CHEMICAL HERITAGE FOUNDATION

KEITH R. JENNINGS

Transcript of an Interview
Conducted by

Michael A. Grayson

at

Leamington Spa
Warwickshire, United Kingdom

on

24 and 25 April 2008

(With Subsequent Corrections and Additions)

ACKNOWLEDGMENT

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

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> Keith R. Jennings, interview by Michael A. Grayson at Leamington Spa, Warwickshire, United Kingdom, 24-25 April 2008 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0419).

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

Keith R. Jennings

1932	Born in Sheffield, England on 5 December

Education

1956	B.A., Queen's College, University of Oxford, First Class Honors, Chemistry
1958	M.A., D.Phil., Queen's College, University of Oxford, Chemistry

Professional Experience

	National Research Council, Ottawa, Canada
1958-1960	Post-Doctorate, under R. J. Cvetanovic
	University of Sheffield
1960-1969	Lecturer
1969-1972	Reader
	University of Warwick, Department of Chemistry
1972-1997	Professor of Physical Chemistry
1974-1980	Chairman of Department
1983-1985	Chairman, Board of Science
1983-1986	Chairman of Department
1989-1995	Chairman of Department
1997-present	Emeritus Research Professor in Department of Biological Sciences

Honors

1985	Thomson Medal presented by the International Mass Spectrometry Congress
1995	Award for Distinguished Contribution in Mass Spectrometry presented by the American Society for Mass Spectrometry
1996	Visiting Gulbenkian Professor, New University of Lisbon
1997	Honorary D.Sc., University of Lisbon, Portugal
1998	Field and Franklin Award for Outstanding Work in Mass Spectrometry presented by the American Chemical Society
	Aston Medal for Outstanding Contributions presented by the British Mass Spectrometry Society

Elected Life Member of the British Mass Spectrometry Society

ABSTRACT

Keith R. Jennings begins his oral history discussing his youth in Sheffield, England. With parents supportive of his education, Jennings excelled, earning a spot at the prestigious King Edward VII Grammar School. Upon completing his examinations, Jennings applied to the University of Oxford where he was awarded the Hastings Scholarship to Queen's College. While at Queen's College, Jennings pursued his B.A. with Jack Wilfrid Linnett. After achieving First Class Honors distinction, Jennings continued his research with Linnett to complete an M.A. and D.Phil.

Following his time at the University of Oxford, Jennings conducted post-doctoral research with Robert J. Cevetanovic at the National Research Council in Ottawa, Canada. While Jennings worked with Cevetanovic he became more interested in the burgeoning research field of mass spectrometry. Returning to England after two years in Canada, Jennings began a post at the University of Sheffield, first as a Lecturer and then as a Reader. While at Sheffield, Jennings pursued research in the mass spectrometry of gas kinetics, fluorine compounds, and metastable transitions. He began building his own equipment and became involved in the emerging British mass spectrometry community.

Jennings discusses Ion Cyclotron Resonance research, time dependant ion fragmentation, and collision induced spectroscopy. After moving to the University of Warwick in 1972, Jennings continued his research on fluorinated compounds, metastables, and the fundamental research of gas phase ion chemistry. Additionally, he became interested in the biological aspects of science and began a mass spectrometry research program around peptides. While at Warwick, Jennings spent much time involved in department administration as the Chemistry department's chair. Ultimately he moved into the Biological Sciences department to further pursue his collaboration with Howard Dalton.

Jennings spends much time talking about the development of the mass spectrometry community in Great Britain, especially the contributions of John Beynon and the historical shift when chemists became interested in mass spectrometry after World War II. Jennings also discusses mass spectrometry curricula and his own teaching experiences both in England and abroad. Throughout the interview, Jennings talks about his research, teaching, and personal collaborations with many prominent members of the mass spectrometry community including Michael T. Bowers, Jean Futrell, Michael Barber, and Martin Elliott.

INTERVIEWER

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

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INTERVIEWER: **Michael A. Grayson**

LOCATION: **Leamington Spa**
Warwickshire, United Kingdom

DATE: **24 April 2008**

GRAYSON: I just want to start by saying that this is 24 April at Leamington Spa in the United Kingdom and I am interviewing Dr. Keith Jennings. As usual we're going to start with where he came from, his childhood, his parents and so on. So do you want to tell me a little bit about your parents?

JENNINGS: Yes, well, my parents, neither attended anything but elementary school. Both left at about fourteen. My mother then went to work in a hosiery factory until she was married. My father was an apprentice at a private engineering company.

GRAYSON: So this would've been in 19 –

JENNINGS: Well, they were born in 1904 and 1905 respectively.

GRAYSON: So in the late teens, early 1920s.

JENNINGS: They were First World War children. And then they got married about mid-1920s.

GRAYSON: Okay.

JENNINGS: I was born in 1932.

GRAYSON: Did you have any siblings in your household?

JENNINGS: No, I'm the only child.

GRAYSON: Okay. They had limited education, but how was their attitude towards your education?

JENNINGS: Oh, very supportive. My father, as I say, started as an apprentice, worked his way up and when he retired he was works manager of one of the branches, two branches of the company.

GRAYSON: And what company was that?

JENNINGS: It was Joseph Thompson Light Engineering Company in Sheffield. They made things like cutting wheels and abrasive things of that nature. Small engineering.

GRAYSON: So he had basically advanced to a pretty reasonable position in the company.

JENNINGS: Yes, he had. He was with the same company all his life.

GRAYSON: Something that doesn’t happen much today.

JENNINGS: No.

GRAYSON: That would’ve been about what, thirty or forty years that he worked there?

JENNINGS: Yes. He started I suppose fairly soon after school. He was certainly working there in the 1930s, because he was kept on during the Depression at a very low rate.

GRAYSON: How did the Depression affect England? I know in America it was really a big deal.

JENNINGS: Well, yes; in the early 1930s there was a lot of unemployment. But key people apparently were kept on at his company, and I think he certainly was kept on a retainer of like two pounds a week.

GRAYSON: Oh, my!

JENNINGS: But things improved once the war started.

GRAYSON: That was better than nothing. So they did support you educationally?

JENNINGS: Very much so. My father was never well-off. He never bought his own house because he said he preferred to keep what money he had available for any emergencies, and the line with me essentially was if I passed examinations which enabled me to go to higher education he would support me until I'd reached my limit.

GRAYSON: Okay. So examinations were at what age?

JENNINGS: Eleven, circa eleven plus year. Elementary school was grades five to eleven; I was given a lot of encouragement. I was one of the brighter people in the class, so I usually found myself at least a year ahead on arithmetic. And I was encouraged to read widely; I needed a local lending library, which in those days was extremely important because not a lot of us had many books at home. And the local lending library really was a great source of information, and it enabled you to expand your vocabulary and the use of English and so forth.

GRAYSON: So did you get any technical volumes from the library?

JENNINGS: Well, what happened was you went to the junior section to begin with, and once you had shown that you could read well and had expanded interests, then the library staff would allow you to go into the senior section, and that's where I first began to read science books, popular science books.

GRAYSON: This is popular science, isn't it?

JENNINGS: Oh, yes; I was certainly ten or eleven when I was doing this. So, I'd read about the solar system and things like that.

GRAYSON: And so the exam came up then.

JENNINGS: This exam came at the age of eleven, and in Sheffield at the time, if we passed the examination, we could go to a number of particular schools, and the better you did in the examination the higher grade school you could go to. And I was fortunate; I was one of twenty-five selected to go to the best grammar school in Sheffield, the King Edward VII Grammar School, which had an intake of twenty-five from Sheffield plus about another five from the surrounding districts. And then three other classes, all of whom came up from the junior school. So to begin with there were about one hundred-twenty boys in the first year of a boys-only grammar school. And we began in what was called the second form, although it was the first year there for us. The ranks were 2A, B, and C -- best, not so good, and rather less good. 2D, which we were, was the new intake, and at the end of the year we were all sorted out according to ability. And what normally happened is that 2A and 2D together each produced half of 3A, and so I got into that and progressed through school that way. This was an extremely good school; very academic.

GRAYSON: What was the name of the school?

JENNINGS: King Edward VII Grammar School.

GRAYSON: And this was in Sheffield.

JENNINGS: Yes.

GRAYSON: That's north of here?

JENNINGS: Yes, it's South Yorkshire. It's about ninety miles north of here. And King Edward VII School was founded in 1905 by an amalgamation of other schools. It began with the Sheffield Royal Grammar School, which I think began in 1604, and Wesley College, founded about 1838. And we were actually in the Wesley College building, and the Sheffield Collegiate School as well came into it somewhere; I can't remember the details. But it's still known as King Edward VII School, but it's no longer a grammar school because of educational reorganization.

GRAYSON: And that's when you first were exposed to science on an academic level?

JENNINGS: Yes. In the early years one did more general science, and the emphasis in the school really was the higher up in the pecking order you were academically the more likely you were to be pushed towards the physical sciences rather than the biological sciences.

GRAYSON: Interesting.

JENNINGS: So I did very little biology at school. After the age of fourteen we had to choose which stream we wanted to go into, and so I chose the science side. There were classics; there were modern studies, economics and so forth, and languages. But as a result I dropped geography, history, biology at the age of fourteen and my school certificate, which was taken at the age of fifteen, was taken in mathematics, chemistry, physics, English language, and English literature. Then French, German and Latin, which may sound a bit odd. But anybody wishing to go to Oxford in those days to read any subject had to have what was called a credit in Latin, and a credit was the second ranking – distinction, credit, pass. I got a pass, so I had to take it again [laughter].

And then after that at the age of fifteen I went on to the Lower Sixth, and there we did maths, physics, chemistry and scientific German plus current affairs in English to some extent. So really one was streamlined into the physical sciences at a quite early age.

GRAYSON: And then you were successful in this environment up to the graduation point, which would've been –

JENNINGS: Yes. I took Higher School Certificate; the last one, in 1950, and then my parents went to see the headmaster about what I might do, and I can still remember – I had a cousin in the Nottinghamshire area where my mother came from, who had a friend who was doing very well as a pharmacist. He had a couple of shops, you know, and was really doing really well economically. And this cousin of mine, who was a teacher, suggested I might follow the same route. So my parents went along to the headmaster and we thought of Nottingham Pharmacy, and the headmaster would have none of it. He said, "No, no, no; you've got to aim much higher than that. You must aim for Oxford."

So my parents, who had little experience at these things, of course, did as they were told. I was put in for a scholarship in November in 1950, which was known as the Hastings Scholarship. It was what was called a semi-closed scholarship. It was available to thirty schools in the north of England. It was established by the Lady Elizabeth Hastings, who, as we were told every year on Founder's Day, left sundry lands and money so the sons of poor northerners could proceed to a high place of learning – Queen's College Oxford. And my year the school did remarkably well; there were seven scholarships awarded, four of them came to my school, so two of us went in chemistry, and I forget – one was in History, and I forgot what the other one did. So that was late November I was told that I had a scholarship.

GRAYSON: Before we go forward, was there anyone of your teachers in your schooling prior to going to Oxford that influenced, had a particular influence on you?

JENNINGS: Yes, the Chemistry Master was quite personable. His street credibility was that he played cricket for Leicestershire second eleven, so he was regarded as quite “with it”. We had no means of checking up on that. But he very much told us that in any exam -- sell yourself; always sell yourself in examinations, and he taught you how to do well in examinations. You have to have the knowledge, but he taught you how to put it across. And chemistry was exciting. Physics, on the other hand, was taught by an elderly Master who joined the school shortly after the First World War, and whose physics was really late nineteenth century. He always referred to James Clerk Maxwell in full, never Maxwell, always James Clerk Maxwell, and we were shown experiments by him of the electromagnetic age. They were, post-Faraday but pre-quantum, if you see what I mean.

To give you an example, after I had passed my scholarship examination in November, I was allowed as a special favor to work with a new triode valve [vacuum tube] which we had and to plot its characteristics. This compares with a rival school, where a father of one of my friends was a Physics Master, who built radio sets. Now, we were not allowed to touch valves in case we broke them. And so physics was always a question of getting through the syllabus and making sure we did this and that. I learned quite a bit of my physics from library books, in fact. We had as a headmaster, a chap called A.W. Barton, who wrote one or two physics textbooks, and of course we had to have his. I found somebody else’s books rather more palatable; I learned from those, from the library again, the same one I was mentioning earlier.

GRAYSON: Now what was the name of your chemistry professor?

JENNINGS: The chemistry teacher was a Mr. Hargreaves. I can’t remember any more about him.

GRAYSON: I guess he was kind of a young swinging guy, compared to the physics teacher?

JENNINGS: Yes, I suppose he was in his forties. Whereas the Physics Master, Harry Redstone, retired I think as I left school.

GRAYSON: How do you spell his name?

JENNINGS: Redstone, R-E-D-S-T-O-N-E.

GRAYSON: Okay.

JENNINGS: He taught us a lot of physics, but it was always a job. He had to get through the syllabus and he'd worry we hadn't got this bit done, or that bit done.

GRAYSON: He didn't make it challenging or exciting or interesting.

JENNINGS: No. The Chemistry Master just sort of walked through it. Another good thing about the school was that we did an awful lot of practical work, which I think was extremely important. And nowadays we would not be allowed to do some of it because of health and safety regulations.

GRAYSON: Oh, yeah!

JENNINGS: And I suspect because of the cost. But practical was very important. Anyway, having passed the examination I was then thrust into the Army because we had to do national service, and it was a rather peculiar time because the government had just decided that rather than eighteen months national service we would have to do twenty-four. Now, you could imagine what effect that had on university intake numbers.

GRAYSON: Oh, yeah.

JENNINGS: And as a result the universities made a supplication which was allowed. If you had a place at the university you could do twenty-one months, not twenty-four. So having passed the examination in late November I was able to start in January for a year the following October. So I did twenty-one months national service in 1951-2.

GRAYSON: And national service consisted of?

JENNINGS: I joined REME, the Royal Electrical and Mechanical Engineers, R-E-M-E, and rather to my father's disappointment I refused to train as an officer; because one had an opportunity to train as a radio mechanic, and I found that more interesting.

GRAYSON: So yeah, the radio would've been a lot more interesting.

JENNINGS: And I had a stroke of really good luck by getting spots on my chest in August.

GRAYSON: Uh oh!

JENNINGS: Because I was in a teleprinter course when I got these spots, and I went to the MO's – "Oh, scarlet fever! You must go to Aldershot Isolation Hospital for three weeks," which I did. But my friends who were in the teleprinter course all had three weeks' embarkation leave in December and found themselves in Korea in January.

GRAYSON: Oh, my!

JENNINGS: I, of course, having missed three weeks of the course, had to wait until the next course came along, and I was then put not on a teleprinter course but on a mobile radio course. It was much more interesting. I learned all about valves and valve circuitry, and that was three years before the invention of the transistor, much to my annoyance.

GRAYSON: Okay. And the MO would be Medical Officer?

JENNINGS: Yes, sir.

GRAYSON: Okay.

JENNINGS: And so I finished my course in early 1952.

GRAYSON: So these red spots, they were – ?

JENNINGS: Oh, it was just a rash on my chest, that's all.

GRAYSON: So it wasn't really anything serious.

JENNINGS: He thought it was scarlet fever – whether it was or not I don't know, but I was perfectly well.

GRAYSON: Good [laughter].

JENNINGS: And I avoided Korea as a result.

GRAYSON: Yeah, that's interesting how a little strange event can change your life.

JENNINGS: So I then spent the rest of my time installing radio sets in tanks, checking the intercom, checking that everything worked before the tanks were allowed to proceed off into the western desert [Egypt and Libya] at the time. They had a lot of sandy-colored tanks to put things into.

GRAYSON: So there weren't any really hot wars going on at that time other than Korea.

JENNINGS: When I was demobilised, I met people who'd been in camps eight miles behind the front line repairing sets and so forth. But I was quite glad to stay in Britain.

GRAYSON: Oh, yes! So how big a force did Britain have in Korea? Do you know?

JENNINGS: I can't remember now, I'm afraid. But I was eventually allowed out in late September 1952, and two weeks later I was at Oxford.

GRAYSON: Okay. So this was a fairly prestigious move for you?

JENNINGS: Oh, yes!

GRAYSON: Academically, that's a non-trivial place. And you were at Queen's College?

JENNINGS: Queen's College Oxford, yes. From 1952 to 1958.

GRAYSON: What was good about that experience?

JENNINGS: Oh, a lot of things. My tutor was Jack Wilfrid Linnett, who went on to become Vice-Chancellor at Cambridge.

GRAYSON: Can you spell that for me?

JENNINGS: Jack, J.W., John Wilfrid, Linnett, L-I-N-N-E-T-T. He was very pleasant and a very understanding, very human person.

GRAYSON: When you say "tutor?"

JENNINGS: Tutor, yes. The way Oxford worked in those days was that you were taught in two different ways. You were taught by lectures centrally, and you had lab classes, centrally, but each college had – almost all colleges -- had a chemistry tutor. And the college would take perhaps six, eight, or ten chemists per year and the tutor would then give one-hour tutorials per week, usually to groups of two, possibly three. He would begin with a random selection of students, and then he would rapidly sort them into ability levels.

So I and another chap were fairly quickly put in as a pair because we were about the most serious of the chemists that year, and then you had weekly sessions with him. He would set you an essay, he'd read the essay, we'd talk about it. He would ask you about how you're getting on. He would talk about specific examples of chemistry, and you could ask him one-to-one questions.

GRAYSON: So that was basically a fairly focused type operation; it was the two of you and this other fellow.

JENNINGS: In the third year, you had individual tutorials. Now, he couldn't cover the whole of chemistry; he covered physical and inorganic chemistry, and another chap called R. M. Acheson, A-C-H-E-S-O-N – Morrin Acheson gave us organic tutorials. He came, I think, towards the end of the first year.

GRAYSON: So these guys do this as part of their graduate studies.

JENNINGS: Well, they were Fellows of the College. A College Fellowship requires them to take part in running the College and teaching in the College. They also have to have the usual university appointments – otherwise they can't afford to live. They work with students while they're doing their research and they're lecturing.

GRAYSON: So they really didn't get paid for the tutoring, but they had other means of income.

JENNINGS: Yes. They have to have really two employees – employers, rather – if you like. The university where they do their research and their lecturing, and the college where they do their tutorial work and the administration, running of the college and so forth.

GRAYSON: And this is part of their obligation to be a Fellow.?

JENNINGS: Yes. So I progressed through the three years, was allowed to graduate, and in Oxford one takes one's finals after three years, and in those days you also took practicals, which have now been abandoned. Your whole future depended on a very short period. I remember we took physical – I think it was physical chemistry first, Thursday morning and afternoon, inorganic Friday morning and afternoon, organic Saturday morning and afternoon. That was it. Three years' work examined in three days. Then you had a week off. Then you had eight-hour practicals on Monday, Wednesday and Friday.

GRAYSON: And what's in the practical?

JENNINGS: Well, you were asked to prepare something perhaps, or carry out some measurements.

GRAYSON: So you'd do kind of a lab experiment.

JENNINGS: Yes, it was an eight-hour practical work where you're set a problem. You might for example be asked to prepare a particular compound and then prepare derivatives of that compound which exemplify its properties.

GRAYSON: Wow.

JENNINGS: So one did one's best and hopefully got through and –

GRAYSON: Yeah, it would've been a challenge, it sounds like.

JENNINGS: Now, after that you disappeared for the summer and eventually got a letter from your tutor telling you how you'd got on. But of course you hadn't finished, because that merely let you through to what was called a B.A. Honors Unclassified.

GRAYSON: Okay.

JENNINGS: A B.A. notice; everybody had a B.A. in Oxford.

GRAYSON: Right, but I mean all of this education is focused on chemistry.

JENNINGS: Yes. Oxford was unusual, and very different from most universities, where you did 100 percent chemistry from Day One. So you ended up knowing a lot of chemistry, hopefully, but not a lot of other things.

GRAYSON: Okay.

JENNINGS: So at the end of that time you had a good idea where you stood in ranking and your tutor would give you advice, and I was told I had quite a good chance of a first but I needed to polish things up a bit. Then you do a fourth year of research work, called the Part 2, and the Part 2 you did working with a particular man – it was like the first year of a Ph.D. in most universities. So after some thought I elected to work with Jack Linnett. I nearly worked with Morrin Acheson. He was a very personable chap, but I decided physical was my area.

And I worked on nitrogen atom reactions, a discharge to produce active nitrogen, it was called in those days, and flowed this into a reaction vessel and admitted organic compounds. Then we got our atomic flame, and I was then able to look at the spectroscopy of the flame, and quite by chance we discovered a new system.

GRAYSON: Oh!

JENNINGS: There are some bands about 3290 Å which we couldn't identify, and with our relatively limited equipment – medium quartz spectrograph was the best we had -- and that we had to borrow. All we could say was that it was either CN_2 or C_2N. We could eliminate hydrogen because we tried deuterium and nothing changed.

GRAYSON: Sure.

JENNINGS: And later on I talked to Ramsey in Herzberg's lab at NRC and they took it up and it was in fact symmetrical NCN.

GRAYSON: This was Herzberg's lab?

JENNINGS: Yes, Don Ramsey worked at NRC and in a general high-resolution spectroscopy lab there. And they followed it up and were able to identify it with very much better equipment than we had.

GRAYSON: Sure.

JENNINGS: Anyway, as a result of that I wrote up my Part 2, had my oral exam, and managed to get a First, so I was quite pleased about that.

GRAYSON: Yes, so what did you do in the oral? How long was that? Several hours?

JENNINGS: No, not quite that long. Everybody gets an oral no matter where they are in the list, so if you're right in the middle of the list – you come in and have a twenty-minute chat with them about what you're going to do and how you found the course and so on. In my case it was probably about forty minutes where I was asked to explain how a particular gauge worked and this sort of stuff. I remember being asked, "What was the likely shape of the ethylene molecule in its first electronically excited state?" Now, what I did was this. [Jennings uses his hands to show the shape.]

See, that's ground state; and this is the first excited state. Everybody burst out laughing – "Ah, a Linnett man!" Because Jack Linnett was very well known for using his body to show molecular structure – I don't know whether it's relevant to this, but I can show you how the CO_2 molecule vibrates.

GRAYSON: Let me get my camera and get a picture of the excited state of ethylene because it doesn't come across on the tape recorder. All right, so the first excited state would be like –

JENNINGS: First electronic excited state of ethylene – that said, it was that shape.

GRAYSON: Okay, and then if you had – if it wasn't excited it would be –

JENNINGS: That shape.

GRAYSON: Okay, now you're talking about CO_2 . Right; okay.

JENNINGS: Now, Jack Linnett used to work on CO_2 vibrations, and he would say well, this is the symmetrical stretch vibration. The best was the asymmetric stretch, which was this one.

GRAYSON: I see [laughter].

JENNINGS: Then I remember one tutor said, "I've just read Ingold's paper. The first excited state of acetylene is this shape." H-C-C-H.[1]

GRAYSON: Oh, the C is underneath.

JENNINGS: Yes. So as I say, Jack was well known for his internal rotations, and saying, "The molecule is like this?" you see?

GRAYSON: Yes, yes.

JENNINGS: And so that was my final question, I still remember it. They all just burst out laughing. It was a good sign [laughter].

GRAYSON: But it conveys the information.

JENNINGS: So that was the end of Oxford then. Oh no, sorry, then I went on to my D. Phil. That was in the summer of 1956 that I got my First. And then 1956 to 1958 I carried on working with Jack Linnett on what must be about the simplest possible reaction you can think of. H plus 'wall' gives a half [H_2], because our interest was in how different surfaces catalyzed a heterogeneous combination of hydrogen atoms. The reason for that is the hydrogen and oxygen reaction at low pressures has an explosion limit which is surface-dependent, and you could correlate the explosion limit pressure with the surface activity. And our measurements were essentially measurement of the surface activity directly.

GRAYSON: So you were using some spectroscopic techniques for that?

JENNINGS: No, no. We used a diffusion technique, imagine a discharge tube and then a flow tube and then at right angles a tube down into which the atoms could diffuse.

GRAYSON: Okay.

JENNINGS: Imagine then two gauges – we won't discuss how they work – but two gauges like that. But in between we would put a small tube which we would coat with different surfaces.

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[1] G. W. King and C. K. Ingold, "The bent excited state of acetylene," *Nature* 169 (1952): 1101-2.

We'd measure the atom concentration there, the atom concentration here. If the surface was not very reactive, of course these two would not be very different, and if they were acid-washed that was what we found. If they're alkaline-washed, then the surface was pretty active and the concentration as they diffused fell quite rapidly. So we were able to look at about six or eight surfaces and correlate the activity of the low pressure explosion limit. So at this stage you might wonder when mass spectrometry's starting. I'll tell you my first experience in mass spectrometry. When I was working on the active nitrogen project, we needed to know if our new system had hydrogen in it.

Well, the way to check that is of course to switch hydrogen to deuterium, and the simplest way to work with that we decided was nitrogen atoms into acetylene. So I had to make C_2D_2. Well that, as you can imagine, is extremely difficult. You've got some calcium carbide and you pass some D_2O vapor backwards and forwards through it. You throw away the first lot because of the surface contamination, then collect the rest. Jack said, "Well, we'd better have it analyzed, see how good it is." We then went to the mass spec service and were delighted when they said it was 97.5 percent pure and 2.5 percent C_2HD, so that was regarded as a success. I was quite enamored of mass spectrometry at the time. Well, that was the nearest one got, and I'll come back to some aspect of mass spectrometry later, I think.

GRAYSON: This was in the late 1950s that you were doing this; 1958.

JENNINGS: Yes.

GRAYSON: Do you have any idea what kind of group that was that did the analysis?

JENNINGS: It was an MS2 mass spectrometer. John Danby did it in the Physical Chemistry Lab.

GRAYSON: This is still while you were at Oxford?

JENNINGS: Yes. He worked in the Hinshelwood group in the Physical Chemistry Lab. Now, I'd been thinking what to do with myself afterwards, and I'd applied to National Bureau of Standards [in the United States] to work with H. P. Broida on atomic reactions. But although I was perfectly normal it took them about six months to decide that I was not going to try and overthrow the United States government, and as a result I got a lot of offers while I was waiting. I applied also to the National Research Council, Ottawa, to work either with Fred Lossing, whom I'd come across and was quite interested in his work. Or with Bob Cvetanovic – he's C-V-E-T-A-N-O-V-I-C, R. J. Cvetanovic.

GRAYSON: Okay. This is in Canada.

JENNINGS: Yes. National Research Council, Ottawa.

GRAYSON: So all of these places that had things you were interested in were in Canada?

JENNINGS: No, Broida was at the National Bureau of Standards, Washington.

GRAYSON: NBS, they call it the National Institute of Standards and Technology today.

JENNINGS: Oh, NIST; that's right, of course. NIST – I remember. Anyway, Lossing had experienced mass spectroscopists apply to him, so I didn't get to go with him. I went with Bob instead. I did two years working on mercury sensitized gas kinetics, which was very interesting. He was a very nice chap to work for, and I enjoyed my two years there.

GRAYSON: This is Cveta –

JENNINGS: Cvetanovic.

GRAYSON: And he's in Canada. And so the selection, what guided your choices there?

JENNINGS: Really talking with Jack Linnett about what were good places to go to, good people to work with, and my interests at the time were still very much in gas kinetics. As I say, I enjoyed two years working with Bob [Cvetanovic] as a photochemist. But of course being at NRC I was able to go to lectures by many eminent people: Fred Lossing, Herzberg, Don Ramsey, various other people, and that was really quite a stimulating two years.

GRAYSON: So the NRC is located in –

JENNINGS: Ottawa. There are two branches. I was in what you would call the annex. There was the original NRC building in central Ottawa, which was where most of the older laboratories were, and then the annex on Montreal Road. Which is, oh, I would say it's three miles out of the city center. That was the Applied Chemistry Division.

GRAYSON: And this would've been in 19 –

JENNINGS: 1958 to 1960.

GRAYSON: 1958 to 1960. Was this a post-doctoral appointment.

JENNINGS: Yes, it was. They had a very enlightened post-doctoral program with people from all over the world, which they cut back in the late 1960s because it was too expensive. One met people from various European countries, from Japan and other parts of America, so it was quite a cosmopolitan group, and really high standards. Then I had to get myself a job back in Britain, and I applied to one or two places and I also had a look at staying in America, but I had family reasons I wanted to go back home. I eventually was interviewed by George Porter, who was then professor of physical chemistry at Sheffield, and I remember he said, well, he was going to be in New York at a particular time, could I arrange to be around then? I think we had lunch at the Waldorf Astoria. I was interviewed during this period and offered a job, which I took.

GRAYSON: This was a job back in the U.K.?

JENNINGS: Yes, as an assistant lecturer, which is the lowest of the low, at the prodigious salary of eight hundred pounds a year.

GRAYSON: Oh, my.

JENNINGS: But happily there was a huge salary increase before I got here; it went up to nine hundred pounds a year.

GRAYSON: Great. Well, that's a 12.5 percent increase or whatever!

JENNINGS: And I had an overdraft on my bank account within about six months [laughter].

GRAYSON: So you were a lecturer at what institution?

JENNINGS: At University of Sheffield. Back in the hometown. But that was purely fortuitous. I had offers from other places, but I thought this was the best long-term prospect.

GRAYSON: And this was close to home, wasn't it?

JENNINGS: Yes, very close to home, yes.

GRAYSON: So you're down here.

JENNINGS: But that was purely coincidental. It was I think the best offer, and it was a good department in those days, very good. So I came back to Sheffield and stayed there for twelve years.

GRAYSON: So did your parents get a chance to enjoy your success?

JENNINGS: Oh, yes, we saw them most weeks. And I'd really got into mass spectrometry until, I suppose, after about two years in Sheffield, two to three years. I wanted something to look at the products of my gas kinetics, and the department didn't have a mass spectrometer. In those days you were encouraged to build your own –

GRAYSON: Sure.

JENNINGS: GLCs [Gas Liquid Chromatographs] and so on, so I eventually managed to persuade them to buy me an MS10. I don't know if you're familiar with MS10 mass spectrometers.

GRAYSON: Not really.

JENNINGS: Well, it was a very simple machine. It was a voltage scanning, permanent magnet instrument with a two-inch radius and a mass range up to one hundered. As sold it would scan from mass twelve to mass forty-five in thirteen and one half minutes. And then mass forty to mass one hundred in another thirteen and one half minutes. It had a potentiometer which went round four times an hour, so fifteen-minutes per revolution. It wasn't connected for about one and one half minutes. So at one end you got mass twelve and all the way 'round to mass forty or, forty-five rather. Then it would disconnect and go around again, or you'd change the range then. So this was my first mass spectrometer.

GRAYSON: It was a really high-powered piece of equipment [ironically]!

JENNINGS: Yes. So I promptly bought a motor which went around fifteen times in an hour [laughter].

GRAYSON: So that's almost four times faster!

JENNINGS: It caused some peak clipping, I'm afraid.

GRAYSON: What did it cost?

JENNINGS: Well, it depends what you regarded as part of the instrument. I mean, you could buy bits; you could buy a diffusion pump. You could bolt it onto a vacuum system, but it would probably be about three thousand pounds in total.

GRAYSON: So I just want to back up a little bit – as assistant lecturer, what were your responsibilities?

JENNINGS: Well, you begin by having a modest lecturing load, usually with several of the elementary classes, plus one advanced course just to introduce you to the more senior students, and you get quite a lot of elementary practical classes too. I mean, we had something there that was the Inter. B.Sc. class, which is the first year class for non-specialist chemists – people that are doing physics or biology or psychology or the like, and you look after them.

GRAYSON: And at the same time you were responsible for starting to develop a research program?

JENNINGS: Yes, yes.

GRAYSON: Okay. So this was the typical kind of starting professor type activity.

JENNINGS: Yes, and after the first year you are promoted to lecturer. I mean, they have you as an assistant the first year just to see if you're sensible, etc.

GRAYSON: Right. It's a stamp of approval kind of a thing.

JENNINGS: Yes, sure.

GRAYSON: That's pretty low man on the pole, isn't it? As a lecturer you are still at the bottom of the power structure.

JENNINGS: Oh, yes. So then you slowly climb up increment by increment.

GRAYSON: So how did you get money to do your research?

JENNINGS: Well, the department would provide a modest amount, and you could apply to places like the Royal Society for a small amount. As an assistant lecturer you probably couldn't get much from the research councils at that time, and indeed I didn't need a lot because I built my own apparatus. I could do my own glass-blowing, so I could make my own vacuum lines and so forth. And only the more complicated bits of glassware you got from your glass-blower; you built them into your apparatus yourself. And provided you had a good rotary pump, an efficient pump, that was okay, and a mercury lamp. So it wasn't a very expensive way to start. I worked on the addition of hydrogen and oxygen atoms to fluorinated alkenes. I chose that because the fluorine would make quite a big change to the electron distribution of double bonds and also they would remain as gases, which of course chlorinated compounds might not have. So this is where I needed a simple mass spectrometer, and it was interesting that I had fluorinated compounds, because that got me into mass spectrometry. One of the first things I did was to put vinyl fluoride, or fluoroethylene, into the mass spectrometer. That had a mass of forty-six – well, you could just stretch the lower mass range to cover that, so you'd do mass twelve to forty-six.

And you can imagine my horror when I saw the thing start at twelve, thirteen, then fourteen, fifteen, sixteen, seventeen, eighteen, nineteen. I thought, "What is this big trapezoidal bump on the baseline?" And I'd read a bit about, it reflecting things, and ions bouncing off bits of hardware in the flight tube. I thought it was probably a problem like that. So I put ethanol in, again mass forty-six, and got a different sort of spectrum, with a flat baseline where the bump was in vinyl fluoride. So the trapezoidal bump was something to do with the vinyl fluoride. It was a metastable transition, which I'd not heard of at the time, so I read all about it. It was due to the loss of HF by the molecular ion. At about this time, John Beynon was talking about metastable transitions on the MS9, so I began to get involved with listening to him, reading about his work, and realized I was doing the same thing on my extremely simple MS10.

Now, as it happens, the width of the metastable peak increases as one over the square root of the accelerating voltage. An MS9 typically worked at eight thousand volts, so the metastable peaks were pretty narrow. On the MS10 I was scanning at around three hundred volts, so instead of being a third of a mass unit wide it was three and one half mass units wide, and that's why I got such a huge bump.

GRAYSON: Sure.

JENNINGS: So I put in a number of other fluoroalkenes, and again got bumps in the right places, so suddenly I found I was looking at broad metastable peaks. And this led me to read up in literature about a number of people's work, so certainly Beynon's, and another chap whose name I can't remember now who I think taught Jean Futrell.

GRAYSON: I think I know who you're talking about, but I can't recall the name [Amos Newton].

JENNINGS: But he'd worked on doubly-charged aromatics, and I thought of a nice experiment with my MS10 which you couldn't do on better mass spectrometers. If you put acetone and benzene in together, and you look at mass fifteen, you'll find a doublet, because the acetone produces thermal CH_3^+ ions and they come out at the normal accelerating voltage. The benzene, because it fragments and releases 2.7 eV of energy, of which 2.2 is carried by the methyl ion, all of your methyl ions are sort of lost, except those that are coming out through the slit and they're coming out through the slit with 2.2 volts of energy. So you need 2.2 volts less to bring them to focus. And I was able to measure this with a meter.

GRAYSON: And this is because of the accelerating voltage scan.

JENNINGS: Yes, and also because it was low. And you could then turn the electron volts down so you stop making doubly-charged benzene ions and sure enough, your doublet became a singlet. So this got me into looking at doubly-charged ions as well on my machine, and about that time the department decided it needed a proper mass spectrometer, so since I was the only person in the department who had any experience in mass spectrometry, I was put on a committee to help select one. And we had a couple of organic chemists plus myself go and have a look at various instruments before we bought the MS9.

GRAYSON: So how big was the department at this point?

JENNINGS: I imagine it would be about thirty strong. I can't remember exactly, but of that order.

GRAYSON: And this was beginning in the 1960s, early 1960s?

JENNINGS: Yes, we're talking now about 1963. Because our MS9 was delivered in November 1964. It was number thirty-three, I remember.

GRAYSON: So that was probably a big day in the department.

JENNINGS: Oh, yes. And now one had to start getting involved really with other mass spectroscopists and learning a bit about what one could do and what the pros and cons of different instruments were and so forth. And this led to my joining a couple of groups; there were very similar names. There was a Mass Spectroscopy Group, or Mass Spectrometry Group, I'm not sure which, and a Mass Spectrometry Discussion Group. The latter were known as the lunatic fringe, because they're the ones who took the sides of the instruments off, bored holes in it, added extra knobs, took the hacksaw to it, and so forth; and generally advanced the technique because they modified instruments. And there was no society, you see.

GRAYSON: But they were two separate groups.

JENNINGS: Yes. They were informal groups. I mean, John Beynon would host a meeting, say, at ICI [Imperial Chemical Industry] Blakeley, and maybe twenty or thirty would turn up. And at the time – as I say, it was probably true in the States – manufacturers' user groups were perhaps the major source of meeting other mass spectroscopists.

GRAYSON: Yes.

JENNINGS: So one would go to the MS9 User Groups in Manchester at what was initially Metropolitan Vickers before it became AEI [Associated Electrical Industries], and you'd meet people there of course with the same instrument, and so any modification they'd done you could consider if you wanted to do and vice versa. So the company gained by listening to what everybody else was doing with their instruments. And the company would incorporate some, and you'd decide if you wanted to invent your own.

GRAYSON: Were these held at random times, or were they regularly scheduled?

JENNINGS: Fairly regular, I think they had about one a year, the users. But the others might meet two or three times a year.

GRAYSON: And so the users would primarily have the MS9, or the people that had equipment from this company, Metropolitan Vickers, or whatever it was at that time.

JENNINGS: Yes.

GRAYSON: Do you have a rough idea of what the attendance was for those?

JENNINGS: Well, of the order of magnitude of something like fifty, I suppose.

GRAYSON: Okay, so it was a pretty good-sized –

JENNINGS: Oh, yes, yes.

GRAYSON: And so were they all MS9 users, or people using various units?

JENNINGS: Well, there was an MS12, which was like part of an MS9.

GRAYSON: Yeah.

JENNINGS: The MS9 without the electric sector.

GRAYSON: Right. So there were people using that level of instrumentation.

JENNINGS: Yes, yes.

GRAYSON: Either single or double-focusing instruments.

JENNINGS: And so one tended to get to know the technical people there, and when we come on to later I can talk more about that. But you see at that stage that was really the only contact one had with other mass spectroscopists and it was about that time that I was able to make really quite a leap forward in getting to know people because we had in the department a chap called Brian Stevens, who was a photochemist.

GRAYSON: Stevens?

JENNINGS: Yes, Brian Stevens; he went later to Florida. But he had a friend, Jean DuBois, who was a photochemist at Wright Patterson Air Force Base. I met him when he came over and I talked about the problems of getting to know other mass spectroscopists. And he said, "Well, we've got a group at Wright Patterson. I could probably arrange for you to spend some time there. A chap called Jean Futrell works there." And as a result, in the summer of 1965 I went to work with Jean for about three months.

GRAYSON: Okay, he was at Wright Patterson then. And he was working on his crossed-beam experiment?

JENNINGS: His big ARL [Aerospace Research Lab] tandem instrument. So I went to work on that for three months, and that was my introduction to the American scene, so to speak.

GRAYSON: I see.

JENNINGS: And I worked with Jean and we produced a paper which went into *J. Chem. Phys* [*Journal of Chemical Physics*].[2] And Jean then wrote to me and said, "Well, why don't you come back next year and present the work at the ASTM E-14 [American Society for Testing and Materials, Committee E-14 on Mass Spectrometry]," which I did. And that was the first of about I suppose twenty-five visits or such.

GRAYSON: Do you remember where it was held? Probably in Dallas?

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[2] K. R. Jennings and J. H. Futrell, "The decomposition of tropylium and substituted tropylium ions," *Journal of Chemical Physics* 44 (1966): 4315-9.

JENNINGS: It might've been; it was the summer of 1966.

GRAYSON: Yeah, those were probably in Texas.

JENNINGS: I think it was in Dallas.

GRAYSON: Yeah, they had a couple of meetings in a row there. I guess they liked something about the place.

JENNINGS: Yes. So I then met Tom Tiernan, who also worked with Jean, and shortly after, I'm afraid, Jean DuBois was killed in an air crash. I think a plane from Cincinnati to their local airport didn't make it. I knew his wife; she was waiting at the airport for the plane to come and it never turned up.

GRAYSON: Ugly.

JENNINGS: Yes. So anyway, that introduced me to the American scene. I met a lot of people at the meetings and as a result whenever I went over I tried to visit a number of laboratories just to meet people, get experience, see what they were doing, tell them about our work, and rapidly got into the habit of going to the States most years.

GRAYSON: Yeah, so would you plan to stay later than the meeting, or go earlier so you'd have more opportunity to interact, or did you –

JENNINGS: Yes. I might go say a week before –

GRAYSON: Oh, okay.

JENNINGS: Or two weeks before, probably have about a three week trip –

GRAYSON: Okay.

JENNINGS: Ending with coming home after the meeting because I was so drained after that.

GRAYSON: Sure. Well, it gives you a chance to get your clock reset before you get into serious discussions with people.

JENNINGS: Yes. And usually I used to begin going to Dayton and got to spend the weekend with Jean Futrell and talk to a number of people.

GRAYSON: Check up on how things were progressing.

JENNINGS: Yes. Give a seminar and find out what they were doing.

GRAYSON: So what – I understand he created this instrument. It was a crossed-beam machine, wasn't it?

JENNINGS: Yes, they were crossed-beam machines, yes.

GRAYSON: So he was trying to get some what, ion/ion chemistry going?

JENNINGS: Yes, or select his reactants and products and then look at the angular dependence of the products.

GRAYSON: Okay.

JENNINGS: I didn't work on that at all. I simply used his ARL as a normal double-focusing machine.

GRAYSON: Ah, okay. Now was that based on a commercial instrument or was it all home built?

JENNINGS: It was a CEC 21-110, but with bits added.

GRAYSON: Yeah.

JENNINGS: And I was looking at the relative intensities of normal ions made in the source, and the products of metastable decompositions made in the flight tube as a function of the internal energy of the ions produced, where you're able to make the same ions from a variety of sources knowing the heats of formation. And the higher the heat of formation the more likely the ions were to decompose in the source, so your intensity ratio of metastable ions to source ions fell. But if you had just enough energy to make them, of course you got many more metastable ions rather than source ions, so you could correlate internal energy with this ratio.

GRAYSON: So you were actually working there for three months. Did you ever do any more research, at Wright Patterson?

JENNINGS: I don't think so, because Jean left to go to Delaware not too long afterwards. So what's that led us to? That's got us to late 1960s, hasn't it?

GRAYSON: Yeah.

JENNINGS: Well, I continued to work in Sheffield with the MS9, and –

GRAYSON: Now, were you recruiting students along the way, or?

JENNINGS: Oh, yes, yes; I had quite a few students by then.

GRAYSON: So when did you really start picking up students?

JENNINGS: My first student began in gas kinetics in 1962, but I suppose I started with mass spectrometry about 1966 or so.

GRAYSON: Okay.

JENNINGS: And we then come to something which you have asked me about. We may as well deal with it now.

GRAYSON: Oh, okay.

JENNINGS: How'd I get into ICR [Ion Cyclotron Resonance]? Because of my interest in – well, you might call it time-dependent fragmentation of ions, looking at them in the first field-free region and the second field-free region of a double-focusing instrument – I thought it'd be nice if one could somehow slow them down and look at them as a function of time, and what fraction would be decomposing, and vary the internal energy with which you made them and so forth. So it seemed to be that a Time-of-Flight instrument was quite good to do this, with a nice long flight tube. And the idea I came up with was to have something like a little railway in this tube with some retarding grids and some accelerating grids so you could move them backwards and forwards down the tube and look at decompositions within a particular time range as a function of the length of the tube, you see.

So this went into SRC [Science Research Council], and – whatever they were at the time – SRC, and I'd asked, I remember, for about nine thousand pounds to buy a demonstration model Time-of-Flight instrument from Bendix.

GRAYSON: Um hm. The SRC is?

JENNINGS: Science Research Council. They're the major source of funding for pure research.

GRAYSON: In the U.K.

JENNINGS: Yes.

GRAYSON: All right.

JENNINGS: So nothing was heard for a while. Then I had a phone call saying, "Have you heard of the Syrotron? It's a Varian ion-cyclotron resonance instrument," and such. Oh!

GRAYSON: How do you spell that?

JENNINGS: S-Y-R-O-T-R-O-N.

GRAYSON: Syrotron.

JENNINGS: It was the first Varian commercial ICR instrument.

GRAYSON: Yes; this was Llewellyn's design, I think.

JENNINGS: Yes, Peter Llewellyn –

GRAYSON: Yeah.

JENNINGS: Was selling it as well.

GRAYSON: So they call it a Syrotron.

JENNINGS: Syrotron, yes.

GRAYSON: It was an ICR box.

JENNINGS: Yes. And I said, "No, I haven't heard of this. And he said, "Well, it's been suggested that you can look at ions in an ICR instrument. You know, keep them over the weekend if you want. I assume you're interested in length of time; would you be interested in having one of these?" So I said, "Well, I'd have to learn more about it," and so on.

GRAYSON: Yeah.

JENNINGS: And it transpired that John Beynon had been to the States. He'd had my proposal to referee. He'd seen this ICR instrument – as he said to me afterward, "I've seen so many Bendix Time-of-Flight instruments that don't work that you don't want to get involved with one of those. But this is a new instrument and Britain ought to have one. So I thought you'd be the ideal guy to have one, so I suggested that you have this instead of a Bendix Time-of-Flight." So I said, "All right, fine." So anyway SRC said well, would I agree? I said, "Yes, yes; that's fine." So they said, "Well, you applied for nine thousand. You wouldn't mind if we doubled your grant then to about nineteen thousand?" "No, no, it's the first time it's ever happened to me!" [laughter].

GRAYSON: "No, I don't want the money" [sarcastically].

JENNINGS: So along came this ICR instrument.

GRAYSON: And this one would've been one of the earliest ones, I would think.

JENNINGS: Well, yes, it was the first one in Britain at the time, and there was one in Bremen as well. And at the time it was rather amusing because it was very much a California [John D.] Baldeschwieler based technique, and the myth went around: Anybody east of the Rockies needn't bother with it because they wouldn't know what they were doing. Jay Henis, I remember had one, and he felt a bit of an outsider and Maurice Bursey of North Carolina had one. Well, you can imagine what we felt like over here.

GRAYSON: Yeah.

JENNINGS: You'd talk to Varian about the instrument and they knew even less than you did about their machine. So eventually the bible was produced; it was Jack Beauchamp's thesis. And we all had a copy of that, which worried us to some extent because it was so erudite. And of course all the questions you ask about magnetic deflection instruments are irrelevant.

GRAYSON: Exactly.

JENNINGS: I remember talking to Peter Llewellyn, saying, "Yes, but what size slits are there?" He said, "There aren't any. There's a one-inch square hole." Things like this. And anyway, we got the ICR machine and it wasn't really useful for looking at ion decays over a long period at all, so we got into ion-molecule reactions instead. Since I'd worked on atomic and free radical reactions that was a reasonable extension to what I'd wanted to do. And we had some ups and downs with the machine; I remember one of their servicemen came along and tried to unscrew something on the flange. It eventually came apart, but he hadn't realized it was welded. It had to go back to the States to be repaired.

And it was about that time that we got into touch with Mike [Michael T.] Bowers, because we found that we're working on rather similar systems. He was working on ion-molecule reactions in simple alkenes, and we were doing the same on fluoroalkenes. It was just an extension of my photochemistry work. And two of my students, or a student and a post-doc, visited him one year when I couldn't go for some reason – I think my father was ill. And that began a longstanding association with Mike. So that's how I got into ICR and then into ion-molecule reactions.

GRAYSON: The machine wasn't good for what you wanted to do originally, but it was a good machine for ion-molecule work.

JENNINGS: Oh, yes. We did quite a bit of ion-molecule work, and then in about 1967 I got promotion from lecturer to reader, which is I suppose like an associate professor in the States.

GRAYSON: Actually, it sounds like a step down, reader compared to lecturer, but I guess, I mean, it's all in the way that the terms are used.

JENNINGS: But then five years later I moved to Warwick to a Chair of Chemistry here.

GRAYSON: So you came in as a chair?

JENNINGS: Yes. And I brought with me my ICR machine, and I'd just got a grant for an MS50 mass spectrometer, which was eventually delivered here. It was very late in coming, but eventually it came.

GRAYSON: So let me just hang loose here for a second. You had the MS9.

JENNINGS: Well, that was a departmental instrument. You see, in those days you could not get an MS9 for your own use. You had to make the case for the department.

GRAYSON: Okay.

JENNINGS: And consequently all my research work was done outside normal working hours.

GRAYSON: Oh!

JENNINGS: During the day a technician would be running standard samples. He would knock off at 5:30 p.m.. I would perhaps have an early meal, come in at 6:00 p.m. and stay till midnight.

GRAYSON: So the instrument was there, but you had to get in line to use it.

JENNINGS: Yes. It was primarily a service instrument which you could use at weekends and evenings for research -- or overnight.

GRAYSON: Did you have any conflicts with the service guy that was working on it, because you would do something on the weekend that would mess it up?

JENNINGS: Oh, no, no. The only conflict you had was if you wanted to modify it in such a manner that it might interfere with service work. I mean, certainly we did one or two modifications which I think the service people were a little worried about. I had to assure them that it wouldn't affect their service work. We did quite a few modifications, but almost all of them could be undone, especially if they were electrical. You wired in a switch. Generally it was messing about throwing the switch or not, as the case may be. I remember the monitor slit quite well. The monitor slit was two hundred 'thou' [thousandths of an inch] wide, and as a result the energy bandpass was quite large. We needed a better shape of metastable peaks. What one needed was a smaller energy bandpass, so I said to AEI, as they were then, "Could I have a variable monitor slit please?" They were very embarrassed, and they said, "Well, we're making one, but we promised the first one to John Beynon. So you can't have it until he's got his." But they said, "You could do something else. You could take the monitor slit out and braze onto it a fixed narrow monitor slit and put it back." It wouldn't be variable. So we took it out and halved the slit, put it back in. So the service people were very worried, but it didn't make any difference to them at all, but I got better shapes for my metastable peaks before I got my variable monitor slit.

GRAYSON: So you didn't have a variable slit, but you had a smaller slit.

JENNINGS: Yes. And then another thing I did at the time, I remember reading some of Rosentock's work about quasi-equilibrium theory, and how they tried to model fragmentation. Among the worries they always had was whether or not a particular decomposition was unimolecular or collision-induced. And if you had a very small metastable peak, was it because your vacuum wasn't very good. I decided you could turn this 'round and say, "Well, if you're worried about collision induced adding peaks, let's really go for it and put a lot of pressure in and add a lot of peaks."

Now, how do you do that? What I did was to loosen the flange slightly and call it a leak. That was how I did my first collision induced work. But then I had a better idea; what one did every night was bake the electric sector plates to make them cleaner. Well, when you baked them of course all sorts of muck came off, so rather than having a leak, I just baked the electric sector plates and whatever stuff came off the plates was my collision gas. So that pleased everybody – cleaned up the instrument and at the same time I could do my collision induced decomposition, because I argued at the time – not quite correctly – that when you're going at eight thousand electron volts the ions didn't really mind what they hit as long as they hit something.

GRAYSON: So there was enough gunk in the instrument that you would always evaporate something for a collision gas.

JENNINGS: You didn't have to make the pressure very high. You normally worked at something like 1×10^{-7} to 1×10^{-6} Torr in the analyzer, where if you put it up to say 1×10^{-5} then you got plenty of peaks. When I went to Wright Patterson, Jean [Futrell] was very upset that I'd done this, you know, laughingly. He'd just spent about ten thousand dollars having a beautiful gas system installed with taps and everything so he could control how much gas it put in and what sort of gas it put in. So he had this big instrument, and I said, "You know, muck comes off and you can look at the decomposition." So again, that's how I started my collision induced decomposition work.

GRAYSON: So you were able to do this work at Sheffield and basically you had to work in the off-hours.

JENNINGS: Yes.

GRAYSON: And then eventually you did get the ICR machine while you were at Sheffield as well.

JENNINGS: Yes. Then because I'd done all this work with metastables and collision induced decomposition work, I was eventually able to get my own instrument, which is the one that was installed at Warwick shortly after I got here. And that was used for a wide variety of things.

GRAYSON: When you left Sheffield you had advanced beyond reader?

JENNINGS: Well, yes. I was reader in Sheffield for five years, and then became professor here [Warwick].

GRAYSON: Okay, and Chair as well?

JENNINGS: Well, yes, that's the second thing. So I suppose at the time I was professor of physical chemistry; we eventually just called it professor of chemistry. So from 1972 onwards until 1997 I was chairing chemistry on and off for fifteen years.

GRAYSON: So that's like twenty-five years in total.

JENNINGS: Hm.

GRAYSON: Okay. Well, why don't we – this is a good spot to halt for today.

JENNINGS: Okay.

[END OF AUDIO, FILE 1.1]

[END OF INTERVIEW]

INTERVIEWEE: **Keith Jennings**

INTERVIEWER: **Michael A. Grayson**

LOCATION: **Leamington Spa**
Warwickshire, United Kingdom

DATE: **25 April 2008**

[Recording begins discussing the King Edward the VII School that Jennings attended in his youth.]

JENNINGS: King Edward the VII school.

GRAYSON: It seemed like it was part of their program to encourage and place as many students in these higher level universities in the U. K. as possible.

JENNINGS: Very much so. We had what was known as the Seventh Club at Oxford.

GRAYSON: Seventh Club?

JENNINGS: Yes, that's King Edward VII, you see? And typically it had a membership of between forty and fifty. Now, they were the people from the school who were still in Oxford, in the three-year undergraduate program, and then the post-graduate program as well. So at any one time there could well be forty or fifty of our school at Oxford. And there would be a rather smaller number at Cambridge. We tended to have more Oxford links than Cambridge links. And then if you didn't make Oxford or Cambridge, then there was a whole plethora of universities to go to. But the top Sixth Form would be expected to go to university I would think. Such as Sheffield, which was quite a good university, Manchester, Durham, Bristol, Birmingham, Liverpool, and so on. And sometimes of course people went to particular universities for particular subjects.

GRAYSON: Sure.

JENNINGS: Loughborough was a technological university which for that sort of thing was quite good, but it didn't have an Arts Department.

GRAYSON: That's interesting because just last year at the theater season in St. Louis they put on a play about a group of students who were being educated and promoted to go into these higher-level schools. That was the whole story of the play, and the contrast between a new lecturer who came into the school and an old hand who'd been there, and how they had different approaches to it. The play was very interesting, but your experience reflects the truth of that tension.

JENNINGS: Oh, yes. See, I was quite reasonably good at geography and history there. I was rather sorry to drop them at the age of fourteen. In fact, our final history year you'll be amused to know we'd done from minus infinity as it were to 1485 in one year; that was the end of one particular collection of kings and queens. Then we did to 1714, which was another lot. Then we stopped English history completely.

GRAYSON: Oh, my.

JENNINGS: And we did American history from the Pilgrim fathers, or earlier – Indians and Spanish and Pilgrim fathers – through [Franklin D.] Roosevelt's New Deal in one year. So I can remember doing [George Armstrong] Custer's Last Stand and General So-and-so in the Civil War and "bleeding Kansas" I remember was one phrase because there was a lot of fighting in Kansas just before the Civil War.

GRAYSON: Yes.

JENNINGS: And this was a heading in a book which I can still recall; had a lot of riots and so forth.

GRAYSON: Well, I think the University of Kansas is located in Lawrence, which is close to this point where there were some terrible atrocities at that time over the different attitudes towards slavery.

JENNINGS: Yes. Then of course there was the War of Independence and things like that. So that was quite interesting; it gave us an insight into another culture.

GRAYSON: The other thing I would like to touch on briefly -- I know during your education the Second World War was going on and you were experiencing it kind of first hand, what was that like?

JENNINGS: Yes.

GRAYSON: When I talked to John [Beynon] he mentioned that he was in London at the time and was having to always go into a bomb shelter on a fairly regular basis because these air raids were a daily occurrence.

JENNINGS: Oh, sure, yes.

GRAYSON: So I was wondering if you were in Sheffield at that time?

JENNINGS: Yes, yes.

GRAYSON: Was it more or less safe from this type of activity where they were continuously bombing the U.K.

JENNINGS: Not completely, no. The government had the view that having children in schools was a bad thing because one bomb on the school and the lot would be killed. So for a while we stopped going to school and we went to neighbors' houses perhaps twice or three times a week in groups of about twelve, so this meant we were dispersed. This didn't last for very long, but I remember going next door but one for some time to receive instruction. I would be about seven I suppose. When the war started I'd be, let me think, approaching seven. Yes, so really I was seven or eight then, okay, so we had two extremely heavy raids in Sheffield, the so-called Sheffield Blitz, in December 1940, which devastated the center of the city, and thereafter we would have sporadic raids and very often we were on the flight path of raids to Manchester and Liverpool.

GRAYSON: Sure -- much more strategic targets.

JENNINGS: We had the sirens go, we had the planes going over, but they didn't bother dropping anything. So yes, we were very much confined to our area during that period. I was about, as I say, nearly seven when it started and about twelve when it finished.

GRAYSON: So it's probably not a pleasant experience.

JENNINGS: Well, no. We had a local barrage balloon which was interesting when it went up and down; some anti-aircraft guns about two miles away which definitely weren't hitting their target. But no, I suppose the thing one remembers really is seeing one's first banana.

GRAYSON: Oh! Okay.

JENNINGS: Things like that, and eggs didn't come in silver and blue tins that said, "Dried eggs, national issue," or something on the side.

GRAYSON: I see. So food was kind of hard to come by during the war.

JENNINGS: Well, certain things like bananas. I mean, I didn't see a yellow banana till about 1945 that I remember.

GRAYSON: You remember your first banana. [laughter] Well, let's see, we stopped last yesterday afternoon where you had gotten to Sheffield [University].

JENNINGS: I'd just got to the end of Sheffield and was going to come here [Warwick University].

GRAYSON: Yes.

JENNINGS: But I mentioned quite a few things in Sheffield because I tended to take this personal links perhaps more seriously than I should've done, so I've got a list of about twelve or fourteen people.

GRAYSON: No, that's great. No, that's good.

JENNINGS: And some of them relate to the Sheffield period and some to later periods.

GRAYSON: Okay.

JENNINGS: So I don't really want to do the Sheffield period now.

GRAYSON: Well, if we can get all the information in with the personal links, I think that would be fine because all the links would be together.

JENNINGS: Okay, fine; right. So let's see where we got to there.

GRAYSON: I think you had just started work.

JENNINGS: Right, well, when I moved from Sheffield to Warwick I brought my ICR instrument with me.

GRAYSON: This was the Varian instrument?

JENNINGS: Yes, the Varian one. And Warwick were just buying a new NMR machine, so they had a spare magnet, so we decided to try and build a second ICR machine. And I happened to have a student with me who was very gifted at electronics, and he built all sorts of things. I can't say that it ever produced many results. It worked, but it wasn't easy to use; not as easy as a commercial one, and the MS50 tended to take over because it was a new instrument. And we could do lots of things with it, and it was entirely my own so it wasn't a service instrument.

GRAYSON: Right. And so the MS50 was in 19 –

JENNINGS: That came in 1973. It was like an updated MS9. Solid state electronics whereas the MS9 had been valves [vacuum tubes].

GRAYSON: Right. And it had many ion optical improvements.

JENNINGS: Oh, yes, certainly, and quite high resolution.

GRAYSON: So what kind of resolving power were you getting with it new?

JENNINGS: I think it was guaranteed to give something like fifty thousand. But, you know, if you pushed it and tuned it up you might get conceivably more. But at very low sensitivity of course. Because that was still at a time when high resolution was perhaps still being pushed, whereas nowadays it isn't pushed maybe as much, except for the FT ICR [Fourier Transform Ion Cyclotron Resonance] work; so yes, we had the MS50 to play with, and that was quite good fun.

GRAYSON: So what kind of problems did you work on?

JENNINGS: Well, we did more of the collision-induced decomposition, of course.

GRAYSON: Did you have to modify the instrument for that?

JENNINGS: Not for the gas phase work, but later on we did surface-induced decomposition. We also built a high-pressure source to put on it to do ion molecule reactions, following the sort of stuff which many Americans had done. There was not a lot of it being done in Britain. But by high-pressure I meant something like one Torr, so we could do the thermal ion molecule reactions in the source. And we did quite a bit of work on that of various sorts. About this time, Rod Mason joined me as a research officer and stayed for a number of years before moving to Swansea as a lecturer about 1986. He played an important role in looking after the group during my periods of heavy administration.

GRAYSON: When you did the collision-induced work, was your collision cell between the ESA and the magnet, or --?

JENNINGS: Usually between the source and the electric sector in the first field-free region. That's something I'll come back to when we talk about people in Sheffield. There's quite a bit to say about that. Well, actually I'd better look at my list of publications; see if I can weed them down.

GRAYSON: I don't know if you want to look at this list; it might be a little easier.

JENNINGS: Well, I've got a few here, thanks very much. I've got some selected ones which are a little more important ones.

GRAYSON: Okay.

JENNINGS: It's about this time we had been working on ion molecule reactions of alkenes, and mixed pairs of alkenes, and it seemed that they appeared to react by following a four-center complex, sort of like a cyclobutane structure. And then fragmenting, but retaining some structural information. And what we found was that we were really breaking double bonds. Now, I hadn't realized how difficult it was for the organic chemist to find out where a double bond was in a long chain, so I turned to one of my organic colleagues. He said, "Well, that's great; if you can locate double bonds, that's useful. I'll give you an acid which is an unsaturated acid. You tell me where the double bond is." And we did. So he was quite pleased with that.

GRAYSON: So how big was this acid?

JENNINGS: I think it was a C18 acid.

GRAYSON: Oh, okay; fairly large.

JENNINGS: Yes, I think it was oleic acid; I can't be sure now. But anyway, we therefore had developed a method of locating the position of a double bond in long chain.

GRAYSON: So how did that work?

JENNINGS: We use vinyl methyl ether or methoxyethylene if you prefer, and this attacked itself, of course, being an alkene, but it also attacked others. If you think of a long chain with a double bond here, your vinyl methyl ether molecular ion would come up, do that, and then they would break like that, so giving you two fragments. Now depending on where the double bond was in the long chain you could see how big the fragment was.

GRAYSON: So they selectively attacked the double bond in the chain.

JENNINGS: So this was an example again of something I'll talk a bit more about in general later, of selective chemical ionization rather than just the acid-based stuff, which most people have worked on. We used a reagent ion to probe a particular structural feature, and Alex Harrison then took this over and improved it by diluting the vinyl methyl ether. Our problem was that by putting in vinyl methyl ether in a fair concentration of course much of it reacted with itself, but enough reacted with the compound of interest. But by diluting it with nitrogen one could make more vinyl methyl ether ions in the presence of fewer molecules, and the reaction then didn't take place. So this particular mix worked quite well, and it was the first example of using mass spectrometry selectively to find the position of a double bond, and then Mike Gross developed other techniques later. But we were quite pleased with that, and that just grew out of quite ordinary work on fluoroalkenes.

GRAYSON: So you were still working with fluorine compounds.

JENNINGS: Yes. And let's see what else we have here. We were still working on structures in metastable peaks because we now have a variable monitor slit, of course. And that tells a bit about the sort of fragmentations going on. We also worked more with linked scans, and again I would say much more about that as I am talking about people.

GRAYSON: Okay.

JENNINGS: Just as an aside, as it were, we worked with the AEI [Associated Electrical Industries, manufacturers of the MS instrument series] people and I suspect because they didn't have the people in their own factory to demonstrate the use of such things; they would say, "Well, look, we can provide you with a technique. Will you demonstrate its use?" So that was a quite useful combination.

GRAYSON: So you had a good relationship with AEI people.

JENNINGS: Yes. I'll talk a bit about the people there later. So we worked with linked scans and demonstrated their utility in various ways. Going back to ICR, another thing that we did that attracted a fair amount of interest, we worked with O^- ions, negative ions, and these we found had a propensity to extract H_2^+ from some molecules to form water, and so we did a fair amount on the mechanism of reactions of that sort, which other people have since followed up. Then we got to building a high pressure source which enabled us to work on ion-molecule reactions and this led us into such work as gas phase acidities and so on.

GRAYSON: So this is really all kind of fundamental research in gas phase ion chemistry.

JENNINGS: Oh, yes, yes.

GRAYSON: You're trying to really understand some of the crazy things that go on in the ion source.

JENNINGS: Yes. It's fair to say that before 1980 we did nothing biological.

GRAYSON: Oh; okay.

JENNINGS: I'm looking here – one thing we did going back to double bonds -- we had a look at doubly unsaturated compounds and if they were not conjugated we could locate both double bond positions. If they were conjugated they reacted quite differently so you could say they were conjugated.

GRAYSON: So let me get this clear. After 1980 you did nothing but biological?

JENNINGS: Oh, no, no, no! No. I'll come onto that in a minute. I'm just trying to see when we started. I think probably the first biological work would be mid-1980s. I haven't got it in this list. I hope I can read it; let's have a look. Now, for a while we worked a little bit on oils, and again I can say more about that with a particular person, but I have spent some time at Shell working on oils, and we have some collaborative work with Shell. And also explosives – people at Porton Down

GRAYSON: I see a paper in 1983 with peptide sequencing by metastable mapping.[3]

JENNINGS: Yes.

GRAYSON: Farncombe, Jennings, Mason, and Schlunegger?

JENNINGS: Oh, yes, yes; it was Schlunegger. Yes. Again, he's on my list of people I collaborated with. But that would be about the beginnings of it. This explosives work was in 1981.

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[3] M. J. Farncombe, K.R. Jennings, R. S. Mason, and Dr. P. Schlunegger, "Peptide sequencing by metastable mapping," *Org Mass Spectrom* 18 (1983): 612-6.

GRAYSON: What was that about? The negative CI is a pretty sensitive ionization technique for most explosive compounds.

JENNINGS: Yes, the people at Porton Down had a unit which worked on detection of explosives, and also on portable detection of chemical and biological weapons. So I was on a committee which helped advise on such things.

GRAYSON: So this was way back in 1980, early 1980s.

JENNINGS: Yes. And they were interested in developing techniques which might enable one to detect either chemical warfare agents, biological warfare agents – much more difficult – or explosives, and they had a drift tube method which as I remember, if there were any explosives around you got different drift times for ions from explosive compounds. But it wasn't particularly specific. So we were asked to see what we could do with negative ions. One amusing story was when we had some acetone delivered which contained 10^{-8} molar RDX. A safety officer got very concerned with receiving explosives into the department, so I pointed out to him that we were the only people in the department who could say this acetone was not completely pure, because 10^{-8} molar RDX nobody else could detect.

GRAYSON: Yes.

JENNINGS: And really the acetone was probably a bigger fire hazard than the RDX.

GRAYSON: Yes. Well, it's amazing how the presence of something in even a minuscule minute tiny itty bitty amount sets people off, because if it's there then it's bad. Most other techniques would never see it.

JENNINGS: Well, we also had a number of problems *vis-à-vis* handling substances, but thankfully our lab superintendent, Peter Rayner, was the university safety officer, and he took a very sensible attitude; if we were working with minuscule quantities that the risks were negligible.

GRAYSON: True.

JENNINGS: You could work with some really explosive stuff, but if you got a microgram of it, it was probably not going to blow the place up.

GRAYSON: Yes. That was R-A-Y-N-O-R, Peter Raynor?

JENNINGS: R-A-Y-N-E-R, I believe it is, yes.

GRAYSON: Okay.

JENNINGS: I mean we had a number of amusing episodes at the time. Perrier water had a scare because there was a very minute trace of benzene found in it, but that was from cleaning problems at some stage, so our storeroom very rapidly put out a notice. "We guarantee that our benzene is entirely free from Perrier water" [laughter].

GRAYSON: So peptides, the peptide sequencing was it done on the MS50?

JENNINGS: Yes. It began to get interesting, but our mass range wasn't particularly good.

GRAYSON: Here's a paper on L-dopa and L-tryptophan, gibberellic acid.[4]

JENNINGS: What years are they?

GRAYSON: 1985 was L-dopa and gibberellic acid was 1985 also.

JENNINGS: I've got a name back here somewhere. Another thing we did, especially when [R.] Graham [Cooks] came over, was to try angle resolved experiments. We built a very complicated device to put on the front of the MS50. It was like a swinging source. If you can imagine, this is the flight tube of the MS50, and we had a source here; the source could do this. [Demonstrates with his hands how the source could swing.] Not as much as that, of course, only small angles. So we fired our beam into something and we could then vary the angle at which we sampled the product. This wasn't, I think, terribly successful. It was a difficult experiment. We got a few results, but we didn't follow it up very much really. It was a difficult experiment and I'm not certain that it was of great interest. A lot of people were interested in angle resolved experiments from crossed-beam work.

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[4] M. F. N. Duarte, D. W. Hutchinson, and K. R. Jennings, "Chemical ionisation mass spectrometry of derivatives of L-dopa and L-tryptophan and their detection in tumour samples," Org Mass Spectrom 20 (1985): 476-8.

GRAYSON: Yes. How much of crossed-beam work was done? Futrell was doing it at Wright Patterson. Was there anybody else that was?

JENNINGS: We had a crossed-beam apparatus, but it never really produced any results. It wasn't my apparatus; I was vaguely associated with it, but I think it's fair to say that it's of interest fundamentally, but the emphasis of the subject shifted towards the biological areas due to better ionization techniques being available. Now I can remember saying to Howard Dalton, who was in Biological Sciences here, that we could almost get up to about two and one half thousand now with our ionization techniques, to which his reply was, "Well, when you put another nought [twenty-five thousand] on there then I'll be interested."

GRAYSON: This is Dalton, D-A-L-T-O-N?

JENNINGS: Yes. He tragically died only about two or three months ago. He was a member of the tennis club which we visited last night.

GRAYSON: Okay.

JENNINGS: And he died on court; he was playing tennis, suddenly felt ill, just dropped. Heart attack.

GRAYSON: Oh, my!

JENNINGS: And he was scientific advisor to Department of Rural Affairs, Food and Rural Affairs, DEFRA. So he was a second government advisor; there was a top one, and he was the second one, and knighted for his work, an FRS and very distinguished. Well, we did quite a bit of work with him later; again, I'll come onto that. But we got onto peptides with FAB.

GRAYSON: So FAB arrived in what, 1980 –

JENNINGS: Yes, in the early 1980s, yes, and that enabled us to do some work with peptides.

GRAYSON: Interestingly when I talked to Beynon he never did any FAB work.

JENNINGS: No. See, he was a physicist and he really was not interested in the biological end of things. Very much interested, very gifted in instrumentation; that was his real strong suit, but I think he was quite content to leave other people to work with very large molecules. We got involved in some biological work when we got around biochemists.

GRAYSON: Well, in 1991 there's some determination of amino acid sequences of cystine-containing peptides by tandem MS. And then 1992 was Bordas-Nagy and Despeyroux. MH^+ ions of peptides; stereochemistry of some stereoisomeric furanosidic derivatives in 1992.[5]

JENNINGS: That's still Fast Atom Bombardment there. I think it was towards the end of the 1980s when I finally got a triple quad. That's what I'm trying to find out.

GRAYSON: The MS analyzer papers in 1990. There's a listing of stuff on 4 sector instruments. [6]

JENNINGS: Right. Armelle Buzy came on the scene then.

GRAYSON: Dominique was around for a while.

JENNINGS: Dominique was there for a long time, yes. Yes, well 1991 with Joe Bordas-Nagy and Dominique would have a paper.[7]

GRAYSON: New trends in mass spectrometry instrumentation; using electrospray ionization.

JENNINGS: That would have been 1993.

GRAYSON: This is the time that you had a quadrupole, do you know?

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[5] J. Bordas-Nagy, D. Despeyroux, and K.R. Jennings, "Comparison of helium and argon as collision gases in the high energy collision-induced decomposition of MH^+ions of peptides," *J Am Soc for Mass Spectrom* 3 (1992): 502-14.

[6] A. D. Wright, D. Despeyroux, K.R. Jennings, S. Evans, and A. Riddoch, "Surface-induced dissociation mass spectra of protonated peptides using a four-sector mass spectrometer," *Org Mass Spectrom* 27 (1992): 525-6.

[7] K.R. Jennings and D. Despeyroux, "New trends in mass spectrometry instrumentation," *Magy Chem Lapja* 51 (1996): 101-9.

JENNINGS: Yes, seems my triple quad must've come about 1993, because papers started to appear in 1994 that look as if they're triple quad, and I can remember some of the people who worked with me then; yes.

GRAYSON: So this was the Finnigan?

JENNINGS: No, no, it was a Quattro, VG Quattro.

GRAYSON: Oh, oh, okay.

JENNINGS: We got up to around I supposed two and one half thousand mass for peptides and sequencing the peptides on the MS50. But one of the problems with a magnetic sector machine, of course it was a scanning instrument.

GRAYSON: Right.

JENNINGS: Which meant that you didn't get a lot of sensitivity, especially at high resolution, and we were really struggling to detect bigger quantities of things. I mean, picogram was regarded as quite good, even the manufacturers thought they were doing very well with picograms. Most of the biochemists said, "Well, you know, frankly femtograms is a lot to us. Can't you do better than picograms?" And the triple quad got us into electrospray; I recognized immediately that when electrospray came along we had to get into that. I thought it was a more useful technique than MALDI [Matrix Assisted Laser Desorption Ionization]. And once we got into that then we began to get into more biological work. Passing over some of the MS50 work we did on explosives and gas based acidities and those sorts of things, surface induced decompositions, we began work then with Howard in biological sciences in the early 1990s.

And one of the first problems we got was could we verify that they had the right sequence for this protein. They had a complete protein and they also had it with the first twelve residues missing, and so we had two proteins to look at. The complete protein we just couldn't get agreement, for which we were sorry. We had a difference of about thirty-four mass units. The one with the first twelve residues missing we agreed to within I think 0.2 of a mass unit, so at least we demonstrated we could do it. And very interestingly they then found that they'd made a mistake in the first twelve and I think No. eleven, if they switched from what they thought it was to something else that made exactly 34 mass units difference. So that was a big success, you see. Howard was impressed. We had saved them from making a wrong assignment.

GRAYSON: So, you did this protein on the triple quad?

JENNINGS: On the triple quad, yes.

GRAYSON: With electrospray ionization?

JENNINGS: Yes, yes.

GRAYSON: And multiply charged ions.

JENNINGS: Yes, yes.

GRAYSON: And collision induced dissociation.

JENNINGS: Yes, yes. As soon as we got into electrospray and triple quad we were into the biological era. I think by then I had got the feeling that most of the problems in what you might call routine chemical analyses were solved. It was merely handle-turning, and looking further for new challenges. I'd always felt that I wanted to change direction every few years; otherwise you tend to get into a rut, and I'd had a good go at oil, I had a good go at explosives, and also at fluoroethylenes, ion molecule reactions, gas phase acidities, surface-induced decomposition; so what was there new to do? And the biologists were really crying out for help; they had a very limited little mass spectrometer, which wasn't very reliable, and they didn't really know what it could do for them.

GRAYSON: So they actually had a system in their own department, or?

JENNINGS: Well, it was in our department, but my post-doc Armelle Buzy, who came over from France – very good girl – spent quite a bit of time up there preparing samples and bringing them down and working with our instrument.

GRAYSON: So how do you spell her name?

JENNINGS: B-U-Z-Y. A-R-M-E-L-L-E. And she really provided a good link. She'd come from central France and she'd worked on peptide and protein sequencing; I think she'd sequenced something like about six hundred residues or so.

GRAYSON: Oh, my.

JENNINGS: So she was ideal to join us because we didn't know much about it at the time, and she was extremely good at working with them on using their techniques, which we again didn't understand, the biochemists, and provided them a very valuable link. I can say how that developed later on, but as a result we did quite a bit of work with Howard, and that led to quite a bit more later.

GRAYSON: So you really embraced the biological aspects of the technique and went forward with that for pretty much the rest of your career there?

JENNINGS: Oh yes, very much so; yes, yes. That was in the early 1990s, and then when we got to 1997 I had twenty-five years in chemistry and I was sixty-four, and it was sort of suggested that perhaps I might think of retiring. And well, I can say more off the record on that than I will on the record, about being pushed that way. So what did I do? I thought of joining Simon Gaskill at UMIST [University of Manchester Institute of Technology] because again we'd had quite a bit of collaboration. But I talked to the chairman of biological sciences, Roger Whittenbury, who had been a good friend for twenty-five years.

GRAYSON: Can you spell his name?

JENNINGS: W-H-I-T-T-E-N-B-U-R-Y. And Roger said, "We're crying out for mass spectroscopists. If you're going to retire why don't you come and join us?" So I said, "Fine! It's much better than going to Manchester and have to move and so on.

GRAYSON: Yeah; sure.

JENNINGS: So rather to the annoyance of the chemists I said, "Right. I'm quitting chemistry and going to biology, and I'm taking my triple quad with me, and my research group."

GRAYSON: Ah!

JENNINGS: So rather than retiring I went up to biological sciences, joined Howard Dalton's group, and opened their eyes to what could be done. And it was really quite remarkable the difference in problem solving. If you take the techniques of biologists and sit with them and have coffee with them, and be part of them, people come and talk to you, bring you things to do. If you're half a mile away in chemistry, you're willing to do exactly the same thing but nobody ever talks to you because you're half a mile away.

GRAYSON: Yes, it is a problem, separation and space can be a real problem.

JENNINGS: And I remember giving a talk which was entitled essentially "What Mass Spectrometry Can Do for You." Because they were pretty ignorant, and at the time they were using sodium dodecyl sulfate methods with electrophoresis to determine molecular weights of things like proteins to plus or minus two or three thousand. I said, "Well, we can probably do it to about one part in forty thousand, and we can tell you if you've got any water molecules associated with it," and they were astonished at that. We'd just done some work for Howard, and I think I'm right in saying the molecular weight was forty-five thousand and two, plus or minus one-point-five. But again, they were astonished you could do this. So suddenly they began to see what we could do, I had a very pleasant four years as a part-time retired emeritus professor working with Howard, and then they stopped paying me. But I still went in and worked with him for another few years. Subsequent to that I managed to arrange to attract Jim [James H.] Scrivens from ICI.

Again, I'll say a lot more about him when we come to people, and he really followed me there and is now a full-time professor in the department going great guns building on what I had. I mean, we took the triple quad, but we did quite well because within a couple years I managed to get another new instrument, even though I was retired. A Q-ToF, which gave us increased sensitivity and performance.

GRAYSON: This was the Micromass instrument

JENNINGS: Yes.

GRAYSON: Or whatever they were at the time – Kratos – or not Kratos, but VG or Pfizer or whoever.

JENNINGS: Yes, they were VG, and then they later became Waters.

GRAYSON: So even though you were retired in an emeritus position you were able to get an instrument.

JENNINGS: Yes.

GRAYSON: Where did the money for that come from?

JENNINGS: That came from the SERC or BBSERC, I can’t remember which – I think it was the SERC. Obviously I was part of the grant application.

GRAYSON: Sure.

JENNINGS: Howard’s work very much helped get it. But the fact that he had, if you like, trained mass spectroscopists willing to use it was quite useful.

GRAYSON: Indeed. It would make a lot more sense for them to fund that than if he just wanted an instrument and he didn’t have anybody who knew how to work, how to use it.

JENNINGS: So that really is a sort of synopsis of, if you like, the timeline, and I still keep in touch a bit. I have lunch with Jim about once a week. I give the occasional lecture in the department. They run a Christmas proteomics course, so I give the introductory talk on that.

GRAYSON: Christmas proteomics – that’s – like a short course?

JENNINGS: They have one every December. I didn’t do it last year because I was away over the holiday, but otherwise I give the introductory lecture each year. A three-day course.

GRAYSON: This is for the biologists or anybody that’s interested?

JENNINGS: Oh, yes; they come from all over Europe.

GRAYSON: Okay.

JENNINGS: It's quite well received – I mean, they have to turn people away. We have about fifteen; there's quite a bit of practical work involved, so that really controls the numbers.

GRAYSON: Well, yes. If it gets to big then it's starting to people don't really learn what they need to.

JENNINGS: And I sort of help Jim a bit with his group. I give them a talk on how to write decent English and things like that, how to write a thesis. I proof read some of their stuff. I comment on their posters before they submit them, and so on. But I no longer get involved with the day to day science now.

GRAYSON: So when you're doing this protein work, do you have to deal with sample clean-up and separation, you're using this . . .

JENNINGS: Electrophoresis?

GRAYSON: Yes, gel electrophoresis that kind of thing.

JENNINGS: Yes. That, and HPLC.

GRAYSON: And I guess the thing now is nanosprays; was that something you were using

JENNINGS: Yes, yes. That's supposed to be more sensitive. We also had a very – how shall I put it – rudimentary MALDI instrument, but I never found MALDI that attractive.

GRAYSON: Well, you've got the matrix issue. And that's the old kind of black magic business, you know. Obviously a lot of people use it and find it useful. I was at a pharmaceutical house up in the Boston area recently. Their attack was to do LC and then continuously spot a MALDI plate with the LC eluent. They would spot as many plates as it took to get the two-hour run. Then they'd just submit the plates to the MALDI instrument in an automated fashion.

JENNINGS: Oh, okay, yes.

GRAYSON: After interrogating the MS data, when they found something that required further investigation then they'd put that plate back in, go back to that spot, and then do more sophisticated experiments. So it was a "high throughput" operation.

JENNINGS: Oh, yes, I'm aware of that sort of thing, but I was never interested in what you might call high throughput routine work.

GRAYSON: But it was an interesting adaptation of the technology -- but then you end up creating tons and tons and tons of data, which they had to backup. They showed me a complete room full of computers just keeping track of the data and storing it safely and making sure that it didn't get lost.

JENNINGS: Well, that's one of the things which I was hearing about in the department earlier today. They now have this new idea of an analytical science center, and they have very good mathematicians who are interested in helping, and very good statisticians. So the data processing side is really well looked after; they don't simply have to rely on commercial software. And Jim has been fortunate in attracting a young lad who is good at bioinformatics. And that seems to be increasingly important, and I can remember the days when we refused to buy a mass spectrometer with a data system because we wanted to do it ourselves. We didn't trust their data systems to do the job. And it was only when I got my Quattro that I got a data system for the first time.

GRAYSON: Reminds me of Brian Green.

JENNINGS: Yes, yes.

GRAYSON: I remember watching him when I was visiting there, watching him approach the computer with great disdain, just barely touching the keyboard. [laughter] He didn't want to get associated with it very closely.

JENNINGS: Yes, I also like machines with knobs on.

GRAYSON: In Gross' group we've got a couple of new instruments, and they're all black boxes with a computer terminal. McIvers' MALDI, FTICR [Fourier Transform Ion Cyclotron Resonance] MALDI instrument is just a nondescript box and you've got a keyboard with a PC to control it.

JENNINGS: Well, if you type in what you want, and what electron volts you want, you type in something, if it's EI for example. Whereas back in the dark days you used to twiddle a knob and look at the scope and see how the signal slowly came up . . .

GRAYSON: I just finished working with Gross and his group of graduate students. They are supposed to run service samples. So we'd have EI and FAB and they don't like these old instruments because they have too many knobs. So invariably the first couple of times they use one of these pieces of equipment you'd hear various voices calling someone down from the staff to help them find the beam.

JENNINGS: Yes; oh, quite!

GRAYSON: So, I earned the moniker of the Ion Hunter, because whenever they lost the beam I'd go downstairs and look at it. Usually, it was something that was totally obvious to me that they just overlooked completely. Like turning up the gain on the amplifier or something like that.

JENNINGS: Something I've left out completely, I just realized – this is going straight to the crux, of it – is of course the four-sector instrument we had.

GRAYSON: Oh, yeah. I saw some references to four-sector.

JENNINGS: Yes. I'm sorry, but we've gone straight on to Quattro and peptides. I'd forgotten. We had our MS50 and the ICR slowly faded away, and then we got the four-sector. Now, that was a real saga.

GRAYSON: Whose machine was that?

JENNINGS: It was a Kratos one.

GRAYSON: Kratos four-sector.

JENNINGS: Yes. Now, we began by saying that we'd modify the MS50. We put a new sector on the front end of the fifty. Then Kratos I think saw the way the market was going and so they didn't want to do that, although I got a grant for it. And they'd rather produce a new mass spectrometer which was horizontal, because of course theirs was vertical and you can't have a 4-sector vertical instrument very easily. Now, there's a lot of sense in having a vertical instrument, because the ion optics were better than the horizontal, and so they tended to get better resolving power earlier than VG did with their horizontal design. But when we went with 4-sector that was shared with Peter Derrick's group, which caused some problems because we had different ideas on how to use it, and that's what reminded me when you said "searching for the beam."

I think one day they had moved the monitor slit sideways to do something, I forget what, and didn't move it back. We spent most of the day trying to find the damn beam [laughter].

GRAYSON: So, what geometry did it have then? Was it just conventional ninety degree sectors aligned appropriately, or?

JENNINGS: I think that it was sixty degrees, but I can't remember now quite honestly because it was such a long time ago that we used it. It was late coming and it was always difficult to get the best out of it, and working with another group who did different types of experiments didn't help. It was almost as bad as working with a service instrument, which I've described just today. And we got some quite good work out of it; I think some of the early 1980s work we almost certainly did on the four-sector.

GRAYSON: So what was the motivation to do accurate mass, or high resolving power MS-MS?

JENNINGS: It wasn't the initial idea; it was a high-resolving MS-MS. But again the problem was of course sensitivity.

GRAYSON: Yes.

JENNINGS: Always sensitivity; and really I think that's what killed accepting these machines.

GRAYSON: Well, the further down the flight tube you go, the fewer ions you're going to have to work with . . .

JENNINGS: Quite, quite.

GRAYSON: . . .and that’s just reality.

JENNINGS: And although we had array detectors, again I never found them as good as they ought to be. The resolving powers of the array detectors were not good. And shortly after I’d left to go to Biological Sciences they scrapped the 4-sector machine completely. It was really a sort of white elephant. It took up a terrific amount of space.

GRAYSON: Oh, yeah. Yeah.

JENNINGS: I mean, you could just about get it in this room.

GRAYSON: Yeah. As well as a fair amount of money, I’m sure.

JENNINGS: Yes, and with the developments in things like triple quads and then the Q-ToF, and of course if you really wanted high resolution, FT, I think the magnetic sectors had really gone.

GRAYSON: Was it adapted primarily to use a FAB source, the 4-sector?

JENNINGS: Yes.

GRAYSON: We had an electrospray source on our ZAB and it didn’t work nearly as well as you wanted it to.

JENNINGS: We could have a variety of things stuck on the front end, but I think FAB was probably – again, I can’t honestly remember now. FAB certainly was what we used on the 50. We probably had electrospray on the 4-sector.

GRAYSON: When did electrospray start to take off?

JENNINGS: Early 1980s, wasn’t it?

GRAYSON: I should know. Gross has the VG 4-sector machine, and I don't think it ever realized its potential. But like you say, the availability of electrospray and other analyzers such as the Q-ToFs and the ICR machines, I think it was probably the death knell of sector equipment for serious high mass protein work. You're just mass limited.

JENNINGS: Of course, yes.

GRAYSON: And that's all there is to it.

JENNINGS: I think the sensitivity is a problem as well.   So do you want to get on to people for a bit?

GRAYSON: Sure.

JENNINGS: [Shuffling papers for a list of topics.] Oh, here we are – sorry. Yes, we'd really done No. Four quite thoroughly yesterday. No. Five, so I'll just go through these.

GRAYSON: Sure.

JENNINGS: Employment experiences, industry – very little. All I really worked in industry was as an ICI summer student when I was an undergraduate. That did quite a bit to put me off industry, actually. I'd worked with ICI dyestuffs for the summer, and my job was preparing the same dyestuff day after day but with different sort of additives to see if we could produce it so that it didn't get dusty but retained its color characteristics. Because the problem was apparently people got drums of the dyestuff, ripped the top off, and covered their face in dyestuff, you see. So could we put some stuff in which would make it coagulate a bit perhaps, and so I prepared stuff after stuff after stuff and sent it off, and they came back a little pinker, a little brighter, a little duller, slightly redder.

And I think at the end, I don't know whether anything came out of it or not. But I talked to other people who were working there, and they were very narrow in their interests. I remember trying to talk about molecular orbitals to them, and they'd say, "I've never had the slightest interest in molecular orbitals." And really I thought, "Well, if this is what working in ICI dyestuffs is like, I'm not very keen."

GRAYSON: This was a summer job when you were in college?

JENNINGS: Yes. I was probably about twenty-one – I'd had my national service. And I worked in ICI Huddersfield Dyestuffs for about six or eight weeks. The only other time was – which I'll come to again in people – was when on my sabbatical year in 1982 I spent about three months working at Shell in Thornton on FAB. So did I have management problems and so on? Not really relevant, I'm afraid.

GRAYSON: Yes, you didn't get to work at that level in the organization.

JENNINGS: Well the only other thing along these lines is a bunch of collaboration with industry but not actually working in industry. I had plenty of collaboration with AEI, with John Beynon, with Jim Scrivens, some with Esso, some with Porton Down. So there's a lot of interaction in industry, but never as being part of industry.

GRAYSON: Okay. Were you interacting with peers on a technical level?

JENNINGS: Yes, yes.

GRAYSON: And this was just in a collaborative thing because of personal connections?

JENNINGS: A lot of it, yes.

GRAYSON: And it really wasn't something – other than sabbatical work -- it wasn't something that was imposed from above?

JENNINGS: Oh, no.

GRAYSON: Somebody, said "It would be a good idea to have come in here and interact with you guys or anything like that.

JENNINGS: I was a consultant for AEI for some years. And later on for VG. And I did some consultancy work for Esso as well.

GRAYSON: I heard about a program that Sy Myerson mentioned when he was working for the Standard Oil Company that they would actually bring in college professors for the summer and give them the lab space and have them do some research and interact with the Standard Oil people at a technical level. It was a kind of enlightened management concept that they could do back in that period of the 1940s, 1950s, 1960s. But they can't do that today because somewhere the bottom line doesn't show the results.

JENNINGS: Quite, quite!

GRAYSON: So if you can't see it in the bottom line, then obviously it's no good.

JENNINGS: So life experience in teamwork situations -- very little other than what I'll come to with people. Academically very much with the biologists, but we've already touched on that. Experience with research management – again, I'm not quite sure how that fits in with my not having been in industry.

GRAYSON: You managed post-docs and graduate students. It's the idea of trying to motivate people to do things.

JENNINGS: Ah. Well, I can say quite a bit about that if you wish me to. I had a research group of up to about ten; never more than ten.

GRAYSON: Must have been post-docs and graduate students.

JENNINGS: Yes.

GRAYSON: Okay.

JENNINGS: I liked to have a group of about six – I can manage six.

GRAYSON: And of course you had to get funds for these people.

JENNINGS: Yes. But I had quite a lot of people come on sabbatical leave. That was extremely useful and very entertaining for the group as well.

GRAYSON: So these were people who, had established themselves and you didn't need to help them and hold their hand in their research projects.

JENNINGS: Oh, certainly – I mean, very distinguished people. Graham Cooks for one; Mike Bowers, Urs Schlunegger, Ian Gregor from Australia among others. Catherine Fenselau came and spent a couple of weeks with us.

GRAYSON: So these arrangements were made just informally.

JENNINGS: Yes, they would write and say that they'd like to come and spend some time, could I accommodate them – Diethard Bohme from Canada was another.

GRAYSON: Then usually they have their own funds.

JENNINGS: Yes, yes, we provided them with lab space. And let them join in with some work we were doing or perhaps bring their problem and work with us on something. So that was extremely good; I mean, I really enjoyed having these people come. We must've had twelve or fifteen very distinguished people over the years. But a thing which I don't know whether you want to go into or not is my administrative work for the university. See, I was in the Chemistry Department for twenty-five years. Just to start at the beginning I came in 1972 when the department was in a very bad way. It had just lost one of its professors, and the other one, I'm afraid, had a drink problem, so he was not well, and the department was very dispirited. It had a poor intake, and I and another chap were really drafted in to try and do something about it. Now, we had a particular problem which was unfortunate in that the grand design had been to have a department called Molecular Sciences.

What was molecular sciences? School children didn't know what it meant, so they didn't apply. We'd produce degrees in molecular sciences; employers didn't know what it meant, so it wasn't very popular. As a university we didn't have a chemistry department, we had a molecular sciences department. We didn't offer a degree in chemistry, so if you were at school you didn't come to Warwick to do chemistry because they didn't do it. So our intake was around two dozen students a year.

GRAYSON: This would be for post-grads at this point.

JENNINGS: This was undergraduates.

GRAYSON: Undergraduate work.

JENNINGS: Yes.

GRAYSON: Oh!

JENNINGS: And because we taught a joint course with biochemistry and you could transfer at the end of the year, of course chemistry was seen in those days as rather mathematical, and so you often lost half your first year. And so the first year I was here – I had come from Sheffield where we had typically sixty or seventy students – I found to my horror we were graduating five students. And really the University said, “Well, do something about it.” So within two years I found myself chair of the department, or head of the department, and during my twenty-five years in chemistry I was head of department for fifteen years. So I had a lot of experience trying to run the place, and it took a few years to start things turning ‘round, but I think now – it’s ten years since I left it – it’s one of the better departments in the country. I think we now have about ninety students. When I left we had built it up to about sixty or seventy.

GRAYSON: But they are students in chemistry.

JENNINGS: Yes, yes.

GRAYSON: Molecular sciences was –

JENNINGS: I’m afraid I had to kill off molecular sciences – much to the disgust of the chap who started it. I said, “Well, you know, the school children will not come. It’s a good concept but it’s too sophisticated for them. I said, “They don’t know what it means.”

GRAYSON: It’s ahead of its time, I would say.

JENNINGS: Yes, indeed. And so we began, we called ourselves Chemistry and Molecular Sciences for a while, but then we dropped the molecular sciences and just became chemistry. And slowly but surely the numbers built up. Now Warwick had a chemistry department, Warwick had a chemistry degree, Warwick taught chemistry, and so they started to come.

Because the department was quite good but we called ourselves this peculiar name. I mean, you asked employers what they understood and you got some very peculiar answers.

GRAYSON: Yes, well, as I said, as a concept it was probably advanced way ahead of its time.

JENNINGS: Yes, and the idea initially, you see, was that there would be a big umbrella department called molecular sciences, and all things molecular would be taught in it. Now, that I'm afraid didn't work because of personalities before I came, and my appointment really was at a time when the concept had broken down and biology escaped to become a separate department. [laughter] And so I was left with, as it, were picking up the pieces.

GRAYSON: So when you came on board how many faculty taught chemistry per se?

JENNINGS: As a department – I'll answer that question in a round about way. As a department we had seventeen staff, of which three, possibly four, were biochemists.

GRAYSON: Okay.

JENNINGS: And they had chosen not to go to the biochemistry department but to stay in the chemistry department as the biochemical end of it. They were committed molecular scientists who wanted to stay within the molecular sciences group. But they were really a very isolated little group, and they slowly faded away over the years, I'm afraid.

GRAYSON: And you had a competing group in biology.

JENNINGS: Sure. And if you think of chemistry as theoretical through to biochemistry, what one tended to do in biology was to have a group here, a group here, a group here. What we did in chemistry, we'd have one of those, one of those, one of those, one of those, one of those, and so they couldn't speak to each other. So we had theoreticians who worked on quantum mechanics and people who worked on yeast – they had nothing in common. And it wasn't a big enough department to sustain this huge spread. So we had seventeen people, and as I say, three or four I would describe as biochemists. Three certainly were biochemists, and the fourth one a very biological organic chemist, and we had to build up from there. So a lot of my early work was spent doing this. And I also became Chairman of the Board of Science, which is like dean of the faculty, for three years. But I've got this sort of stuff written down here if you're interested.

GRAYSON: This would be also on your C.V.?

JENNINGS: Yes, this is the C.V. that's got major committees and things.

GRAYSON: So this consumed quite a bit of your time.

JENNINGS: Yes. This is why I didn't want a big research group. Some people have fifteen or twenty organic chemists; my research group at the time felt that, they were a bit second or third on my list of priorities. If you were a chairman of a department you were on the Senate, you were on this that and the other committee. And sometimes this meant I was only in the department about half the time. But that had to be.

GRAYSON: So you eventually were able to get rid of this concept of molecular sciences?

JENNINGS: Yes.

GRAYSON: And get people to think in terms of chemistry.

JENNINGS: Yes, and I was there for about ten years before we finally dropped molecular sciences completely, and I think after five years we called ourselves Chemistry and Molecular Sciences. Then in another five years we dropped molecular sciences.

GRAYSON: Now, you were hiring people at this time as well, and firing people, or did they leave?

JENNINGS: Not for a while. We had a system whereby – I'm sure it's the same everywhere – you had to have a number of student units to justify a member of staff. So a student unit for undergraduates was based on how much of his degree you examined, so if you taught the biochemist you might get 0.4 for the biochemist, because most of them were 0.6 for biochemistry, you see; 0.4 then would be chemistry. So forty biochemists would generate sixteen points. And since some of your chemists were also taught biology, you only got 0.6 for your chemistry students to begin with, so all this decimals were added up and you had to have initially about ten units per member of staff.

GRAYSON: Okay.

JENNINGS: Well, because of our already bad position we had nothing like that; so there was no question of any member of staff being replaced if they left and we slowly had to build up. But unfortunately as we built up so also, the numbers required drifted up.   By the time I finished I think it was about seventeen per member of staff. But one good thing was that initially because they were keen to build up research, a post-graduate student counted three units. Now, a post-graduate student you had entirely to yourself, and it counted three units as well.

GRAYSON: That was advantageous! [laughter] Well, it encouraged the development of research with post-graduate work.

JENNINGS: Sure, sure, so we did our best to build up the post-graduate side of it. We slowly got there, one or two people left. We had some fairly bright young people who left for chairs elsewhere; but we replaced them. It wasn't until I'd been there about ten years before we really shook off the problems from molecular sciences. Now, in addition to that at the university the other thing I was a member of Heads of University Chemistry Committee, and I became chairman of that national committee for three years, and of course Science Research Council committee, similar to the National Science Foundation. I was chairman of the physical chemistry part of that for a while, and on the committee for quite a bit longer.

GRAYSON: When you brought up the molecular sciences ideas, the Chemical Heritage Foundation has a little motto or vision that it's "the place for the chemical and molecular sciences." And the reason, I believe – and I don't know that this is true, and maybe this shouldn't show up on this tape, but they want to be a repository for things in the biological community as well as just chemistry. So by using this idea of chemical AND molecular sciences they hope to attract advocates outside of 'classical' chemistry.

JENNINGS: Sure. Brings in the biologists. Well, that was certainly the initial idea here. I mean, the chap who started it all actually used to give a lecture entitled, "Organic Chemistry is Dead."

GRAYSON: Oh, my! [laughter]

JENNINGS: Because it's got to become bio chem now.

GRAYSON: Yes; oh, yes. Well, there's certainly a lot of truth to that.

JENNINGS: Yes. But to go around schools giving that talk I mean, would you go to a university and say, "Oh, organic chemistry is dead?"

GRAYSON: Maybe fifty years from now you could say that.

JENNINGS: Yes, it was a good idea but it was just too sophisticated for every school child.

GRAYSON: That's too bad. So then you eventually were able to hire people and –

JENNINGS: Yes, slowly but surely we could hire people and change the distribution of the department somewhat.

GRAYSON: Now was there as much of a difference on the analytical side?

JENNINGS: No. We can explore that now, then. Analytical chemistry in Britain was seen as the bottom of the subject, always. And so you never called yourself an analytical chemist. If you applied for grants and it was seen as an analytical chemistry application it very rarely got funded. Unlike the States, where being a successful analytical chemist is good, here it's solving other people's problems. You're not bright enough to think of your own, so you solve other people's problems by chemical analysis, you see, and that really meant that analytical chemistry in Britain, for a long time was thought of as gravimetric analysis and burettes and pipettes and that sort of stuff.

GRAYSON: It was a second class profession.

JENNINGS: Yes. And if now is the time to stray into what that meant for mass spectrometry, you know, I had talked a bit about this last night.

GRAYSON: Yeah, let's go ahead and explore that.

JENNINGS: Okay. Well, mass spectrometry in Britain was really in the hands of the instrument manufacturers, and Britain was good at making mass spectrometers. It sold them to the United States, Europe. But our leading man was John Beynon, who was excellent as an instrumentalist, but he was a physicist, so his accent was on high resolution, accurate mass measurement, sensitivity, but we still had the words “cracking patterns.” And the emphasis in mass spectrometry certainly when I joined it was oil industry. Shell, Esso, and such people were the big buyers, and the instruments were really made for them to buy. And they wanted high resolution because of the sulfur problem in oil and so on.

GRAYSON: And that was a fairly stringent requirement. Resolving power had to be up in the fifty, sixty, or seventy thousand range in order to do the oxygen-sulfur doublets in hydrocarbon feeds.

JENNINGS: Yes, sure. And of course that started growing up initially in the States. I mean, I’ve often said to people had GLC [gas liquid chromatography] been invented earlier we might not have had any mass spectrometry. But in Britain, I mean all you ever heard as a student was about J.J. Thomson’s instrument and isotopes, and so mass spectrometry was for discovering isotopes. Full stop. But of course the Americans decided at the beginning of the war that it was a way of analyzing light hydrocarbons, so CEC began to make them, and then Metropolitan Vickers did, and after the war they continued to make them, but still for the oil industry. So interpretation of mass spectra, in Britain certainly, was just not thought about. So the chemists really had not much interest in mass spectrometry. It didn’t appear in undergraduate courses, and it really was completely ignored. And when mass spectroscopy started to grow – as I say, we had a mass spec discussion group, and a mass spec group and so on – it was a motley collection of people. You couldn’t say they were chemists; they were oil industry people, they were instrumental people, and so it didn’t naturally adhere to the chemistry – chemical community – I’m sorry, Chemical Society.

And although the Chemical Society had discussion groups for NMR, for chromatography and various other things, it never had one for mass spectrometry, and when it was suggested that we join the Chemical Society, that was resisted because we’re more than chemists.

GRAYSON: Right.

JENNINGS: We’re much more than chemists, you see, so we don’t join them. We form our own society, so the British Mass Spectrometry Society was formed in I think it was [1964] or thereabouts. And we operated outside the chemical community. We had our own meetings – we still do – and we are not seen as part of mainstream chemistry. Now what that has meant is that it’s never really got into the undergraduate curriculum. Even as late as the 1980s the organic chemists were simply saying, “All I want is a molecular weight. I can deal with everything else with NMR and infrared.” So the idea of interpreting a mass spectrum was foreign to them, and I think, as I said the other night, last night, it wasn’t until Dudley Williams came back from [Carl] Djerassi's lab that there was any attempt in Britain to think of interpreting mass spectra, mainly because of course we started with hydrocarbons, which are the most difficult to interpret.

GRAYSON: So what time period would it have been when Williams came back from the U. S.

JENNINGS: Dudley came back about 1965. And there was the very influential book, Budzikiewicz, Djerassi and Williams book, and that was the bible. But I can remember as a young man reading McLafferty's book *Advances in Mass Spectrometry* [*Interpretation of Mass Spectra*], which was, I think, published about 1962.[8] There I could see for the first time that people could actually explain this thing called the McLafferty rearrangement; which was very similar to the Norrish Type II rearrangement in photochemistry of ketones, which I understood as a photochemist. And so what did one do? And this is why I collaborated with Dudley to start putting on summer schools, and at the time we had something called the Royal Institute of Chemistry, which was a slightly more applied organization than the Chemical Society, which was really a bit purer, if you like. And the Royal Institute of Chemistry, which eventually amalgamated with the Chemical Society about 1971, put on summer schools and I collaborated with a variety of people in these.

I'm just trying to see – I have it down here somewhere. Yes, we put on courses 1968, 1970, 1972, 1976, and 1979, and I was co-organizer of the school in 1970 and 1972. And we got together a number of people – Allan Maccoll, for example, among some others – and we tried to give a course where we talked about how the instruments worked, what you could do with them, and how you interpreted the spectra.

GRAYSON: And how long was this course?

JENNINGS: Typically a week. Well we had a very amusing example – this is an aside – in Sheffield, and we had Wednesday afternoon off to let the students have a bit of a rest. And the course organizer from the RIC, Dudley and I went out for a ride into Derbyshire, and we stopped to have tea in a pub in Hope. I don't know if you know, Dudley was quite a good pianist.

GRAYSON: No!

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[8] Fred W. McLafferty, *Interpretation of Mass Spectra; An Introduction* (Ann Arbor, Michigan: University of Michigan Press, 1966).

JENNINGS: We were having tea, and I think in the evening they had dance or something, and there was a piano, and Dudley said, "Oh, I wonder if they'd let me play?" So he walked over and inquired, and they said, "Sure!" So Dudley played things like "A Nightingale Sang in Berkeley Square" and some of the popular tunes; at the end of which the proprietor came and offered him a job.[9] If he'd like to come a play regularly. [laughter] But thankfully for mass spectrometry he didn't take it.

GRAYSON: Yes. Well, those jobs don't pay very well, I think.

JENNINGS: No. But that was what we did nationally to try and produce some interest.

GRAYSON: So are these students then primarily from U.K. or did they come from all around Europe?

JENNINGS: Mainly U.K., but we had some European ones.

GRAYSON: And what was the enrollment typically?

JENNINGS: I can't remember exactly, but –

GRAYSON: Beginning at ten, twenty, or fifty?

JENNINGS: I would say twenty-five to thirty, something like that.

GRAYSON: And these people were motivated because they were getting mass spectrometers?

JENNINGS: Some; yes, yes.

GRAYSON: Did you have senior people who wanted to know more about it, or mostly junior people?

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[9] "A Nightingale Sang in Berkeley Square," 1940 msuic by Manning Sherwin and Jack Strachey lyrics by Eric Maschwitz.

JENNINGS: They tended to send junior people, I think. “We bought a mass spectrometer; better have somebody know how to use it, so send Joe along to learn what it’s for.”

GRAYSON: Were they primarily from academia, or from industry?

JENNINGS: There’d be a mix. But I think in most cases they would send what you might call the senior technician who’d be using the instrument rather than the academic. But at least we were hopefully spreading the word about what you could do with it.

GRAYSON: Did you get many people from government labs?

JENNINGS: I think we got one or two, yes. I can’t remember now much about the composition of the people because it was about thirty years ago. But, you know, we did our best and we produced brochures and take-home material and this sort of stuff, and I’ve always felt very strongly that we’ve got to do this for the subject because there’s still a resistance to thinking of it as a properly based scientific subject. Whereas with NMR you can predict the spectrum if you know the structure. And with mass spectrometry you can’t.

GRAYSON: You can make some intelligent guesses.

JENNINGS: But if I said to you, “Predict the spectrum of methyl propionate,” you could give me an idea. Another thing I can remember from one of these courses, we were discussing how many students could distinguish butyric acid from methyl propionate. I remember saying, “Well, you don’t need a mass spectrometer to do that. Just use your nose!” [laughter] Most people I don’t think could’ve done that at that time. Now I would hope they could. But at the time we were having a really quite serious discussion as to how many people could do that. In the early days a discussion as to where the energy came from when metastables occurred. I mean why did these things suddenly break up?

We’d read about [John A.] Hipple and [Edward U.] Condon’s work and all that stuff back in the dark days, but it was really when the double-focusing instruments started to come online that you began to see metastables and people began to be concerned about what they meant.[10] And if they were any use – I mean, you had metastable suppressors in the old days!

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[10] J. A. Hipple and E. U. Condon, “Detection of metastable ions with the Mass spectrometer,” *Physical Review* 68 (1945): 54-5.

GRAYSON: Oh, yes; yes.

JENNINGS: Whereas I was much more keen on having normal peak suppressors.

GRAYSON: Yes, I remember my experience originally was with an old 103, CEC 103C –

JENNINGS: Oh, yes, yes, I remember those.

GRAYSON: And they had a metastable suppressor on it. You have to get those metastables out of the way so you can see the peaks.

JENNINGS: Quite.

GRAYSON: So do you think that situation is improved at this point in time in Britain?

JENNINGS: Only since the biological revolution, I think. I remember in chemistry in the 1980s certainly we had a course, spectroscopic techniques, where the students were given folders – this is the UV visible spectrum, this is the infrared spectrum, this is the proton NMR, this is the C-13 NMR, this is the mass spec. Deduce what you can about the structure of the compound. I would begin with the mass spec. Joe would begin with the infrared, another would begin with the NMR, and depending on which person the student got, he got a quite different view of how you interpreted the spectra. I remember one chap, Alan Cox, who said to me, "What is the use of a technique which when you put cyclohexane into the mass spectrometer the biggest fragment ion is the loss of methyl?"

He said, "It's useless as a technique." To which all I could say, of course, was, "Well, if you put it in an NMR machine all it tells you is all the carbon atoms are the same." Doesn't do much else, does it? But that illustrated the problem which people had. You'd find that many of the older staff would interpret the structure perfectly adequately and then say, "Well, you see, the mass spec there can wait; it agrees with what I've said." That was the end of the mass spec. All the fragments are irrelevant.

GRAYSON: Well, this reminds me of a book by Carsten Reinhardt that came out about a year and a half ago, and he dwells on this issue with mass spectrometry even in the States.[11] It was

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[11] Carsten Reinhardt, *Shifting and rearranging: Physical Methods and the Transformation of Modern Chemistry* (Philadelphia: Chemical Heritage Foundation, 2006).

primarily seen as a tool by the petroleum industry and the cracking pattern was recognized as having some significance, but not that much. All they were interested in was getting the instrument to be stable enough so they could use it for quantitative analysis.

JENNINGS: Oh, yes, yes.

GRAYSON: And then what the various peaks meant was not really all that important.

JENNINGS: It was a fingerprint.

GRAYSON: Yes, a fingerprint. Reinhardt then develops from there the point that a lot of people considered -- as I think Fred [McLafferty] said you put something in a bag with a bunch of marbles and shake it up and these pieces come out. What does it mean? That was the way people were looking at mass spec data, just a cracking pattern.

JENNINGS: Well, that was because of electron ionization.

GRAYSON: Sure, where you had all that excess energy in the molecule. It had to do something.

JENNINGS: Get your sample, knock it to bits, and then wonder what it was you'd knocked to bits.

GRAYSON: Yes, I remember when I was at Monsanto with the CEC 103, they had low ionization voltage technique where they had a very precise device so you could get the ionizing voltage at exactly – I think it was like 10.8 volts. Because then it would minimize a lot of the cracking on the alkanes and alkenes and you could get primarily molecular ions.

JENNINGS: But did you also control your repeller voltage?

GRAYSON: Yes. Yes, indeed. Indeed, that was the other part of the problem with it, because that could be a source of electron energy.

JENNINGS: And you could be two volts out with the repeller voltage.

GRAYSON: Yes. Indeed. But people were dealing with this mixture issue, and in many ways a mass spec is the worst instrument in the world to deal with a mixture, of any of these analytical techniques.

JENNINGS: Yes. That's what I said, if GLC had been invented in 1935 I'm not sure how many mass specs would've been built.

GRAYSON: Now, that's an interesting thought. [laughter] Hopefully someone would've found a use for them.

JENNINGS: But I think it was the decision by the Americans that had been taken early in the Second World War for a light hydrocarbon analysis – a mass spec seemed to be the thing to use. Thankfully GLC didn't come along for another fifteen years or so.

GRAYSON: So anything more to put onto that particular point we've been discussing. Do you think the educational aspects have caught up? For instance I know in the United States there are several schools that have groups that teach mass spectrometry course. At Washington University in St Louis, Mike Gross has a graduate level course in mass spectrometry and some people who are not even in his group are taking this mass spec course. And I know that goes on at a number of other schools in the States. I was wondering if a similar kind of thing is done here in the chemistry curriculum.

JENNINGS: Yes, but then you said post-graduates, right? Well, see the course I was talking about would have been a second year undergraduate course here.

GRAYSON: Okay.

JENNINGS: And whereas it is thought essential these days to teach them about carbon-carbon stretching for infrared and this that and the other, proton NMR and C-13 NMR, I'm still not certain that such a course would always include much in the way of mass spectrometry at the undergraduate level.

GRAYSON: That's interesting. Most of these undergraduate courses are taught out of textbooks. I'm wondering how many textbooks cover mass spec?

JENNINGS: Well, see, Williams and Fleming's book would be the common one here. That for many years has been the standard, *Spectroscopic Methods in Organic Chemistry*.[12] Now Graham's book I found quite useful as well, and I had a copy of that which had a variety of things in it. I liked that, of course, because it had quite a little mass spectrometry too.

GRAYSON: My youngest son [Scott Michael Grayson] teaches organic chemistry at Tulane,– undergraduate – so I'll have to see what if anything they say about mass spec in his course, because it'd be interesting to know. And they should really gravitate down to that level because the reality of it is knowing the molecular weight of a compound is a very important piece of information.

JENNINGS: Yes.

GRAYSON: And you don't get that from NMR or infrared.

JENNINGS: But you see the organic chemist would say, "Well, that's all I use the mass spec for. I've already got molecular weight; I have no use for all those fragment ions."

GRAYSON: Yes. But they still provides some information about the structure too.

JENNINGS: Well, it's interesting you mention post-graduate, because I've got here in my C.V. post-graduate teaching of mass spectrometry, and I guess perhaps just go through them. There's lectures in the Royal Institute of Chemistry courses which I've already mentioned, lectures in a mass spectrometry course at Lubljana, Yugoslavia, and that was with John [Beynon] at the Jožef Stefan Institute in 1969, and that was similar to the RIC courses. I think about six of us met and talked to the students for a week.

GRAYSON: So these were the courses that you held abroad on the continent?

JENNINGS: Yes, the Jožef Stefan Institute in Lubljana organized one and I was invited to lecture there. I think probably via John, because John knew somebody there. The other one I was mentioning last night, Venezuela, Caracas, we were invited to lecture there and I was co-

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[12] Dudley H. Williams and Ian Fleming, *Spectroscopic Methods in Organic Chemistry* (New York: McGraw-Hill, 2008).

organizer with John of that in 1976, and when we arrived there was little local organization at all, and we just took charge. We had to; otherwise we were in a mess.

GRAYSON: So it was you and John –

JENNINGS: John and one or two others. There were one or two Americans, I can't remember their names now. But this is really for most of South America. People were from Ecuador, I remember, from Argentina, and so on. That was the one which at the end we had innumerable thanks in Spanish until we were asked if we lecturers would like to say anything, so we nominated John to get up and he spoke in Welsh for about five minutes. What he said nobody but John knew. [laughter]

GRAYSON: Of course, you didn't know what they were saying in Spanish either.

JENNINGS: No, quite! So John was getting his own back.

GRAYSON: I wonder what they thought when he started rattling off in Welsh; that must've been a little bit of a surprise.

JENNINGS: He didn't speak for long, but as I said he just thought, "Well, we put up with about an hour of Spanish." [laughter] And then I helped Graham with a short course at Purdue [University] in 1982. But most of my time has been spent on MSc courses – it's interesting you talked about post-graduate courses – in a huge number of places. I've done many here. I've given a short course in Edinburgh and at UCSB in Santa Barbara. I was in San Paolo for three weeks giving a course some years ago. Delhi, to schoolteachers, two-week course.

GRAYSON: Now, what do school teachers get out of that? When you say school teachers this would be high school?

JENNINGS: Sorry, they would've been college lecturers. Quite senior people from all over India. It was a post-graduate course at the University of Delhi.

GRAYSON: So when you say MSc that's Master of Science?

JENNINGS: Yes, it's our junior post-graduate degree. It's typically a one or two-year course. Where I've had many links is Portugal, I've given several MSc course lectures in Lisbon, and also in the universities of Madeira and Aveiro, and I've organized an Erasmus Conference, Theory and Practice of Mass Spectrometry, in 1990. That was for the European students, and as I say we've had mass spec courses in the department and I give the introductory course on proteomics, a lecture on proteomics. So I've put a lot into this type of thing, and as you've probably gathered it's because it's absent in so many courses and there are not people willing to teach it. I mean currently chemistry doesn't have a mass spectroscopist here [Warwick] – well, they may have now. They hopefully appointed somebody just today.

GRAYSON: Okay.

JENNINGS: But many departments in Britain will not have a mass spectroscopist.

GRAYSON: Well, there's also this issue that I ran into I believe with Gareth Brenton, is that the powers that be are turning off chemistry in a number of schools.

JENNINGS: Yes.

GRAYSON: Is this coming down from on high from some kind of a high level committee?

JENNINGS: I suspect it was initially because there was a fall-off in chemistry recruitment, and some universities decided that it was economic to merge science departments. Certainly a number have combined chemistry and biology or chemistry and physics, and in Wales the University of Wales has a number of campuses and Aberystwyth I know has closed their chemistry department, and one or two of the staff there I know moved to Swansea – Jeremy Jones, for example, moved to Swansea. Now I think the Swansea numbers were not high. Cardiff is still able to attract students. I know Bangor had amalgamated chemistry, I think it was with biology. I suspect it's largely economics.

If your department is not attracting the students which will justify the staff numbers, and your staff numbers are going to fall below what is seen as a viable minimum, what do you do? And we have been through a period where chemistry numbers did fall – they're rising again now – but I suspect the cost of putting on chemistry, especially practicals, doesn't help.

GRAYSON: So this is something that's driven by you might say market forces?

JENNINGS: Well, I think it's individual universities taking the decisions. There's nobody on high saying, "We've got to cut ten chemistry courses."

GRAYSON: I see. And so as a result some schools are then getting those people who wanted to stay in chemistry and they're able to strengthen their program or keep it functional. And others are just, you know, closing down that curriculum altogether.

JENNINGS: Right, quite.

GRAYSON: Well, that happens. I know at Tulane after the Hurricane [Katrina] they got rid of the engineering department and merged the chemical engineers in with the chemistry department. And I know schools have closed their dentistry schools and this kind of thing. So I guess that's just part of the natural evolution.

JENNINGS: Well, Warwick floated for a while an Environmental Sciences course when it was popular to be environmental, but we rapidly saw that attracted the bottom of the pile as far as students were concerned. If they were good they did a major science subject, and if they couldn't get into a major science subject they went into environmental. So the quality of the students attracted was not good, and after about ten years it was subsumed into various departments. It largely went into biology, in fact.

GRAYSON: There's a phenomenon going on in the United States which is kind of curious with all these television series and programs focusing on the "forensic" science. Apparently in the United States there's a huge boom in people wanting to study forensic science, and go into criminal forensics after they graduate. It's become very popular.

JENNINGS: The answer to that is a good forensic scientist is a good pure scientist first. I think we've got one or two post-graduate forensic science courses, which I think is the right way. If you've got a good science background then you can build on it to go forensic. But to go directly to forensic without a good background I'm not sure is a good thing. Well what is popular here for some reason or other two or three years ago was psychology. Innumerable people wanted psychology degrees, and chemistry even started a chemistry and psychology degree – I don't think it got very far, but it had a number of takers.

GRAYSON: This is looking at the effect of chemical compounds on psychological behavior?

JENNINGS: I think it was a way of trying to boost numbers for both departments. But very often chemistry with another subject means you can drop the math side of chemistry. They tried chemical physics but that didn't take off at all because it's too mathematical. So I mean we have got Chemistry and Medicinal Chemistry, which is quite a successful degree, but that drops some second year physical chemistry, you see. They students still want to run away from calculus.

GRAYSON: That's too bad. Okay, so where are we in the grand scheme of things, Keith?

JENNINGS: Well, another – just continuing with post-graduate teaching, if I may. On an international scale, NATO [North Atlantic Treaty Organization].

GRAYSON: What's the origin of these NATO courses?

JENNINGS: Well, NATO gave funding for scientific meetings of a teaching variety at a post-graduate level, and many years ago Pierre Ausloos got to go with me to a conference.

GRAYSON: How do you spell that?

JENNINGS: A-U-S-L-O-O-S. Pierre. He was Belgian, but he was at NBS [National Bureau of Standards, now National Institute of Standards and Technology] at the time with Sharon Lias. L-I-A-S. And they and myself together with I think I said last night the Rose Marx from Paris, Nico Nibbering from Amsterdam, Hans-Friedrich Grützmacher from Bielefeld.

GRAYSON: How do you spell that Gutzmacher?

JENNINGS: G-R-U-T-Z-M-A-C-H-E-R. And Maurizio Speranza from Rome.

GRAYSON: That's S-P-E –

JENNINGS: Speran-Z-A.

GRAYSON: Okay.

JENNINGS: We essentially formed an international committee which time after time produced NATO schools. We began in Biarritz in 1974 with Ion Molecule Interactions, and I think we kept essentially the same title, Ion Molecule Reactions or something like that.

GRAYSON: And so NATO has the money to do this?

JENNINGS: Yes. NATO produces funding but you have to put in a budget and a list of lecturers and a list of how many students you hope to get from each country.

GRAYSON: So this would then be a proposal that you put together.

JENNINGS: Yes. And the idea is to have it very often in isolated places so you don't get distracted. Also to invite people from countries where science is not strong, so you'd joke that you'd always have Portugal in because that was useful. And typically you could invite something like, I don't know, about eighty students. But you'd have to say where they were from; you'd have to say you would invite three Greeks, five Turks, and so on.

GRAYSON: So even before you arrange this conference, you have . . .

JENNINGS: You have the lecturers and the course title.

GRAYSON: But you also have an idea of how many students will come from where. You don't know exactly who you're going to invite, but know they will be from certain countries.

JENNINGS: You do have a quota. And you try to stick to the quota. Obviously you can't always. So if you say, "Well, we hope to have eighty students; we're going to allocate say ten to the U.K., six to the Netherlands, ten to Germany," and so on. And you try and stick to that, but they don't hold you to it. But it gives an indication of what you're trying to do. And then you write a report and you say how well you managed to do this, and you must produce a book.

GRAYSON: You *must* produce a book?

JENNINGS: Yes, that’s important. Which is what I was saying last night about the last one I went to. The book never appeared despite submitting all my material very quickly – three lectures.

GRAYSON: So how did you find out about this NATO school thing?

JENNINGS: I think it probably came from Pierre who said ‘There are funds available. I think we should do something. Would you help?” And I said “yes.” You identify somewhere to have it. You put in your plan. You hopefully get your money. You then advertise it; you let it be known that you have a quota, and then people start writing in and then the director usually decides who can and can’t come. I mean occasionally you have to say, “I’m very sorry, we’re full up.” Occasionally – occasionally you can have people who happen to be there at the same time but are not receiving any funding, and the Italians are very good at that.

GRAYSON: I see.

JENNINGS: We were in Tetouan. There must have been about fifteen or twenty Italians, and I don’t think they’d all been funded, but it was a bit like a jamboree. You know, maybe five were funded, and another ten came because their professor could afford to fund it from somewhere else. They were not officially attending; they would not get copies of the book. They were not on the list, but they happened to be in the lecture theater at the same time, that sort of thing, you know.

GRAYSON: I see; just happened to be dropping by.

JENNINGS: Yes. But I must say we always were a little more strict about numbers.

GRAYSON: So NATO provides some funds to get you started?

JENNINGS: They would say, “Well, we can afford, we can offer you X.” It might be we can offer you your board and lodging if you get yourselves there. Or it may be a little less; we can offer you something, anyway.

GRAYSON: And there was a conference fee charged to attendees?

JENNINGS: Oh no. Let's say we were having a two-week conference. You're expected to stay for the two weeks, especially if you're a lecturer. We always had some arguments about that, because some people like to fly in, give the lecture, and fly out. That's not the idea at all. They must be there to communicate with the students.

GRAYSON: Sure, there's interaction that goes on outside the classroom.

JENNINGS: So we had a bit of argument with some lecturers about that. And no, we said, "We'll provide you with accommodations for two weeks, and you to come and play your part and attend lectures and mix with people, but you've got to get yourself there." Hopefully the university will provide you with transport money, and then you hopefully had internationally known people. I mean, we'd have say Jack Beauchamp, Mike Bowers, Graham Cooks, Mike Gross, Nico Nibbering, and we had these roughly every four years. I have the dates here.

GRAYSON: You would have about eighty students or so for these?

JENNINGS: Thereabouts, yes. Eighty to ninety usually. Yes, 1974, 1978, 1982, 1986, 1990, and 1995. Then I lectured at one in 2003. It's Garmisch, by the way, Garmisch-Partenkirchen, the one I couldn't remember last night.

GRAYSON: Oh.

JENNINGS: But it was 1982 that we had the tragic event I mentioned last night of Henry Rosenstock dying of a heart attack in Vimeiro, Portugal.

GRAYSON: Oh, that's very sad.

JENNINGS: He again was typical of the sort of people we'd ask to lecture.

GRAYSON: Sure, people with well-known reputations. This conference kept its ion molecule flavor?

JENNINGS: Yes, or gas phase ion chemistry. It began with ion molecule interactions and ion molecule reactions and then gas phase ion chemistry to broaden things a little bit.

GRAYSON: And most of the students were from Europe?

JENNINGS: Not all. We had some from Canada and the States. But preference was given to places such as Portugal or Greece or – if there were such students. And we were also encouraged for political reasons, I believe, to have non-NATO people from Europe. So if a Pole or Russian wanted to come, this would help international relations apparently. So we had a few of those.

GRAYSON: So Eastern Bloc countries were encouraged if anyone was interested in coming.

JENNINGS: Yes. So these were held two or three in France, one in Germany, one in Portugal, and the last one I mentioned was in Morocco.

GRAYSON: And typically these were two weeks long.

JENNINGS: Yes. You arrived on a Sunday and left probably on a Friday afternoon of your second week.

GRAYSON: And so by the time you had your lecturers and students you'd have approximately 100 people who were together in a remote place. They interacted both outside and inside the classroom, and so you got a lot of good exchange.

JENNINGS: Oh, yes. I mean, we'd typically have the lectures in the morning, have the afternoon off, then have a 5:00 p.m. lecture, then an evening workshop, so you could go out and enjoy the local area in the afternoon or sit around talking sitting out on the grass.
They were usually in the summer.

GRAYSON: So it was a very low key but intense educational program.

JENNINGS: Yes. It did bring a lot of people together and fostered international relations and helped countries where there was not much locally to instruct them, like the Portuguese. Very often we'd have say six or seven from Portugal come.

GRAYSON: Did NATO do these for other areas science?

JENNINGS: Oh, yes, they were all sorts of things. Photoelectron spectroscopy I know was quite common, and gas kinetics.

GRAYSON: So it was primarily science?

JENNINGS: Yes, yes. Well, that's what I know of. There may be others; I'm not sure.

GRAYSON: That's kind of interesting. So if someone had a wild idea about a topic, they could approach the people at NATO and propose this.

JENNINGS: Yes. If you had a well-planned course, well-costed, for which there was deemed to be a reasonable demand, then I think you had a good chance of getting it. So that was really an extension of teaching.

[END OF AUDIO, FILE 2.1]

JENNINGS: So why fundamental gas phase ion chemistry? Well, we've dwelt on that a bit, I think. My background was very much in atomic and free radical gas kinetics and spectroscopy, and reaction mechanisms and energetics, so I felt extending it to ions was logical. And I was always interested in mechanisms and energetics. I thought getting beyond the cracking pattern problem was of interest. Now, why ion cyclotron resonance, I think I told you about that.

GRAYSON: We discussed that in some detail.

JENNINGS: Chemical ionization –I always felt that was a much more sensible way to do mass spectrometry. It's much more like chemistry, whereas electron ionization, as I said, is rather a brute force method of knocking the things apart. First of all, you produce an ion, which is not what you want to know about, but you may cause isomerization before you fragment. Then you get the fragments and wonder what it is you started with. Whereas the classical chemical approach is to take the molecule which you're interested in and react it with reagents, and that's what chemical ionization does. So that struck me as a more sensible thing to do, and you have a much wider range of reagents than electrons.

GRAYSON: I don't know if you're aware, but Burnaby Munson and Frank Field published an early paper when they were working on chemical ionization and submitted it to *JACS* and it was rejected because – well, I guess they were in a petroleum company, and *JACS* is of course a very prestigious journal.

JENNINGS: Oh, yes, quite.

GRAYSON: So I guess they couldn't explain a couple things that were minor. But apparently the reviewer thought, "Well, you know, these guys are a bunch of hooligans. They don't know what they're doing. Can't make any sense out of it and why should we publish this garbage?" So that's one of the things I found rather curious; if you look carefully you'll find that most really important advances, when they're first proposed to either a granting committee or an editorial review; they'll frequently be rejected because it's too far outside the box

JENNINGS: Oh, yes.

GRAYSON: And it's kind of sad that that's the way science has to progress. So they got it published, and obviously chemical ionization became a very useful technique.

JENNINGS: Oh, yes, important. It's selective, it's controllable in the type of reactions, type of energetics. You can do a lot more with it. You react with a molecule of interest in its ground state, which is what most people want to know about.

GRAYSON: Yes.

JENNINGS: So that's, you know, why we're pleased when we could do the double bond work, because that's really almost like classical organic chemistry. And also the reaction of the opposite, the minus ions, which were quite peculiar. And why tandem mass spectrometry? Because it allows one to select specific reactant and product ions rather than looking at the whole mess where you don't know what came from what. And so it hopefully gives you detailed information on reaction mechanisms, so if you're looking at the resultant mass spectrum you now have a method of saying what came from what. Whereas just a standard spectrum – if you're clever like Fred [McLafferty] you can work that out. [laughter] Later it became essential for use with the softer ionization techniques, but one had to use collision induced decomposition. See, when I first started collision induced decomposition the motivation was because Henry Rosenstock and his friends were concerned about how they know the metastables were really true molecular things and how it worked. So I started that, and then the quasi-equilibrium theory said that the fragmentation was independent of the way that you put the energy in. I said, "Okay, let's put it in a different way." And my first paper was actually to show that the spectrum I got for I think it was tri-deutero toluene was extremely similar to what you got from electron ionization of 50 eV.[13]

So I could say that collision induced decomposition gave you very similar information to what electron ionization gave you, to which of course the assembled throng said, "So what? Why bother?" But of course I was smiling when FAB came along. So once the softer ionization techniques came along collision induced decomposition, which I'd been playing with for some time, suddenly became everybody's favorite game.

GRAYSON: Sure, because now you needed something to create those fragments

JENNINGS: Well, why FAB? The main thing of course was that it extended the mass range considerably, and for me gave me an extra push with a ton of new CID techniques. And for the first time you could start to talk to biologists.

GRAYSON: Well to me that's the thing that FAB provided. Whole classes of molecules that you'd never get to fly with electron ionization. I did a chart which showed the number of publications using FAB as a function of year, and within two or three years after it was published the number takes off just like a rocket. Well, it was easy to build a FAB source for the most part. A lot of people, if they had an FI/FD source they were halfway there anyway. And the instrument companies hopped on it right away as well.

JENNINGS: I'll come back to FAB in a minute when I talk about interaction with Alan Quayle. But yes, I think it stopped you having to worry about derivatization. You had trimethyl-silyl ethers and those kinds of things.

GRAYSON: Yes, I remember that whole era.

JENNINGS: And in 1972 or 1973, you had the hydroxyl group.

GRAYSON: Right. I'd like to stay and spend a little bit of time talking about surface induced collisions. I mean, this – in my sense this is something that people did but I don't think it really has caught on as significantly as collision induced.

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[13] K. R. Jennings and J. W. Linnett, "Spectra of flames supported by active nitrogen," *Nature* 180 (1957): 1272.

JENNINGS: No, no.

GRAYSON: And so how did you do surface induced collisions?

JENNINGS: We bounced things off electric sectors. But I don't think we learnt anything very exciting from it. You could do it, but we were not interested in the surface. We were interested in the things that were bouncing onto the surface, so I think collision gases were just as good at it.

GRAYSON: So it was another thing you could do but it didn't really provide you with anything terribly useful.

JENNINGS: No, you wouldn't solve any problems with it.

GRAYSON: Except one of the problems with gas collision process is the cross section isn't always that great.

JENNINGS: True.

GRAYSON: And when you collide with a surface, then you should in theory I guess get a better cross section – but then many things also stick to the surface.

JENNINGS: Well, it's interesting you mention the cross section stuff. Almost everybody used helium to begin with on the grounds that it didn't deflect your ions out of the beam so you didn't lose them. We found the heavier the ions you worked with the less it was important, because they were big and they kept going. So when we started working on things like sugars, we began with helium and didn't get very good results, but when we moved to argon we got much better results. So I think much of the early mythology, if you like of CID, was done on small molecules where you did have small ions, and you did have to worry about knocking them out of the ion beam. So helium was the best bet. And if you used things like argon or xenon – xenon was expensive, but you could try it – you certainly got a lot more energy then, but your ions scattered all over the place.

But when you got to the bigger biological ions, I don't think it really mattered as much, and because they were big like sort of sponges moving along if you gave them a good thump with an argon or a xenon atom then they'll wobble about and bits would drop off and that's what you wanted. So actually we published a paper on how it was better to use heavier collision partners with – I think with sugars. I can't remember now; I think it's carbohydrates of some sort.

GRAYSON: Yes, I think the Q-ToF we had at Washington University in St Louis, the collision cell used argon. There was a completely separate gas system that was set up just for the collision gas.

JENNINGS: Yes, and with a Q-ToF you presumably don't have to worry about slits.

GRAYSON: Right, yeah.

JENNINGS: Again, it's a problem with magnetic sectors.

GRAYSON: Yes. Yes, that is an issue.

JENNINGS: Now, you've got the impact of MALDI on this area. We had a very ordinary MALDI instrument, never terribly satisfactory, I didn't think. And I suppose my interest was such that I never got very involved with MALDI. I found electrospray so much more interesting.

GRAYSON: When did you first start working with electrospray?

JENNINGS: When I got my Quattro, really. Prior to that I think we could mess about a bit with the magnetic sectors, but it was difficult. But that was really what I think propelled the use of other mass analyzers.

GRAYSON: Oh, yes. Now, there are two kinds of electrospray techniques; APCI [Atmospheric Pressure Chemical Ionization] and electrospray. So they're kind of the same, but different. So you were primarily using the electrospray.

JENNINGS: Yes.

GRAYSON: Did you do any APCI work, or?

JENNINGS: I don't recall any, no.

GRAYSON: It seems to me that APCI is a viable ionization technique for certain classes of compounds that are not so polar. Whereas electrospray is typically for the more polar compounds.   So although the sources are very similar, they really are different ionization techniques.

JENNINGS: Yes, I suppose since we focused specifically on biological compounds, peptides and proteins, we went entirely to electrospray.

Now, about the most significant analyzer development. I don't have anything written down for that, but I'd have said recently the terrific step forward made in Time-of-Flight. FT is very interesting, but not for every lab, in my opinion. It is expensive, it's difficult to get into, and the ease with which Q-ToF type instruments can be used by all sorts of people I think has made that quite a significant advance. I remember Time-of-Flights before they had reflectrons, before they had source focusing, where you were lucky to get one hundred and fifty [resolving power], I think, and they always showed you the mercury isotopes.

GRAYSON: Oh, yeah.

JENNINGS: Just because they didn't fragment. For normal butane then you've got about one hundred and fifty resolving power. And then they managed to get up to about four hundred and the first one I was offered, in fairly recent times, matched the four hudred. Bruker offered us one; I said no thanks, it wasn't really worth working with.

GRAYSON: So Bruker had a Time-of-Flight?

JENNINGS: Yes; it was a relatively small thing. But once you came to Q-ToF type instruments and the source focusing devices – I forget what they're called.

GRAYSON: The old Bendix used to have what they called time lag focusing. It's basically the same thing as on these modern T-o-Fs   It just was reinvented for the new era of Time-of-Flight. I remember when Time-of-Flight mass analyzers essentially disappeared from the scene.

JENNINGS: Yes.

GRAYSON: When I first started working at McDonnell Douglas, that's all we had was a Bendix Time-of-Flight, and you would go to a mass spec conference and they looked down their nose at you because you had this toy.

JENNINGS: Well, they never caught on over here because it was AEI [Associated Electrical Industries] and VG [Vacuum Generators] who really dominated the field.

GRAYSON: Yes.

JENNINGS: And Time-of-Flight was regarded as a bit peculiar.

GRAYSON: Well, that's definitely changed.

JENNINGS: Indeed, and people might use it for things like shock tube work. But serious mass spectroscopist didn't mess with Time-of-Flight.

GRAYSON: Yes, exactly. As I say, when I went to these conferences they just kind of ignored me because all we had was a Time-of-Flight and what could we do with that? The thing that is interesting if you look at the literature published with those Bendix instruments; the first thing they say in the introduction is they've modified the instrument. So I think it was a great tool for people who wanted to do things out of the ordinary.

JENNINGS: Well, that's what I applied for my money for when I got the ICR, you see, to play with a Time-of-Flight.

GRAYSON: That's right, yes.

JENNINGS: John said that so many Bendix instruments were not working and he didn't think I should get involved with one of them.

GRAYSON: That's interesting. The evolution in Time-of-Flight mass analyzers is almost like a miracle; because I remember a time when people predicted that the future of that analyzer technique was totally dead and that sectors and quadrupoles were going to win the day. Time-of-Flight was just totally out of it.

JENNINGS: Yes, but the improvement of the source design and the reflectron and the multiple pass things changed all that.

GRAYSON: Yes. It's very amazing; truly amazing. I can't believe the resolving power. I've seen it, I've used one, and it's just totally mind-boggling to me.

JENNINGS: And they're relatively easy to use, now; and sensitive. That's really what killed the magnetic sectors, I think. I remember Kratos struggling to show they can just about detect a picogram; a picomole rather.

GRAYSON: Well, those sector people joined the Time-of-Flight business. Kratos has the Time-of-Flight, Bruker has Time-of-Flight, and VG, Micromass, Waters, whatever you want to call them, has a Time-of-Flight. Have you heard much about this new Orbi-Trap?

JENNINGS: No, I didn't know of it until a couple days back. They were talking about it when I had dinner with Ron Heeren and Jim Scrivens and Peter Sadler. They were talking about it then. Is it not as good for getting molecular weights, is that right?

GRAYSON: I am not sure. Mike Gross had some money to buy a new instrument, and I know that he spent a lot of time researching an Orbi-Trap and an FTICR. They already have an FTICR instrument. They have two in his lab at Washington University in St Louis, and they have a Q-ToF, so they have the instruments. Of course, FTICR's got resolving power that goes way beyond what the Q-ToF will do, but they were hot on the Orbi-Trap and then they cooled off, and now I think they got back onto it. One of the issues is you don't need the cryogenic magnet, so you're saving yourself a lot of liquid helium.

JENNINGS: Tremendous time and cost, yes.

GRAYSON: Yes those costs are down. I think they've managed to get the cost of the instrument up in the same range as an FT instrument though. And my understanding is the instrument's accuracy is good. From what I've been able to gather from the little bit that I've read about it is that below mass one thousand it's not as sensitive and as accurate, but above mass one thousand it competes well with the FTICR. I think it may give the FT machines a pretty good run for the money when enough people have got them and used them.

JENNINGS: That was the impression I was getting, and the accuracy of mass measurement though still seems to be a problem to beat one ppm, I think.

GRAYSON: Yes, it's tough.

JENNINGS: Magnetic sectors were essentially getting there years ago.

GRAYSON: I don't know how an Orbi-Trap would work with 'top-down' proteomics where you try and look at the molecular ion of the protein, as opposed to the 'bottom-up' approach where you slice the protein up in pieces and look at those to build it back up. Fred McLafferty has been promoting this 'top-down' approach where you look at the molecular ion of the protein or large peptide and use ECD, Electron Capture Dissociation, technique to fragment it.

He likes that because he compares it very much to what you would get from an electron impact breakdown, so you get to see the significant fragments. So he has a disciple at the University of Illinois who just received an award at the last Pittsburgh conference; Neil Kelleher who is doing this top-down proteomics work.

JENNINGS: Well, we had one of his disciples give us a lecture only two weeks ago.

GRAYSON: Oh, okay.

JENNINGS: [Roman A.] Zubarev. So I think he's interested in possibly coming here. I doubt that he's quite what they're looking for, but he was giving a lecture and I heard him only two weeks ago talking about the ECD work.

GRAYSON: Okay. So you've got a sense of what's going on there. The thing that's amazing to me is the continual evolution of the field. I know people who at one time said, "Well, mass spectrometry is stuck, as a technique for analyzing hydrocarbons, or doing small molecules, or doing non-biological samples." And every who has written it off has been basically shown wrong.

JENNINGS: Oh, yes. About every five years it takes another leap.

GRAYSON: Yes. And it's just – it's like a technique that won't quit. Now of course they've get all these combination instruments that are almost like alphabet soup, with quadrupoles and linear traps and orthogonal Time of Flights. People are putting these things together in ways that no one ever thought of before. Now they're doing this drift experiment, the ion mobility thing.

JENNINGS: Oh, yes, well, this is work done here. Jim Scrivens does that.

GRAYSON: And so they're combining that as well with the mass spec.

JENNINGS: In fact, had one of our funding agencies moved a bit faster, I'd have been doing some of that. I retired before they came up with any money. Just as an aside, we're interested in mad cow disease, and mad cow disease is thought to arise from misfolding of proteins, which on the whole don't change chemically.

GRAYSON: Right.

JENNINGS: So how do you detect that? Ion mobility. So we put together a deal with Mike Bowers, who is piloting this at Santa Barbara, and the department here, and we applied for money to work on it to try to produce a test for mad cow disease before the person or the cow had died, so you did not have to sample its brain. Now, that fell foul of the funding people because it wasn't pure and it wasn't applied, so the pure people said, "This is really not for our side of it. This is for the DEFRA funders." And DEFRA said, "But there's a lot of pure work got to be done to get the stuff right and so on." And after about two years I retired. I think it took three and one half years for some money to come through. But I did a lot to write the original proposal for that, but it just got put back and put back and eventually I think, the chemists, the biologists and the government all had to fund bits of it or something.

GRAYSON: I see. The problem touches in two different areas.

JENNINGS: Sure. But certainly ion mobility I think is quite useful. And then Waters is producing quite nice instruments to do that. I think Jim was getting one. So that gets us onto personal interactions, which I have quite a lot to say on.

Let's start with John Beynon. Well, he was about the first person I interacted with, and as I think I mentioned last night I remember going to Blakeley to hear him talk and hear his group talk about broad metastable peaks, which they were seeing in nitrophenols at the time. And they gave an analysis on how the width of the peak varied with the accelerating voltage and how you could deduce the energy release. And I found that very interesting because as I told you I'd then done a bit of work on the MS10 and got these broad peaks, which I never published. I wrote it up in a conference for MS10 users which I'm sure nobody ever read. But when our MS9 appeared in 1964 in November we got it working, and I was able to submit a paper, or a note, rather, in the following January on the doubly-charged benzene ion, because by modifying the treatment I was able to show how you could look at both ions.

So my technician thought I'd gone mad when I said, "Well, put some benzene in, could you, Bill, and start scanning down from about mass one hundred and ten." He said, "Well, that's a waste of time!" and I said, "Just you wait." And around one hundred and one and one hundred and two a nice broad metastable peak came up which was a sixty-three plus ion from the doubly-charged molecular ion, and I'd keep it going right down, and at five-point-seven there was a methyl ion, so you could see both. And so this showed the doubly-charged benzene ion did fragment into bits. I'd seen the methyl plus on the MS10 with voltage scanning. So then we looked at a lot of aromatic ions.

GRAYSON: So was this one of the first times people had observed a fragment ion at a higher mass than the parent?

JENNINGS: I believe so, yes.

GRAYSON: Because it's counter-intuitive. I mean, like the technician says, why should he start at 100+ and why would you find a mass larger than the parent?

JENNINGS: Well, it normally comes with M_2^2 / M_1 for singly charged ions fragmenting. It comes at $2M_2^2 / M_1$, I think it is when a doubly-charged ion fragments to give a singly-charged product. So it's going at twice the normal speed because it's doubly-charged, and when it fragments it's still going at nearly twice the normal speed, so of course it appears at a different place in the spectrum. Anyway, I think we were the first to show that you could see both bits, but this then brings me onto another gripe I have about mass spectrometry – where do you publish? We put this in *Chemical Communications* and that was referred to quite a lot because it was as I say a first observation.[14] We then observed about another twenty or thirty from various aromatics, and when we published those we'd published in the *Transactions of the Faraday Society*. Hardly anybody has ever referenced that paper.[15]

You know, we have pictures of them and a whole table of twenty-thirty of them. But Faraday Society readers don't do mass spectrometry, and mass spectrometry people don't read the *Transactions of the Faraday Society*. So the paper was essentially lost.

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[14] W. Higgins and K. R. Jennings, "Metastable transitions ofthe doubly-charged benzene ion," *Chemical Communications* (1965) : 99-100.

[15] W. Higgins and K. R. Jennings, "Doubly-charged metastable ions of aromatic hydrocarbons," *Transactions of the Faraday Society* **62** (1966): 97-103.

GRAYSON: Why did you select that journal?

JENNINGS: It's the standard physical chemistry journal in Britain, and we thought the research was physical chemistry. But it didn't impinge on anybody really, and I remember talking to John Beynon about where he published, and he said well, it was difficult. He published in *Zeitschrift für Naturforschung* and I said, "Why!" And he said, "Well, they accept the papers." So I published one or two things in there; I know Mick [Michael] Barber did, but I never got any referee's comments back, which worried me, and I think the editor decided whether or not it was publishable.

And I didn't really feel comfortable about having papers where I didn't even get a thing saying, "Yes, I've read this and it seems okay," you know? Just something saying it was accepted. And I think most of us found it difficult to know where to put things, and really in a sense that was why we started the mass spec journals.

GRAYSON: What was the first one you guys started?

JENNINGS: I think it was probably *Organic Mass Spectrometry, The International Journal of Mass Spectrometry and Ion Processes*. But again, you see, it separated us from mainstream science, which I've always felt is perhaps one of our mistakes.

GRAYSON: You could do an analysis of the journals in which mass spec papers appear over the decades. I don't think a lot of it gets into *JACS* [*Journal of the American Chemical Society*] or the typical "chemistry" journals in the United States.

JENNINGS: Analytical chemistry gets quite a bit. And then *JASMS* [*Journal of the American Society for Mass Spectrometry*].

GRAYSON: But then JASMS is also basically another specialty journal, as you know.

JENNINGS: Yes.

GRAYSON: Of course, some of these journals are so narrowly focused..

JENNINGS: Well, Jim Scrivens and I were saying the other day that really one of the best journals for content is the *International Journal* [*on Mass Spectrometry and Ion Physics*, but one of the poorest journals for referencing is the '*International Journal*.' The impact factor is very low. You put something into *Rapid Communications*, which most people regard as a rather low-quality journal, and all sorts of people will read it, all sorts of people will reference it. So if you're interested in citations, that's the one to go to. But the '*International Journal*' I'm afraid is a bit like the Faraday was for me. You publish in there, but it rarely gets referenced. It's too physical, you know; there's too much instrumental material. Too theoretical. It's not sort of what happened when I put my peptide into the mass spectrometer.

GRAYSON: Well, I think – is there a biological mass spectrometry journal now? That sounds vaguely familiar.

JENNINGS: Yes, there was *BMS* [*Biological Mass Spectrometry*]. Well, that rather scathingly was regarded as "OMS [Organic Mass Spectrometry] Failed" for a while. Anyway that was my first link with John, and then we had the Venezuela trip together, and the Lubljana one.

GRAYSON: Where was that other second one?

JENNINGS: The Lubljana one, the Jožef Stefan Institute I mentioned earlier. But we were just getting together to get him a visiting professorship in Sheffield when I moved to Warwick, so we managed to get him one here. He then came for about a five-year period and he did one or two lectures each year.

GRAYSON: So he started at Swansea – he was already at Swansea then, was he?

JENNINGS: Yes, yes.

GRAYSON: Because he had this kind of interregnum between ICI and Purdue.

JENNINGS: Exactly, I know. I think he went to Swansea a little after that. He still had been at Buxton, because I know he thought Buxton to Sheffield was easier than Buxton to Warwick. Yes, then there was the Purdue affair, so I went to Purdue when he was there and actually had a chat with the chairman about whether I'd be interested in going; this was before Graham went. But I have to say that Purdue didn't attract me, and it was out in the middle of nowhere.

GRAYSON: Yeah, right.

JENNINGS: Really the idea of living there – I couldn’t imagine living there.

GRAYSON: It’s this American tradition of land grant schools that, are put in the middle of nowhere, as you say, because the land is there. I think Purdue is probably a land grant school. It’s like Cornell [University]; kind of the middle of nowhere. But so many schools, University of Illinois, they’re all located in the hastings.

JENNINGS: Well, I was there giving a seminar, and John said, “Well, while you’re here why not have a chat with the chairman, see what you think of the place,” and so on. So I think he was coming to the end of his time at Purdue.

GRAYSON: So I gathered from my conversation with him that this professorship he got from the Royal Society was something of a surprise. To him, at least that’s the way he expressed it to me. These things don’t come every day.

JENNINGS: Oh, no; they’re quite rare.

GRAYSON: They don’t hand them out like candy.

JENNINGS: No, it was a great achievement to get it.

GRAYSON: It was a very significant achievement. I just wonder what the mechanism behind selecting people for these things must be.

JENNINGS: I’m afraid I wouldn’t know; I’m not in that category.

GRAYSON: You don’t have any insight as to what it is.

JENNINGS: No.

GRAYSON: But obviously someone is looking at people's scientific record and just thinking that this fellow was worthy of receiving this honor. And it's really a tremendous honor.

JENNINGS: Yes, I haven't really known many of those. Alan Carrington was one; he worked on the electron spin resonance, and long ago, A.G. [Alfred Gordon] Gaydon, who worked on spectroscopy and dissociation energies.

GRAYSON: How do you spell his last name?

JENNINGS: G-A-Y-D-O-N. The reason I know him is he was my external examiner for my D. Phil. He wrote to me and said, "Ah! Got these new bands, have you? Please bring your photographic plates along so I can have a look at them" so I go along carrying my plates.

GRAYSON: Ah!

JENNINGS: He was an eminent spectroscopist who had very limited eyesight owing to an explosion problem, but he wrote a nice book, *Dissociation Energies* [*and Spectra of Diatomic Molecules*], which I remember reading with considerable interest.[16] It was a time when the dissociation energy of nitrogen was still either 9.76 electron volts or 7.8, I think it was; it depended on how you interpreted the spectrum. And [George B.] Kistiakowsky showed it was 9.76 with his shock tube work – but anyways, sorry, it's beside the point.

GRAYSON: So then when you came to work John had a five-year appointment as a visiting professor here?

JENNINGS: Yes, and that meant we had joint meetings. We use to hire a minibus and my group went off to Swansea for the day and his group came here for the day about every three months. We each got students to present their work, you know, to each other.

GRAYSON: So this was a day-long affair.

JENNINGS: Yes, well, you saw how long it takes to drive. If we left at 7:00 a.m. we'd be down there for about 10:00 a.m., leave about 6:00 in the evening.

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[16] Alfred Gordon Gaydon, *Dissocation Energies and Spectra of Diatomic Molecules* (Ann Arbpr, Michigan: The University of Michigan, 1950).

GRAYSON: So this would've been a great opportunity for the students to get some criticism of their work.

JENNINGS: Yes, and also to present a twenty-minute talk in circumstances which needn't really worry them too much. They could make their mistakes among friends, if you like. And get some good feedback. And they could see how other people did things, and that was very, very common. We had a joint post-doc for a while as well, Dilip Sen Sharma, and that went quite well.

GRAYSON: So at the end of five years?

JENNINGS: You couldn't carry on. This was supposed to be a one-off five-year thing. You couldn't renew them, as it were; the university didn't allow that.

GRAYSON: So they didn't want anyone to be too comfortable with this arrangement.

JENNINGS: Yes, quite. You couldn't sort of have your friends come in forever. [laughter] Since then we've kept in touch at conferences after that. And then there is the ICR connection as well that I mentioned earlier. The next one you had down for interaction was Klaus Biemann – very little actually to do with him, probably because he always seemed to be working on peptides and as I said for many years I didn't have anything to do with biological stuff. I visited his lab once – I can't remember when now – and I met him occasionally at conferences. But I have to say I don't think we interacted very much. I mean, I got on very well with him, don't get me wrong.

GRAYSON: Did you use his book at all?[17]

JENNINGS: Not very much, no.

GRAYSON: What about Beynon's book?[18]

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[17] Klaus Biemann, *Mass Spectrometry: Organic Chemical Applications* (New York: McGraw-Hill, 1962).

[18] John Beynon, *Mass Spectrometry and its Application to Organic Chemistry* (London: Elsevier Publishing Company, 1960).

JENNINGS: Not a lot, really.

GRAYSON: It didn't cover the kind of things you were interested in.

JENNINGS: Right. It was about technique. I mean, he gave a detailed description of the gallium inlet system, for example, which I was remember using back in the dark days.

GRAYSON: Yeah, back in the dark days.

JENNINGS: I read McLafferty's book and then [Herbert] Budzikiewicz, Djerassi and Williams got very well thumbed![19] And as I said McLafferty's *Advances in Mass Spectrometry* was about 1962 or 1963 I think; I found that a good book. Biemann's was obviously a good book, but not on the subject area I worked in. I know it was very well regarded.

GRAYSON: But I think that really sealed his appointment at MIT [Massachusetts Institute of Technology] I think when he wrote that he was tenureless, and shortly after that, he was granted tenure.

JENNINGS: A book I did read quite a bit, or two books, Field and Franklin, of course, and the book edited by McDowell that had quite a lot of chapters in it by different people.

GRAYSON: M-C-D-O-W-E-L-L?

JENNINGS: Yes, he was at U.B.C., wasn't it? Yes. University of British Columbia, Vancouver. And yes, that was a well thumbed book; it was almost dropping to bits when I finished it. And Kiser's book I remember.

GRAYSON: Bob's, yes.

JENNINGS: And McDowell's book – that was fairly physical. But as far as interpretation was concerned there's nothing that would beat Djerassi's book, I thought.

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[19] Herbert Budzikiewicz, Carl Djerassi, and Dudley H. Williams, *Interpretation of Mass Spectra of Organic Compounds* (San Francisco: Holden-Day, 1964).

GRAYSON: Yeah, and then they had this huge series of papers that went on forever.

JENNINGS: Yes, quite! [laughter]

GRAYSON: I remember fifty-seven or whenever, I can't remember how long they went.

JENNINGS: Well, we had an ESR chap in Britain, Martyn Symonds, who I think published something like three or four hundred papers. He had "Studies in ESR Spectrometry Part 378" as titles on all aspects of ESR.

GRAYSON: Right!

JENNINGS: Then we come to Mick Barber. Mick actually was at Queen's when I was, but he was three years behind me. Queen's College Oxford. I first met him when he was there as an undergraduate. I had just started my Part 2 when he appeared in college.

GRAYSON: So was he in physics or chemistry or?

JENNINGS: He was in chemistry. He actually followed the same course that I did three years behind me. Because he also went with Jack Linnett, did his D. Phil. with Jack. And we met a bit more than we might've done because we were both interested in music.

GRAYSON: Oh, okay.

JENNINGS: The college had a musical society, the Eglesfield Musical Society, which gave one concert a term. We both sang in that.

GRAYSON: Oh, okay. So this was a choral group composed of just people in Queen's College?

JENNINGS: No, no; people from outside could join. But most of the basses and tenors were from Queen's.

GRAYSON: And Mickey Barber was in that with you.

JENNINGS: Yes. He was much more musical than I. I think he could play the organ, if I remember right.

GRAYSON: Too much foot coordination for me.

JENNINGS: Quite.

GRAYSON: So that was a very early association.

JENNINGS: Yes, yes, and I remember as I was finishing he was just about to start his Part 2. But some of his work then actually led on to much of my early work. He and Martin Elliot showed how you could use a knob on the MS9 called the Delta V knob which allowed you to vary the accelerating voltage by about 2 percent, and this enabled you to look at metastables in the first field-free region if you were losing a hydrogen atom or a hydrogen molecule. And they were very difficult to see because they didn't move very far down in the normal spectrum.

GRAYSON: So this had been close to the molecular ion region.

JENNINGS: Yes. So this was Barber and Elliot, ASTM, 1964, Montreal, and they showed how you could use the Delta V knob, you see. And I got a machine and I said to Martin and Mick, "It's a pity this Delta V knob doesn't work a bit more than 2 percent, isn't it? Why can't you make it 50 percent or 100 percent?" Oh, they didn't know about that, and I said, "Well, I'd like you to." So Martin devised a circuit which allowed me to vary the voltage by 50 percent, and that was my first paper on benzene metastables in the first field-free region, which was erroneous, because Martin's circuit wasn't quite right. You had two hundred and seventyvolts on either side of the electric sector plus and minus, zero in the middle.

GRAYSON: Right.

JENNINGS: As you varied the accelerating voltage, unfortunately the zero moved, so when your ions went down the middle they were starting to experience a retarding potential and that made one part difference in eighty. So I found myself having to explain why $C_6H_6^{++}$ could somehow produce $C_6H_7^{++}$ ions. Where was it picking up the proton, the hydrogen mass -- I don't know. Anyway, Martin said he was very sorry but there was a slight error in his circuit. He then produced the right circuit and things were fine thereafter.

GRAYSON: Now, was he at –

JENNINGS: He worked with Mick Barber at AEI then.

GRAYSON: Oh, so Barber was with AEI. So originally he was in the instrumentation side of things. With Elliot, Martin Elliot.

JENNINGS: Yes, yes. See, he would leave Queens in 1961, because I left in 1958.

GRAYSON: And then did he go right into –

JENNINGS: I think he must've gone into AEI then.

GRAYSON: Okay. So you were talking to these guys about this, and they were in a position to actually make the change you suggested.

JENNINGS: Yes, and it was 1965 that I began to work with Martin, and it was late 1964 that I said, "I'd like this knob to cover a much bigger range please. Could you tell me how to do it?" Because of course it was a service instrument and they'd be worried if I left my circuits on.

GRAYSON: Yes.

JENNINGS: So anyway, I did that and then they financed a student who worked with me to demonstrate how useful it was, and then of course everybody started doing the same thing. Once we showed how we could see metastable peaks, people were doing it all over the place.

GRAYSON: And these would have been ions that decomposed right out of the ion source.

JENNINGS: Yes, yes. And it was the summer of 1965 that I went over to work with Jean Futrell and did the same with him, on the tropylium ion formed in different ways from different precursors. So everybody started doing the same thing about the same time, but I think we were among the first to show how useful it was.

GRAYSON: Well, I have all those old programs and abstracts, so I can certainly pull up this paper or abstract anyway.

JENNINGS: Well, Barber and Elliot, ASTM Montreal, 1964 was the Delta V paper, and they are usually given credit for using that method first. I'm given credit for having extended it to a larger mass range. We extended it by fiddling about with the circuitry. To begin with we learned to do losses up to half the mass and then extended it to losses up to three quarters of the mass.

GRAYSON: Yes, so the Delta V circuit basically just changes the accelerating potential.

JENNINGS: Yes. Without changing the electric sector.

GRAYSON: You want the electric sector to stay at what it would for the normal accelerating potential, and by having this broad sweep in the Delta V circuit you're able to change the energy of the ions that come out of the ion source.

JENNINGS: Yes.

GRAYSON: Then when the decomposition occurs in the first field-free region the energetics will be such that –

JENNINGS: You looked at ions at the same energy all the time, because the same energy –

GRAYSON: Which has been defined by the ESA plates. So however they were created they were always going to have the same energy.

JENNINGS: Well, you dropped the instrument to 4 kV normal operation, then you started to raise the accelerating voltage to 5 kV, then a bit dropped off so it was only 4 kV's worth going through the center, then to 6 kV and a bit dropped off, it was still only 4 kV going through – do you see what I mean? So by the time it was 8 kV half of it had to drop off to get through the 4 kV. So it could go up to say one hundred, giving fifty that I could collect with the circuit. If you go down to the 2 kV operation, I could go to one hundred leaving twenty-five.

GRAYSON: So that was setting the ESA plates to lower value than standard operation.

JENNINGS: Instead of having 540 volts across them you have 270 volts across them, then 135 volts across them.

GRAYSON: And then this machine did an 8 kV normal operating range.

JENNINGS: Yes. Now, I worried them occasionally. We went on to working with metastable peak shapes; what one finds is if you have the release of very little energy your metastable peak looks like that, and as the energy release gets bigger it does this. So you can play with the electric sector voltage, and change the peak shape, and I did that. I remember putting the best part of a kilovolt across the electric sector. Oh, Martin had a fit! He said he didn't know the insulation could stand that.

GRAYSON: Yeah. It wasn't exactly designed, for that.

JENNINGS: I think it was eight hundred or nine hundred– I can't remember exactly. But anyway, with Mick Barber and Martin Elliot I had a lot of fun. We worked together lot of times, and also with Brian Green before he joined VG, and with Alan Wolstenholme. So my interaction with Kratos was very intense. We worked together on all sorts of things, and we used this Delta V variation – we didn't use the Delta V knob, we added a potentiometer which went from nought to nine hundred and ninety-nine, and the number in it, if you put one in front, gave you the fraction you'd lost, so 1.444 would tell you immediately what was going on. We did fragmentation of peptides that way in the early days, and we used a simple, limited data system to try and analyze things.

GRAYSON: So did you rely strictly on metastables, or did you have a collision cell in the first field-free region?

JENNINGS: In the beginning it was just with metastables. That was in 1965 and 1966, and I didn't start collision induced decomposition until about 1967.

GRAYSON: But using the same technique.

JENNINGS: Yes, my first thought was "Let's see if this goes up much if I put some gas in it." and all hell broke loose with metastable decompositions. So that's really my interactions with Mick Barber.

GRAYSON: Were you privy to his work on the development of FAB ionization?

JENNINGS: Yes. I'll come to that in a minute. Then we have Henry Rosenstock; I visited NBS quite a few times, talked to him and Vernon Dibeler and John Herron.

GRAYSON: So Vernon Dibeler is a name going back to the beginning of American mass spectrometry.

JENNINGS: Indeed. I can just remember him, yes.

GRAYSON: Okay, because I'm looking at the early programs of ASTM meetings and his name keeps popping up.

JENNINGS: Well, I was interested in appearance potentials and so on, and we did some work on that ourselves, on ionization efficiency curves and so on. And as I say, Henry often referred to his book as "the book of random numbers." [laughter] He said, "Oh, you mean the book of random numbers?"

GRAYSON: That's rather interesting. Maybe somebody else calling it that, but I can't see him calling it that.

JENNINGS: I used to have entertaining discussions with him about these things, and then unfortunately as I say he died at this NATO meeting in Portugal in 1982. But I certainly followed a lot of his work and was interested as a spectator, and also the quasi-equilibrium theory. Never actually doing anything with it, but seeing what it led to, and his work with Wahrhaftig and company.

GRAYSON: So you just remember meeting Dibeler? And you didn't have very much of an interaction with him?

JENNINGS: No. I met him once or twice when I visited the NBS– it was really good to mix with and actually see the people whose work you read about.

GRAYSON: Definitely. Did you do collaborate with Rosenstock?

JENNINGS: Oh, no. We always talked at conferences. I'd talk about what we were doing, and he would comment, and so on, and I always got on very well with him. He was a very nice chap. Then we get to Nico Nibbering; I first I got to know him when I got this letter from him, very formal, saying, "Could he please visit my laboratory to see this Delta V knob?" Or the Jennings knob, as he referred to it. He actually put it on his MS9 in Amsterdam; and they referred to it as the Jennings knob. [laughter] It was in good fun!

GRAYSON: It does have a certain distinction.

JENNINGS: I've known him for about forty years or so now.

GRAYSON: So he was rather formal in approaching you?

JENNINGS: Yes, I think he was just starting out and this was about 1966 or so, 1967, and we visited each other quite a bit. We had met, and of course the NATO schools, we worked together on those. And I visited Amsterdam in 1993 and I was the John van Geuns lecturer at the University of Amsterdam.

GRAYSON: Okay. That's on your C.V., so I can get information from there.

JENNINGS: And I also had a post-doc, one of his students, Ronnie van Doorn, a really nice chap, came and worked with us for a couple years and later, Andries Bruins worked with me. And I examined some of his students and he examined mine, so we've kept in quite close contact. In fact we visited with Nico about a year ago. So we go back a long way with Nico.

GRAYSON: Now, he just retired too –

JENNINGS: Yes, sort of.

GRAYSON: Sort of, okay. Yes, some of these people don't really retire, I guess.

JENNINGS: Moving on to Jean Futrell, I've already mentioned him, haven't I? It was at Wright Patterson Air Force Base, going to work over there in the summer of 1965, and then he invited me back to present the work in 1966 at ASTM E-14 meeting.

GRAYSON: So how did you first get associated with each other.

JENNINGS: Well, I think I told you about Jean DuBios who was over talking to Brian Stevens about photochemistry, and Brian went to work with Jean DuBois and I was talking to him and he said, "Well, we've got a mass spectroscopist, why not come in and work with him?" I said, "Great, yes." So I went over in the summer of 1965. Jean [DuBois] talked to Jean [Futrell], Jean [Futrell] raised some money, and I went over and worked on the tropylium ion heats of formation stuff, and spent three months there. And that really was my introduction to American mass spectrometry; I met a few visitors and then the following year, of course, I went to the ASTM E-14 meeting. That was the first time I'd been to the States since my post-doc days. And the following year, the first meeting, you know, you suddenly began to meet people you'd read about.

GRAYSON: And you got to enjoy those hospitality suites.

JENNINGS: Yes, yes. [laughter] The "hostility" suites.

GRAYSON: Hostility suites. I can't believe that I did some of the things that I did at those.

JENNINGS: It was always very good to drink ginger ale and tell other people you were drinking Scotch and ginger ale.

GRAYSON: Yes. Well, typically they could go on until ungodly hours.

JENNINGS: Quite so, quite so.

GRAYSON: They've really tamed things down at the meeting now.

JENNINGS: I should hope so!

GRAYSON: It's probably a necessity for the size of the meeting.

JENNINGS: But I kept in touch with Jean [Futrell]. I mean, we saw each other last I think at the Prague meeting in 2006. But you know we exchange Christmas cards and that sort of thing, but we haven't done anything scientific together for some years now. And Mike Bowers, I started with him really about '68 when we found we were both working on ICR of alkenes. We were both non-Baldeschwieler people, you see, so that threw us together a little bit because the Baldeschwieler group were terribly close knit and gave you the impression that if you hadn't been through Baldeschwieler's lab, well, what were you doing working on ICR? You couldn't possibly know what you were doing.

GRAYSON: Okay, one of those superiority things.

JENNINGS: Yes. So the outsiders tended to band together a little bit, and Mike actually had to build his own ICR machine, he didn't have any funds to buy one. So we got to know each other quite well and started to visit each other, and he'd spent two periods here on sabbatical leave. I spent one period there on sabbatical leave. We published probably ten or eleven papers together.

GRAYSON: Where was he when you first got together?

JENNINGS: University of California, Santa Barbara, he's been there ever since. I think he worked in the Jet Propulsion Lab for a bit, and then went to Santa Barbara as a young lecturer and he's been there ever since. So yes, I've known him since the late 1960s.

GRAYSON: So how many other of these non-Baldeschwieler people were there?

JENNINGS: Well, there was Maurice Bursey from North Carolina. Jay Henis worked I think for Monsanto.

GRAYSON: How do you spell that last name?

JENNINGS: I think it's H-E-N-I-S. Yes, there was a lot of one-upmanship with comments like, "Well, how anybody living east of the Rockies could possibly know anything about ICR," things like that.

GRAYSON: Very uppity.

JENNINGS: It was all in good fun. And I had a very good post-doc from Mike by the name of Lew Bass, who was a theoretician.

GRAYSON: And how do you spell that one?

JENNINGS: Well, Lou and then B-A-S-S. I think it's spelled L-E-W, not L-O-U.

GRAYSON: Oh, L-E-W. Okay.

JENNINGS: Mike has been a lecturer at most of the later meetings, not all of them, but most of them. Oh, the other thing was I think he has been instrumental in nominating me for one or two awards. I got the Distinguished Contribution Award, and the Field and Franklin Award, and I suspect he's at least in part key to that. He's still in touch with Warwick because he was over here last September and I think he's coming over next September according to Jim; so my association with him has now been handed on to Jim Scrivens; which is why I know all about the ion mobility work. But in fact one of Jim's people was in his lab in January, doing some measurements.

Well, next on my list I had Ausloos and Lias, and we already discussed that NBS and NATO, really. And Graham, well I've known Graham since he was a Ph.D. student in Cambridge with Dudley Williams.

GRAYSON: Where were you in your career when you met Graham?

JENNINGS: I was in Sheffield then as a young lecturer, and Graham had got his Ph.D. in Natal, but left South Africa to come to Cambridge, did another Ph.D. with Dudley Williams, and he was also interested in metastables at that time. And so we got to know each other then when he was in Britain and kept in touch really ever since.

GRAYSON: So you met because of the common scientific interest.

JENNINGS: Well, he was working with Dudley Williams, and I interacted with Dudley quite a bit. In fact Dudley's on my list to talk about in a while. And Graham's invited me to Purdue several times; I've given, helped with a course of lectures there, and he spent sabbatical leave here.

GRAYSON: So he elected to go to the States. Do you know if there's any motivation for that other than just better opportunities?

JENNINGS: I think it's probably better opportunities, yes.

GRAYSON: When did he get out of Cambridge with the second degree?

JENNINGS: He was in Cambridge I would say about 1966 to 1968, something like that; thereabouts. Then he came over to spend time with us I would imagine in the 1980s – I could look it up, but I can't remember exactly.

GRAYSON: Well, he's been extremely productive at Purdue.

JENNINGS: Heavens, yes. Did some marvelous work.

GRAYSON: I think he's one of the motivators behind this Orbi-Trap instrument. I know he consults with whatever it is now, Thermo Fisher or what-have-you. I can't keep track of the way these instrument companies buy each other.

JENNINGS: Indeed.

GRAYSON: It used to be Finnigan, Thermo Finnigan. So I think he's been one of the prime movers on the Orbitrap

JENNINGS: Now, Allan Maccoll. Born in Scotland, but with a strong Australian accent. [laughter] He lived in Sydney for a long time.

GRAYSON: Oh really? So what was that about?

JENNINGS: Oh, he came back to University College, London. He was the prime motivator behind starting *Organic Mass Spectrometry* journal and the British Mass Spectrometry Society. He used to work on gas kinetics, pyrolysis and the like. He was sort of like a father figure almost to us all, and he died some years ago at eighty-four But he'd be ninety now if he were still around; his wife died only about two years ago. But he was very strong on fundamentals.

GRAYSON: So where did he operate out of?

JENNINGS: University College, London. I examined his students, he examined mine. And we also liked to talk about – let's see, let me get this right – yes, Pimms No. 1 and Pimms No. 2.

GRAYSON: Pimms.

JENNINGS: Pimms. You know the drink Pimms here?

GRAYSON: Vaguely familiar with it.

JENNINGS: Yes, well, to him, PIMS was Photo Ionization Mass Spectrometry.

GRAYSON: Oh, I see.

JENNINGS: And Penning Ionization Mass Spectrometry. That's one of his jokes. Allan's characteristic pose is fifth along on the top row.

GRAYSON: I'd like to take a picture of this. I see he has a large mug of beer about to be imbibed.

JENNINGS: A very hospitable guy, and you know I think very few people would say they didn't like him. People really got on very well with him.

GRAYSON: So when I talked to John he seemed to think there was some confusion about the first chair of the British Mass Spectrometry Society. He thought he had been more involved in it in the beginning, and then apparently the record shows that he wasn't.

JENNINGS: Ah, I'm not sure, actually. I have to say that one of the few people who didn't get on with Allan was John.

GRAYSON: Oh; okay.

JENNINGS: I don't quite understand this. I think there must be some background to it which I'm not privy to, but certainly John was undoubtedly the leading mass spectroscopist in Britain. Nobody ever questioned that. But he wasn't an academic, and I think Allan as the academic, perhaps did more towards the pushing the subject with a society and the journal. John has always been very keen on a world mass spectrometry society, which I think Graham is involved with now. Nico is, I think. I've never seen the point of this. It's not like you can have joint ventures with the Martians or anything like that, can you? But there was certainly a bit of tension about; at one stage. Currently there is nothing like the Americans' [society] at all.

There are a few British academics now working who are rarely very involved with it. But it's become much more commercial with people from industry, Pfizer's so-and-so analytical something or other. So there was a time when British academics ran it – Allan Maccoll, Dai Games, myself, John, but we've all sort of either retired or fallen away from it. Now it's run almost entirely by business people.

GRAYSON: What is the membership of the BMSS? Do you have any sense of it as several hundred, a thousand?

JENNINGS: I would think it's at least several hundred, yes. But you know I don't know the background of the very early days.

GRAYSON: That's the only comment that John Beynon made that I thought was a little negative. The fact that he thought he had had a better, bigger role in getting BMSS started than apparently other people did.

JENNINGS: Well, to be fair, he was running what you might call the mass spectroscopy group, which was a very informal get-together at ICI. And then a chap called Peter Knewstubb tended to run – that's K-N-E-W Stubb, B-B –the mass spectrometer discussion group, but that was very much more for small mass spectrometers. They might make their own Time-of-Flight

– and there was as I think I told you what was called the 'lunatic fringe' because they would bore holes in their mass spectrometers and take a hacksaw to them and things like that. John's discussion group was really more for those using a commercial instrument, and out of this somehow the British Mass Spectrometry Society grew at a time when I was just getting involved So I don't have any real background to what happened.

GRAYSON: And so you're interaction with Maccoll was just as an associate. Did you examine each other's students?

JENNINGS: Yes, and we met at conferences. I remember going to a conference to celebrate I think it was his seventieth birthday as a speaker and things like that.

GRAYSON: So he's an esteemed colleague.

JENNINGS: Oh, yes, very much so. As I say, he was almost like a father figure at – in a different way from John. John dominated things, he took control of the situation. In a room, John was in charge.

GRAYSON: I see; okay.

JENNINGS: He was that sort of person.

GRAYSON: Sounds like Frank Field.

JENNINGS: Yes, you'd know he was there. You'd never find John quietly in the corner. [laughter] He was always telling jokes; he had an infinite supply of jokes. I don't know where he got them all from.

Well, next I had Elliott and Evans, and that really is a carry-on from Barber and Elliott, because Mick Barber left AEI to go into UMIST [University of Manchester Institute of Technology] about 1967-ish, I think. And then Martin Elliott started to work with Syd Evans at Kratos and I worked together with them for probably about 15 years, although in the end Martin ended up moving to Micromass. But much of the work on CID, linked scans, MS9, MS10, MS50, 4-sector, was all done with them, and as I think I said last night; often they were able to develop a technique but they didn't have a chemist to use it, so I was almost used as an adjunct. They'd say, "Well, here's a technique, what do you think you could do with it?" I was quite happy with that.

GRAYSON: Now Syd, that was spelled S-Y-D, wasn't it?

JENNINGS: I think it was S-I-D-N-E-Y.

GRAYSON: Okay. When I was here visiting on pleasure a long time ago, I sat with Elliott and Evans and another fellow Alan Errock?

JENNINGS: Oh, that's Alan Errock, yes, yes. Now, he was the electronics guy.

GRAYSON: Yes, and we had a conversation amongst the four of us which I recorded. I'm not sure if it has been transcribed. That was a freelance thing that I did because I was in the UK and able to get these guys together for a session. So some of that from the AEI side I'll be able to get from that interview.

JENNINGS: Yes, well, Alan Errock was often the man who devised the electronics for the instruments.

GRAYSON: He was a great host. I visited him separately when my wife and I were in London.

JENNINGS: I didn't know him as well as the other two. But Sid was the one who would discuss the hardware. He'd know where the slits were and how many thou [thousandths of an inch] they were, and what the tolerance was for one thing and the other, and things like that. Martin I know had written a quite good article on ion sources; I think that was his forte. But if you said to Sid, "I want to put a plate somewhere and modify it," or something or other, then it was Sid who would know the workshop drawings in his head and tell you that you couldn't do that because there was something in the way. Whereas Alan was the one who would know what the circuit was like for the magnet stabilization power supply for example. But they were a fine group to work with; I really enjoyed my association with AEI/Kratos.

GRAYSON: Well, it was a special time in these companies where people who had technical expertise were respected for it and were given opportunities to use it and show how they could make a contribution. But today I don't think that management is sufficiently enlightened to understand the importance of people who have good strong technical and scientific knowledge. They see them as just someone who sucks away salary and they don't see anything other than, "Where's my quarterly profit from you?"

JENNINGS: Yes, yes.

GRAYSON: And it's a sad situation, but it certainly dominates American industry today.

JENNINGS: Well, certainly in the 1960s and 1970s industry was like what was going on at the universities, and I actually considered joining AEI. I talked to John Waldron about this, and he essentially said, "Well, I'd be careful. You seem very happy in University surroundings and so be careful about it," and he was right, I think.

GRAYSON: Well, Gareth Benton had spent some time with them before he went back to Swansea.   The attitude in the industry was very much – not totally academic, but it had a very strong flavor of being permissive with people to do what they thought best and to use their intelligence to move the field forward. Rather than being managed by some guy who can only observe how much money is being spent and how much is coming in.

JENNINGS: Quite, yes.

GRAYSON: That's such a sad approach to things.

JENNINGS: Yes, I used to turn up at AEI at about 10:30 a.m. and we'd talk science nonstop till 3:30 p.m. and then go home across the Snake Pass and back to Sheffield.

[END OF AUDIO, FILE 2.2]

GRAYSON: Ready to start recording our next session here in the afternoon of 25 April

JENNINGS: Well, as I said, Mick Barber left to join UMIST about 1967, and I think Sid Evans joined AEI about 1970.   I think he came from Edwards High Vacuum, and he and Martin Elliott were the two with whom I worked closely for a number of years. Linked scans were developed at AEI and then handed to me to work with. I was supposed to do something useful with them and hopefully I did.

GRAYSON: So what were the initial linked scans – obviously there are some simple linked scans and there are some more sophisticated ones.

JENNINGS: There was one with B/E constant; that was the best one.

GRAYSON: And what did you get from that, daughters?

JENNINGS: If I remember rightly, yes.

GRAYSON: Then there were these more sophisticated ones.

JENNINGS: There was a B^2/E constant which was never very useful, I don't think.

GRAYSON: Yes, it's hard to do the squared part with a magnet. That was parents?

JENNINGS: I can't remember now. You could do that but I didn't find it really useful. But the B/E scan was quite useful.

GRAYSON: And this was with forward geometry – the MS50, was a forward geometry instrument.

JENNINGS: Yes. And the 4-sector eventually came from them, but they were very bad on delivery. I mean, the MS50 for example arrived nearly a year late. It should've arrived in Sheffield. And I learned a new word: Slippage. [laughter] "There's been another fiveweeks' slippage; very sorry." It eventually came, but it was nearly a year late.

GRAYSON: Well, since you were moving from Sheffield to Warwick it probably was just as good that it didn't show up on time.

JENNINGS: Indeed! Sheffield were not very happy with the lab they'd prepared for me, – it was down in the basement. But no, working with Martin Elliott and Sid Evans was fun because they were real scientists, and you could suggest things to them and they'd think about it and come back to you and tell you why we'd better do it this way, not that way, but the effect will be similar, you know. I didn't know much about what was inside the flight part in particular, but

Sid could say, “Well, no, you can’t do that because there’s something or other in the way.” But they were very happy to think of anything you suggested, and they would accept you. The only criticism I had was if I sent them a manuscript Sid would usually get it lost on his desk somewhere.

GRAYSON: Oh my!

JENNINGS: It would take about three months to come back. Although the worst example of that, going back many years, was in Jean Futrell’s lab; it was Wayne Sieck – I don’t know if you knew Wayne. A very nice chap; he’s dead now, unfortunately. But Wayne used to have a desk that was absolutely chaotic. He’s the only chap I know whose phone rang and stopped before he could find it. [laughter]

GRAYSON: Not good. What’s his last name?

JENNINGS: Sieck, S-I-E-C-K. But he was a lieutenant in the Air Force, was with Jean and he was very kind to me. I went and stayed with them for weekends and things like that. He had a great sense of humor. Very good fun, but his desk was an absolute shambles. [laughter]

GRAYSON: I can hardly imagine not being able to find your phone.

JENNINGS: Right. Well, so move on to Alan Quayle. Alan Quayle was the first editor of the international journal, and he was at Shell Thornton, and he very kindly agreed to have me work at Shell for about three months during my sabbatical year in 1982. I worked with one of John Beynon’s former students, Roger Morgan, and it was just as FAB was becoming available. And Roger had the idea that the surface of the tip might be important, so we had a probe made with a detachable end, and Roger had innumerable ends made of copper, titanium, glass, God knows what. And we spent about three months fiddling about with these and actually the general view was it didn’t matter at all what the FAB tip was made of.

GRAYSON: True, but worth finding out.

JENNINGS: But I then wrote my report, and it pleased Alan Quayle. He said, “It’s nice to have a well-written report. I don’t get these very often these days.” [laughter]

GRAYSON: Was it A-L-A-N, or A-L-L-A-N?

JENNINGS: A-L-A-N Quayle. He was a very nice chap, a sort of self-effacing chap, very quiet, and he died some years ago. But he was a very fair and pleasant editor of the *International Journal* [*of Mass Spectrometry and Ion Physics*]. He did his best to ease things through if he felt it was worth it to help you. I think he was really undervalued, because when he retired, I don't think the *International Journal* [*of Mass Spectrometry and Ion Physics*] made much of an occasion of it.

GRAYSON: So what was he doing on the scientific side other than the editorial?

JENNINGS: He was just in charge of the mass spec services at Shell Thornton Research Center. Near Chester.

GRAYSON: So by this time he was pretty much at the administrative level.

JENNINGS: Oh, yes, yes. He was very senior. Yes. Then we come to Dudley Williams. We already said quite a bit about him, in fact almost as much as there is to say. But he came back from Djerassi's lab and we kept in very close contact, did these RIC summer schools together.

GRAYSON: What was his motivation to go to Djerassi's lab?

JENNINGS: I don't know really. I didn't know him before he was there. He was originally I think a graduate of Leeds.

GRAYSON: Was it a post-doc appointment?

JENNINGS: I think it must've been a post-doc in Djerassi's lab, yes, and then he went back to Cambridge because he had a lectureship. We were both fairly new with the MS9 so that was the link. That's where I got to know Graham as well, when he was working with him and we examined each other's students and so on and we worked on the RIC courses together.

GRAYSON: Actually there's one point that we missed altogether. Did you teach any undergraduate courses at either Sheffield or Warwick?

JENNINGS: Oh, heavens, yes! A terrific amount.

GRAYSON: So you had a teaching load, you had an administrative load at Warwick, and you had a research load.

JENNINGS: Yes, typically I would do about one hundred hours' teaching a year.

GRAYSON: And these would be organic chemistry?

JENNINGS: No, no; physical chemistry.

Let's digress a bit then. Early on I took over the first year thermodynamics course, which is, you know, sort of a lecturer's graveyard, really. But I wasn't very happy with the way it was being taught, so I did that.

GRAYSON: Most people who teach thermodynamics aren't happy with the books that are available. That was one of John Fenn's commentaries when he was asked to teach the thermodynamics he actually created his own course notes, which then became a book for him.[20] He didn't like the way it was taught, or the books that he had to teach from. It can be extremely esoteric.

JENNINGS: Oh, yes. I think you've got to show that it's useful; and I did the third year statistical thermodynamics, which again is not a popular course.

GRAYSON: I understand that.

JENNINGS: One of my students apparently said to one of my friends, "Well, his lectures are okay, but I wish he'd find something else to lecture on." [laughter] And a second year kinetics course. But the thing I did which I haven't mentioned, which really bears on what we were talking about much earlier; recruitment of undergraduates, one of the sciences' problems. What we decided we'd do was go out to schools and give lectures. So a number of us got up lectures for schools, which were then advertised, free of charge, and we'd go one hundred miles if need be. I gave a lecture on "Why Do Chemical Reactions Occur?" and this was a mixture of

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[20] John B. Fenn, E*ngines, Energy, and Entropy: A Thermodynamics Primer* (Pittsburgh: Global View Publishing, 2003).

thermodynamics and kinetics, of course, but very much popularized, and I had about ten or a dozen experiments.

So going on with that I needed at least an hour to set it up before I started the lecture – I had things that fizzed or they changed color or were clock reactions, usual stuff; got quite a bit out of the *Journal of Chemical Education* for that. And I must've given that one hundred times.

GRAYSON: This was essentially to pre-university students?

JENNINGS: Yes. It was really to get them interested in chemistry and possibly to get them interested in Warwick. We had to ration it in the end, it had become a very popular lecture, and, the lab staff got very adept at preparing all the stuff for me, so I got my three or four cardboard boxes to put in the back of the car. And I quite enjoyed it, but it was very time-consuming.

GRAYSON: I can imagine!

JENNINGS: When you had to go somewhere fifty or seventy-five miles away – indeed, once I remember I did one in the morning, one in the afternoon, so that was really very tiring in a way. But we hoped we did something for chemistry. We hoped we did something for Warwick. But about half a dozen years doing this, and I think it paid off in the end.

GRAYSON: Yes. Were you able to do an analysis of incoming students to determine whether or not it had an effect?

JENNINGS: No.

GRAYSON: But you were casting your bread on the water and hoping for the best.

JENNINGS: Yes. Even as Department Chairman I would take about a 50 percent teaching load. And towards the end we had a scheme whereby those with a large research group had a slightly lower teaching commitment, but in general everybody had quite a bit of teaching to do. We were just not a big enough department, and in Britain it's very rare that an academic does not teach undergraduates. Post-docs tend to give tutorials or do lab work, but not lecturing.

GRAYSON: So this was a real serious part of your time.

JENNINGS: Oh, yes. Very much so.

GRAYSON: Okay. It's a facet of your career that we haven't really talked about, and if we don't mention it we assume that it's all been doing research, but in fact you were spending quite a bit of time teaching.

JENNINGS: Well, I thought, certainly as chairman, I wanted to give a first year course so students saw who I was, or at least I communicated with them.

GRAYSON: Good idea.

JENNINGS: So when I handed over the first year thermodynamics to someone else I took a first year kinetics course, or a first year molecular structure course. I don't think anybody else wanted to compete for statistical thermodynamics in third year, so I got that. And I did second year kinetics.

GRAYSON: So what was your class size typically?

JENNINGS: In the first year towards the end it would be sixty or seventy, and probably about fifty in the second year and maybe forty in the third year.

GRAYSON: So it wasn't one of these terror courses where you had a huge lecture room with three hundred students. But still, sixty to seventy undergraduate students is an appreciable load.

JENNINGS: To begin with, the very first lecture I gave was to about one hundred students. That was when I'd gone up to Sheffield, because in those days Sheffield was quite a big department which did enormous numbers of service courses. I think it gave five different first year courses, and I got what was called the Inter B.Sc. course, which was the lower level course for non-specialist science students. It was really very elementary stuff.

Something I was asked in my interview here [Warwick], which was quite an amusing aside, the professor of biochemistry was very concerned that we got a professor of physical chemistry who appreciated the difficulties of students with mathematics, because the previous chap had lectured to first year students and he'd used concepts like Hermite Polynomials, which to first year biochemists you might as well be talking pure Greek.

Anyway, he left and I was asked at my interview had I ever taught people who were not too adept in mathematics, and I said, "Yes." I related how in one of my first year lectures at the beginning of kinetics, I wrote dx/dt [an expression of rate of change in differential calculus] as equal to something on the board, and a hand went up, and a very nice young girl said, "Excuse me Sir, but why didn't you cancel the 'd's?" [laughter] So I said, "Well, you'd better come and see me afterwards." And at this stage, the vice chancellor who was chairing the interview committee looked somewhat puzzled, and so one of the chaps said, "Well, I'll explain it to you afterwards, Jack." He was a lawyer, you see, so I think he would've canceled the 'd's also! Anyway, this reassured the professor of biochemistry that I had taught such students in the past.

Anyway, going back to my list. I think the rest of Dudley [Williams] we covered in previous discussions; Graham Cook's interpretation, RIC courses, and then I'll come onto the last one in a bit. Then there's Urs Schlunegger.

GRAYSON: What's his first name?

JENNINGS: Urs; U-R-S – "Bear" from Bern. He actually wrote a book on metastable ions some years ago, and so we corresponded and got to know each other because of this, and he came and spent a sabbatical term with me.[21] We did a lot of work together, and he invited me to Bern on several occasions, usually to give a course of about five lectures. So these again were post-graduate lectures, not an MSc course particularly, but a short course of lectures to post-graduate students in Bern. And he was very much an instrumentation chap, a very interesting character in many ways. He built his own ion trap, which he had in his garage.

GRAYSON: Oh, my!

JENNINGS: He also was an advisor on detection of drugs and explosives and so forth for the Swiss Army, Swiss police, or something. But very much a workshop chap; he really built things. I'm still in touch him, an annual exchange of letters and Christmas cards and so forth. We visited each other on a number of occasions. Not for the last five years or so, but I certainly went several times to do lectures there. Then Jean-Claude Tabet, you know him? T-A-B-E-T. Jean-Claude – again, let me get my rogue's gallery out [Referring to a picture of his graduate and post-doc students]. That is Urs Schlunegger.

Now Jean-Claude is a larger-than-life figure. He works in Jussieu, Paris, and has great sideburns like this down here, and always grasps you like this and hugs you when he sees you, and he's very amusing. He's got about the worst scientific English I've come across; marvelous French scientific English, you know. Hugely amusing when he speaks, but terribly nice chap,

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[21] Urs P. Schlunegger. *Biologically Active Molecules: Identification, Characterization and Synthesis. Proceedings of a Seminar on Chemistry of Biologically Active Compounds and Modern Analytical Methods* (Berlin and New York: Springer, 1989).

and I spent a sabbatical month with him. He said would I go over and work with one of his students on ion trap design for a bit.

GRAYSON: So what kind of work was he doing?

JENNINGS: He does ion molecule reactions with ion traps, and all I did really was work with this young woman on getting the trap to work and sort of settle her in really; she was a first year student – gave one or two lectures, as usual.

GRAYSON: Now, on these sabbaticals did you take a sabbatical year and tour around the country, or did you take it at one place?

JENNINGS: I took a sabbatical year and I spent a bit of time in Warwick, a month in Paris, three months at Shell, and three months in Santa Barbara. Later, I spent time in Portugal as a Gulbenkian Visiting Professor.

GRAYSON: So you spent a month with this Jean-Claude Tabet. That was a separate event.

JENNINGS: Yes, that was within the year. I'd say October or November sometime.

GRAYSON: Okay, and you worked with a young woman, a first year student getting started with ion molecule reactions.

JENNINGS: Yes. My main thanks to him was having a sort of perpetual post-doc for about six or eight years, Dominique Despeyroux. Now, she came as a post-doc, and it was very amusing. I was at an ASMS meeting and I arranged to meet Jean-Claude because he had a student who wanted to talk to me. And I saw him at the other end of a corridor, and Dominique told me afterwards she said, "Ah – I see Professor Jennings down there," and she then composed herself and was very respectful and on her best behaviour and came along and stood there looking very demure [laughter]. Then we agreed she'd come to work with me. She was an excellent post-doc, and stayed with me for something like six or eight years. She married a Scotsman who worked in the department and the two of them are now working at Porton Down on detection of explosives and chemical and biological weapons. So she's now in quite a senior position. She was a very good post-doc, and looked after my group for me for some time, and I have a lot to thank Jean-Claude for, for pointing her in my direction.

Right, moving on – oh, Brian Green – I thought I should say something about him, because back in the dark days, of course, at AEI we worked with Brian and Alan Wolstenholme and Mickey Barber on the sequencing of peptides and the use of their very primitive data system. Then I really lost touch with him a bit, other than seeing him at conferences, because he moved across to VG and I was a Kratos consultant so we didn't speak very much until Kratos disappeared. Then after a suitable time I became a VG consultant and worked with him and visited him at Floats Road where, you had to clear your voice from the cigarette smoke and brush the fag ends off the top of the Q-ToF. [laughter. Brian Green is a chain-smoker.]

But we worked with him a bit on hemoglobin and so on because that was one of his major interests, and I think we had one or two papers with him on that. And then through Jim Scrivens also we worked again with Brian on one or two other things. But he's good fun to work with, and a very unassuming chap but very gifted, I think.

GRAYSON: Yeah, Brian's a lot of fun. He's such a wry character.

JENNINGS: Indeed.

GRAYSON: And as you say, he's a lot of fun. I remember seeing him at the last ASMS meeting he attended which was I believe Nashville.. I don't think he's been going back there since. But I do remember seeing him there and mentioning the fact that he wasn't going to be coming back any more because he just didn't want to mess with the complications of travel.

JENNINGS: Yes; well, he is getting on a bit. Like a lot of us. And then I come to Jim Scrivens. Now, Jim I've had a very close association with for ages. Jim was a student at Manchester and did his Ph.D. on NMR. Then in about 1980 or so there was a meeting at the Royal Society on mass spectrometry, and Jim as a young ICI chap was sent along, and I lectured on it and I think John [Beynon] did; a Japanese guy did, I forget his name, Nakanishi and one or two other well-known mass spectroscopists. This impressed Jim, and Jim decided that he was going to make a career in mass spectrometry at ICI, which he has done extremely successfully.

GRAYSON: So he was going in as an NMR person.

JENNINGS: But he worked in the molecular structure department, a junior chap but very bright, I might say, and worked his way up and became almost John's successor at ICI.

GRAYSON: Now, was Beynon at ICI at that time or had he left?

JENNINGS: No, he'd left. Well, by the time Jim attained any seniority; but he got up to something like the top twenty scientists in the company, in the divisional research associates and things like that. And I think he quite liked the talk I gave; I can't remember what it was on now. But as a result we got together, and at the time one could get what were called CASE student trips, C-A-S-E, Cooperative Awards in Science and Engineering. These were student trips which were partly funded by the government and partly funded by industry, and Jim very quickly decided that he'd like to fund students to work with me. So over a period of about fifteen years I probably had three or four students funded by Jim, or partly funded by Jim, working on things of interest to him. Now, Jim said we could never work on the compounds they were interested in; we could work on innocuous compounds to develop methods which they could then use, because their competitors mustn't see what compounds they were really interested in. So we were used really to develop methods and also train students.

GRAYSON: He was doing some synthetic polymer stuff at one time?

JENNINGS: Oh, yes, yes.

GRAYSON: Okay, that's what I thought. The name, it seems familiar, because I was doing some polymer work at McDonnell Douglas before I left and his name kept coming up in the literature that I reviewed or looked at.

JENNINGS: Yes, we did quite a bit of polymer work with him, and he had a VG 4-sector, so we would swap notes from time to time; he was interested in metastables and CID. And he was a visiting professor here after John – couldn't have both. He gave a number of lectures I remember on chemometrics. An interesting link was that my oldest son decided he wouldn't mind doing some mass spectrometry was interviewed by Jim and got a job working as a technician for Jim. And he's still working in the same area, but Jim has left and the company's been sold to somebody else, but he's still working in the same lab. So I've really had a very long association with Jim, and towards the end we got him to come part-time to the biological sciences department. I was, as it were, fading away into retirement we got him to take more time at Warwick. And when ICI essentially said, "Well, once you're over fifty if you leave you can do so on a decent pension." We managed to attract him here as a full professor and he's now my successor in Biological Sciences.

GRAYSON: So when did he start that?

JENNINGS: I think full-time a year ago last January.

GRAYSON: Oh, okay, so it's not that long ago.

JENNINGS: No. He was for a while I think one-third time and then two-thirds time. It was a gradual switch from ICI to here, but that was very useful to the department, and now he's got about five or six mass spectrometers, group of about eight, and doing extremely good work.

GRAYSON: Good.

JENNINGS: So I think the department's very pleased that we managed this, and he's happy, settled in very well doing his work here. So I see him every week, we keep in touch, and as I said, I help his students on preparing posters or proofreading their reports or papers and so on. So that's really a long association and it's still going on.

GRAYSON: That's interesting. When Bieman left MIT then that lab just shut down. I asked him a little bit about that. Now, I don't know if he gave me all the inside information; you know, there's a lot of politics involved in these situations. But he said he felt that the department wanted to do something different and they'd exhausted their interest in mass spec. Cathy Costello was with him then at the very end but she had to leave. She went to Boston University and is doing well there. He just turned it off what had been one of the most prestigious academic mass spectrometry operations, certainly in the country, maybe even in the world.

JENNINGS: Well, there's some politics here which I'll perhaps tell you when that's switched off [the recorder]. But you may have noticed that after twenty-five years in chemistry I've arranged for a successor in biology. But I can explain that later if you want. [laughter] And Mike Gross, I'd forgotten about Mike Gross. Again, I've known Mike since he was a student with McLafferty, and I visited Nebraska, he's visited here, and although we've never worked together we've talked at a lot of conferences and at NATO Schools and our interests have overlapped quite a bit. As I told you, we did the double-bond work and he developed new methods of doing that which were probably better in the end.

GRAYSON: Yes, well, gas phase ion chemistry is what he wanted to do.

JENNINGS: And I contributed to a book he edited and things like that, so we've kept in close contact and I'll again enlarge on that in just a minute or so. Now, a group I haven't mentioned is Lisbon. For reasons which are difficult to think of historically, I got very closely involved with Portugal way back. Professor Alzira Almoster-Ferreira – I can spell that one if you like.

A-L-Z-I-R-A then the word “almost” with E-R on the end, Almoster – Almoster, they call it. That’s A-L-M-O-S-T-E-R hyphen F-E-R-R-E-I-R-A. So Alzira for short. She visited me in Sheffield and was very interested in metastables, but they didn’t have very good equipment and they didn’t have much money, so she did her best with limited resources. She tried to do a bit of quasi-equilibrium theory work, but she couldn’t get very far, and she invited me over and I began to get involved with people in Portugal. I lectured to them on a MSc course on two or three occasions. Slowly but surely I got involved increasingly with the group and related groups, and in the end I had one of her people come over to do a Ph.D. with me, Teresa Fernandez, and she worked with me from 1983 to 1986, I think it was. She’s now back on the staff in Lisbon. And one of her friends, Filomena Duarte came over for a couple of months or a term also and worked with us.

GRAYSON: How did that last name go?

JENNINGS: Filomena with an F.

GRAYSON: F.

JENNINGS: Yes. Portuguese, you see.

GRAYSON: I see.

JENNINGS: Duarte, D-U-A-R-T-E. I went back and talked with them, and then I got involved with other people there. Helena Florencio, that’s Helen with an A on the end and Florencio is F-L-O-R-E-N-C-I-O – she is Alzira’s successor in the department and she’s now the senior professor there in mass spectrometry. So I’ve done some collaboration with her too, and as a result of all this to-ing and fro-ing and being involved with them, I was asked to serve on a committee to assess the research strengths of the Portuguese university chemistry departments and also to advise on their teaching. Then Alzira nominated me for an honorary D.Sc. at the University of Lisbon, so that had an amusing side to it because I decided I’d better have my thank-you speech in Portuguese.

GRAYSON: Okay.

JENNINGS: So I wrote out what I wanted to say and we had a woman teaching Portuguese in the department from San Paolo, so she translated it for me. Then she made a tape with gaps in between so I could keep saying what she’d just said and try to learn it. Then I wrote out what she'd written and then wrote over it in phonetic English what it sounded like. When I was given my degree I got up and read my piece, you see, and it sounded like perfectly good Portuguese except apparently I said it was always a pleasure to visit your father when I meant it's always a pleasure to visit your country. But as a result people thought I could speak Portuguese.

GRAYSON: Uh oh!

JENNINGS: So after this they all came up and started talking, and I said, "I'm sorry, I don't speak Portuguese at all, no!" [laughter] But anyway I got my honorary D.Sc.. There's another Portuguese university, the New University of Lisbon, and they invited me as Gulbenkian visiting professor in 1996 so I had a full term there working with somebody in the physics department and then again giving some lectures. That was with Maria de Lourdes Costa, so that's Maria, then D-E, then Lourdes, L-O-U-R-D-E-S, then C-O-S-T-A.

GRAYSON: So it seems to be dominated by females.

JENNINGS: Women – yes, all women, yes.

GRAYSON: That's interesting. I mean is this part of the science culture there?

JENNINGS: Yes, there are an awful lot of women in chemistry. And I'm still in touch with them. Helena recently asked me to serve on a committee to advise on setting up a Portuguese mass spectrometry center, and Maria has asked me to help with organizing a conference which takes place in the end of June this year.

GRAYSON: Now, all these conferences and summer institutes and whatnot, these are all taught in English?

JENNINGS: Yes.

GRAYSON: So students, irrespective of what country they come from, are expected to understand English.

JENNINGS: Yes. In Portugal there may well be one or two students who lecture in Portuguese to present their papers, but all invited lectures will be in English, and certainly all major lectures are English. And whenever I lecture in Holland or Switzerland or Germany it still is in English.

GRAYSON: Except for an occasional presentation in Welsh by John Beynon.

JENNINGS: Yes. Or in Portuguese but badly translated. [laughter] It's now the common language for science.

GRAYSON: Yes, but it's respectful of you to deliver your acceptance in the language of the country that's giving you an honorary degree, but it does make it difficult when they find out that you really don't know the language.

JENNINGS: Well, it was particularly appreciated because the previous chap -- I think he was Italian, an arts man -- thanked them profusely in Italian for half an hour. Then I got up and thanked them in Portuguese in four minutes, so they were doubly pleased. [laughter]

GRAYSON: Yes, some people given the opportunity to speak in front of a group can go on forever.

JENNINGS: I know. So that was rather funny, but the Gulbenkian Professorship was interesting because one of the requirements of that is to give a popular lecture on "The Importance of . . ." whatever it is you're working on "in the twenty-first century". So my title was "The Importance of Mass Spectrometry in the twenty-first Century." So, you start two thousand years ago with a guy called Democritus who said let there be atoms, and there were atoms. You work your way up and end up talking about the mass spectrometry of peptides and proteins. But all sorts of people came; there were about one hundred there, secretaries and students and people off the streets.

GRAYSON: Well, that's good. There's nothing wrong with educating people who aren't specialists in the field.

JENNINGS: No, no.

GRAYSON: Particularly if you make it something they can comprehend and understand and make them see the impact in their lives.

JENNINGS: Well, I've also given a course of lectures at Aveiro, and one of my former students whose now a professor – well, no, he's retired in fact – shows how old I'm getting. One of my former students is retired from being professor in Aveiro. A-V-E-I-R-O, is on the coast. It's probably about one hundred miles north of Lisbon.

GRAYSON: That's familiar. I think Mike Gross had a graduate student from Aveiro.

JENNINGS: Yes, I think he may have had Grassa.

GRAYSON: Yes, exactly.

JENNINGS: That's my student's wife.

GRAYSON: Oh, okay.

JENNINGS: She's still working, because I saw her in Prague, and much to my amusement Tony came along as the accompanying person because he was a retired professor.

GRAYSON: Sure.

JENNINGS: Yes, he was an interesting character. He came and worked with me in about 1970. Yes, he came to Warwick with me. Tony Ferrer-Corriea. F-E-R-R-E-R hyphen C-O-R-R-I-E-A. His father was I think deputy minister of education or something like this, and professor of law in Coimbra; a very highly respected person. Tony went with me first as a student, then as a post-doc. And again a very accomplished character that went back as a full professor to Aveiro when he left. So you can see my links with Portugal have been quite strong – still are, in fact.

GRAYSON: Well, that's good.

JENNINGS: Right. Well, that's got to the end of that. We've dealt with NATO school. We've dealt with summer schools as far as graduate courses. BMSS I could say a little about. I can't resolve for you who actually formed it, but I was a founding member of it, and I have been chairman of it on two occasions – I can't remember when they were now.

GRAYSON: Well, if it's on the C.V that is good enough.

JENNINGS: I'm not sure whether I have it on there or not, actually. No, I don't. No, I've just got professional affiliations, British Mass Spectrometry Society. Oh, yes, former chairman, 1969-71 and 1984-1986.

GRAYSON: So it was a couple of year term.

JENNINGS: It was a two-year thing and we had something known as a Maccoll lecture. I think I'm the only person who's given it twice, which was a mistake on the part of the committee because they hadn't realized I'd given it before. But Allan Maccoll had this lecture named after him and annually – we meet two years in three. We don't meet when there's a major European meeting, but when we have a meeting the fundamentals lecture is the Maccoll lecture.

GRAYSON: Okay. So there is still a fundamentals aspect.

JENNINGS: Yes, yes. It's probably getting less fundamental than it used to be. [laughter] But certainly up to about the mid-1980s I'd say that academics played a pretty prominent role in it. But as academic mass spectrometry in Britain has perhaps fallen away a bit, so involvement in fundamentals has fallen away. Jim Scrivens, Simon Gaskell, I think most of the academics, certainly Dai Gaines, are not involved any longer with BMSS on the committees, anyway. The young woman in Edinburgh I know is, Perdita Barran.

GRAYSON: But it is alive and well.

JENNINGS: Oh, yes, very much so.

GRAYSON: But not as academic as before.

JENNINGS: It's more marketing and very much in applications, I should say.

GRAYSON: Well, I think that that's the case with ASMS too is the applications have started to really dominate things.

JENNINGS: Now the other things I've got on here – awards, but you've probably got something about those, haven't you? Thomson Medal in 1985, then the American things.

GRAYSON: The Thomson Medal is awarded by International Mass Spectrometry Society?

JENNINGS: Yes. Then there's the ASMS '95, when I got the Distinguished Contribution Award and the honorary D.Sc. in Lisbon and the Field and Franklin Award. And the Aston Medal at British Mass Spec Society, 1998. I believe I put awards in twice. I don't know why; I'm sorry about that.

GRAYSON: Well, that's understandable. There's so much stuff going on.

JENNINGS: I was trying to reorganize things and somehow I got awards in twice.

GRAYSON: Ah – yeah.

JENNINGS: Extended visits outside Britain, we've already talked about those. Membership on editorial boards isn't very interesting, I don't think you want to know about that. Coworkers, well, there were more than one hundred or so grad students, post-docs and visiting scientists. I've also had one or two weeks in Africa, as you've probably gathered.

GRAYSON: Yes.

JENNINGS: I visited Malawi to give some lectures in about 1970. The most interesting thing that happened there was I got off a plane at 10:30 and the chap said to me, "Now, what have you been done for?" I said, "Malaria, cholera," you know. He said, "How about polio?" I said, "No." He said, "Well, we'll go straight to the hospital then. You can have a polio in a teaspoon full of sugar." Afterwards he said, "How are you feeling now?" About 11:30 it must have been. I said, "Fine." "Good," he said, "Well, I've got somebody who is off ill. Would you mind taking a practical class at 2:30 p.m.?" I was in action at 2:30 p.m. doing first year practical chemistry with the Malawian students.

GRAYSON: So what prompted you to go to Malawi?

JENNINGS: A former colleague in Sheffield had gone out there, a lecturer. He was very much a character who wanted to go and help train the poor and in fact stayed in Malawi. He'd gone out initially for a year, but he stayed there for probably about ten years, until he retired. And so I went out for about three weeks and gave a few lectures, and the most important lasting thing I did there, I believe, was because I could glass-blow, and Malawi seemed not to have any glassblowers, I repaired a distillation flask by putting the sidearm back on. But the University of Malawi had a technician; it was that sort of level of support. They tended to promote cleaners to be technicians if they were reliable cleaners, then they were probably reliable to put out apparatus and things like that.

As far as any qualifications were concerned, they had one elderly New Zealander to organize things, but they didn't have a glassblower, they didn't have any electronics people. And then the other link I had was Lesotho. L-E-S-O-T H-O. Yes, in Lesotho the Besotho speak Sesotho, because they have a language which inflects the front of the word, not the back of the word.

GRAYSON: Okay.

JENNINGS: And they used to have British people as external examiners, and a former colleague in Sheffield was external and asked me if I'd succeed him, so I had two visits to Lesotho, one of which was immediately prior to the Hawaiian ASMS meeting. You examined all the papers, you talked to the staff, you talked about development. I was asked to advise on the development of an MSc course, things like that. They're desperately in need of help for people, and advice.

GRAYSON: But there is a drive to improve the educational system at the university level?

JENNINGS: Yes, yes. They need it. For example a lad of about twelve will be given a cow. At thirteen he'll get another one, at fourteen another one, at fifteen another one, and when he turns about eighteen he'll have about six cows and that's his set up for life. What'll he do then? He'll walk round with his cows and a stick. His cows will eat whatever they can find, and he'll just make sure that nobody walks off with his cows, and that's life. And there was a river I noticed with Lesotho on one side, and the Union of South Africa on the other side. They're both growing maize; the heights are like this, [showing the different levels of the plant with his hands] that was South Africa, this was Lesotho. And you felt they didn't need somebody to go and monitor how they were teaching first year thermodynamics. They needed somebody to monitor how to grow food.

I was given a tourist leaflet which illustrates the problem. It said that Lesotho has two miles of railroads. They were the two miles that the Union of South Africa trains needed to park when they come into the country to move them back again. They had four classes of roads. Two hundred miles of paved roads, then dirt roads, then not so good dirt roads, and then Class Four roads where it is advisable to let people know before setting out on these roads. No part of Lesotho is below three thousand feet above sea level – it's a very high place. Marvelous for looking at the heavens; I mean to see the Southern Cross and the Milky Way, they couldn't afford electricity so it's extremely dark.

GRAYSON: Sure, yeah.

JENNINGS: Marvelous for that. But a very poor country. My second visit was another two weeks there, and I saw quite a bit of it and talked among the staff and did what I could to advise on the MSc course, which actually was not to have one. But they had one nevertheless. Of course, their first level degree courses are only past degree standard; it wasn't an honours degree standard. I think they would've done better to have raised that. So that's really another bit of life.

GRAYSON: So that must've been a pretty interesting experience. Where'd you stay? What kind of a facility?

JENNINGS: Oh, you stay in somebody's home.

GRAYSON: Okay. And I guess things like water -- you may have to mind what you ingest.

JENNINGS: Yes, you have to be careful, but I stayed in Maseru, which is the capital – no, sorry, it was a main town, but Roma was the capital. We used to drive to Roma, which was about half an hour or so. But Maseru for example had a French restaurant, I remember.

GRAYSON: This would have been a colony of some European country at one time?

JENNINGS: Yes, I suppose.

GRAYSON: French, Belgian, English?

JENNINGS: Had to be English in the past; that's why we still have roots. But it's a totally enclosed, small, primitive.

GRAYSON: They don't have access to the ocean and seaports?

JENNINGS: Oh, no. It's this totally enclosed bit of land in the middle of South Africa. You fly into Johannesburg and then fly in a little plane to Maseru. It's a bit difficult getting out of the place. They had forty-eight seater planes, but the runway wasn't long enough to take off fully loaded, you could only have forty-four passengers. So I hope they were not all my size; otherwise we'd never get off the ground. [laughter] But it was an interesting time.

GRAYSON: I guess! This was 1970, 1980?

JENNINGS: It would be 1982. Because Hawaii was 1982.

GRAYSON: Right, exactly. So I wonder how that has changed in the last twenty-five years?

JENNINGS: Oh, I don't know, but most of the instructors were expatriots. There was a Canadian chap who taught physical chemistry, there was a Dutch chap who taught organic.

GRAYSON: Well, how are we doing? Are we pretty much wrapping it up here?

JENNINGS: No, I've left one thing I'll now find. Sorry to keep going on.

GRAYSON: No! That's good. That's what I'm here for.

JENNINGS: I have here something I ought to make a copy of, I suppose. I don't know for what this was prepared, but it's entitled "Summary of Major Research Contributions and Brief Summary of Research Achievements." Would that be of interest if I copied that?

GRAYSON: Yes, definitely.

JENNINGS: It may have been for the Field and Franklin [Award] or something like that. Sorry to be so disorganized.

GRAYSON: No problem. Nice thing about this audio recorder is it has a huge capacity.

JENNINGS: Good.

GRAYSON: The whole business with computers has become so ridiculous these days. A gigabyte is nothing; it's just normal technology. There's probably more computational power in that little recorder than in an old IBM mainframe back fifteen or twenty years ago.

JENNINGS: Yes, I'd begun life with a BBC Micro with 32k of memory.

GRAYSON: Last year at ASMS an old fellow who had squirreled away a quadrupole mass spectrometer from HP [Hewlett Packard] called me up about five or six months before the meeting wanting to know if we could do a little historical display. So ASMS picked up the tab and we had the instrument shipped to the meeting site and then I prepared some posters to go with it, and one of them was totally devoted to the data system for the mass spectrometer, complete with 16k of memory! If you were rich you could get 32k!

JENNINGS: Well, remember the PDP8 that had 8k or something like that? That was the first data system we had on the MS50. Like a piano keyboard or something, and I remember I had a chap called Rod Stradling work with me, who was one of Allan Maccoll's people, and he was extremely good at computers. He produced an absolutely stripped down thing which was just good enough for our purposes. Where it went wrong, when the guy came to have a look at it there, wasn't enough memory for him to use his test equipment. [laughter] So that was pretty primitive.

GRAYSON: Oh, primitive was the word!

JENNINGS: Well, one of the things I haven't mentioned is my retirement. I retired in 1997 and Mike Bowers along with quite a few other people had arranged a retirement symposium for me, which I knew nothing about until I tried to arrange to be away; and it was not allowed. But you can see who came; I've got the program here. I'll just read the names. You needn't take any notes; I'll give you the program if you want. But Mike Bowers, Dudley Williams, Mike Gross, Nico Nibbering, Urs Schlunegger, Jim Scrivens, Graham Cooks, Simon Gaskill, Helmut Schwarz, Gareth Brenton, and John Beynon came. So that was a fairly humbling experience.

Oh, and Chairman Allan Maccoll, I'd forgotten him, John Monaghan who is now the chairman of the BMSS, or president, I think and Tony Stace.

GRAYSON: Fairly high-powered group of people in the field.

JENNINGS: Yes. And then there were twenty-five people who brought posters, so it was really rather nice of them.

GRAYSON: It's a nice tradition.

JENNINGS: Yes. And this was a whole day symposium; people came from far and wide, and very nice finish to my career in chemistry. Howard Dalton came from Biological Sciences. So there are some other things that I could talk about, but perhaps I could talk about those without this on […]. Before we close, perhaps I could pay tribute to my wife, Christine, who has put up with my working late in the laboratory and being away for several weeks of the year for many years. Without her understanding, it would have been impossible for me to have had such an enjoyable time working in mass spectrometry with so many gifted co-workers whom we both see as part of our scientific family.

GRAYSON: Okay. This concludes the recorded interview.

[END OF AUDIO, FILE 2.3]

[END OF INTERVIEW]

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