That's pretty good. But there was some concern after the fact?

STANDING: There was some concern after the fact. They had instituted this monthly blood test, so there was some concern, but those of us who were presumably exposed didn't worry too much about it.

GRAYSON: Well, [. . .] there's probably some fairly hot radiation coming off of uranium—

STANDING: There was lots of radiation in the cyclotron, and thinking of it now, I mean, I changed the filament on the ion source and so on, and it was probably pretty hot. But no . . .

GRAYSON: No ill effects?

STANDING: No ill effects, as far as I could tell. [Yes]. **<T: 40 min>**

GRAYSON: So, tell me a little bit about this fast-cycling cloud chamber. What was the idea behind that?

STANDING: That was a . . . you know, most cloud chambers are sort of like this. And [. . .] a fast-cycling cloud chamber was a very narrow . . .

GRAYSON: Volume?

STANDING: Volume that had an electrode at the bottom that went up and down like this. And this was to produce the expansion which you need in cloud chambers. As I said, it didn't work, all right? I was never really able to make it work, let's put it that way.

GRAYSON: So this would be to improve the amount of data that you could get from observing collisions?

STANDING: Yes. That's right. The idea was that the beam from the cyclotron or cosmic rays would come in along the axis and you have an expansion, and then you could see the track. That was the idea.

GRAYSON: It didn't work, though. Okay.

STANDING: It didn't work.

GRAYSON: So, what did you do your first year there, after your first—

STANDING: Well, after I did the general exams in 1950 that was sort of the license to do a thesis. And at that time, Rubby, I think because the cyclotron was being rebuilt, had gone to Caltech [California Institute of Technology] for the year. I think it was Caltech. And so I didn't have . . . I was more or less on my own.

GRAYSON: So, I guess I was curious how many cyclotrons were around in that time period. There probably weren't too many.

STANDING: Oh, there were sort of half a dozen in the US. Very few.

GRAYSON: All right. And they were basically I guess, with the war just ending, more and more schools wanted to get into the—

STANDING: [Yes], I think the Princeton cyclotron was used during the war to do various experiments. Anyway, when it burned they decided to rebuild it and it was rebuilt, but in that time, Rubby Sherr, who was my advisor, went away to Caltech. So, he said much later, in fact, at his one hundredth birthday party last year—he didn't survive to one hundred. He died in between ninety-nine and one hundred. But anyway, he had a birthday party, and he said at that time that I was the most innovative graduate student he'd had.

GRAYSON: And why did he characterize you that way?

STANDING: Well, I think because he went off to Caltech, and I still did a thesis, or two theses, when he was gone. So I—

GRAYSON: So you had to come up with your own research plan?

STANDING: I had to come up with my own research plan. And the first one I came up with was double beta decay with an emission of zero neutrinos. That is, the neutrino is emitted in the first one and absorbed in the second decay.

GRAYSON: Oh, my. That's a fairly—

STANDING: In other words, it is a Majorana particle. [. . .] <**T: 45 min**> [Ettore] Majorana was an Italian physicist who had hypothesized this process.

GRAYSON: So this was a theoretical construct that you—

STANDING: Well, this was to try to find these double beta decays. There had been a guy named [Edward L.] Fireman at Princeton [. . .] who had set [. . .] a limit on the lifetime of this process, which was something like 10^{17} years, [. . .] and he had detected a certain number of particles in a counter experiment and later on in a cloud chamber. And the idea was that I would measure this process, and it got far enough along that I had the iron for a magnet that I was using was cast, but not the . . .

GRAYSON: Pole faces?

STANDING: Not the coils or the vacuum chamber. And at that time, it was scuppered by, in that case by Milt White, who was sort of in charge of me, since Rubby was away. And the idea was that there had been other experiments done that set higher limits, and it wasn't clear that my technique was going to be able to measure it. So, that was the first thesis. That was double beta decay.

GRAYSON: That was kind of esoteric, but you had a theoretical construct that permitted you to devise an experiment.

STANDING: [. . .] I was trying to do the experiment to detect this process.

GRAYSON: So, what was your source of beta?

STANDING: What?

GRAYSON: What was the source of your beta ray, beta particle?

STANDING: Well, I was using a magnet that was cylindrically symmetric, but the idea was that the particles would follow so-called trochoidal orbits, [. . .] which had been studied, again in

France, by a guy named [Jean] Thibaud [. . .] and he published a paper in it back in 1934.[3] And the nice thing about it was that this was in the fringing field of the magnet, so the positrons would go one way, and ordinary electrons would go the other way. And so, one of the processes that was thought to produce background for this was internal pair production, a positron and a negatron. And so this was able to distinguish clearly between them. But it didn't have high enough efficiency, according to—I think it was correct, because the problem still isn't completely satisfied. [. . .] This is like '48, '50, this is sixty years on. **<T: 50 min>** So it turned out to be a difficult problem, so it was a good idea to scupper it. So, then I went on to do an experiment on the cyclotron that was working by that time.

GRAYSON: So, I assume that when they rebuilt the cyclotron, it was up to the highest standards, and the latest and greatest that you could have at the time, technology-wise.

STANDING: It worked better than it ever had before, and it was a 50 MeV [megaelectron volt] cyclotron.

GRAYSON: So this puts you in a different category with regard to cyclotrons around the country?

STANDING: [Yes]. It was competitive, I'd say, at that time. [Yes]. Anyway, I started on a second thesis, this time coupled to the cyclotron pretty directly on p-d [proton-deuteron] reactions in [. . .] nitrogen-14 and various other things. But my thesis was on nitrogen-14.

GRAYSON: And there was a choice? Why did you choose nitrogen-14?

STANDING: Well, there were some difficulties with previous measurements on nitrogen-14. Let's see . . . [D. Allan] Bromley, who went on to be the Presidential advisor, I think, had done an experiment at [the University of] Rochester on carbon-13 DN, I think, nitrogen-14.[4] And there were some contradictions between his experiment and another experiment. I can't remember what it was. And the difficulty in doing p-d reactions was that they had an appreciable negative Q. In other words, you put in relatively high energy protons, and you got deuterons out at much lower energy, so that made it difficult, because there were lots of background down at lower energy.

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[3] Jean Thibaud. "Positive electrons: focusing of beams, measurement of charge-to-mass ratio, study of absorption and conversion into light." *Physical Review* 45, no. 11 (1934): 781.

[4] D. Allan Bromley was on the White House Science Council under President Ronald Reagan and served as President George H.W. Bush's Science Advisor from 1989 to 1993. D A. Bromley and L.M. Goldman. *The Parties of the Ground States of N-14 and C-14*. No. NYO-3209. Rochester Univ., 1952.

So the p-d reaction was more difficult than the d, p reaction because of this Q value. So, anyway, it was one of the first p-d reactions that we had done, and it confirmed Bromley's measurement on the parity, so-called, of nitrogen-14. And I was able to fit the so-called [. . .] reduced widths, which were the measurement of the probability to calculations by Tony [Anthony Millner] Lane, I think. [. . .]

GRAYSON: So, that was your second—

STANDING: That was my second thesis, and that was successful. I made the measurements, but I took a job as assistant professor back here at the University of Manitoba. It's hard to feel **<T: 55 min>** the atmosphere at that time. Now, of course, it's very competitive, and when you advertise a job, you get all kinds of applications. At that stage, it was mainly, "[Yes], he's warm, we'll hire him." [laughter]

So they were, I think, overjoyed at the prospect of getting me onto staff, and I'd taken this job, but unfortunately, I took the job expecting to have the thesis all written up and it wasn't. I had finished the experiment, but I hadn't written the thesis up. So, I went off to Manitoba at that stage [. . .] as a staff member.

GRAYSON: On the faculty?

STANDING: Assistant professor, I think.

GRAYSON: In 1956?

STANDING: Fifty-three, I think.

GRAYSON: Fifty-three?

STANDING: [Yes], '53. And that was a big mistake, because I hadn't finished writing the thesis up. But I worked very diligently for the next year or two, and finally got it written up by 1955, by which time, Rubby had got back to [. . .] Princeton.

GRAYSON: Oh, so he spent several years away?

STANDING: He spent, I think, just maybe a year. Maybe he got back at the end of ’53 when I left, but he was back in time to read the thesis. And I—

GRAYSON: But you hadn’t written up the . . . . When did you write it up?

STANDING: What?

GRAYSON: When did you write up the thesis?

STANDING: I wrote up the experiment that I had done.

GRAYSON: But you hadn’t submitted a dissertation?

STANDING: What?

GRAYSON: You hadn’t submitted a dissertation?

STANDING: [Yes]. I submitted the dissertation on the experiment that I’d done, but of course you had to write that up, and I think it’s still here.[5] It’s one of these books there. Anyway, by that time, Rubby had got back, and he was able to read my thesis. He at that stage was quite ignorant of the process. “What [is] a p-d reaction?” But anyway, he read the thesis, and I went and took my final oral exam in ’55 and was granted the PhD degree from Princeton.

GRAYSON: But you had already started at Manitoba?

STANDING: I’d already started. I had been two years teaching at Manitoba. [Yes].

GRAYSON: So, finally, by the end of ’55, you’re official?

[5] Kenneth G. Standing, “The Angular Distribution of Deuterons from Nitrogen-14 (Proton, Deuteron) Nitrogen-13 and Lithium-7 (Proton, Deuteron) Lithium-6.” PhD thesis, Princeton University, 1955.

STANDING: I was . . . well, I was officially an assistant professor before that, but it was sort of understood that I would get a PhD, and I did in ’55, two years later.

GRAYSON: So, is there any reason why you decided to just come back to Manitoba without having completed your—

STANDING: Well, I guess the reason was basically a personal one. I was tied up with a girl in Winnipeg.

GRAYSON: Now the truth comes out.

STANDING: Romantic.

GRAYSON: So you wanted to get back to your girlfriend.

STANDING: Well, that’s right.

GRAYSON: Very good. [. . .] The university here at Manitoba was willing to go **<T: 60 min>** along with the idea that eventually you would get your official—

STANDING: [Yes]. They were hard up. [laughter] They didn’t have much choice in the matter. But of course, if it had gone on for years and years they would have been kind of disturbed, I think.

GRAYSON: Yes. Well, a couple of years isn’t too bad, but it’s better to get it all done.

STANDING: That’s right.

GRAYSON: So I assume, what, when you came back to Winnipeg, you got married—

STANDING: Well, I started an experiment to do . . . . I had an excellent student named [Jovan] Jovanovich. [. . .]

GRAYSON: So you were at a research program?

STANDING: [Yes], I started a research program at that time, as well as teaching, which was a very heavy teaching load that time which was—

GRAYSON: [Yes], I was kind of curious, as how—

STANDING: —normal at that time.

GRAYSON: How many contact hours did you have back then?

STANDING: Oh, I guess I was teaching three or four courses.

GRAYSON: Oh, wow.

STANDING: I'm not quite sure how many contact hours that amounted to, but—

GRAYSON: Probably ten to twelve.

STANDING: —two lectures per week in each of these courses, I think.

GRAYSON: This would have been like introductory physics and then maybe some higher-level courses?

STANDING: It was introductory physics was one, and I had a graduate course in nuclear physics, and a course in, I think, [electricity and magnetism] or something like that.

GRAYSON: So that would have kept you pretty busy in class preparation.

STANDING: [Yes], it kept me busy. But with Jovan, I [. . .] started an experiment on gamma-ray scattering, where we used a multi-curie source, a cobalt-60 source, that was up at the health sciences lab. They used it normally for radiations.

GRAYSON: Cobalt-60 is kind of hot, isn't it?

STANDING: [Yes], it's 1.33 MeV is—and then there's a smaller gamma ray as well. Anyway, we measured [gamma ray] scattering, which was a hard experiment.

GRAYSON: For gamma rays?

STANDING: [Yes], for gamma rays.

GRAYSON: Wow. Well, they're probably—

STANDING: We were looking for so-called Delbrück scattering. [. . .] Named after a man [Max Delbrück] who was then at Caltech. And that was scattering from the electric field, essentially. But we set an upper limit on that, which was better than anybody else had done. And anyway, we did this gamma-ray scattering experiment.

GRAYSON: And the gamma ray—the cobalt-60 source was located in—

STANDING: Up in the Health Sciences lab. One of the peculiar things about the University of Manitoba is that there are two campuses. There's this [Fort Garry] campus, which is the main campus, and there's the Health Sciences [Bannatyne Campus] that's up by one of the hospitals, about twenty minutes away by car. So, anyway, this was up **<T: 65 min>** in a shielded room at the Health Sciences campus, and they used it normally during the daytime. They used it for irradiating tumors. We had it overnight.

GRAYSON: Oh, wow.

STANDING: So that was some difficulty.

GRAYSON: Yes. Because basically, you would have an experiment to set up and run—

STANDING: We had it on a cart and we moved it out in front of the beam, and we were outside the shielding wall. There was a shielding wall. And we were outside the shielding wall and pressed the button to start the . . . to open the shutter, and start the beam going for . . . .Our targets were fairly large things, about this big.

GRAYSON: Like maybe six to eight inches on the side?

STANDING: Pardon?

GRAYSON: Six to eight inches on a side, the target?

STANDING: [Yes], six inches, six by six maybe, something like that. And we did basically on lead.

GRAYSON: Lead.

STANDING: So, that was a successful experiment. We published first of all in *Nature*, and then in the *Canadian Journal of Physics*.[6]

GRAYSON: So, usually getting in *Nature* is considered to be a feather in your cap. They don't accept just anybody's work.

STANDING: [Yes]. That's right. And [. . .] the theorist, [Gerald E.] Brown, who gave a talk saying how great the experiment was.

GRAYSON: So, this helped the theoreticians in their development of the theory?

STANDING: [Yes]. A number of people had done experiments before that I guess you might say weren't very good.

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[6] K.G. Standing and J.V. Jovanovich. "Coherent scattering of 1.33 MeV. gamma-rays by lead." *Nature* 182 (1958): 521-523; K.G. Standing and J.V. Jovanovich. "The elastic scattering of Co60 γ-rays." *Canadian Journal of Physics* 40, no. 5 (1962): 622-653.

GRAYSON: And you had managed to do the best measurement?

STANDING: We did it better. [Yes].

GRAYSON: So, this gentleman Jovanovich, he was, like, your doctoral student?

STANDING: He got a master's degree—

GRAYSON: Master's. Okay.

STANDING: —from this, and then he went off to Washington University in St. Louis.

GRAYSON: I know they had a cyclotron there.

STANDING: Well, they had. I think it's now the . . .

GRAYSON: It's a mass spec [spectrometry] lab.

STANDING: —lab that . . . mass spec lab. [Yes].

GRAYSON: [Michael L.] Gross has. [Yes]. Where I used to work.

STANDING: [Yes].

GRAYSON: They still call it the cyclotron building though.

STANDING: It could be. It could be.

GRAYSON: But yes, it was—I think back in the fifties, having a cyclotron on campus was kind of a premier kind of thing for physics departments.

STANDING: It was a prestige item. [Yes]. Anyway, he went on and did various things at Washington U and then went on to Brookhaven [National Laboratory], etc., etc., okay. In the meantime, I went off to University of Bristol. I had a sabbatical, I think, coming up, and I went to University of Bristol in ’58,’59, and we—

GRAYSON: Where is that?

STANDING: What?

GRAYSON: Where is Bristol?

STANDING: Bristol’s in England.

GRAYSON: That’s what I was thinking. [. . .] You went abroad for—

STANDING: [Yes]. That’s right. So I went to . . . . And if you—

GRAYSON: This was for a year?

STANDING: If you looked at my CV, I had some sort of distinction at University of Bristol. I had a Nuffield Fellowship at University of Bristol. [. . .]

GRAYSON: Nuffield Fellowship. Very good. **<T: 70 min>**

STANDING: Lord Nuffield [William Morris] was the big . . . he made cars, Morris Minor and so on, but he’d established a certain fund that, among other things, supported me while I was at Bristol.

GRAYSON: Well, that’s good. I want to back up a second, though, just for a while, to Princeton. There’s a number of people who were in the mass spec game at Princeton, and I think they were all in physics department. Did you interact with any of those?

STANDING: No. By that time, they moved on. It was . . .

GRAYSON: The guy who did the ion source—

STANDING: Well, Rubby, the guy I worked for, had done his original experiment on what you might call mass spectrometry back in the thirties. And let's see. I think—

GRAYSON: I'm trying to think of this one fellow who [. . .] had an ion source. It was probably instrumental in the development of the electron impact ion source, but I can't think of his name off the top of my head right now [Walker Bleakney]. But—

STANDING: [. . .] Anyway, by the time that I arrived, it was during the war, of course. The fellow who had been mainly concerned with mass spectrometry was doing shock waves. [. . .] I think the other development at Princeton was a great big ring that was . . . . But again, that had died by the time I arrived. So there was no mass spectrometry at Princeton.

GRAYSON: I was just kind of curious, because—

STANDING: Now there had been in the thirties.

GRAYSON: I know one of the graduate students that got out of [University of] Minnesota went to Princeton, and I can see a picture of him in my mind but I can't think of his name.

STANDING: [John T.] Tate?

GRAYSON: No, Tate stayed at Minnesota. It was one of his students who measured the ionization levels of mercury in—he essentially created what is now the grandfather of electron impact ion source, and—

STANDING: Well, there was of course the very eminent physicist who worked in the Manhattan Project who was at Minnesota. [Alfred O.C.] Nier.[7]

[7] Alfred O.C. Nier, interview by Michael A. Grayson and Thomas Krick at the University of Minnesota, Minneapolis, Minnesota, 7-10 April 1989 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0112).

GRAYSON: Oh, [yes].

STANDING: Nier.

GRAYSON: No, this would have been . . . I think this guy got out of Minnesota just before Nier. Well, I'll figure it out and I'll just—

STANDING: All right. Well, anyway, I didn't have any contact with them, and I had contact with Nier much later on. I met him at a meeting. Anyway, coming back to Bristol, I was involved in one of the first experiments on gamma ray astronomy. And that was unsuccessful. [laughter]

GRAYSON: You have a pretty good record here.

STANDING: [Yes].

GRAYSON: So, the idea would be to detect gamma rays from—

STANDING: [Yes], that's right. We sent up a lead sphere that was about this big and a counter inside it and a moveable shutter that went around. And the results were sent down by telemetry.

GRAYSON: This was a balloon?

STANDING: So, we cooperated with the people from Aldermaston, [England]. The weapons lab [Atomic Weapons Establishment], who were anxious to do something useful.

GRAYSON: This was a balloon experiment, I guess?

STANDING: This was a balloon . . . **<T: 75 min>** well, most of the experiments that were being carried out then were balloon experiments. They sent big chunks of emulsion up by

balloon. The Bristol lab was famous because [C.F.] Powell had discovered the pi meson, I think.[8]

GRAYSON: Oh, okay. Pi meson?

STANDING: [Yes]. And anyway [. . .] the Bristol lab was famous for studying cosmic rays. But I started this offshoot, which was gamma ray astronomy, which wasn't successful when I was there, at least. It was somewhat successful later on, and very successful in people who did things by satellites.

GRAYSON: So do you think perhaps you needed to get to higher altitude in order for it to work?

STANDING: Well, I think the lab in Bristol wasn't very well set up to do this sort of experiment, really. So, that was interesting.

GRAYSON: So, you were hoping to find sources—

STANDING: Find gamma rays from the Crab Nebula, I think.

GRAYSON: And so from far away, not particularly close by.

STANDING: That's right.

GRAYSON: And you had a lead sphere with detector inside of it?

STANDING: It was a detector inside a lead sphere, and with a shutter going back and forth in front of the thing. And we didn't detect any appreciable difference.

GRAYSON: So you're looking for change in radiation as it changes position—

[8] C.F. Powell, G P.S. Occhialini, D.L. Livesey, and L.V. Chilton. "A new photographic emulsion for the detection of fast charged particles." *Journal of Scientific Instruments* 23, no. 5 (1946): 102.

STANDING: That's right. We were looking for if there'd been a—we were pointing . . . you could point this, because was on a balloon. And you could point this at the Crab Nebula, and you could then put the shutter in front and not in front of the aperture, and measure to see if there's any difference.

GRAYSON: Difference. Okay.

STANDING: And there wasn't any difference.

GRAYSON: Very good.

STANDING: And since then, gamma ray astronomy has got to be a big deal, but not then.

GRAYSON: So were you one of the first persons to try and do this gamma ray measurement?

STANDING: We were one of the first, anyway. [Yes].

GRAYSON: Very good.

STANDING: They started x-ray astronomy, and [. . .] it turned out quite successful. So, anyway, that was—

GRAYSON: So, you stayed there for a year.

STANDING: In '58, '59. [Yes]. I stayed there for a year. And at that stage, they decided to build a cyclotron at Manitoba.

GRAYSON: They wanted to get in the cyclotron game.

STANDING: That's right. Another cyclotron. And [they] brought me back to be in charge of building it, which I was foolish enough to do at the time. I could have got a job at Bristol, actually.

GRAYSON: Oh, really?

STANDING: [Yes].

GRAYSON: At the university there?

STANDING: [Yes]. But I chose to come back to Winnipeg to build this cyclotron. And—

GRAYSON: So, this sounds like it was a challenge.

STANDING: Well, it was a challenge. [. . .] The head of the department had been talked into the fact that [. . .] you could build a cyclotron fairly cheaply.

GRAYSON: Oh.

STANDING: And modeling after one at UCLA [University of California, Los Angeles].

GRAYSON: So, who was the talker that **<T: 80 min>** convinced him about this cheap cyclotron construction?

STANDING: Well, this was [B.G.] Whitmore, who was chairman of the department at the time. And he had gone down and visited the people at UCLA.

GRAYSON: He wanted one. [. . .]

STANDING: He wanted one, because [. . .] there had been a number of people who had been working with radioactive isotopes of one kind or another, and they had the idea that it would be useful to have a cyclotron to make isotopes, radioactive isotopes. [. .]

And the idea was that this cyclotron would be patterned after the one at UCLA, and in fact it was, except it used a—if you want to change the magnetic field in a cyclotron, generally

speaking, people used—these were sector field machines, so they had sectors, hills and valleys, and in the valleys people tended to put in coils to change them out to regulate the magnetic field.

And we did something unusual. We used Invar blocks that were controlled in temperature. So you went through the Curie point, where above the Curie point there was no magnetism. Below the Curie point, there was. So, this was the interesting, if you like, variation that we used on the UCLA machine. So, anyway, we built this, and it worked.

GRAYSON: It worked this time?

STANDING: [Yes], it worked.

GRAYSON: It sounds like it was not that easy to put together, or get it done, and you were in charge.

STANDING: It took some time to put together, and we had a conference finally after it started to work in '65. And one thing that was important for succeeding things was that Ron [Ronald D.] Macfarlane came.[9] He was then at McMaster [University], and he attended this conference that we had to commemorate the working of the cyclotron. So, he was a nuclear chemist at McMaster before he moved to Texas [A&M University]. And he attended this meeting, and I met him at that time. So that was important for succeeding measurements or succeeding developments.

GRAYSON: So it sounds like there might have been some cost overruns associated with building the cyclotron.

STANDING: Well, [yes], I guess it was cost overruns, but we kept being supported by the Atomic Energy Control Board [AECB] of Canada that had quite a lot of money at the time.

GRAYSON: So did the school put in any money, or was this money for the cyclotron all from the government?

STANDING: It was all from the government, I think. I think there was maybe some university money. There was—

[9] Ronald D. Macfarlane, interviewed by Michael A. Grayson at Texas A&M University, 26 May 2011 (Philadelphia: Science History Institute, Oral History Transcript #0877).

GRAYSON: So the university had to sell the idea to the government?

STANDING: That's right. That's right. So, the university managed to sell this idea, and the machine finally worked.

GRAYSON: And hopefully, it did what it was designed to do?

STANDING: It did work. Did work. And **<T: 85 min>** so that was '59 to, well, say '67, something like that.

GRAYSON: So, how long did it take for the construction of the cyclotron?

STANDING: Oh, it took about . . . let's see, started '59. We had the conference in '65. So'59 to '64, five years, something like that.

GRAYSON: And you were in charge of this whole process—

STANDING: I was in charge of building the machine.

GRAYSON: So that meant you had to deal with all the contractors or the—

STANDING: [Yes], that's right. We managed to hire two engineers that were helpful and looked after a lot of these things. But I was sort of in charge of the whole thing.

GRAYSON: So, did you have any other responsibilities at that time, teaching or anything? Did you—

STANDING: Not really. They released me from teaching on the grounds that I was needed to build the cyclotron. So that was so. And that went on, and . . . . Okay, so I went to University of Grenoble [. . .].

GRAYSON: When was that?

STANDING: In France.

GRAYSON: When? [. . .]

STANDING: In 1967, ’68.

GRAYSON: So you—

STANDING: Because they were building a cyclotron that was the sort of . . . I think it was a bit higher energy. I can’t remember.

GRAYSON: So, in order to get the higher energy, what, you used higher magnetic fields?

STANDING: Bigger machine.

GRAYSON: Bigger?

STANDING: [Yes].

GRAYSON: The radii?

STANDING: It was bigger. It was bigger. And the idea was that I should do experiments on the machine, but it never worked while I was there. [laughter]

GRAYSON: I see. So that was—

STANDING: It didn’t work until later.

GRAYSON: You had a lot of “didn’t work” experiences in your life.

STANDING: That's right. That's right. It didn't work.

GRAYSON: So, I guess that explains this period from like 1958 to the mid—actually, '62 or '67, when you didn't have any publications. You were involved so much with the cyclotron process.

STANDING: [Yes], I was involved with the cyclotron.

GRAYSON: So, there was no time to do research or to do any publications.

STANDING: [Yes]. Although I think there were some publications that don't appear in here, because this was mainly for [successful] things. There were some publications, but nothing very important.

GRAYSON: Because I looked at your publication record from SciFinder and there's this, you know, this period where there's just nothing happening, and I was kind of curious about—

STANDING: [Yes]. That's right. That's right. I was in charge of building the cyclotron.

GRAYSON: You were a manager.

STANDING: Yes.

GRAYSON: So to speak.

STANDING: That's right. Anyway, I went to Grenoble in '67, '68, and the machine didn't work while I was there.

GRAYSON: So what did you do?

STANDING: Well, I did various things. We lived up in a peripheral town called Seyssins, [France], and I ran off a cliff and completely clobbered the car that we had but fortunately, not me.

GRAYSON: Oh, wow.

STANDING: I was okay. It was up in the mountains, you see, these small mountains, but big enough that it could clobber the car.

GRAYSON: So, this was obviously an accident of some kind. It was—

STANDING: Pardon?

GRAYSON: It was an accident. You were driving too fast for the road, or you missed your turn, or—

STANDING: I was driving . . . well, I'd lent the car to somebody, and he'd complained that the accelerator and the brake were too close together. And I think this was the problem. I hit the accelerator when I should have hit the brake. Anyway, it rolled over three times, and finally ended up next to a tree, but fortunately, didn't burn or anything. And I was okay.

So, anyway, to deal with physics, I didn't do very much at that time. Well, I tried to keep my hand in. They had a staff to build the cyclotron so there wasn't any opportunity to contribute to the design. And as I say, it didn't work while I was there, so—

GRAYSON: That's unfortunate.

STANDING: It didn't work till later on.

GRAYSON: Now these trips—by this time, you were married, I assume.

STANDING: Pardon?

GRAYSON: You were married by this time. You'd gotten married?

STANDING: I married and had three children by that time, so we rented a house up in this place called Seyssins, this outlying town.

GRAYSON: So you had your family there?

STANDING: I had the family there.

GRAYSON: Well, that's good.

STANDING: [. . .] And then there was a strike in France. *Grêve*.

GRAYSON: What's the name of that? [What do] they call it?

STANDING: *Grêve* is a French word for strike. I think there's a circumflex in there somewhere. And so that shut down the lab and shut us down pretty well. So I put the family on a plane in Geneva, [Switzerland] to travel to England, and I drove the car from Grenoble to Cherbourg, [France] and caught the last ferry from Cherbourg to England during the strike. And the strike didn't end for some time.

GRAYSON: Really? [. . .] So you ended up in England. What did you do then? You just—

STANDING: [. . .] This was, oh, a month or two before the term started back here, and I did a little bit of work on my gamma-ray scattering. That was still going on, okay. Then I came back as director of the cyclotron, in '68.

GRAYSON: Can't get away from that cyclotron.

STANDING: What?

GRAYSON: Can't get away from that cyclotron.

STANDING: That's right. That's right. I was still a nuclear physicist. I hadn't yet learned the joys of mass spectrometry. So, I was director of the cyclotron, and that involved me and . . . if people came in with a problem I was sort of expected to look after it. And among other things it aroused my interest in biology, first of all.

GRAYSON: Because of the people coming in to—

STANDING: Well, people came in . . . . First of all, there was an experiment producing nitrogen-13 for tracers, and then—

GRAYSON: This would be tracers in a metabolic—

STANDING: [Yes], tracers with Norm [Norman] Campbell in microbiology. And then a proposal for analysis of the Canadian grain crop for protein content.

GRAYSON: So this is a fairly highly specific problem.

STANDING: This was a specific problem that had **<T: 95 min>** been . . . . There was an outfit called the Grain Research [Laboratory] in Winnipeg that was doing this and doing it by the so-called Kjeldahl method. [. . .]

GRAYSON: Kjeldahl. Okay. Sure.

STANDING: [Johan Kjeldahl] was a Dane, I think.

GRAYSON: This is a chemical?

STANDING: And that involved treating . . . you put the sample into a test tube of some kind, a container, and you added acid and looked at the ammonia that was produced.[10] So this was very time-consuming, and I know I never saw this, but the Grain Research Lab evidently had a whole bank of these glass vessels that were analyzing for nitrogen.

[10] J. Kjeldahl, "A new method for the determination of nitrogen in organic matter." *Z. Anal. Chem* 22, no. 1 (1883): 366-382.

GRAYSON: Why were they interested in nitrogen?

STANDING: What?

GRAYSON: Why were they interested in nitrogen?

STANDING: Well, they were interested in nitrogen because that determines the protein content. And the protein content of grain is very important for Canada, because this determines the baking quality. So, the Canadian grain crop was sold largely on the basis of high protein content. [. . .] Anyway, they had these banks of Kjeldahl apparati down at the Grain Research Lab, and the search was on for a better way of doing this. And some people had used nuclear methods for doing it already.

For example, I think the most . . . the main thing that had been used was the nitrogen-14 end-to-end reaction giving nitrogen-15. And then you counted the positrons from nitrogen-15—that was a positron emitter—and you counted the positrons, the annihilation radiation from the nitrogen-15. But the trouble was that this was very non-specific. If you bombard almost anything with proteins or nitrogen or neutrons, you produce positrons. And if you tried to look at the annihilation radiation, so-called, the 500 kilovolt radiation, that two photons are given off when the positron decays, this was non-specific.

GRAYSON: And so it really . . . it gave you some measure, but it didn't really tell you what you wanted to know?

STANDING: That's right. That's right. [. . .] If there was impurity of some sort in the grain, it might detect that as well as the nitrogen, okay. So I had—going way back, before I arrived in Princeton, there had been some experiments that were done partly by Rubby Sherr, [H.R.] Muether, and White, that [. . .] had been looking at the radioactivity of carbon-10 and oxygen-14.[11]

GRAYSON: Carbon-10. Carbon-10. Carbon-10. That's—

STANDING: Never mind the carbon-10. The oxygen-14 had the favorable property that it had . . . nearly all the decays went to an excited state at 2.3 MeV. So, aha, this was finally specific

[11] R. Sherr, H.R. Muether, and M.G. White. "Radioactivity of C-10 and O-14."*Physical Review* 75, no. 2 (1949): 282.

for detection of nitrogen. But this was the reaction we were using was to simply p,n on nitrogen-14 which gave oxygen-14.

GRAYSON: Oh, I see. Okay.

STANDING: So the idea was you'd measure the oxygen-14, and that gave you a measure of the—or you measured this gamma ray, and that gave you a measure of the nitrogen content, which was normally interpreted as a protein content of the grain, okay. One problem was that the lifetime of oxygen-14 was only seventy-odd seconds.

GRAYSON: That's not too long.

STANDING: What?

GRAYSON: Not too long.

STANDING: Not too long. So what we did, by this time I had a postdoc who was interested in—oh, yes. I guess I was replaced as cyclotron director in '74, roughly, so I was free to [. . .] follow my own interests. And I had got interested in this, and I had a postdoc, Don Dohan, who was interested in it. And we developed an apparatus to do this using the O-14 activity. So we had a conveyor belt. We irradiated the sample in by the cyclotron, and we brought the sample then through the shielding wall on a conveyor belt, and put it in front of a pair of big scintillation counters and measured the activity. And the activity as measured then would tell you what the nitrogen content was of the grain.

GRAYSON: And that would be specific to the protein?

STANDING: That was—2.3 MeV is a rather definitive sort of energy. So, this was definitive. [. . .] And we worked on this, and the exciting thing was that Don Dohan, who had been working with me on it, had come to the end of his period, and I think he got a job with Argonne.

GRAYSON: Argonne <**T: 105 min**> National Laboratory? [. . .]

STANDING: Yes. And so I'd hired a guy by the name of Brian [T.] Chait.

GRAYSON: So that's where he came from.

STANDING: That's where he came from. [He] was then at [University of] Oxford and was interested in doing things that had a practical impact rather than just a nuclear physics impact.

GRAYSON: So Brian started out as a nuclear physicist?

STANDING: He was a nuclear physicist as well. He'd [. . .] come from South Africa to Oxford and done a thesis on nuclear physics at Oxford. So that's where Brian arose.

GRAYSON: Interesting. [. . .] He came here?

STANDING: What?

GRAYSON: He came to the University of Manitoba?

STANDING: [. . .] I was able to attract him to the University of Manitoba, because he was interested in doing things that had a practical import.

GRAYSON: Very good.

STANDING: And so he had accepted at job at [University of Manitoba] as a postdoc, okay. Well, things had gone on in the lab that analyzed for the Canadian grain crop—what do you call it again? Grain Research Lab. And the head of that, that I didn't have anything to do with, had been under a good deal of pressure to solve this problem, because he had this great bank of Kjeldahl apparati. So, he got up and made a speech to something or other that said, "We have the solution to this. It's measuring the infrared reflectance of the grain."

Now, we had various objections to that. We knew about the technique, but anyway, this was a solution that they had adopted at the Grain Research Lab, and of course they were the judge of these things, so they weren't able to tell how accurate it was. But anyway, our funding for this project dried up at that stage, and this is more or less when Brian was in transit, I think, or maybe shortly after he arrived. And so we got interested in biological problems, and, in particular, we'd realized that you didn't only want to know the—

GRAYSON: Nitrogen?

STANDING: —protein content of a grain or biological sample, but you wanted to know a number of other things. You wanted to know what the structure was, and so on.

GRAYSON: Right.

STANDING: And at that stage, I consulted Bob [Robert C.] Barber, who was a member of Harry [Henry E.] Duckworth's group in the department. And he said, "Why don't you go down and talk to Ron Macfarlane?"[12] So, as I mentioned a few minutes go, I had met Ron already. I knew him. And I phoned him up, and he was happy to . . . . By that time, he'd moved to Texas A&M with Dave [David] Torgerson. And so that was the idea, that we might build a plasma PDMS [plasma desorption mass spectrometry] machine. Okay. So we're getting closer to mass spectrometry. [laughter] Finally.

GRAYSON: Well, I mean, your career started out as a physicist, nuclear physicist.

STANDING: It started out as a nuclear physicist. [Yes].

GRAYSON: Nuclear physicist, and you're doing nuclear physics, and building cyclotrons and doing all this other stuff, but somehow you got sucked into this grain problem.

STANDING: That aroused my interest in biological questions, I guess. So, then I went down and visited Ron Macfarlane in Texas A&M, and he was very kind to us.

GRAYSON: What year would that be, when you went down to see Macfarlane?

STANDING: That's about late seventies. Late seventies. And we decided then to build a PDMS machine. And we managed to get about five thousand dollars from graduate studies to do this. By this time I wasn't in charge of the cyclotron anymore, so I lost that source of funding. But we were foolish enough to say we were going to build a PDMS machine.

[12] Ronald D. Macfarlane, Oral History Transcript #0877.

GRAYSON: With five thousand dollars?

STANDING: [Yes]. So, we started off, or at least Brian started off, and we started off designing the machine, which you could see if you're interested.

GRAYSON: [Yes]. I'd like to look at it.

STANDING: It's just a pipe.

GRAYSON: Oh, sure. Right.

STANDING: So we did that, intending to build a PDMS machine with fission fragments, and we got so far enough along to order a californium source. And by this time, Brian had . . . . Well, by this time, first of all, [Alfred] Benninghoven had done experiments where he'd measured the . . .

GRAYSON: He was doing surface—

STANDING: [Yes], he'd measured the mass spectra from peptides, relatively small things, and the question was whether we could do something similar, but at much higher energy.[13]

GRAYSON: Higher energy?

STANDING: Much higher mass. [. . .] So Brian looked up and found a way of making low-energy ions. Benninghoven hadn't been able to do very large molecules, because he'd been using mass spectrometers that were only capable of dealing with large masses.

GRAYSON: Large energy—large—

STANDING: What?

[13] Alfred Benninghoven and W.K. Sichtermann. "Detection, identification, and structural investigation of biologically important compounds by secondary ion mass spectrometry." *Analytical chemistry* 50, no. 8 (1978): 1180-1184.

GRAYSON: Large energies?

STANDING: Quadrupoles. He was using quadrupoles and they were limited in mass range. So, anyway, it was a gamble, but we decided it was a gamble worth taking. We got so far as to order a **<T: 115 min>** californium source, and in fact, deposited it up at Pinawa, [Canada], where Dave Torgerson was by that time.[14]

GRAYSON: [. . .] That was a place that . . . to keep it safe . . . a safe-keeping place?

STANDING: To keep it safe. To keep it safe. To keep the californium safe. We never did—

GRAYSON: Do that experiment?

STANDING: —get it back. Anyway, one advantage of that was that by that time, Dave Torgerson had gone to Pinawa, and so he was available to advise us, okay, so [. . .] we decided to go ahead and build the spectrometer using cesium ions.

GRAYSON: This would be a surface analysis kind of thing? [. . .]

STANDING: No, for protein analysis, basically. And with the hope of seeing large molecules as Macfarlane had seen by PDMS, but Benninghoven had not been able to see because of the limitations of his spectrometer. But we were using a time-of-flight machine similar to Ron Macfarlane's.[15] So we didn't have that drawback. So we started, and I guess the first [. . .] effort we really proposed was at a conference at Orsay, [France]. Well, it was actually at the Institut Curie in 1980. We'd started to build this machine in '79, and by 1980 . . . . This is the twenty-seventh conference in Seattle, [Washington] of ASMS [American Society for Mass Spectrometry].

GRAYSON: All right. So, it's kind of like—

STANDING: You can turn over—

[14] The Whiteshell Laboratories nuclear research facility, an operation run by Atomic Energy of Canada Limited, is located in Pinawa.
[15] Ronald D. Macfarlane, Oral History Transcript #0877.

GRAYSON: —it's kind of like a FAB [fast atom bombardment] experiment, isn't it, in a way?

STANDING: We'd managed to analyze potassium iodide.

GRAYSON: Well, you've got to start somewhere.

STANDING: [Yes]. Anyway, we went on and—

GRAYSON: Hang on. Let me put some information in here. [. . .] Which year was this ASMS—

STANDING: That was the—

GRAYSON: Twenty-seventh conference?

STANDING: Twenty-seventh conference in Seattle in '79.

GRAYSON: Let me just get the page number here, and I can—I have all these on—there's PDF files, so I can get that.

STANDING: [. . .] By that time, we'd progressed a bit beyond potassium iodide, and we were able to . . . I think at that stage, we reported a comparison between fission fragment desorption and cesium ion desorption. If not, we proposed this at Benninghoven's meeting, which was later that summer. **<T: 120 min>** Benninghoven had a meeting which was IFOS [Ion Formation from Organic Solids]. I . . . never published. [. . .] There was a whole series of IFOS meetings.

GRAYSON: IFOS meetings. Okay.

STANDING: Anyway, that was IFOS I that we submitted a paper, but [. . .] Benninghoven never got around to publishing it. IFOS II was that fall, I think, and was, in fact, published. And by that time, we had progressed a bit beyond . . . we progressed to organic materials.

GRAYSON: So you're beginning to stretch this technique a little bit, pushing it harder to heavier compounds.

STANDING: Well, I don't know that we pushed it harder. We looked at different samples.

GRAYSON: Just to see how it worked? [. . .]

STANDING: [Yes], to see if we could do large molecules.

GRAYSON: Well, would this be—

STANDING: So we finally arrived at mass spectrometry.

GRAYSON: Yeah. So, this is in [. . .] about 1980, where you turned out to be—

STANDING: This is '79 to '80. [Yes].

GRAYSON: [. . .] Very good. Well, why don't we take a break, then?

STANDING: All right.

GRAYSON: And maybe we can relax a bit and grab lunch. What do you say?

STANDING: [Yes]. Let's see. What's the time?

[END OF AUDIO, FILE 1.1]

GRAYSON: So, I think we're back on track here. And you had finally gotten to the point where mass spectrometry became part of your professional career.

STANDING: [laughter] Yes, that's right. Everything else has been a preliminary. And as I remarked, the remarkable thing is that our distinctions, if such are, arrived after I officially retired.

GRAYSON: So, what did you—let's see. We're probably in . . . what period here? 19—

STANDING: Nineteen seventy-nine to '80 was when we got started on mass spectrometry.

GRAYSON: And that's when Brian Chait kind of showed up on the scene.

STANDING: That's right.

GRAYSON: But you were using secondary ions—

STANDING: That's right.

GRAYSON: —to impinge a surface, to produce the primary ions.

STANDING: Well, we used primary ions to bombard the surface, and they produced secondary ions which we looked at. They were characteristic of the masses that we wanted to examine.

GRAYSON: And you were interested in using—these primary ions were designed to elucidate or ionize larger molecules?

STANDING: [Yes]. That's right. Because Benninghoven had done it already for small molecules, and there was a question of whether you could do the same as fission fragments had done for large molecules.

GRAYSON: And your primary ions were usually—

STANDING: Cesium, cesium ions to start with. [Yes].

GRAYSON: And this worked pretty well?

STANDING: This worked well.

GRAYSON: What was the first large molecules that you were able to produce with secondary ions?

STANDING: Oh, what did we do?

GRAYSON: Diribonucleoside monophosphate? Does that sound familiar?

STANDING: Could be. Could be. We did some . . . . We published the design of the mass spectrometer in 1981, and we compared the results of the low-energy bombardment and californium also in 1981.[16]

GRAYSON: So, it would have been about thirty years ago—

STANDING: That's right. That's right. We then proceeded on to . . . well, let's see. Secondary ion mass spectra of protected diribonucleoside monophosphates.[17] That was in '82.

GRAYSON: So, these were **<T: 05 min>** fairly large molecules, and—

STANDING: [Yes]. These were fairly large molecules.

GRAYSON: Do you recall a mass range or the mass . . .

[16] B.T. Chait and K.G. Standing. "A time-of-flight mass spectrometer for measurement of secondary ion mass spectra." *International Journal of Mass Spectrometry and Ion Physics* 40, no. 2 (1981): 185-193; Werner Ens, K.G. Standing, B.T. Chait, and F.H. Field. "Comparison of mass spectra obtained with low-energy ion and high-energy californium-252 fission fragment bombardment." *Analytical Chemistry* 53, no. 8 (1981): 1241-1244.
[17] Werner Ens, Kenneth G. Standing, John B. Westmore, Kelvin K. Ogilvie, and Mona J. Nemer. "Secondary ion mass spectrometry of protected diribonucleoside monophosphates with a time-of-flight mass spectrometer."*Analytical Chemistry* 54, no. 6 (1982): 960-966.

STANDING: I don't know.

GRAYSON: Well, we can find out. That's not that hard to sort that out. It seems to me that, looking at the publication record, there's this fellow by the name of Werner Ens who appears on the scene.

STANDING: Oh, yes. Well, I haven't got to the end of the story about Brian yet.

GRAYSON: Well, let's go ahead and do that.

STANDING: Brian Chait.

GRAYSON: Right.

STANDING: He came and worked and got things running on the primary ion bombardment, and then I finally had to let him go. By this time, we knew that Frank [H.] Field was looking for somebody at Rockefeller [University], and I didn't have enough money to keep Brian, so I had to let him go to Rockefeller.[18]

GRAYSON: So, that would have been—

STANDING: So, at that time [. . .] Werner Ens showed up as a graduate student. So, he and a little later Ron [Ronald C.] Beavis were my first graduate students in mass spectrometry.

GRAYSON: So, you carried on with the ideas that you developed at the time Brian was here?

STANDING: [Yes]. I stopped doing nuclear physics at that time and started doing mass spectrometry. So, that was a definite transition.

GRAYSON: And you were using a home-built instrument?

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[18] Frank H. Field, interview by Michael A. Grayson in Durham, North Carolina, 9-10 December 2009 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0636).

STANDING: A what?

GRAYSON: Home built?

STANDING: Yes. Yes.

GRAYSON: So, this was a . . . . What kind of accelerating potential did you use for—

STANDING: Oh, we used fifteen, twenty kilovolts.

GRAYSON: Oh, fairly substantial.

STANDING: [Yes].

GRAYSON: And you started to—now that you had Ron Beavis here—

STANDING: [Yes]. Ron Beavis showed up a little bit after Werner. Anyway, Werner showed up about the time that Brian left, [. . .] as a graduate student. So, he was my graduate student at that stage, and about a year later Ron Beavis arrived.

GRAYSON: So, you just basically continued this analysis of biological molecules from there on? That was mostly what you were doing, then?

STANDING: That's right. That's right. And Brian by that time had gone off to Rockefeller, and the first joint paper we published there was with Brian, a comparison of fission fragments in low-energy ion bombardment, which he had done at Rockefeller and we had done in Manitoba.[19]

GRAYSON: I see. So it was a joint paper with the two—

[19] Werner Ens, *et al.*, "Comparison of mass spectra."

STANDING: [Yes], that's right. That was . . .

GRAYSON: So, I guess Field, Frank Field, had a fission fragment machine at Rockefeller?[20]

STANDING: [Yes], that was published in '81. And Brian by that time had got established at Rockefeller, and so we did this comparison.

GRAYSON: And I had a question about **<T: 10 min>** your decision—this kind of backs up a little bit, but that's okay. Your decision to stay in academia. Did you ever consider the possibility of working in industry or government, or did you just—

STANDING: Well, of course, we started to collaborate very intimately with SCIEX in Toronto, [Canada]. And I suppose there was a possibility of getting hired by SCIEX, but I never explored it in great detail.

GRAYSON: And you never had an inclination independently on your own to look at the possibility of a job in industry?

STANDING: Well, we thought a number of times that we might start making mass spectrometers here, but the obstacles seemed to be pretty large.

GRAYSON: It's a tough business.

STANDING: It's a tough business.

GRAYSON: No doubt.

STANDING: So, we got this collaboration with SCIEX. That started later on. Well, actually, SCIEX had been sort of dipping their foot in the river for some time, but anyway, we finally started a collaboration with them. By this time, I had . . . . Let's see. I went on a conference in Russia, and thereupon acquired Anatoli [N.] Verentchikov. He was our first Russian.

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

GRAYSON: [. . .] So, why did you go to Russia for this conference?

STANDING: Well, I can't remember. It was one of the conferences in the mass spec business. And at that time, I—let's see. There was a difficulty with the planes. Anyway, I spent some time in St. Petersburg, [Russia] with Anatoli that was unexpected, because there was a mix-up with the planes.

GRAYSON: Your travel plan?

STANDING: Travel plans. [Yes].

GRAYSON: Oh, my.

STANDING: And so I was in St. Petersburg—I think it was still Leningrad, [Soviet Union]—for a whole day, and Anatoli spent the day taking me around to various places to see. And I persuaded him to apply for an NSERC [Natural Sciences and Engineering Research Council] postdoc fellowship to come to Manitoba. And he came, and he had been involved in the experiments at St. Petersburg that were in parallel with John Fenn's. And so anyway, he came to Manitoba, and designed a—let's see. **<T: 15 min>**

GRAYSON: Did you publish anything with Verentchikov?

STANDING: Yes, particularly the . . . let's see. I'm just trying to find it.

GRAYSON: I'm looking, too. I can't see anything.

STANDING: Here we are. Number twenty-two on my CV. Reflecting time-of-flight mass spectrometer with electrospray ion source and orthogonal injection.[21] That was *Analytical Chemistry* in '94.

GRAYSON: Okay. All right.

[21] Anatoli N. Verentchikov, Werner Ens, and Kenneth G. Standing. "Reflecting time-of-flight mass spectrometer with an electrospray ion source and orthogonal extraction." *Analytical Chemistry* 66, no. 1 (1994): 126-133.

STANDING: So Anatoli was well settled in Winnipeg by that time. And then he went back to Russia originally.

GRAYSON: Did you design a reflectron here?

STANDING: Yes. He designed this machine that's described in ’94.

GRAYSON: Did you ever buy any mass spectrometers, or you just worked with home—

STANDING: Well, later on, later on. We never bought anything, but we were given mass spectrometers from SCIEX. [. . .]

Anatoli was the first of our Russians, and the next one was Chernushevich, written up there, Chernushevich.

GRAYSON: Well, there's a lot of stuff written up there.

STANDING: [laughter] Well, anyway—

GRAYSON: Oh, there it is.

STANDING: Igor. Igor Chernushevich. And by that time, SCIEX has decided that they would like to collaborate with us. [. . .] They knew about quadrupoles, and we thought we knew about time of flight. So at that stage we joined forces and used their knowledge of quadrupoles and our knowledge of time of flight, to make a hybrid machine. [. . .] <**T: 20 min**> So, Igor was already here in ’94, and [Xuejun] Tang was already my graduate student at that time. And at that stage when I was going off to Orsay, I had designed a machine with a mirror and Tang was left to get this working, which he did. [. . .] He's now at . . . . What do you call it, the people who make the insulin?

GRAYSON: Novartis?

STANDING: No.

GRAYSON: Johnson [and Johnson]?

STANDING: No. In the middle of Indiana.

GRAYSON: Oh, [yes]. Can't think of it right now, but I know who you're talking about.

STANDING: That's where he is now, Tang.

GRAYSON: So, I notice this fellow Ens is on a lot of your publications from when he got here until forever, it seems.

STANDING: [Yes]. He was originally my student. Then he . . . let's see. Maybe postdoc. Then he went to Uppsala [University] for at least a year. And then he came back and was hired on staff.

GRAYSON: Now was he hired in the physics department or—

STANDING: In the physics department.

GRAYSON: In the physics department. So now all this mass spectrometry that's leaning over to biology is being done in the physics department.

STANDING: That's right.

GRAYSON: And so you're still in the physics department.

STANDING: [Yes]. Still in the physics department, although, you know, in terms of teaching we taught mainly physics, I guess.

GRAYSON: So, how long were you obliged to teach?

STANDING: Up till the time I "retired" in '93.

GRAYSON: So, you had to carry a teaching load?

STANDING: I had some teaching load. I can't remember how it worked out.

GRAYSON: So, when Werner Ens came back, he still continued to collaborate with you for many years.

STANDING: [Yes], he's still on staff.

GRAYSON: Because I think he's got like, I don't know, literally tens of papers that you're co-authored with him on.

STANDING: [Yes]. That's right. That's right. So, he's been a collaborator with me for a long time.

GRAYSON: Even after he got his degree and got a staff position and so on and so forth.

STANDING: That's right. Although [. . .] he was at Uppsala for about a year, I think. So, if you look through the records, you'll find that he was co-authoring things at Uppsala for a while.

GRAYSON: [. . .] You were still publishing together, but he was at Uppsala, and you were collaborating on the—

STANDING: Not while he was at Uppsala, no. I think he was just collaborating with the people at Uppsala.

GRAYSON: But he's been a serious collaborator with you over the years?

STANDING: [Yes]. He was my student originally and then finally ended up as a staff member.

GRAYSON: So, what skill set does he bring to the effort?

STANDING: Well, just the normal skills that a physicist brings to these efforts.

GRAYSON: It's just that you just collaborate a lot on all—

STANDING: [Yes]. **<T: 25 min>** He's never developed much of an interest in biology, particularly after Brian went to Rockefeller. He developed a strong interest in biology and a strong collaboration with a biologist there. And Ron Beavis was also my graduate student, and he went to . . . well, he went to Memorial University, and then he went to work with Brian.

GRAYSON: What was the first university?

STANDING: Memorial. In Newfoundland.

GRAYSON: He got involved in the MALDI [matrix-assisted laser desorption/ionization] business.

STANDING: That's right, with Brian particularly.

GRAYSON: Now were you doing MALDI? Did you ever do any MALDI work here?

STANDING: [Yes]. We did MALDI soon after [Franz] Hillenkamp did.[22] That is, we [. . .] just switched the . . . . Instead of bombarding with cesium ions we just bombarded with photons.

GRAYSON: And so when you were doing your original work, you always used cesium ions. You didn't try any other—

STANDING: Originally, we just used cesium ions.

[22] Franz Hillenkamp, interview by Michael A. Grayson at University of Münster, Münster, Germany, 20 August 2012 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0704).

GRAYSON: During all this period, did you have to fight for funding?

STANDING: Shortly after we started to get reasonable results we got money from NIH [National Institutes of Health], and that was good.

GRAYSON: But you're Canadian.

STANDING: [Yes]. That's right. The official statement is that [. . .] research can only be funded outside the country if it's not likely to be duplicated in the USA, and I was able to persuade them that that was so for a number of years, and then it died.

GRAYSON: Well, it's getting harder and harder to get money from anybody.

STANDING: That's right. And finally, our grant from NSERC died as well, a couple of years ago. So I'm now retired retired. [laughter]

GRAYSON: Really retired. [. . .] The funding levels, I guess, were adequate to get the job done.

STANDING: [Yes], from NIH, they were certainly more than adequate. From NSERC, they were always borderline, but I had the NIH money to fall back on.

GRAYSON: So, you managed to keep graduate students?

STANDING: [Yes], graduate students and postdocs were paid mostly out of NIH funds. When the NIH money ended, which was, what, ten years ago maybe, then things got a little tighter.

GRAYSON: Were there any instrument grants? [. . .] There are some funds in the United States that are given primarily just to purchase instruments.

STANDING: They were the standard grants. What do you call them?

GRAYSON: R01 or whatever?

STANDING: [Yes], R01s. [Yes].

GRAYSON: But SCIEX was providing you with instruments eventually?

STANDING: SCIEX provided us with this machine at one time.

GRAYSON: A machine?

STANDING: Well, let's see. First of all, when Igor was there **<T: 30 min>** they provided us with quadrupoles, and that was interfaced to a time-of-flight machine that we had already. So that was the first hybrid machine that we made.

GRAYSON: Who developed the interface?

STANDING: It was a combination. There wasn't much of a problem except levels and so on. Things like that. So they sent a couple of people out from SCIEX to Winnipeg to make these measurements and make sure that everything was right in terms of height.

GRAYSON: And you had somebody on the time-of-flight side helping to—

STANDING: That was Chernushevich, particularly. So, they started . . . . I had a proposal, and I talked to them at the ASMS meeting at the start of that year, and they sent somebody out in the fall and by Christmas we had the hybrid machine working.

GRAYSON: So, they gained from that also? SCIEX—

STANDING: They gained . . . at that time, they had no expertise in time of flight. Since then, they've hired Igor and hired Sasha Loboda, and I'm not sure who else is in the time-of-flight department, but anyway, they have at least much expertise as we have now.

GRAYSON: So, would this be one of the earliest combined quadrupole/time-of-flight instruments, or were other people doing it?

STANDING: It was officially the second, I think. Let's see. Do I have that? Oh, here it is. [Yes]. The people at . . . I'm not quite sure what it was called then. Anyway, they published in *Rapid Communications* in '96.[23] And we got on to publish in '97.[24]

GRAYSON: So—

STANDING: So, that was a collaboration with . . . . Well, we had it working before that, but I guess that was the first thing we have down on paper.

GRAYSON: So, it looks like Bordoli, Bateman?

STANDING: That's right, in '96.

GRAYSON: [. . .] They published the first . . . Q-ToF [quadrupole time-of-flight] concept.

STANDING: They called it the Q-ToF, and I think patented the name, or—

GRAYSON: Oh, really?

STANDING: —did whatever it was. And we came along shortly behind them.

GRAYSON: But now, was any of the work that you did along this line—was that patented?

STANDING: What's that?

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[23] Howard R. Morris, Thanai Paxton, Anne Dell, Jean Langhorne, Matthias Berg, Robert S. Bordoli, John Hoyes, and Robert H. Bateman. "High sensitivity collisionally-activated decomposition tandem mass spectrometry on a novel quadrupole/orthogonal-acceleration time-of-flight mass spectrometer." *Rapid Communications in Mass Spectrometry* 10, no. 8 (1996): 889-896.

[24] Andrej Shevchenko, Igor Chernushevich, Werner Ens, Kenneth G. Standing, Bruce Thomson, Matthias Wilm, and Matthias Mann. "Rapid 'de novo' peptide sequencing by a combination of nanoelectrospray, isotopic labeling and a quadrupole/time-of-flight mass spectrometer." *Rapid Communications in Mass Spectrometry* 11, no. 9 (1997): 1015-1024.

GRAYSON: Did you patent any of these things?

STANDING: [Yes], we patented the collisional cooling, and that's still yielding us money.[25]

GRAYSON: Oh, it is?

STANDING: [Yes].

GRAYSON: So, that patent is held by—

STANDING: Still valid for another year or two.

GRAYSON: And who is it assigned to?

STANDING: It's assigned to various people, but particularly . . .

GRAYSON: **<T: 35 min>** Does the university have a slice of it?

STANDING: [Yes], the university has 50 percent, and the "inventors" have 50 percent. I think I have, oh, 10 percent of that or something.

GRAYSON: I see. But it is, in fact, providing income? You get money from it? You still get money from it?

STANDING: It's still providing income, although that's personal income to us. At least, the university treats it as that.

GRAYSON: But they get 50 percent anyway, though, right?

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[25] Andrew N. Krutchinsky, Alexandre V. Loboda, Victor L. Spicer, Werner Ens, and Kenneth G. Standing. "Spectrometer provided with pulsed ion source and transmission device to damp ion motion and method of use." U.S. Patent 6,331,702, issued December 18, 2001.

STANDING: What?

GRAYSON: The university gets 50 percent?

STANDING: They get 50 percent off the top. [. . .] The agreement was that two hundred thousand [dollars] would be divided up, and if the . . . I can't remember just how it is, but if there was a lot of extra money, that would be divided as well. And last year, we got I think the . . . instead of two hundred thousand [dollars] it was three hundred thirty [thousand dollars] or something like that. So the university got half of that. [. . .] Our "inventors" got the other half divided.

GRAYSON: Now that's just on that one patent?

STANDING: What?

GRAYSON: That's just on the one patent, on collisional cooling?

STANDING: That's just on one patent.

GRAYSON: So, you have other patents, but they didn't pay anything?

STANDING: That's right. There are a number of other patents, but none of them made any money. [laughter]

GRAYSON: Well, having one make some money is good.

STANDING: That's right. That's right. Not everybody does.

GRAYSON: Well, it's good to know, because I know a lot of people do a lot of work and they patent things, but it doesn't really end up actually providing them with much satisfaction or remuneration—

STANDING: Yes. Yes.

GRAYSON: [. . .] So, let me see what else we want to cover here. Well, you kind of had a dual career, first in nuclear physics.

STANDING: I had a career first of all in nuclear physics, and then in mass spectrometry. So, I was . . . by the time—let's see. I was born in ’25, so I was fifty-five [years old] before I started doing mass spectrometry. So, that explains partly the record on my CV that nearly everything, all the distinctions, were after I retired formally.

GRAYSON: So, I noticed also that your publication rate jumped up almost immediately after you got into the mass spec game.

STANDING: That's right. [Andrew N.] Krutchinsky. Krutchinsky.

GRAYSON: [. . .] I made this chart, the number of publications. This is from SciFinder, so it's probably not, you know, totally . . . but it shows that you were basically kind of not producing a lot in the—

STANDING: Nineteen eighty was the start of mass spectrometry.

GRAYSON: And then after that you're going three, four, five papers a year or more.

STANDING: [Yes], or thirteen. That's the maximum.

GRAYSON: That was a big year. So that's a really intense amount of work, while you had—you had a fairly good number of co-authors on these. So, were all these graduate <**T: 40 min**> students, or—

STANDING: Well, we had a number of co-authors. For one thing, we had collaborators in the chemistry department. First of all, John [B.] Westmore, who was early on, and then Harry Duckworth.

GRAYSON: Duckworth is in the physics department?

STANDING: No, this is young Harry Duckworth, that we're going to have dinner with.

GRAYSON: And what department was he in?

STANDING: He's in chemistry.

GRAYSON: Chemistry.

STANDING: He was head of chemistry. We had collaborators in chemistry, John Westmore and Harry. And we had various collaborators spread across the . . . . And then we developed a rather strong collaboration in plant viruses.

GRAYSON: Plant viruses?

STANDING: Plant viruses, with our friend . . .

GRAYSON: This says Seifers—

STANDING: Steve Haber. [Yes], Dallas Seifers was at Kansas State [University], and through Steve Haber we made contact with Dallas Seifers and there was a three way collaboration that lasted for quite a long time.

GRAYSON: And you were looking at plant viruses?

STANDING: Plant viruses. [Yes].

GRAYSON: So what is the motivation for studying plant viruses?

STANDING: Well, to [. . .] characterize the plant virus so that you knew that they were different from the existing viruses.

GRAYSON: And to develop possible defenses against them? Was it—

STANDING: Not really. We weren't concerned with that aspect of things. That was Seifers' job. We simply characterized them. In other words, we sequenced the proteins.

GRAYSON: What's the typical number of proteins in a plant virus?

STANDING: Well, one protein is characteristic of the plant virus, generally.

GRAYSON: So, it's just a particular protein that signifies that you've got a plant virus on your hand?

STANDING: [Yes]. That you have this particular plant virus.

GRAYSON: And so you were using . . .

STANDING: Mass spectrometry to define this or to sequence this plant virus.

GRAYSON: So did you . . . . I'm just trying to think. You say in order to get the proteins you've got to digest the thing, or how do you do that step where you separate out—

STANDING: You digest it and put it on a target, and then you bombard it.

GRAYSON: With MALDI?

STANDING: Yes. By that time it was MALDI.

GRAYSON: But then you have a mixture of these things, right? Mixture of the proteins on the target? If you—

STANDING: Well, that's the problem. [Yes]. But we managed to define a number of . . . if you look at the CV, there are a number of . . .

GRAYSON: So were you using a tandem mass spec approach, then?

STANDING: Yes. **<T: 45 min>** And by that time, I haven't got to the end of my story, by that time, we had a collaboration with Oleg [V.] Krokhin [. . .] who was really a . . . he did things by putting things through columns.

GRAYSON: Liquid chromatography?

STANDING: Liquid chromatography. [Yes]. He was really a liquid chromatographer.

GRAYSON: [. . .] So, he was doing liquid chromatographic work. So, does that put you in the electrospray business, are you still doing MALDI?

STANDING: Well, that put us in the electrospray business or hybrid business. So we do electrospray first, and then put the results into the time-of-flight machine.

GRAYSON: [. . .] Very good. So, you really got seriously into this mass spec business for—

STANDING: Well, I guess so. [laughter] I guess so.

GRAYSON: And what kinds of experiments were you doing at the end of your career in the—

STANDING: I guess . . . well, at the end, we were doing a set of experiments with the viruses, and with some people in Israel that were interested in citrus chlorophyllase. And . . .

GRAYSON: So, it looks like you get to do a lot of collaborations with the people from different—

STANDING: [Yes], we did. [Yes]. We certainly . . . I guess almost entirely since we started off doing mass spectrometry we were involved in collaborations, because we're not chemists and

we don't produce the raw material. So the chemists or biologists of one kind or another would produce the raw material, and then we'd try to analyze it.

GRAYSON: So you were kind of like a handy lab that people could access?

STANDING: [Yes], we were . . . originally, at least, we didn't charge anybody for things. So, you can regard it as a service, but we liked to regard it as a collaboration.

GRAYSON: Sure. Well, so how did the physics department—

STANDING: Well, it's odd to find it in the physics department. [Yes]. That's right.

GRAYSON: Did that ever make your management—what did your management think of this proposition?

STANDING: Well, the physics department was happy enough, as long as we got enough publications.

GRAYSON: Publications.

STANDING: And of course, I was retired for . . . I've been retired since ’93, so they didn't have any financial commitment to me.

GRAYSON: Oh, so all this—

STANDING: They did to [. . .] Werner.

GRAYSON: So, basically, they liked the PR, is what—

STANDING: They liked the PR. There was **<T: 50 min>** enough PR to satisfy the department and the faculty, and of course, we kept doing things for other people in the faculty, in microbiology, and so on and so forth. We kept analyzing samples for them.

GRAYSON: Now, when you started charging for this service and these collaborations eventually, how was that worked out? Was that . . .

STANDING: Well, we charged particularly the company that's along the . . . what do you call it? Which had lots of money anyway, so that was fine. But that sort of died, and we haven't really explored that very much.

GRAYSON: So, this was work you were doing for a commercial enterprise?

STANDING: [Yes]. We've been somewhat . . . . The people up in Health Sciences [Centre Foundation], John Wilkins and company, have . . . they charge the other medical departments, but, of course, they almost all have lots of money. [. . .] So he got Oleg, who's our liquid chromatographer, works most of the time up in Health Sciences. He works there part time, and he works here part time. He has a couple of students still working here.

GRAYSON: So, you've had a lot of interactions with people outside of the department, outside of your particular field of study.

STANDING: That's right. That's right.

GRAYSON: Yeah, that's [. . .] probably enlightening, I would assume.

STANDING: Pardon?

GRAYSON: Enlightening.

STANDING: [Yes]. I think by and large, they were fairly satisfied with it, and of course, we were able to contribute an instrument that was pretty much up to date.

GRAYSON: So, I was curious, how did that grain measurement thing ever work out? You never used mass spec on the grain—

STANDING: It never worked out. Never worked out. They opted for the—of course, there were disadvantages to that too, because people had to send the samples in to Winnipeg. You weren't going to build a cyclotron at every country elevator. So, there were disadvantages, but I think basically the answer was that the people in the Grain Research Lab were deciding what was acceptable. So, as far as I know, they have a network of machines that examine the infrared reflectance of the grain.

GRAYSON: So they went with the infrared?

STANDING: That's right. And it—so this is fine. This is fine. But we don't think it's very accurate, but what can you do?

GRAYSON: So, you've probably mentored a number of students over the—

STANDING: Students and postdocs. [Yes]. Particularly a number of students, including some very good ones, but I guess mainly postdocs. **<T: 55 min>** The disadvantage of being in a physics department, that you already pointed out, that many of the students that are in the department are in the department because they don't want to study biology, and so it's relatively hard to get students, and, in fact, get students that are able to handle the physics department requirements of graduate [school]. So I think that's one reason that NSERC cut us off finally, because we didn't have enough students. [. . .] Or so they claimed.

GRAYSON: You're in the physics department, so if a student comes to work for you, but he does biological measurements with a mass spectrometer, does he have to come out—what kind of a degree does he—

STANDING: That's all right. He gets a degree which is a degree in science, and there isn't—I haven't seen any particular problem with that. It's at the input end that the problem is. That is, you would like to attract people in this area that are going to be able to handle the graduate physics courses.

GRAYSON: I mean, since they're working for you then they have to do some physics.

STANDING: They still have to get a degree in physics.

GRAYSON: Physics. Okay.

STANDING: So, some of them have been capable of doing that, but some are repelled by the idea of having to take courses in physics.

GRAYSON: That's understandable, particularly if they're coming in with a chemistry/biology interest.

STANDING: That's right. Background. Now I guess we could have maybe explored that a bit farther so that they could have taken courses in other departments, but we haven't done that.

GRAYSON: Now, did you ever get saddled with being a department chair or any of those—

STANDING: No, I was invited to run for the department chair at one time, but I opted out. I managed to opt out. Our present chair is in the other mass spec . . . you know, where there's still the other mass spec lab that's working in the department, that's a descendent of elder Harry Duckworth.

GRAYSON: That would be a more physics-oriented instrument?

STANDING: That's physics, very much physics-oriented doing things that are off the major channel, etc., etc. But they've just about finished, I think. Because Bob Barber, who's retired, and I think he maybe has one graduate student, but that's the end of it.

GRAYSON: [. . .] Once many years ago I had a time-of-flight mass spectrometer, you know, one of the old Bendix [Corporation] instruments.

STANDING: Oh, [yes]. One thing I didn't mention, I should have mentioned, is that at the time we developed the time-of-flight mass spectrometer I don't think we were aware of the Bendix, and in particular, your criticism or your recording of the general attitude to the Bendix machine. I don't think we were aware of that, as I remember.

GRAYSON: I don't know if they sold any in Canada or not, <**T: 60 min**> but . . .

STANDING: Yes, they must have done some, because John Westmore, our collaborator in chemistry, our original collaborator, had a Bendix machine that he had got from Pinawa. And when, I don't know, they didn't need it, or something or other. And he had it sitting in his lab, but never got it working.

GRAYSON: Oh, really?

STANDING: [Yes].

GRAYSON: So, do you know what ever happened to that, or . . .

STANDING: I don't know. I guess it was scrapped eventually.

GRAYSON: One of the things that we'd like to do is see if there's any old Bendix machine around that we could collect at the Chemical Heritage Foundation, because—[26]

STANDING: Well, we're kind of committed to giving our old machines to the National Lab in Ottawa, [Canada]. Now whether . . . we could certainly ask if they are still interested, because they sent a guy out here to look over the thing, and they were interested in our time of flight and also interested in the Duckworth machines. And that was last year, or two years ago. Anyway, nothing much has happened since then, so I could certainly ask them if they're still interested.

GRAYSON: Well, that would be a good thing to do.

STANDING: I can show you a couple of examples in the lab.

GRAYSON: I'd like to see that when we get done here. So, the machine I was using . . .

STANDING: The Bendix?

[26] On 1 February 2018, as a result of the merger with the Life Sciences Foundation, the Chemical Heritage Foundation changed our name to the Science History Institute.

GRAYSON: The Bendix, had this wonderful situation where they had very good resolving power for mercury isotopes, but not at all for an organic compound at the same molecular weight. And I actually presented a paper in Atlanta [Georgia] showing that if you put a retarding potential in front of the detector, you could separate out the neutrals and the charge fragments and there was the original ion. So, there was [. . .] decay going on in the flight tube. So, for the organic compounds, you would never—with that linear instrument and everything line of sight of the detector, you were never going to get the drawing power . . . . I mean, no matter what you did to the ion source in terms of [inaudible] and all that kind of stuff.

STANDING: We did a paper on metastable decay that's been referred to a lot, of cesium ions, cesium iodide ions.[27] Number eight in the . . . is a [*Physical Review*] *Letters*, has been referred to nearly two hundred times. And that was with cesium iodide cluster ions. And that exploited the idea that when you get a decay in the flight tube, that the products keep on going with essentially the same velocity.

So that if you look at the counts in the upper counter you get a smooth distribution, as compared to people who looked at it at NRL . . . they used a magnetic sector machine, and of course, in that case, if you get a disintegration in the flight tube it goes off to the side. So they saw these anomalies in the distribution, **<T: 65 min>** and we were able to show that the anomalies were not a question of the original production but a question of what's happening in the flight tube.

GRAYSON: So it's a metastable decay kind of a—

STANDING: That's right. It was a metastable decay in the flight tube.

GRAYSON: You say that was fairly heavily referenced, that paper?

STANDING: Yes. There were . . . what was it? One [hundred] ninety-nine references now.

GRAYSON: [. . .] The resolving power of the time-of-flight instruments today is essentially mindboggling compared to the time-of-flight instrument that I used. So [. . .] there's an electronic component of being able to control the electronics and the ion source better. There's the reflectron that helps you, okay, and it helps get rid of a lot of the metastable decay and it gives you a longer flight path. But it seems that it's almost magical. I don't know. I mean, I still have a hard time comprehending the improvement in resolving power.

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[27] W. Ens, R. Beavis, and K.G. Standing. "Time-of-flight measurements of cesium-iodide cluster ions." *Physical Review Letters* 50, no. 1 (1983): 27.

STANDING: Well, Marvin [L.] Vestal has pushed this farther than anybody else, and he's used a very long flight path and he uses the technique that the original . . .[28]

GRAYSON: Delayed extraction? [. . .]

STANDING: [Yes], delayed extraction, as well as a mirror. And that is . . . . He and I . . .

GRAYSON: You jointly published this paper?

STANDING: Well, it's in press, I think.[29]

GRAYSON: It's in press. Oh, okay. All right. So I haven't seen it yet. This is going to be appearing in?

STANDING: *International Journal* [*of Mass Spectrometry*].

GRAYSON: *International Journal*? Okay. I'll have to keep an eye out for it.

STANDING: It's supposedly in connection with—I wrote the first part of the paper, and Marvin wrote the latter part.

GRAYSON: So, like I say, I understand what all is happening, but I still have a hard time believing the resolving power that you get out of these instruments today. It's definitely—

STANDING: [Yes], well, he has got some quite remarkable results on resolving power.

GRAYSON: What types of resolving power did you get on your instruments here?

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[28] Marvin L. Vestal, interview by Michael A. Grayson at the Orange County Convention Center, Orlando, Florida, 3 March 2010 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0680).
[29] K.G. Standing and Marvin L. Vestal. "Time-of-flight mass spectrometry (TOFMS): From niche to mainstream." *International Journal of Mass Spectrometry* 377 (2015): 295-308.

STANDING: Oh, we're . . . let's see. What do we have?

GRAYSON: I mean, it had to be good enough—it had to be better than the old Bendix, that's for sure.

STANDING: Oh, [yes], very much better than the old Bendix. And . . .

GRAYSON: [. . .] I think you were a group that was working on developing the time of flight when almost everyone else had forsaken it.

STANDING: When what?

GRAYSON: When almost everyone else had forsaken it.

STANDING: Yes, that's right.

GRAYSON: You and Bob [Robert J.] Cotter was the other, I think—

STANDING: That's right, Bob Cotter. He kept pushing time of flight, and wrote the book on it, of course.[30]

GRAYSON: So, between the two of you, you kind of kept it alive until it came back into vogue.

STANDING: Well, I think I'd credit Ron Macfarlane with a lot of the impetus there, because he had detected these very large things.[31]

GRAYSON: Well, what Ron had done was show—I mean, there was always this idea that the time of flight had this huge **<T: 70 min>** mass range. But the Bendix instruments never got much beyond, I don't know, four or five hundred, if they got that far. And Macfarlane's work

[30] Robert J. Cotter. *Time-of-flight mass spectrometry: instrumentation and applications in biological research*. Amer Chemical Society, 1997.
[31] Ronald D. Macfarlane, Oral History Transcript #0877.

showed that you could indeed get these large ions, large molecules, to fly. So, you could see a mass—you could start to use that "infinite" mass range that a time of flight has. And I think that was a motivator to get other people to really start looking at it, you know, really carefully.

STANDING: I think he encouraged a lot of other people, including ourselves and the people who did the original hybrid machine to look into time of flight more definitely. And I think he's still active.

GRAYSON: Macfarlane?

STANDING: [Yes]. Looking at Cathy [Catherine J. McNeal], whatever his collaborator was and finally wife. She went on to be a cardiologist of some sort, and he's been working on things connected to that.

GRAYSON: [. . .] I'm trying to get a sense of how the ideas and research on different areas coalesced into suddenly making the time of flight back into a viable commercial instrument, because, I mean, as a commercial venture, the time of flight went down in flames with the Bendix because they couldn't compete with the quads. And they didn't really deliver on their promise that they had this huge mass range. And the resolving power stunk. [. . .] If it weren't for people like Macfarlane and you and Cotter, then the whole thing probably—

STANDING: Might have died.

GRAYSON: Might have died totally, but somehow there was some synergy, some reaction, interaction, that this thing kind of—

STANDING: Well, I guess we have to give credit to Ron Macfarlane mainly, because he encouraged us to originally do the fission fragment desorption. We didn't follow up on that, but that was because we thought we had something better.

GRAYSON: Had a better way, you think, you thought. As an ionization method?

STANDING: That's right. That's right.

GRAYSON: But you still used the time of flight, and—

STANDING: That's right, to do the measurement.

GRAYSON: Was that a coincidence counting instrument?

STANDING: What?

GRAYSON: Was that a coincidence counting instrument?

STANDING: No. No. It was just—it was originally . . . . We used a data recorder to record the thing essentially.

GRAYSON: So you had an electron multiplier detection?

STANDING: Yes. Yes. We used microchannel plates, and I think Marvin is still using microchannel plates.[32]

GRAYSON: See, I think that whole concept of microchannel plates came out of the Bendix instrument.

STANDING: Yes. Well, it came . . . oh, I don't know. At the time we started, they were already well known in physics.

GRAYSON: So, you're saying that Macfarlane kind of got you going, and I'm not sure what was motivating Cotter.

STANDING: I don't know what motivated him. He of course—Catherine Fenselau was involved in more conventional mass spectrometry.[33] And Bob Cotter, I think he more or less went to his own <**T: 75 min**> tune.

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[32] Marvin L. Vestal, Oral History Transcript # 0680.

[33] Catherine Fenselau, interview by Michael A. Grayson at University of Maryland, College Park, Maryland, 13-14 April 2012 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0710).

GRAYSON: It's unfortunate that he died before I had a chance to talk to him, because that would have been an interesting . . . to get the time-of-flight story all together, because like I say, at one time it was . . .

STANDING: It was down the drain.

GRAYSON: It wasn't a very viable . . . certainly not in a commercial sense. Whereas now it's just like everywhere.

STANDING: Well, it's the workhorse of most labs, I think.

GRAYSON: It's an interesting change of position.

STANDING: That's right. That's right. Well, we [. . .] made some contribution to this, anyway.

GRAYSON: Well, yes, you did most definitely. So I was curious, what do you consider your most significant publications?

STANDING: Well, I suppose the most significant in terms of impact has been the collisional damping. And the original with Andrew Krutchinsky.[34]

GRAYSON: [. . .] And so what exactly is collisional damping? Can you explain that for the layman?

STANDING: Well, collisional damping is if you have a . . . . Well, say you start with MALDI ions that are well-defined in time but have energies all over the map. Then you put these into a region where the pressure is elevated, not to atmosphere, but somewhat more than normal, than high vacuum. And so you get them collisionally damped.

GRAYSON: So, this would be like in an RF [radio frequency] only quadrupole section? Or, I mean, you got to keep the ions in a bottle—

[34] A.N. Krutchinsky, I.V. Chernushevich, V.L. Spicer, W. Ens, and K.G. Standing. "Collisional damping interface for an electrospray ionization time-of-flight mass spectrometer." *Journal of the American Society for Mass Spectrometry* 9, no. 6 (1998): 569-579.

STANDING: This is in just a—

GRAYSON: Volume?

STANDING: —region. So, you get the collisional damping region, and then the ions . . . . This is tied in with orthogonal injection. So . . .

GRAYSON: So that—

[END OF AUDIO, FILE 1.2]

GRAYSON: You were saying about . . .

STANDING: Collisional damping?

GRAYSON: [Yes]. We're back. I wonder what happened.

STANDING: That the ion goes through a region of increased pressure, and then is injected orthogonally to the final direction.

GRAYSON: So, who is the inventor of the orthogonal—

STANDING: Well, I think a number of us got the idea, but the person who really took it and ran with it was Andrew Krutchinsky, and he's getting the largest portion of the . . .

GRAYSON: Of the patent money?

STANDING: Yes.

GRAYSON: It's telling me my battery is low. Let me make sure I'm plugged in over here.

STANDING: What are you looking for?

GRAYSON: It says my battery is low. But I don't know why, because it's plugged in.

STANDING: Seems to be all right at that end.

GRAYSON: Yes. Yes. It's flashing a battery low signal.

[END OF AUDIO, FILE 1.3]

GRAYSON: We can use my laptop to start recording and carry on from where it was we are. I'll turn this around. [. . .] And I'll just say that we're continuing our oral history session with Professor Standing using the recorder that's on my laptop because the battery charger for the recorder that I was using before died. So we'll have to figure that out after the fact. But at any rate, I believe we were talking about the time-of-flight mass spec development, and how it got—it did come back from the ashes and became a much more powerful tool, and is probably the major instrument that's in use today in so many laboratories.

STANDING: Now our [. . .] original contact, at least, was just about entirely with Ron Macfarlane.[35] And . . .

GRAYSON: You might have to speak a little louder, because we're just—we're picking up the microphone over here, so—

STANDING: Oh, [yes].

GRAYSON: You can see the little green thing gives you an idea of the volume that's going.

STANDING: Oh, okay. All right. Well, I'll shout a bit.

GRAYSON: There you go.

[35] Ronald D. Macfarlane, Oral History Transcript #0877.

STANDING: Our original contact I think was with Ron Macfarlane, and we went on from there.

GRAYSON: Were you aware of the work that Bob Cotter was doing?

STANDING: Well, from this point of view, he was a little bit delayed. He came along a little bit later, and we weren't really aware of the stuff that he did until later on.

GRAYSON: And were there any other people that you were aware of doing time-of-flight work besides you?

STANDING: Well, there were the people in Orsay that had done this correlated business, and that in fact encouraged me to spend a year at Orsay in ’84, ’85, I think. So, there were those people, and there were various other people that appeared at this conference in 1980 in Europe. The interesting thing about the conference in 1980 was that none of them were professional mass spectrometrists. They were all imports from other fields. I don't know exactly what Yvon’s[Le Beyec] background was, but he and Serge Della-Negra had this low-energy machine.

GRAYSON: Low-energy?

STANDING: Nuclear machine. They were interested in mechanisms, primarily.

GRAYSON: So, this is a conference of people who were doing time of flight, but weren't really from the field?

STANDING: I think so. Most of the original people in time of flight had come into it from other areas.

GRAYSON: Now, before we lost our recording earlier, we were talking about your most important publications, and you talked about the collisional damping as one. Were there other—

STANDING: Well, I think the . . . obviously, the original publication we had **<T: 05 min>** with the time-of-flight machine was setting some sort of standard.[36] That was way back in '81. That was the publication of our first machine.

GRAYSON: So, any other possible ones that you can think off of the top that got a lot of press?

STANDING: Well, I think the general idea of coupling liquid chromatography to time-of-flight machine was a contribution. We certainly weren't alone in that, but we did quite a bit of that.

GRAYSON: Well, without my friend here [. . .] unless you have any things to talk about that I haven't discussed or covered, I think we might be close to wrapping things up. We've talked about your relationship to Ron Beavis and Brian Chait, both of [whom] went to Rockefeller after they spent some time here. Now Beavis, did he come as a graduate student, or was he a postdoc?

STANDING: He came as a graduate student originally, and he received his PhD with me.

GRAYSON: So, he's got a physics degree?

STANDING: He has a physics degree. That's right. And then he moved on, and finally ended up . . . he's mainly concerned with analyzing data.

GRAYSON: The big data crunch business, trying to understand what's going on there. I'm just looking at some of your co-authors here. A guy by the name of [Victor] Spicer?

STANDING: Vic Spicer is our long-time sort of analyzer. He is our computer expert, and if we have questions about what to do with the computer or how to do it, we ask Vic.

GRAYSON: There's a name here, Hélène Perreault?

STANDING: Oh, Hélène Perreault is in chemistry. So, she's a . . . well, a real mass spectrometrist. I'm not sure. She certainly was in Boston, [Massachusetts] for a while with

[36] Chait and Standing. "A time-of-flight mass spectrometer."

Cathy [Catherine E.] Costello. And I'm not sure she came directly from that lab, or a more indirect way.

GRAYSON: What about John Wilkins?

STANDING: John Wilkins is the head of mass spectrometry at the Health Sciences. So he has a lab up there, and well, he employs [. . .] Krokhin, for example. He's a fundamental guy who gets the money for them.

GRAYSON: So, do you have a good relationship, I guess, with the Health Sciences Center?

STANDING: [Yes], it's somewhat flexible, you'd say. Oleg [. . .] Krokhin, works half time up there, so he's our liaison man. But our relations with John Wilkins are certainly very pleasant. **<T: 10 min>** But we don't have any actual collaborations going on with him.

GRAYSON: And the university tolerates you these days?

STANDING: They tolerate me. They haven't tried to kick me out of my office yet. [laughter]

GRAYSON: That will come.

STANDING: That's right. That's right. Well, as you can see, I have mental problems. I have a very great difficulty in remembering things. I can usually remember if I'm given enough time, but not immediately.

GRAYSON: Well, your memory looks pretty good to me.

STANDING: Well, at age eighty-nine, coming on ninety, I guess I'm not too bad.

GRAYSON: So, you were born in what month?

STANDING: In ’25.

GRAYSON: The month was?

STANDING: April.

GRAYSON: April. So, April—well, not too far away. About halfway there.

STANDING: I'm halfway between eighty-nine and ninety. My mentor at Princeton died when he was in his hundredth year, so that's I guess advantageous.

GRAYSON: I still can't think of the name of that fellow that was at Princeton at the time when you might have—he might have been there at the same time you were, but he probably was there before you, because he probably got there in the thirties, late thirties.

STANDING: He was there before me, because I know what you're thinking of. [. . .] He had a big machine. [Walker] Bleakney. Bleakney.

GRAYSON: Yes. Bleakney. Yes. That's the name.

STANDING: He was earlier on, and by the time I arrived in Princeton he was doing shock weights.

GRAYSON: But that's the same person we were thinking about.

STANDING: He had done more conventional mass spectrometry before.

GRAYSON: Well, unless you have any comments, words to the wise that you want to give, this would be the end of this part of the interview. [. . .]

[END OF AUDIO, FILE 1.4]

[END OF INTERVIEW]

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