

CHEMICAL HERITAGE FOUNDATION

JOHN H. BEYNON

Transcript of an Interview
Conducted by

Michael A. Grayson

at

Swansea, Wales
United Kingdom

on

22 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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JOHN H. BEYNON

1923 Born in Ystalyfera, Wales, United Kingdom on 29 December

Education

1943 B.S., Swansea University, Physics

Professional Experience

1943-1947 Fighting Vehicles Research Establishment
Tank Armament Research

1947-1969 Imperial Chemical Industries, Ltd.
Manager of Physics and Physical, Polymer and Analytical
Chemistry

1970-1974 Senior Research Associate

1965 University of Minnesota
Boomer Memorial Fellow

1969-1975 Purdue University
Professor, Chemistry; Director of Mass Spectrometry Center

1964-1969 Swansea University
Honorary Fellow

1974-1986 Royal Society Research Professor and Director of the Mass
Spectrometry Research Unit

1976-present Research Professor, Physics and Chemistry

1972-1975 University of Essex
Visiting Professor

1982-1985 Visiting Professor

1976-present Institut Jozef Stefan
Research Associate

Honors

1960 Founder Chairman British Mass Spectrometry Society
1967 Founder Member American Society for Mass Spectrometry

1973 Sigma Xi Research Award, Purdue University
1979 Marice F. Hasler Award
1980 Jozef Stefan Medal
1981 Medal of the Serbian Chemical Society
1984 Techmart Trophy of the British Technology Group
1984 Jan Marc Marci Medal, Czechoslovak Spectroscopic Society
1985 Gold Medal, International Mass Spectrometry Society
1985-1986 President, Association for Science Education, Wales
1986-1990 Chairman, Schools Curriculum Development Committee, Wales
1987 Frank H. Field and Joe L. Franklin Award for Outstanding Work in Mass Spectrometry, American Chemical Society
1988 Gold Medal, British Mass Spectrometry Society
1990 Gold Medal, Italian Mass Spectrometry Society
1993 Founder President, European Mass Spectrometry Society

ABSTRACT

John H. Beynon was born in Ystalyfera, Wales, the older of two sons whose parents ended their education at secondary school. Beynon grew up in a coal mining town and attended a local university, the University of Wales at Swansea (Swansea University), during the early years of the Second World War. Graduating with a degree in physics, Beynon decided that the pursuit of a PhD was a waste of time and money and he committed himself fully to wartime work, including the development of weapons system used to track targets while a weapon was in motion. He spent much of his career in industry, principally working at the Imperial Chemical Industries (ICI), a British chemical company. Upon his arrival at ICI, Beynon's supervisor, A.J. Hailwood, immediately gave Beynon the task of building a mass spectrometer, a device with which he had no conceptual underpinnings. Creating this technology, however, proved to be pivotal in Beynon's career. Even without a PhD Beynon made himself and his work central to the development of mass spectrometry as a field of study and as a tool of chemical analysis and knowledge.

Uncertain about remaining in industry his entire life, Beynon spent time at Purdue University, Swansea University, and the University of Essex. Being outside of industry allowed Beynon the opportunity to publish his research for the wider scientific community, ultimately contributing over 350 articles and other publications to the annals of science. He founded the Mass Spectrometry Unit at Swansea University, and was also a founding member of both the British Mass Spectrometry Society and the American Society of Mass Spectrometry. All through his long career Beynon trained a number of students (one of whom is Gareth Brenton; Brenton's reflections on his mentor are recorded in the appendix to this transcript) and did much to advance the field of mass spectroscopy.

The interview concludes with Beynon's reflections on the politics surrounding the formation of an international mass spectroscopy committee. Throughout the interview Beynon details many of the scientific discoveries that came of out mass spec research, as well as a number of the refinements and improvements to mass spec technology.

INTERVIEWER

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

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INTERVIEWEE: John H. Beynon
INTERVIEWER: Michael A. Grayson
LOCATION: Swansea, Wales, United Kingdom
DATE: 22 April 2008

BEYNON: I had an honorary issue of *Organic Mass Spectrometry*, I think, for my 65th birthday. I don't have a copy of it on hand, but you'll be able to find it.

GRAYSON: I can get a copy of that, I'm pretty sure. Is that a Wiley journal, do you know?

BEYNON: Yes, a Wiley journal.

GRAYSON: Okay. Those are all online now, so I can get that. Do you know what year that would've been?

BEYNON: I don't know if they did it exactly on the year of my birthday, but it was the 25th anniversary of the foundation of the society.

GRAYSON: Okay, now when you say, "The society," you're referring to the British....

BEYNON: Yes, the British....BMSS.

GRAYSON: The BMSS, British Mass Spec Society. Okay, as opposed to ASMS, very good. Okay, so that would've been roughly....

BEYNON: 1980 something.

GRAYSON: Somewhere in the '80s. All right, I'll just make a note of that, because I have access to those journals.

BEYNON: I'm sure that I've got a copy somewhere in this apartment, but I don't know where it is. I couldn't find it. We've just moved in here. We've had to downsize very markedly, in fact, the area of this apartment's only about one-third of the area that we moved out of.

GRAYSON: Oh no.

BEYNON: So we're congested and haven't sorted ourselves out.

GRAYSON: Oh my. I sympathize with you.

BEYNON: Well, we've thrown away so much stuff and sold so much stuff. Anyway, enough of that, but I have got here the two honor issues that were done for my 80th birthday, and there's one of them.

GRAYSON: This is from the *International Journal of Mass Spectrometry and Ion Physics*.

BEYNON: And the other one is from *Rapid Communications in Mass Spectrometry*. The *RCM* journal, and this is the bound one that they did for me. And that one they didn't do just articles. They did people's sort of reminiscences of things they'd done with me, and the good times we'd had together.

GRAYSON: Yes. I think Brenton sent me a PDF files of these personal reminisces and also a proof copy for review of your publications so that I could look over the literature and see what topics we should explore.

BEYNON: And then there were two other things here. When I came to Swansea, I gave a lecture in the Welsh language. It's the only time I ever have done, so I don't know how this would be on the internet, but there's an English translation of it, and when I was visiting Purdue University in 1989, Graham Cooks did an interview with me.

GRAYSON: Yes, I think I have some recollection of that.

BEYNON: That's the only copy I have of it. You're welcome to take it away but if I can have it back. So that's the sum total of what I've got prepared for us.

GRAYSON: So I will be able to follow up on this. I need to return this to you. These have signatures of all the people that worked with you?

BEYNON: Some of the people signed on the inside.

GRAYSON: And this is the *RCM*? That's very nice.

BEYNON: This has a picture of my wife in it. There's me and my wife.

GRAYSON: Oh, great, so this *RCM* was done when you were –

BEYNON: When I was 80 years old in 2004.

GRAYSON: Okay.

BEYNON: The end of 2003.

GRAYSON: I see. Well, before I forget, I do want to say that I had been in contact with Michael Gross and Klaus Biemann, and both of them asked me to pass on their best regards to you since they knew I was coming to visit, so I just wanted to make sure I did that before I forgot. Why don't we go ahead and start? I appreciate having the interview that you did with R. Graham Cooks.

BEYNON: One other thing, you said in your letter that ASMS has generously said that they'd pay for a supper for us.

GRAYSON: Yes.

BEYNON: Well, I don't think I'm really in the state to go out for a supper.

GRAYSON: Okay.

BEYNON: And my wife's handicapped as well, so what I suggest is, if you're agreeable, that we can have lunch here today – there's a restaurant over there.

GRAYSON: Okay, very good.

BEYNON: And if ASMS want to be generous, they can pay for that lunch.

GRAYSON: Very good, why don't we do that?

BEYNON: Yeah, okay. I thought we would save time. And finally, I suppose you want to conduct the interview in English, not in Welsh.

GRAYSON: I don't know. I suppose we could say some things in Welsh.

BEYNON: [Speaks Foreign Language]

GRAYSON: Okay, which means?

BEYNON: It's, "If you want me to say something in the Welsh language . . ."

GRAYSON: Our transcriptionist will have a problem. Well, what I'd like to do then is go ahead and get started. This covers some of the material that I think Graham Cooks did in his earlier interview that was done in '89 when you were visiting him at Purdue. Okay, what I can do is get a copy of this made and return it to you. I don't recall that I actually have seen this transcript.

BEYNON: Do you want to take a minute to read through?

GRAYSON: It probably wouldn't hurt to just browse through it real quickly.

BEYNON: The pages are numbered. You can take that clip off it.

GRAYSON: You can tell me a little more about your parents. I like to get a little bit of insight into a person's early history prior to when they started out. For instance, the educational level that your parents had, and what their background was, and whether or not you have any siblings.

BEYNON: Well, both my father and my mother, their education ended at secondary school level. They lived and were married in the Swansea Valley in a little town called Ystalyfera which is spelled in there. And that's where I was born.

GRAYSON: Relative to here, where would that be?

BEYNON: 13 miles north of Swansea.

GRAYSON: So it's more inland.

BEYNON: Yes.

GRAYSON: Okay, and did you have any brothers or sisters?

BEYNON: I had one brother who has now died, a younger brother. The village I was born in was a coal mining village. There were three coal mines there and that was really the basic industry. Everybody worked in the collieries.

GRAYSON: So your father was in the mining?

BEYNON: No, he was in the office of a mining company. My mother was a housewife.

GRAYSON: Was there much of an emphasis on education in your household, in your family?

BEYNON: Yes, well, my schooling started when I was four years old, and I went to the infant school, which was a bit of a cultural shock for me, because I should explain that my father is Welsh. My mother couldn't speak Welsh so we spoke no Welsh at home.

GRAYSON: Oh my.

BEYNON: So I didn't hear any Welsh spoken up until the age of four, I never heard any Welsh spoken. I went to school, to this infant school, to find that all the teaching was done in the Welsh language, so I literally learned to count and so on in Welsh, which is a bit different than counting in English.

GRAYSON: Oh, how is that different?

BEYNON: Well, it counts, for example, when you get up to 13, 14, 15, 16, in Welsh, it's 13, 14, 15, one and 15, two and 15, three and 15, or two nines – some genius must've discovered that eighteen was two nines -- four and 15 or one less than twenty, and finally twenty. And then you tend to count like the French do, in twenties, rather than tens. So 60 is three twenties and so on.

GRAYSON: So this was a bit of a shock. If everything was taught in Welsh, then you really were initially at a bit of a disadvantage?

BEYNON: Yes. You pick it up as a child, I suppose, because all the other children spoke Welsh.

GRAYSON: But did you hear Welsh before you went to school?

BEYNON: No, not really.

GRAYSON: It reminds me a little bit of my experience, I grew up in south Texas, on the border, and the first school I went to, the teacher spoke to us all in Spanish, so she addressed me in Spanish, because she assumed everyone else spoke Spanish. So when she found out I was an Anglo, I could've hit her over the head with a baseball bat, the fact that I didn't understand what she was saying.

BEYNON: The Welsh language is a very old language. The Welsh have a very strong literature that goes back earlier than the English literature in this country, but the Welsh alphabet is different than the English one. There are some letters missing. For example, in the Welsh alphabet, there is no letter J, K, Q, X or, Z, but there are some extra letters, double letters.

GRAYSON: Like Dd?

BEYNON: Like Dd, which is pronounced like the ‘th’ as in ‘the’. So the alphabet starts A, B, C, Ch, D, Dd, E, F, Ff, G, Ng, H, I, L, Ll, and so on.

GRAYSON: I see. I’ve noticed in some of the signage here I see the Dd, which is pronounced like ‘th’ in ‘the’.

BEYNON: The “ll” sound is a unique sound. To make that sound, you put your tongue to say the letter L, and blow.

GRAYSON: You started school when you were four years old?

BEYNON: Yes.

GRAYSON: That’s a pretty tender age. Was that typical at that time for children to start school at age four?

BEYNON: That’s right, yes. But then I moved into the next school, which I suppose you call the high school. We used to call it the secondary school. It was supposed to be at age eleven, but I did it at age nine.

GRAYSON: Okay, so you were a bit precocious?

BEYNON: I suppose I must’ve been, and then I went through the secondary schooling and when I’d got to the fifth and almost final year, the war broke out.

GRAYSON: Okay, so this would’ve been ’40?

BEYNON: 1939. I was fifteen years old and well, it was normal to go to university at age 18, but I’d finished my schooling completely when I was 16, and because it was wartime. Because the universities were in a state of flux, I was allowed to go to university at the age of 16.

GRAYSON: Okay.

BEYNON: Now the law was then that you were allowed to stay in university only until the age of 18, at which age you were called up into the forces, but if you were doing a scientific subject and were passing your exams, you could have one extra year after the age of 18 before you were called up into the forces, but because I'd gone in at the age of 16, I completed my studies and achieved my honors degree one year after I was 18. So I then left the university. It was wartime. I did my degree in physics. I did no chemistry, just physics and mathematics.

GRAYSON: Okay and this was at what school?

BEYNON: In the University of Wales in Swansea.

GRAYSON: So basically you stayed in this area pretty much your whole youth?

BEYNON: That was my first degree. Now my parents didn't have much money and there was no money for me to go on to do a higher degree, and in any case, the war was on, and I was going to be called up. I was prepared for this in the university by being made firstly to take an extra course in radio and had been told that I had to take my exam in radio before my degree exam, and that if I didn't pass the radio exam, I wouldn't be allowed to sit the degree exam.

GRAYSON: So some mastermind in the educational system had this idea.

BEYNON: I found that I was going to be called up into the Air Force to do radar work, and to become what was then known as a wingless wonder. So I joined the Air Cadets, and served with them through my university time, but then when I got my degree....

GRAYSON: Now this degree that you got, this was . . .

BEYNON: Bachelor of Science in 1940. Well, I was ready. I was being prepared for the Air Force, so they called me up to the Army, typical governmental, bureaucratic nonsense. I found myself sent to a research establishment for designing armament for tanks.

GRAYSON: Okay. Well, that's a start.

BEYNON: So that's what I did during the war, was work on tanks.

GRAYSON: Even though you had been trained with this radio background, you were basically not doing any of that when you finally got to the armed services.

BEYNON: Exactly, and the other thing was that because of the war, I didn't stay on at a university to do a Ph.D., and in fact, when the war ended, I had, by that time, met Yvonne. We wanted to get married. We had no money and decided it was a waste of time to go get a Ph.D., so I didn't do a Ph.D. In fact, I have never done a Ph.D., and don't have a Ph.D. degree.

GRAYSON: Okay. Well, that's interesting. There are a few of us. Then the end of your formal education was a bachelor's in physics.

BEYNON: That's it. I was given an interview to stay on in the civil service. Fortunately, I failed that interview. [laughter] When I went seeking my fortune, I didn't know quite what to do. One of the places I applied to was ICI, Imperial Chemical Industries. I was interviewed by them at their divisional headquarters in Manchester where they had the largest organic chemical research establishment in the British Commonwealth. The research department had 750 chemists in it, 250 of whom were Ph.D.'s, nearly all of them organic chemists. There was also the technical service department, which was almost as big.

GRAYSON: When you say, "Technical service," this encompassed?

BEYNON: All of the chemicals that were made. They gave help and advice to the people who were buying them.

GRAYSON: When you say, "Service," were they doing analytical work?

BEYNON: Oh yes, in the research department, there was an analytical department, but it was quite small. Now ICI at that time made 12,000 different chemical products. The Dyestuffs Division, which was the division I was interviewed at, made 6,000 of them. It made all the dyestuffs, paints, waterproofing agents, all the complicated chemicals. There was a department of pharmacy which later expanded to become the Pharmaceuticals Division, and it was really what you might call the "complicated chemicals division" of the company. They didn't deal with the bulk things like caustic soda and so on, and there were no physicists there.

GRAYSON: I was getting to this question, why would you look for a job in the chemical industry with a physics degree.

BEYNON: But the interesting thing was that of all the 6,000 products that were there, for example, the dyestuffs, the paints, and the surface-active agents were not sold for their chemical compositions. They were all sold only for the physical effects that they produced.

GRAYSON: So they needed a physicist?

BEYNON: So they thought they needed some physicists, and I was one of the very first ones they recruited.

GRAYSON: I see. Did you reply to an ad?

BEYNON: No, I just applied cold.

GRAYSON: You just showed up cold! This would've been 1946?

BEYNON: '47.

GRAYSON: And you were still not married at that time? The war was over.

BEYNON: Well, I wasn't released from this tank work until 1947, but I went straight from the tank work to get married. I got married in May 1947 and immediately went to ICI.

GRAYSON: If you would, I would like to find out what you did for the tank work. You worked there for several years -- three, four years.

BEYNON: Well, I designed equipment for firing on the move so that when you had your gun sighted on the target, if you went over a bump, the tank went down and the gun went up. Then the gun automatically came back up. You could do this using rate-measuring gyroscopes and a series of switches. The gun was controlled both vertically and horizontally.

GRAYSON: So the gyroscope sensed the movement and then provided the information necessary . . .

BEYNON: To keep it absolutely fixed in space. It was an uncanny feeling, actually. If you were inside the tank with your eye glued to the telescope, looking at the target, you could see as the tank moved; that the target was getting closer and closer, but you had no idea as to whether you were going to the right, or the left, or straight at the target.

GRAYSON: Oh, so it actually would rotate the turret.

BEYNON: Yes, as well.

GRAYSON: If you had to do an avoidance maneuver, it would still keep the target sighted.

BEYNON: Our electronics wasn't very good in those days, and it wasn't a very good system.

GRAYSON: But it was a first step.

BEYNON: The Germans couldn't do it, so this was one of the things that was done.

GRAYSON: And this was all done with tube type electronics, valves, and whatnot.

BEYNON: Yes.

GRAYSON: And relays and that kind of thing.

BEYNON: Exactly.

GRAYSON: Wow. Did that take up pretty much all of your time then, or were there some other projects that you did?

BEYNON: Well, everything to do with firing the gun. I used to design muzzle brakes for reducing the recoil of the gun and well, everything to do with tanks. For example, for clearing minefields, there was the so-called flail tank that used to have weights attached to the ends of chains that hit the ground ahead of the tank and exploded the mines as the tank moved along.

GRAYSON: So they would essentially eject these weights on the end of chains in front of them to activate any mines that might be there.

BEYNON: 20 pound weights on the end of ten foot long chains. There were all sorts of other things which were very highly secret. We designed tanks that could lay bridges....could carry a folded bridge on the top of the tank, get to a river, and literally unload the bridge over the river which the tank could then go across.

GRAYSON: Do you know how much of this technology was actually used?

BEYNON: Well, I think it was all used. And instead of armor piercing shells, which were the normal shells on a tank, we had other devices, one of which for example I remember was called the flying dustbin, which was a mortar of a huge diameter. I can't remember now, but from memory, I would've said probably two feet diameter. A tank, you see, was in those days a vehicle that could go ahead of the infantry and if the infantry were being shelled, the shells could explode on the tanks without causing any damage. You were protected in the tank from these sorts of explosions.

It was only the specialist armor piercing shell that was any danger. I should say that the British tanks and the American tanks, when America came into the war, were vastly inferior to the German tanks in terms of thickness of armor and depth of penetration achievable by the armor-piercing shells. The German 88 millimeter gun could penetrate any British or American tank at about half a mile further away than the British or American tank could penetrate the German tank.

GRAYSON: So that was certainly an advantage for the Germans.

BEYNON: And there were some very big tank battles during the war at the time I was working. In North Africa, for example, over a period of time we would collect, say, 1,000 tanks. The other side would collect a similar number. They'd go out and have a battle, and at the end of the battle, one side might have 100 tanks left and the other side none. And the side that had 100 tanks still functioning ruled North Africa. Your chances of being knocked out in a battle like that were very, very high. It used to take about three shots or maybe four shots to hit your

target. You'd fire the first shot. Let's say the shot fell short. You'd then double correct so that the next shot fell beyond, and then you could interpolate quite accurately, and you would expect to hit or be very, very close with the third shot, but in any case, to hit with the fourth, and I think the statistics were that the average life of a tank was four shots fired in battle. If you were really hit, then chances were that you would catch fire. It was very difficult for some of the crew to get out of the tank. The driver and the co-driver had to exit the tank through the turret, and this wasn't always possible, depending on the position of the turret.

GRAYSON: Yeah, so what was the staffing then, the driver, co-driver, and then I guess they had some other people?

BEYNON: There was the driver, and the co-driver, and then in the turret, you would have the gunner, and the loader, and the commander. So it would be a five man crew on a normal big tank.

GRAYSON: So the commander essentially did the tactical work and directed the rest of the troops in the tank.

BEYNON: Yes. The conditions weren't very good inside the turret. When the gun went off, it was like being punched in the stomach very hard with the blast, and people were literally physically sick when the gun went off due to the blast.

GRAYSON: Oh my. Did you experience any of this yourself firsthand in terms of testing?

BEYNON: Yes, I got to test all these things.

GRAYSON: I see, so this was a rather, how would you say, brutal experience for you, even though you weren't in the war fighting.

BEYNON: It was an exciting experience, and then I remember also when aircraft came along; the low flying aircraft with a rocket, the tank had virtually no defense against them. They could hedge hop, fire a rocket, and it was as the saying goes, 'every egg a bird'. They would hit with very good, accuracy, and the tank would be unable to get its gun aimed in time to do anything before the plane would come around again.

GRAYSON: So there wasn't really much defense.

BEYNON: Airpower was easily capable of knocking out these tanks, so control of the air was extremely important.

GRAYSON: And you did this for several years?

BEYNON: Well, I started in 1943, just when I'd got my degree, and I wasn't allowed to leave, and was so to speak, conscripted until 1947, which is when I left, and got my degree, and went to ICI.

GRAYSON: Okay, so were you working for an industrial concern, or were you working for the Army? You were in the Army.

BEYNON: Well, I was working in an Army unit all the time. I wasn't wearing a uniform, but there were uniforms all around me, and I lived in an officers' mess, and the whole unit, the head of the unit was a colonel in the Army.

GRAYSON: So you were considered....were you enlisted or not?

BEYNON: No, I was not.

GRAYSON: So you were not really part of the Army per se.

BEYNON: Right.

GRAYSON: Yet, you were conscripted by the Army.

BEYNON: I would've been better off to have been in the Army. I'd have got more money.

GRAYSON: What kinds of salaries did they pay?

BEYNON: Very, very poor. When I started, I earned four pounds per week.

GRAYSON: Okay. Wow. That's not very much.

BEYNON: The pound was very strong against the dollar in those days.

GRAYSON: Just like it is today.

BEYNON: No, no, much more, about four dollars twenty to the pound.

GRAYSON: Okay.

BEYNON: So I was earning, you might say.... about \$16.00 a week.

GRAYSON: So that's not real big pay, but at least at the time it sufficed.

BEYNON: There wasn't much to spend your money on in this country during the war.

GRAYSON: I can imagine.

BEYNON: There was rationing of food and I think in amounts that Americans generally don't realize. I think the ration of butter...I can't remember, but it was either one ounce a week or two ounces a week. You never saw an egg, or a banana, or an orange, or anything like that.

GRAYSON: And I guess this was for two reasons, some of this stuff couldn't be had, and the other was things were being used in the war effort.

BEYNON: Yes, but mostly it was that more food had to be imported, and cross the Atlantic, and pass all the U-boats.

GRAYSON: Yes, and you couldn't get here without getting blown up.

BEYNON: And then of course, there were the air raids. You'd get an air raid. Well, I was located just outside London.

GRAYSON: So you'd moved from Manchester to London for this job.

BEYNON: This was during the war and I hadn't yet gone to Manchester.

GRAYSON: Oh, you hadn't gone to Manchester. That's correct. You were working on tanks.

BEYNON: I was in Surrey, and there was an air raid virtually every night.

GRAYSON: When did these start?

BEYNON: Well, they could start any time.

GRAYSON: But I mean year-wise. It was in....

BEYNON: 1940.

GRAYSON: So they started right immediately with the war?

BEYNON: Yes and whether one plane came over or 100 planes came over, the inconvenience was just the same, because the air raid sirens went off so everybody then had to go to the air raid shelters. Most people had a shelter, usually a corrugated iron shelter in their garden that they could go to, you'd have to go there to sleep. Then in the early morning, the all clear siren would go, but you can imagine that this went on, night, after night, after night. Occasionally there was extra excitement when the bombs came down near you.

GRAYSON: You may not have experienced any bombs in your area, but because an air raid was ongoing, you had to be in a safe place in case something would happen.

BEYNON: Exactly.

GRAYSON: You say everybody had their own shelter that they could go to if they had a home?

BEYNON: Yes, it was just corrugated iron.

GRAYSON: Kind of like a Quonset hut type structure?

BEYNON: Yes; but it was buried in the garden. You'd have a bed in there, and you'd sleep in there.

GRAYSON: I see. Wow, and this went on for how many years?

BEYNON: About '40 to '45. Five years.

GRAYSON: How close did one of these things ever come to your particular shelter?

BEYNON: Well, they used to come pretty close very often. Even Swansea, which was way away from London, was visited by bombers regularly, and the actual city was flattened. In the center of the city, half a mile square, there wasn't a single building left. In fact, throughout Britain, I understand one building in ten was damaged or destroyed by bombing during the war. It was more a feat of endurance to live through the bombing rather than bravery. I think there was nothing much you could do about it except suffer it.

GRAYSON: You just dealt with it.

BEYNON: Yes and with the monotony of the available food and the....

GRAYSON: When you were doing with this tank work, was this something that was being done six days a week, or seven days a week, or did you work a regular week? Some of the war effort and various activities were almost nonstop.

BEYNON: Well, this was solely a research unit, and in general, we worked six days a week, but we would work seven if there was any immediate reason to do so. For instance, getting some of the outfit ready for a special exercise.

GRAYSON: And so this was all driven by upper echelons in the Army that wanted to improve the tanks for fighting with the Germans?

BEYNON: Yes. There were various kinds of tanks that were used for different things. There were infantry support tanks, for example, that were just used for going ahead of the infantry into the bombardment, and being offensive, firing ahead. The infantry would come up behind or the tanks could be used to attack in their own right. There were cruiser tanks that were not so heavily armored and could move quite rapidly, these would be in general, about 30 tons in weight, and could do about 50 or 60 miles an hour over an open road, although they weren't allowed to generally be on the road, but the really big tanks like the Tiger tank the Germans had, that would weigh something over 60 tons. The armor at the front would be five or six inches thick and because the front of the tank sloped, you didn't go through five or six inches perpendicularly. You went through at an angle, so effectively, the armor was thicker than that, and the armor was also very good on the sides, but especially on the front. But underneath there was very little armor, so minefields were a big danger.

GRAYSON: So you had reasonably good intelligence on how the Germany tanks were constructed.

BEYNON: We'd capture them now and again, and bring them back.

GRAYSON: Well, if you're gonna move that much mass, how big of an engine did you have?

BEYNON: Well, this was one of the difficulties. I know when America came into the war, they didn't have in production an engine suitable for a tank.

GRAYSON: I can imagine.

BEYNON: The bus engines were too small, and the aircraft engines were generally too big, so the first American tank that I saw, a General Sherman tank, had four bus engines in it, all driving the same shaft. So that when you turned the ignition on, four ignition lights came on.

GRAYSON: A real gas guzzler, huh?

BEYNON: I think they used to go about two gallons to the mile or something of that sort. All the fuel was carried of course in the sides of the tank, which meant that if the side was hit, you're quite likely to get a fire, and all the shells for the gun, they were stored around the turret so that the loader could bend down and pick them up. This was quite a job, because the actual shell plus the propellant; the whole thing weighed about 40 pounds, and the loader had to pick this up, and load it into the gun in a very restricted space. Then the gun fired and ejected the shell case. That came out red hot, so you had these red hot things rolling about on the floor. You were getting the blast from the gun firing; not very pleasant.

GRAYSON: It seems to me that being in the Tank Corps would've been an almost certain death sentence.

BEYNON: It was a really dangerous unit to be in, very dangerous.

GRAYSON: So eventually when the Army let you go.

BEYNON: I went to ICI and arrived for my first morning, was shown to my desk, and on my desk there was a note that said, "Remit for Mr. Beynon, build a mass spectrometer," which came as a huge shock to me, because I didn't really know what a mass spectrometer was.

GRAYSON: So whose idea was it?

BEYNON: That was my boss' idea. Hailwood. A.J. Hailwood.

GRAYSON: Where did he get this idea?

BEYNON: Well, I don't know. There was a large Department of Pharmacy, and they were making drugs, and they wanted something which could follow the metabolites of these drugs in the body, and analyze them. Isotopically labeled drugs were the answer, but you couldn't use radioactively labeled drugs, because of the danger to the subject, so you had to use stable isotopes, and he'd worked out that the mass spectrometer was the way to analyze these.

GRAYSON: So this would've been 1947? There was some work done at Columbia University in New York by a group that was using isotopically labeled oxygen and nitrogen to follow the metabolism in subjects, so I guess he must've been aware of this.

BEYNON: Well, I don't know what he was aware of, but anyway, I set out to build this instrument with the one assistant.

GRAYSON: And the assistant's name was?

BEYNON: It was Albert [Bert] Williams, A.E. Williams. He had come back from the Air Force where he'd been a navigator in bomber command. He'd survived more than 30 bombing trips over Germany. I think he might've been ranked something like a Wing Commander, but he came back to ICI, where he'd been a lab assistant before the war, and was just put as an assistant under me.

GRAYSON: What was his education?

BEYNON: He'd been to high school, and he was going to night school to try to get a degree, and he worked with me throughout my career at ICI.

GRAYSON: Okay, I thought I saw his name.

BEYNON: I owe him really a very, very great deal.

GRAYSON: Well, I noticed he's in a lot of your publications in the early period. So the two of you then are given this task.

BEYNON: I had to go out and buy....there were no electronic bits whatsoever at ICI, and because it was just after the war, it was very difficult to get parts. For example, to get copper wire for the magnet coils; you could only buy it in one pound reels.

GRAYSON: Couldn't make very much of a magnet with a one pound reel of copper wire.

BEYNON: You could if you had 50 of them and joined them all together.

GRAYSON: So could you buy 50 at a time?

BEYNON: And then the transformers that we needed, they were home made. Steel was in very short supply. The power supplies on the mass spectrometer were really lethal. Our first instrument, it had an accelerating voltage of 2,000 volts, and the power came from mercury arc rectifiers that could deliver up to, I think a quarter of an ampere, so it was not recommended to touch that.

GRAYSON: Not if you wanted to live very long!

BEYNON: But it had one remarkable benefit that if anything went wrong, let's say, inside the ionization chamber, a little dirt or something in the ionization chamber, which produced a short circuit, there was sufficient power to burn that short circuit away so that the source was to a great extent, self cleaning. It was so difficult to take the source out, because it was a glass machine, and you needed a skilled glass blower to be able to reinsert the source into the correct position, and make a glass seal at the same time. Anyway, we made this instrument.

GRAYSON: Okay, but I need to explore some things that are happening here, because you had to make some selections with regard to what you were going to do in terms of analyzer geometry and method of ionization.

BEYNON: Well, I spent, I suppose, several months in the library reading everything I could about mass spectrometers and what they could do, and finally decided that the sort of design that we wanted was an instrument that had been built in Canada by Graham, Harkness, and Thode.¹

GRAYSON: So this was something that they had published back in 1940 something?

BEYNON: Yeah, this was published in the *Journal of Scientific Instruments*.

GRAYSON: Okay.

¹ R. L. Graham, A. L. Harkness, and H. G. Thode, "An AC Operated Mass Spectrometer for Isotope Abundance Measurements," *Journal of Scientific Instruments* 24 (1947): 119-28.

BEYNON: [Doorbell rings.] Oh dear, there's somebody at the door. I wonder, could I ask you, could you answer the door?

GRAYSON: I can surely do that. Mail for you, sir, a big, heavy box from Wiley.

BEYNON: I get five copies of *Rapid Communications* and *Mass Spectrometry*; which is now published once every two weeks.

GRAYSON: So what are you going to do with five copies?

BEYNON: Give them to the university.

GRAYSON: Okay. So you did some research before you started building this instrument, and the design that this fellow had followed, was it just a single focusing instrument?

BEYNON: It was a single focusing.

GRAYSON: The magnetic sector was 60, 90, 180 degrees.

BEYNON: 90 degree magnetic sector. The flight tube was a copper wave guide through the magnet. With metal to glass seals at either end. The ion source was the one that Al Nier had designed. It was remarkable in that Nier just built one, got everything absolutely right, nobody ever improved it as far as I know. Everybody copied it.

GRAYSON: You got that from the literature as well?

BEYNON: Yes. The detector was just a Faraday cup, just a piece of metal with a $10^{11} \Omega$ resistor into a direct current amplifier, and it could detect down to 10^{-15} amp, but with a time constant of one second. It was very, very poor compared to what's done today with multiplier detectors. But a damn sight better than J.J. Thomson had.

GRAYSON: Sure [laughter].

BEYNON: The minimum current he could detect was a microampere.

GRAYSON: Yes. Do you recall what the radius of the 90 degree sector was?

BEYNON: Six inches.

GRAYSON: Six inches. What was the mass range and resolving power?

BEYNON: Well, the mass range that we were working in, which seemed to be the range of these metabolites, they were quite small molecules, was up to 200 or 300.

GRAYSON: And the resolving power would've been nominal mass, I guess?

BEYNON: Well, the resolving power was a couple hundred. I think when you do research, people look for what they call your achievements, but I think in general, individuals shouldn't be credited too much with achievements, because I think they get their ideas in the midst of a crowd of people who are all thinking about the same problems, who are all discussing them, sharing their ideas, and all right, somebody finally says the word that says, "That's the discovery," but he owes that discovery to all the people he'd been working with as well.

GRAYSON: He's getting ideas from many different inputs, and then he synthesizes this information to create those.

BEYNON: But there are some things that I think....and it might've been my training as a physicist that alerted me to this sort of thing, that resolving power, this was something that I think I did clarify, if you like, but I've never been credited with it, and it answers the question "What do you mean by resolving power?" What do you mean when two things are resolved? Does it mean you can just tell that there are two things there, which is what's meant when you say, "These two stars are resolved." They're resolved simply if you can tell that there are two there.

What, do you say about two peaks in a mass spectrum? Do you just have to have a valley in between them to tell that there are two there? The answer to that is no, you don't. You can put the two peaks so close together that you get an inverted valley, and the combined peak shows little blips on the side. Even then, a little blip on the side of a peak is sufficient to show that more than one component is present and is sufficient to measure its location and to do a

mass measurement; and I think I was the first person when I published my book² in 1960 to talk about resolution in terms of valley, 10% valley, or 50% valley, or inverted valley, so that's one thing that I achieved that I don't think was ever recognized. People just thought, "Well, that's obvious," but it wasn't, because it was never used before. And the other similarly simple thing that I think I uncovered was the so-called nitrogen rule, which I think I discovered when I noted that for all the elements that occur commonly in organic chemistry; carbon, hydrogen, the halogens, sulfur, phosphorus, boron, etc., the mass of the most abundant isotope is even if the valency is even and odd if the valency is odd. The sole exception is nitrogen with an odd valency and a most abundant isotope of mass 14. Thus, for the compounds with which we were dealing, of molecular weights of up to 500, the most abundant molecular ion peak would be of even mass if the compound contained an even number of nitrogen atoms and odd for an odd number of nitrogens. When listing all possible molecular formulae corresponding to a particular peak, this 'rule' enabled many possibilities to be discarded, making it more likely that a unique empirical formula could be obtained. Measuring the relative abundances of the isotope peaks associated with the molecular ion peak to supplement this rule made it even more probable that the empirical formula could be found. That was something I discovered, which for obtaining possible formulae when you measure accurate masses, you could cut the possibilities down to a great extent by using this "nitrogen rule", and I don't think anybody'd ever used it 'til I did.

GRAYSON: That's interesting. I know that McLafferty did a lot of work synthesizing ideas about the interpretation of mass spectra. I know that he states the nitrogen rule, but I don't know that he has any reference to where it came from. The nitrogen rule was published in your book?

BEYNON: Yes. Well, I'd claim that that was the first.

GRAYSON: Very good. Well, that's something that's quite interesting and may be worth doing some research. So now you're building this mass spectrometer; how much of a budget did you have for this?

BEYNON: Well, it was really a week by week budget. I'd say "We need so-and-so. I need a hundred pounds." "Good god, you had a hundred pounds last week." There was that sort of budget.

GRAYSON: So you went to this fellow, Hailwood, and said, "Give me some more money."

² J. Beynon, Mass Spectrometry and Its Applications to Organic Chemistry (London: Elsevier Publishing Co, 1960).

BEYNON: Yes, he was very good that he did give us more money and we built this machine.

GRAYSON: Did he want to have a progress report every now and then. “Well, I’ve given you 500 pounds. What have you got to show for it?”

BEYNON: Of course, but in general he let us work and didn’t keep interfering too much.

GRAYSON: Well, that’s a little bit strange management attitude.

BEYNON: We adopted the philosophy that we wanted this machine to be reliable, and so we’d have to build it so that it was rated in terms of ….well, if we wanted a resistor that was going to have to dissipate one watt of energy, then we would buy a resistor that could dissipate ten watts. And we built everything very, very conservatively. We built it like a battleship. And I think that did pay off in the end.

GRAYSON: When you did this you had to build all of the electronics yourselves.

BEYNON: Sure.

GRAYSON: And so some of your physics came in for circuit design, and that kind of thing.

BEYNON: So my radio training came in at last. But also I had to learn all sorts of things like how to make glass-to-metal seals.

GRAYSON: So that technology still had a ways to go at the time. Nowadays you can buy these seals pre-made.

BEYNON: You couldn’t buy them then. You could buy the metal and I remember, you had to pre-treat it. It had to be heated in a stream of hydrogen at 1100 degrees. We used to do all that in the backyard.

GRAYSON: What kind of metal? You couldn’t just use any old metal for these.

BEYNON: It was a commercial alloy, but I can't remember the name.

GRAYSON: Well, it used to be Kovar, but it might've been a different alloy then.

BEYNON: I don't know what alloy it was, but anyway, the alloys were known and available, and you had to make a graded glass seal, because it wouldn't seal directly onto Pyrex glass. You'd have to seal the metal onto one particular glass, and then fuse that glass onto a different glass, onto a different glass until you got this graded seal down to the glass that you wanted.

GRAYSON: They provided glass blowers to do this for you?

BEYNON: We had one glass blower who was a real expert. He was absolutely an artist, a genius. He trained in Holland.

GRAYSON: Oh my, so you finally then went through this long period of construction. How long did it take for you to get the instrument completed?

BEYNON: It took us two years to get going and then came the problem of how do we test it? We were complete novices at this. There were mass spectrometers in the United States that they were using for oil analysis and only oil analysis. Nothing was ever put in except their hydrocarbon samples for fear of altering the cracking pattern. These commercial spectrometers were 180 degree magnetic sector and were scanned by varying the accelerating voltage. We scanned by varying the magnetic field so as not to affect the electric field within the ionization chamber.

GRAYSON: You did a magnet scan? That was a challenge, wasn't it?

BEYNON: Yes.

GRAYSON: Because this meant you actually wound the coils for the magnet and you designed the pole faces, and you figured out how to scan that magnetic field with a current driven electronic system?

BEYNON: Yes, it was scanned manually most of the time. If you wanted a spectrum; you needed two men. One man put his hand on the scanning knob, rotating the knob very slowly

and reading the bottom of the peak, the baseline, and then going up the peak, altering the sensitivity, shunting it, then reading off the value of the shunt and the top of the peak, and then down into the valley on the other side. Two men could do about four spectra in a day.

GRAYSON: Oh my, and so this would be over a mass range of maybe 150?

BEYNON: Well, from carbon 12 up to at least mercury, which was our mass marker at mass 200. We had mercury vapor pumps. Perhaps we'd go up to 300 or 400, or occasionally we were very brave or had a particular sample, we'd go up to 500.

GRAYSON: So as you went up in mass, you'd go down in magnetic field strength, right?

BEYNON: No, up in mass, up in magnetic field strength.

GRAYSON: Okay, up, so you started pushing current through your magnet. Okay [laughter]. What was the magnet maximum current that it would draw? It must've been in the ampere range, several, five, ten?

BEYNON: I can't remember now.

GRAYSON: It probably would've been in the order of quite a few amperes.

BEYNON: No, lower than that. It was a high voltage, low current system as far as we could make it. The magnet coils were relatively high resistance and had many, many turns of wire.

GRAYSON: And you actually wound this yourselves?

BEYNON: Yes. It took weeks and weeks to wind the coils.

GRAYSON: So you had mercury diffusion pumps on it. Were these glass pumps?

BEYNON: Yes.

GRAYSON: Did you have a cold trap of any kind?

BEYNON: Yes, it was a glass u-tube cold trap that used liquid nitrogen. Dry ice didn't give a good enough vacuum due to the vapor pressure of mercury and of water at dry ice temperatures [$\sim 80^{\circ}\text{C}$].

GRAYSON: Ah, so the liquid nitrogen would minimize the mercury vapor pretty much. Was liquid nitrogen hard to come by in those days?

BEYNON: Liquid nitrogen was available very easily and cheaply. It was made in Manchester in the industrial park there, and we'd get deliveries daily.

GRAYSON: Okay. You had to have a sample inlet system. What did you use for sample inlet?

BEYNON: Well, there was glass and a capillary leak into the ion chamber. They were all described in my book that I wrote in 1960.

GRAYSON: Okay. Very good, I can look that up.

BEYNON: Yes. When it came to testing this machine... Before we actually got on to using it, it was a question of what could we put in as a sample to calibrate the mass scale -- we didn't know. We weren't chemists and didn't know any chemistry, but we decided that, because there was a convenient gas supply in the lab; we'd put some of this gas in. Now this was coal gas, so we put coal gas in, which I expected would give us a few peaks due to hydrogen and carbon monoxide and so on, but in the event, we got a real forest of peaks, including one big peak at mass 78.

I didn't have a clue as to what 78 might be, so I tried to find the empirical formula in terms of carbon, hydrogen, nitrogen, and oxygen . . . we checked on all of them. There was a book I discovered, *Beilstein's Formula Index*, which gave molecular formulae for different molecular weights and which I suppose was well known to chemists. I'd never heard of it. In *Beilstein* you just looked up the empirical formula, and it told you all the chemicals that could have that formula -- the ones with oxygen in and with two nitrogen's and so on, and we found the simplest formula for the peak at mass 78. It was C_6H_6 , so I looked up C_6H_6 in *Beilstein's Formula Index*, and it gave quite a number of compounds with this formula, linear compounds with double bonds and triple bonds, and also of course, benzene. So when I saw 'benzene', I

thought, “Well, I’ve heard of that, so it must be benzene.” So I then went ‘round ICI proclaiming to all the chemists, “Do you know that the town’s gas supply contains benzene?”

Hailwood, when he came to see our first spectra, said to me, “How did you discover that there’s benzene in the coal gas?” So I told him how and he said, and I think this was a very, very clever thing for him to say, he said, “If an idiot like you can analyze benzene like that in an unknown mixture, I don’t think we’ll waste our time looking at metabolites in pharmaceuticals. We’ll use this machine for doing qualitative analysis of unknown samples.”

We had this huge organic chemistry research department, you see, producing all manner chemicals. The colorists and so on wanting to know the impurities which were affecting the color. Wanting to know, for competitors’ products, what traces of impurities were present that might show how these things had been manufactured, and so that’s what we did. We started looking, providing an analytical service. Here’s a chemical, what is it?

GRAYSON: This started in 1949?

BEYNON: 1949. Now this instrument worked, and it worked very reliably, and it wasn’t pensioned off until the 1970’s.

GRAYSON: Oh really? Did it have some improvements over that period of time.

BEYNON: Well, it was continuously improved. We finally got a pen recorder for it. Before that, we used a galvanometer lamp and scale. It was a two man operation, reading the peak heights on a galvanometer scale. But we finally got electron multipliers instead of the Faraday cup, a pen recorder instead of the galvanometer and a proper scanning system.

GRAYSON: And it had a useful life into the ‘70s. Wow, and then what happened to it?

BEYNON: Well, it just...we had other machines by that time, commercial machines, and it gradually fell out of use, because all the manpower was working on the commercial machines, which were generally more advanced, but it had a long and respectable life. Also, with this machine we worked on ways of improving the resolving power including development of a means of closing the slits.

GRAYSON: Okay, so originally it had fixed slits?

BEYNON: We had a device on the collector slit where there was a little bar magnet attached to the mechanism and we could rotate this bar magnet from outside the vacuum and close the slit. With this, we got the resolution at 10% valley up to about 600, and we were able to resolve isobaric peaks in a mixture of ammonia and water, I think it was. I've still got a spectrum somewhere, that showed the doublet of CH_4^+ and O^+ I think, or NH_2 and O , I can't remember which, but I think it's a matter of record, and it's shown again in my book.

GRAYSON: Did anything get published in the literature from the spectra from that instrument?

BEYNON: Yes, sure.

GRAYSON: Okay, so there's a paper here in 1954 that was published in *Nature*.³

BEYNON: That would be the one that showed the first high resolution spectrum, I think. That was before we had a pen recorder.

GRAYSON: And that would work...

BEYNON: ...using a galvanometer lamp, lamp and scale and it's from these spectra that we got the idea that we ought to go to double focusing.

GRAYSON: So that '54 paper, that was work done on this instrument?

BEYNON: Yes.

GRAYSON: I have to get a copy of that. So this Hailwood fellow saw a new direction for use of the instrument. Did the rest of the people in the company begin to see this as something that was valuable?

BEYNON: Chemists are very conservative people. It takes them years to accept something, but a chemist would bring us something, we'd identify it for him. He'd listen to our arguments, take them all in, and say, "Yes, very good. Now I'll go away and prove this properly."

³ J. H. Beynon, "Qualitative analysis of organic compounds by mass spectrometry," *Nature* 174 (1954): 735-7.

GRAYSON: So when he said, "Prove it properly," what did he intend to do?

BEYNON: Well, do some chemical proof. I don't know.

GRAYSON: I did interview Klaus Biemann, and he indicated in the earliest publications that they had, the editor gave him a lot of grief because he wanted a melting point for the crystal of the thing he had synthesized to prove that he had done it rather than the mass spectrum, because crystalline melting points were the way it was done.

BEYNON: Because they wouldn't trust the mass spectrum.

GRAYSON: But still, people were giving you samples. Eventually you got some traction with them.

BEYNON: Mass spectrometry was the method of choice for taking a first look at any unknown sample. We worked, for example, on all stages of the nylon process. I think we identified impurities in every part of this process. Well, we did so many jobs. I remember on one occasion, ICI were offered a process. I think it was for Terylene or what you Americans call Dacron. ICI were being offered terephthalic acid that was supposed to be very, very pure, but which had been made by some very simple process that they wanted ICI to buy off them.

GRAYSON: As a patent license.

BEYNON: Yes. We got a sample of this to look at, we were able to see that it contained dimethyl terephthalate, that they'd esterified it, and they'd done a step in the purification that they weren't announcing to ICI. We also detected various other things in there, including trichloroethylene. They'd cleaned out part of their equipment with this when they were doing the purification, so we were able to announce to them, "You crooks. You've purified this by this and this mechanism, and you washed out your apparatus with trichloroethylene."

GRAYSON: So what'd they have to say about that?

BEYNON: "How in the hell did you know all that?" They never sold that process.

GRAYSON: So this was to the advantage of ICI that had a financial impact; so someone in management understood that there was some financial value derived from having this instrument.

BEYNON: So that's when we said, "Now we want double focusing ." Nobody had double focusing. This was a complete step in the dark now. We went to a company in Britain, Metropolitan Vickers who later became AEI and GEC, and asked them if they would make a double focusing machine to our design. What resolution would they guarantee? They said they would make a machine and they would guarantee a resolution of 2,500 until we put a chemical sample in. They said, "Once you put your chemical muck in, you'll wreck it." So the Board of ICI agreed that we could buy this machine without really any guarantee of resolution.

GRAYSON: I need to explore some more details of this interaction. So were you the person in charge of interacting with Metropolitan Vickers?

BEYNON: Yes.

GRAYSON: And who did you interact with on their side?

BEYNON: With their Managing Director, John Waldron.

GRAYSON: Okay, and so at this point in time, Metropolitan Vickers had built mass spectrometers?

BEYNON: They were building a single focusing machine quite similar to the one that we had, a 90 degree sector.

GRAYSON: Did it have a model number?

BEYNON: MS2.

GRAYSON: And this selection was made on the basis "These guys build mass specs, let's go to them." or did you actually do a little vetting to find out if there were other people who were in the business? Or did you select them as the best of a number of companies?

BEYNON: Well no, they were the only one.

GRAYSON: They were the only one to do this, okay, and Waldron was the person that you interacted with.

BEYNON: Yes.

GRAYSON: Now when you say, “To your specifications,” did you just specify the resolving power that you wanted, or did you specify the geometry?

BEYNON: We specified the geometry and everything else.

GRAYSON: So the geometry for that instrument was a 90 degree magnetic and a 90 degree electric.

BEYNON: We called it (none too accurately) the Nier-Johnson geometry. We selected it because of all the double focusing geometries, which all would’ve given similar resolutions; this was the one with the shortest ion path. We decided a short ion path was an advantage.

GRAYSON: Why was it an advantage?

BEYNON: Because you wouldn’t have too many collisions with gas molecules on the way through with your ion beam.

GRAYSON: Okay. Now the Nier-Johnson geometry really is a 90 degree electric and a 60 degree magnetic sector. That was the one that they devised in Minnesota, but you had a 90/90 sector?

BEYNON: We had a 90/90. Well, I can’t really remember why we changed, but I think it was to do with the length of the ion path.

GRAYSON: I don’t know if Nier ever patented that geometry or not.

BEYNON: I don't know. I don't think he could, because it was a well known geometry.

GRAYSON: Do you know the story about Johnson who worked with Nier?

BEYNON: No.

GRAYSON: Nier gave Johnson, as a master's degree project, to design a double focusing instrument using electric and magnetic sectors. Johnson did the design, essentially all the theory work, and then took his master's degree and left. He never saw the instrument work, so it is interesting that this design that was so important in mass spectrometry was never realized by the person who designed it. A later graduate student helped Nier actually build the instrument. So let's see, we were working on this design that you proposed to Waldron.

BEYNON: They started building it, and I was sent by ICI to go and work at Metropolitan Vickers on this, so I worked for six months at Metropolitan Vickers on it.

GRAYSON: And were you paid by them for this?

BEYNON: No, I was paid by ICI. But I worked there with Robert Craig, R.D. Craig. Well, we got this machine. We switched it on, and it gave 2,500 virtually immediately, so we then started playing with slits and things, and within a week, we'd got the resolution up to 13,500.

GRAYSON: Wow, 13,500, that's pretty amazing.

BEYNON: Now this was a small machine with a six inch radius. I can't remember whether it's six inch electric or six inch magnetic, but they're comparable sizes, the two sectors, and it was called the MS8. It was a prototype, and when Metropolitan Vickers saw how well it worked, they said, "We won't make another of these. We will rebuild it in twice the size, a 12 inch radius, and call it the MS9."

GRAYSON: And this was in 1960?

BEYNON: Really, I can't remember the exact date, but in my publications, you'll see where the MS8 starts coming in.⁴

GRAYSON: Okay, I'll have to do some research on that. So you actually did publish some literature with the MS8.

BEYNON: Well, we published this 13,500 resolving power.

GRAYSON: Okay.

BEYNON: Which was very, very high for those days, because it was unheard of resolving power.

GRAYSON: Yes. I'm trying to think. Did they [Metropolitan Vickers] have the MS7 by that time?

BEYNON: The 7 was doing something completely different, it was a spark source.

GRAYSON: That was the spark source with Mattauch Herzog geometry.

BEYNON: Yes.

GRAYSON: Okay, but when they designed the MS7, they weren't really going for resolving power. It was designed for elemental analysis of metallic samples.

BEYNON: No. Ours was a mass spectrometer organic samples. ICI made me go to chemistry classes. Their philosophy was that I and three or so other physicists that they had recruited by this time would go to a class given by one of the organic chemists. I remember he said, "First thing you've got to do is you've got to buy a chemistry book, all of you, and we'll buy....*Organic Chemistry* by Conant." I think that was the book. And his lectures were very brief. His first lecture was "Read the first three chapters and I'll see you next week. Goodbye."

⁴ J. H. Beynon, R. A. Saunders, and A. E. Williams, "The high resolution mass spectra of aliphatic esters," *Analytical Chemistry* 33 (1961): 221-5.

GRAYSON: My, my. This was taught at University of?

BEYNON: This was taught within ICI. It was one of their ICI chemists lecturing to the physicists.

GRAYSON: I see, so this is internal education.

BEYNON: Well, we treated this very lightheartedly. We glanced at the thing, came back for the second lecture and then to our horror....

GRAYSON: He gave an exam?

BEYNON: He said "You, come out to the front here. Now on the blackboard, answer question number one." So he made you look a fool in front of your contemporaries, but our course didn't try to teach us organic chemistry as a properly trained organic chemist would learn it. We were taught, first of all, nomenclature, that if we were given a formula, we could name it, and if we were given a chemical name, we could write the formula, so he says, "First of all, that enables you now to talk intelligently to organic chemists." And then we learned certain properties of organic chemicals. By looking at a formula, we were able to judge whether it was likely to be a solid, a liquid, or whatever, what its melting point or boiling point might be, how thermally stable it was, what it might dissolve in, things of that kind.

GRAYSON: So this was a very hands on, practical kind of experience.

BEYNON: And that meant, you see, when all our customers came with their chemicals then, at least we were able to discuss things sensibly with them, and plan what we were going to do with their samples.

GRAYSON: Interesting, so how long did this course last?

BEYNON: A few months. That's all.

GRAYSON: Okay, so it was fairly brief, but intense.

BEYNON: But then my learning continued informally for the rest of my career since I was talking to chemists. So I began to acquire more and more chemistry, and more and more knowledge of certain processes, like the nylon process.

GRAYSON: So this was really pretty helpful in your work with the rest of the troops in the field.

BEYNON: As a physicist, you were given all sorts of peculiar jobs at ICI, anything that turned up that they would say, “This doesn’t look like chemistry, it must be physics.”

GRAYSON: Okay.

BEYNON: So static electricity, for example, in grinding up their dyestuffs and pigments, there were all sorts of fires and explosions from time to time. “Why was that? That’s not chemistry. That must be physics.” So I find myself in a factory sorting it out.

GRAYSON: So you were a troubleshooting jack of all trades.

BEYNON: I was ICI’s fire and explosions officer for the whole company, not just for Dyestuffs Division, for a long time. I remember one occasion, to do with the Dacron process. The process involved starting with *para*-xylene. We have xylene as a mixture of the three isomers. Now how do you make *para*-xylene? How do you separate *para*-xylene from *ortho* and *meta* xylene. And the answer is, as physicists will tell you, *para*-xylene has got a much higher melting point than the *ortho*- and the *meta*- isomers because of its symmetry. So all that you do is you take xylene, you mix it with dry ice, and it goes into a sort of snow of *para*-xylene, and you then centrifuge this and you’re left in the centrifuge with *para*-xylene and the *ortho* and the *meta* go through as liquids, but then the excitement comes. To get the *para*-xylene out of the centrifuge, you must scrape it off with something. Now, as soon as you insert a “plough” of any material; e.g., metal or plastic, the whole centrifuge lights up like a firework display. When they started to operate this process, they had three fires in the first four days, so I was to stop this from happening.

GRAYSON: So these fires would start in the centrifuge after they decanted the liquid xylenes?

BEYNON: Yes. As soon as they took steps to get the solid out, to put a sort of...I don’t know what you call it...a plow in to scrape the stuff off the centrifuge, the whole thing would charge

up, you see, and begin to flash and catch fire. I couldn't find any material that wouldn't charge up when it was rubbed on the xylene, so the only solution I could come up with was get rid of the air and seal the plant. Fill it with nitrogen. As far as I know, the process still operates to this day filled with nitrogen.

GRAYSON: Sure, without oxygen, you're not going to have combustion, clever.

BEYNON: And with the things like the nylon process or the Dacron process, because they were such big plants, the value of a mass spectrometer was small compared to the value of the sales of these products, so the works manager could make you push the mass spectrometer way beyond what you wanted to do. We used to be looking for impurities in some of the nylon intermediates, for example. We'd be looking for impurities in, say, adiponitrile down to parts per million, putting more and more sample into the ion chamber until one day we lit the mass spectrometer up like a discharge tube, wrecking the source and everything else.

GRAYSON: Oh sure, definitely.

BEYNON: Well, the answer to that was, now do it again, because the output of my plant in one day is worth more than your instrument. So we used to work at very high pressures, and push the machines. But it was a very forgiving kind of instrument because it had been so conservatively designed.

GRAYSON: So this was your original instrument.

BEYNON: The glass instrument.

GRAYSON: So you're saying it blew up on occasion and then you had to have it rebuilt. Now at this particular point, you were the mass spec guru for the company. Did you have any other people come in that were trained in mass spectrometry or people that came in with backgrounds in the field to help you?

BEYNON: We had the only group in the whole company, not just in the division, but in the whole company. We used to get samples from everywhere. In those days in this country, unlike the United States, I think, analytical chemistry was rather looked down upon.

GRAYSON: In the U.K.?

BEYNON: Yes. In America, I think it was appreciated for what it was, but to say that you were an analyst or an analytical chemist was to put yourself down in this country. People used to annoy me by saying, “With your mass spectrometer, all you’re doing is routine analysis,” and that infuriated me. Because I firmly believe to this day that routine analysis is analysis which a person doing it has made routine. But I looked upon it as having the privilege of looking at the samples of 250 Ph.D.’s and their ideas were incorporated in these chemicals, and I had the privilege of looking at them all, and learning about all of them, far from routine.

GRAYSON: I think the attitude that you mentioned with regard to analytical work being looked down upon, I think it’s also true in the United States. There are some centers where analytical chemistry has some dominance, but I think to a larger extent, over at Washington University in St Louis, the analytical chemistry side of things is not considered to be as important.

BEYNON: No. In Purdue, where I went to in the States, analytical chemistry had a very high standing.

GRAYSON: Oh yes. The thing that seems to me a little snobbish about that attitude is if these high-powered synthetic chemists didn’t have an instrument to tell them that they had made what they were trying to make, they’d be back to doing melting points, and then you would hear some squawking. So I think that they’re a little shortsighted in their attitude towards the analytical side of things.

BEYNON: They are used to thinking of the analyst as a sort of servant. Not somebody that did any useful thinking.

GRAYSON: Right. Well, I guess there’s always snobbishness in science that pops up. So let’s see, how are we doing with this general overview that I was working on, so you had good support from your management for a long time at ICI?

BEYNON: I had great support from the management at ICI.

GRAYSON: Was that characteristic because of the individual that was managing you, or was that a philosophy that was done in the company?

BEYNON: It was dominant in the company. I think it was based on the fact that I suppose we were making money for them. Our analyses were paying dividends and they were prepared to invest in us.

GRAYSON: Did you ever have to make that point to management?

BEYNON: Oh yes, to argue for a bigger instrument, we had to explain to non-specialists why we wanted higher resolving power. In fact, I remember they once showed their ignorance but their enthusiasm at the same time when we wanted to buy this double focusing machine. I said, "We'll have a double focusing machine and we'll get the resolving power up to 2,500," and they said, "Why don't you get a triple focusing machine and push the resolving power up to 25,000? We'll pay for that!" They were prepared to put their money down, trust our judgment and give us what we needed.

GRAYSON: Okay. I like the argument. Another sector was better. That's very cute. I like it. So how long did you stay at ICI?

BEYNON: Well, I stayed at ICI ...from 1947 to 1969. And I was then headhunted by Purdue University.

GRAYSON: Why did Purdue come to you?

BEYNON: Because they had a big double focusing mass spectrometer, the Hitachi RMH2, which had been ordered by Fred McLafferty, who was at Purdue, and Purdue and Fred McLafferty had come to the parting of the ways, so Fred was off. He was going to Cornell. Purdue weren't about to let him take the RMH2 with him, but they had a big NIH grant for this RMH2. They wanted somebody to run it, and they didn't know who to ask, so they asked me. I went over and I decided I'd give it a go. I agreed to go there for a one year period, and at the end of one year, Purdue and I would look at one another and see whether I wanted to stay and whether Purdue wanted me to stay. So Yvonne and I went out to Purdue for a year. We spent the whole of 1969 there. We had our ups and downs and discovered that there was a flaw in the design of this RMH2 and that it would never give really high resolution at good sensitivity. This particular one, there was a big computer that went with this machine and there were about half a dozen people I remember working on the computer. I discovered, and I was very proud of having done it, this design flaw in the RMH2, and when I announced it, NIH said, "Well, you won't be needing that computer now. I'm going to take the whole thing away," and I began to be looked at as the guy who destroyed this important part of Purdue's analytical program.

GRAYSON: Yes, so your physics background was probably what led you to be able to sort out what was wrong with the instrument. What exactly was the problem with the optics?

BEYNON: Well, they'd got the relative radius of the electric and magnetic sectors and their placements a bit wrong so that second order distortions of the image didn't cancel properly. Anyway, we had to find out what we could use this machine for, and we developed the IKES and MIKES methods, the ion kinetic energy experiments. Now this proved not to be immediately accepted by all the chemists. I mean ALL the chemists, even the ones who should've known better...I won't name names, but the

GRAYSON: Oh, you can certainly name names.

BEYNON: For example we had an NIH inspection or whatever you call it with Klaus Biemann coming, and his recommendation of it was "Don't mess about with these metastable peaks. Nothing will come of them." We thought we were going to lose all our NIH money. The IKES and MIKES depend entirely on metastables.

GRAYSON: Sure, it's a totally metastable concept.

BEYNON: Yes, anyway, we continued, came to the end of 1969. I decided that I wanted to go back to ICI. Purdue decided that they wanted me to stay.

GRAYSON: Why? Even though you "destroyed" their effort in mass spectrometry.

BEYNON: We'd got our own way in the end. I don't want to go into politics and point fingers or anything, because it's nonproductive.

GRAYSON: But it is part of the story. So you ended up staying at Purdue?

BEYNON: No, so I ended up doing a deal with ICI and Purdue. ICI said that I could spend all my annual leave plus a bit more at Purdue every year, and I started going to Purdue for four months of the year and ICI for eight months of the year. And living out of a suitcase, and I had students at Purdue.

GRAYSON: So when you did this arrangement, did you do it in four months' straight?

BEYNON: No. I'd go five or six times; trying to keep the place going.

GRAYSON: Okay, because I mean if you had students, you had to keep a presence on a regular basis.

BEYNON: And in fact, the very first year, it proved that it wouldn't keep going because as soon as I left, the whole place began to fall apart. There was nobody controlling it, and I said they had to get somebody to run it while I wasn't there, and finally Purdue said, "Okay, who's the best young mass spectroscopist in the United States?" I said, "Graham Cooks" [R. Graham Cooks].

GRAYSON: Okay, now Graham was at Kansas at the time?

BEYNON: He was in Kansas, yes. So they asked Graham if he'd like to come and run it, and he did come without any tenure or any promise of tenure, and in fact, when he turned out to be so good, and obviously deserved tenure and would have got it wherever he was, the chicken became too big. The bird was too big for the nest. There was no room for both Graham and me. Meanwhile at ICI I had been promoted a few times and was now the manager of physical chemistry research, physics, being thought to be a branch of physical chemistry.

GRAYSON: Well, I guess that's okay in a chemical company.

BEYNON: So ICI said, "Now you've gotta decide. Do you want to come here and be a manager or do you wanna go to the USA and be an academic?" I said, "I wanna do both," so we got this four month, eight month arrangement and ICI brought out a new title in the research department, senior research associate, where the power and pay and everything else was the same as that for senior manager, and I became their first senior research associate. Turned out not to be a good idea, although they gave me a budget. They told me I could work on anything at all that I liked, but I didn't have the manpower, so when I wanted to do something, I had to go and try to beg help from one of the managers, and they weren't about to give their staff away to me, so I decided that I would retire from ICI.

GRAYSON: So you had money, but you didn't have the bodies to help you out.

BEYNON: Yes, so I made a plan that I would go to Purdue for eight months of the year. I would retire from ICI and take my pension immediately. I was 50 years old at the time, and I worked out that with the little money that I could save while I was at ICI Purdue, and my pension, I would be able to survive the four months I wasn't working at Purdue, and then by coincidence, I got offered another job.

GRAYSON: Okay, I just want to clarify this situation now. You could work at Purdue for eight months, but not for the whole year.

BEYNON: That's it. Purdue offered me a 12-month in the year job, but Yvonne and I didn't want to completely break with the UK.

GRAYSON: And so your pension from ICI supplemented those four months.

BEYNON: Yes.

GRAYSON: Now did that mean you moved to Purdue two-thirds of the time?

BEYNON: Well, there was never any need to finally decide on that, because I got offered another job. And this was a very prestigious position, an academic one. The Royal Society offered me a Royal Society Research Professorship. Now this is a post where the Royal Society pays the salary, the Royal Society gives you a research grant to enable you to keep a postdoc or two and a student or two, and the university, really all they have to do is provide space for you. The Royal Society paid for you, and so any university is always glad to accept you, and you could hold this position at any university of your choice.

GRAYSON: That's pretty nice.

BEYNON: You could hold the Chair in any University in any country that had been part of the old British Commonwealth, so I could've held it in Australia, Canada, South Africa, India, wherever. Well, I decided..."I'm 50 years old," I thought, "I'm getting on now. I'm retiring. I've taken my pension [commuted down] from ICI. There's no going back on that. I'd like to go back to my original university. I'd like to go back to Swansea," so I announced to the Royal Society that I would like to hold this chair in Swansea. Well, they were astonished. They were expecting me to say Oxford, Cambridge, but in the end, I had my way, and I started this Royal Society research unit in Swansea.

GRAYSON: So what did Swansea say to you? They had to be agreeable too.

BEYNON: Oh, they said, “Welcome.”

GRAYSON: They still had to provide you space, right?

BEYNON: Yes.

GRAYSON: Space in universities is usually a fairly precious commodity.

BEYNON: Anyway, they saw that they were getting an extra chair for nothing.

GRAYSON: Well, that’s what it looks like, so that was in 19?

BEYNON: ’74.

GRAYSON: Did you have any idea as to how this Royal Society arrangement came to pass? Do you know anything about the background of your choice?

BEYNON: No, nothing....came right out of the blue.

GRAYSON: So this must’ve been a fairly surprising event, right?

BEYNON: It really was. Astonishing!

GRAYSON: What, did it show up in a letter in the mail one day?

BEYNON: I think it was a telephone call.

GRAYSON: Okay, because that’s a fairly prestigious honor.

BEYNON: Yes, and I remember, I was very naïve, but when I started in Swansea, and there came the subject of my research grant, okay, the Royal Society rang me up and said, “Now this research grant of yours, where do we send it?” I said, “Well, I suppose you send it to the University, to the chief finance officer,” and they said, “Don’t you have a bank account?” [laughter] The Royal Society gave the money in this way, so that I could avoid frustration. Whatever I needed, there was no need to seek permission. I could just write a check.

GRAYSON: Oh.

BEYNON: So my research grant went into my bank account. The university had no say about it. They didn’t even know how much it was. They were very upset about this, and they tried every way they knew to get the money. On the other hand, there were advantages to me. When I called in at my local bank, as soon as I walked in through the door, whoever saw me come in would buzz the manager, and he’d come out to greet me, and shake my hand. There was another thing that the bank liked very much. The Royal Society had decided that with this research grant that would go to the research professors, if the money was put into some account that gathered a lot of interest and one researcher did this, and another researcher didn’t, then the one who was doing it had an unfair advantage. So they made it a condition on this grant that it should not be put into any account that got any interest, so the bank manager had my account sitting there for free, so he thought I was a great person.

GRAYSON: So how much was the grant?

BEYNON: Well, I forget the exact money now. Money is changing in value, but it was enough, as I say, to keep a couple of postdocs and graduate students, and then of course I was allowed to go out and look for research grants myself.

GRAYSON: Okay, but any subsequent research grants that you got while at the university, they would be involved in the finances?

BEYNON: Yes.

GRAYSON: But the Royal Society grant; you basically controlled that completely.

BEYNON: That's right. Any other grants, they were subject to whatever the terms and conditions of the granting body was, but I did get some grants. For example, from the Science Research Council, the equivalent of NIH in the UK. The Royal Society too had other grants on offer that I could apply for. They had one for example called the Paul Instrument Fund. This chap, Paul, had been a very, very rich man, and had left all his money to the Royal Society in a fund where the capital couldn't be touched, but the interest could be used for building new and improved instruments. Bigger and better didn't count. It had to be novel.

GRAYSON: That's interesting.

BEYNON: And the interest was sufficient that they could give mass spectrometers and so on away, plenty of money there, and the Paul Instrument Fund grant again, was to you personally, not to the university. I became very unpopular with the finance people in the university. They didn't like me. The university liked me; however, because my salary was paid.

GRAYSON: Now your position at Swansea was in the chemistry department, right?

BEYNON: Well, to begin with; so that I could share in the chemistry students and so on that were studying for their PhDs in chemistry. But again, just as being a research associate, you need people. I was now competing with the other professors for the research students, and they didn't like that, so I declared independence and told the university I didn't want to be part of the chemistry department. I would stay in the chemistry building, but I would be an independent department, so that's how I continued.

GRAYSON: But you still were going to be competing in the pool of graduate students, right?

BEYNON: Yes, but now in a much wider sphere and I wasn't restricted to chemistry students. I also took physicists and mathematicians

GRAYSON: So formally you were your own department?

BEYNON: Yes.

GRAYSON: And so if you got a graduate student, it was because they wanted to work for you.

BEYNON: That's right.

GRAYSON: It wasn't because they parsed these fellows out.

BEYNON: No.

GRAYSON: Okay and you were willing to go with that?

BEYNON: Yes. Chemistry and other departments would try to persuade the students not to come to me, but they came.

GRAYSON: It seems like a love/hate relationship.

BEYNON: Yes, so I was very unpopular in a lot of places, and very popular in others, like the bank. I had to more or less start again. I had no equipment. I had a lab, but it was empty.

GRAYSON: But you did get space from them. Now did you have to give them any money for this space?

BEYNON: No.

GRAYSON: But the fact that you were a Royal Society Research Professor lent some prestige to the university.

BEYNON: Sure. I was the only Royal Society Research Professor in the university.

GRAYSON: Okay, so then this was an arrangement that started in....

BEYNON: '74. I had to give up the Purdue appointment.

GRAYSON: You gave it over to Graham.

BEYNON: I'd given up already the ICI job.

GRAYSON: Okay, so you cut a few strings and burned a few bridges.

BEYNON: Yes. I was starting again now in 1974.

GRAYSON: And then you've got space. How much space did they give you?

BEYNON: About one half of a lab.

GRAYSON: One half of a lab? Does that include an office or anything like that?

BEYNON: And an office.

GRAYSON: Okay. Now you've got to build from there. So what's your first step?

BEYNON: First step, decide what instrument you needed and I decided on a new design of instrument, a reverse geometry.

GRAYSON: Is this the first time the reverse geometry instrument was used?

BEYNON: No. Reverse geometry had been used before, but not for any particular reason. It was just thought if you've got an electric and magnetic sector, it doesn't matter which way the beam goes through. It's all the same; but with IKES and MIKES, I had shown that you should really reverse what had become the conventional geometry of the AEI and VG instruments. Interestingly; in Germany, MAT had adopted the reverse geometry.

GRAYSON: Do you know why?

BEYNON: I really don't know why. They claimed immediately that they did it because they'd seen this possibility of MIKES. But that was completely untrue that they'd had any ideas of this

possibility. But anyway, I designed a new instrument of reverse geometry especially for doing MIKES work. Now this was made by VG in this country.

GRAYSON: So now when you say, "Designed," that means that you did the ion optics and radii and the whole nine yards.

BEYNON: I did the general concept of the ion optics and the actual detailed arithmetic was done by VG. But it was with my approval, and I got an order in for one, so they were building one that they knew they were going to sell.

GRAYSON: Okay.

BEYNON: And they called it the ZAB.

GRAYSON: ZAB.

BEYNON: Zero aberrations is the optimistic name [laughter].

GRAYSON: Optimistic name [laughter].

BEYNON: Or zero alpha, zero beta was the other. There were no first order alpha or beta aberrations. Zero alpha, zero beta or whatever -- it was called the ZAB and it proved a very successful instrument.

GRAYSON: Now that they deviated from the Nier-Johnson geometry in terms of sector angles and so on.

BEYNON: Well, in fact, the Nier-Johnson one was done in a vertical plane, because of the fact that the magnetic field of the Earth has its biggest component vertically. So it went through the magnet and didn't affect it, but we decided that this ZAB should be made in a horizontal, because benches can be added, and components can be added much more easily than using skyhooks to put things into midair. Anyway, the ZAB was a big success and as I say, sold throughout the world. It sold very well in the United States. It sold in Japan, even. As you know, you don't sell sector instruments in Japan very easily.

GRAYSON: No, but the angles of those sectors in the ZAB are unusual. They were a departure from anything that had been done before. Is that something that the VG or the Micromass, whoever they were then, came up with?

BEYNON: Yes, and they were working with me very closely. I was spending a lot of my time with them.

GRAYSON: Who was the man on the instrument side you were working with?

BEYNON: Brian Green.

GRAYSON: Brian Green, no surprise. He's another person I would really love to interview.

BEYNON: He's a remarkable person; a very modest, self effacing person, but very, very capable in many, many ways.

GRAYSON: And so between you, you worked out this ZAB geometry.

BEYNON: That's right. And then they sold another one before construction, and they sold it to DuPont, to Chuck McEwen.

GRAYSON: That's interesting.

BEYNON: And Chuck decided that he didn't really want to take the risk of having a reverse geometry machine. He was very happy with conventional geometry, so he said he would buy one, but only if they didn't reverse the geometry [laughter]. I said I would only buy one if they did reverse the geometry, so then it was a battle of wills as to which way are we going to do it, and finally VG decided to take the risk of a new concept and they built the reverse geometry.

GRAYSON: Okay, so did McEwen not buy this instrument?

BEYNON: No, he did buy one after all.

GRAYSON: He did buy the reverse geometry instrument, but he really wanted it primarily as a high resolving power, double-focusing instrument.

BEYNON: In which capacity it worked very well, giving $\sim 10^5$ resolving power at 10% valley. I believe a lot of people who bought the reverse geometry used it as a conventional, high resolving power, analytical instrument.

GRAYSON: Okay, so where did you get the money for the instrument? Did that come out of your Royal Society grant; because you had to purchase this equipment, right?

BEYNON: No, it came from a Science Research Council Grant.

GRAYSON: Okay, and you then put it on the floor at Swansea and started running it in 19?

BEYNON: I don't know. Well, 1974 was when I came to Swansea, and I think it was at least two years before I got it. It was probably about 1976.

GRAYSON: You had no teaching obligations?

BEYNON: Correct.

GRAYSON: Okay, so you had two years. Were you trying to recruit students during that period or are you doing anything there?

BEYNON: Yes, I am. I recruited three research students.

GRAYSON: Even though you didn't have an instrument?

BEYNON: That's right.

GRAYSON: And what did they do?

BEYNON: Well, they learned about mass spectrometry. They learned how to do various things, and we did start designing a little machine which we were going to build just for our own amusement.

GRAYSON: Did that ever come to pass?

BEYNON: No, I did start also building a machine to copy J.J. Thomson's original design, and I did accurately build that.

GRAYSON: This would be the parabola mass spec?

BEYNON: Yes, with a minimum current detectable 10^{-6} amps. Resolving power was about eight. A remarkable man, Thomson; head and shoulders above anybody else.

GRAYSON: Yes, so I know that there was a German by the name of Wien, Wilhelm Wien who was doing similar things at that time, but apparently he didn't come upon the idea that Thomson had with regard to the mass spec side of things.

BEYNON: Well, Thomson, with this little instrument that...nothing more than a toy, he discovered the electron and measured its mass-to-charge ratio. This showed for the first time the existence of sub-atomic species. He discovered stable isotopes. I've made a list of his achievements on a slide. I can't, I'm ashamed to say, remember them all just at this moment.

GRAYSON: So what was the purpose of this Thomson replica? Just to experience that whole historic event?

BEYNON: Yes.

GRAYSON: And where did that end up going?

BEYNON: Well, then I saw what an awful instrument it is, and how terrible the spectra are. You can't even tell they're parabolas properly, you know?

GRAYSON: Oh, really?

BEYNON: When you look at the illustrations of Thomson's spectra, the so-called parabolas are just sort of lines on a piece of paper, and in his first experiments, Thomson had no recording system at all, you see?

GRAYSON: Right, that was a bit of a challenge.

BEYNON: Yes, he had to look at this fluorescent screen detector in complete darkness, memorize what he saw, then turn the lights on and draw what he thought he'd seen, and then have another look to check how accurate his drawing was.

GRAYSON: So who were your first students then?

BEYNON: Well, the first students were....one girl, Janet Harris [chemist]. There were also Stuart Howells, a laser specialist, and Gareth Brenton [both physicists], and Chris Proctor [a chemist, now Research Director of British & American Tobacco].

GRAYSON: Brenton?

BEYNON: Yeah, but he came to me as someone half-way through a Ph.D. on positrons.

GRAYSON: So, well, let's see....before I go too far along, when did you write the book?

BEYNON: 1960.

GRAYSON: So that was still when you were at ICI.

BEYNON: Yes.

GRAYSON: Well, this is a fairly industrious project.

BEYNON: Yes.

GRAYSON: So did they support you in it?

BEYNON: Well, they gave me access to the library at any time, and I used to work seven days a week, five days a week for ICI and two days a week, Saturday and Sunday, writing the book. And I used to try to put in ten hours on Saturday, ten hours on Sunday. I did that for two years.

GRAYSON: My, my. You were really motivated to do this. Why were you so motivated?

BEYNON: I don't know. You just start it and you think, "I'll damn well do this," and it turned out that the time was such that you could more or less cover the whole of mass spectrometry up until then, and the number of mass spectroscopists in the world wasn't all that great. I reckon I knew 75% of all of them personally. I think in that other interview that you've got there, I talk about going to ASMS meetings and how big they were.

Can I change the subject very rapidly?

GRAYSON: Sure.

BEYNON: Since its 12:30, I think we should be going on down for lunch..

GRAYSON: Very good.

[END OF AUDIO, FILE 1.1]

GRAYSON: Okay, so I want to talk about the book business a little bit. You said that you published nine books?

BEYNON: I think it's nine.

GRAYSON: And so these were all topics in mass spectrometry?

BEYNON: One or two of them were reaction rate theory.

GRAYSON: This is esoteric chemistry, the reaction rate theory, right? It's not physics.

BEYNON: True, if one must classify things according to these rigid rules. Most of my own research was inter-disciplinary which often made it difficult to know when applying for a grant to know which Committee [Chemistry, Physics, etc.] to approach with the Application.

GRAYSON: So you must've picked up a lot more chemistry than you got from that course that ICI taught you.

BEYNON: Yes, I picked it up talking to chemists, and living in a chemistry environment.

GRAYSON: So you really end up being a chemist for all practical purposes [laughter]. Yes, and let's see, there was this other issue that I wanted to touch base on, which is escaping me right now, but I'll come to it. I notice that you had a long term interest in nomenclature where you were involved with IUPAC [International Union of Pure and Applied Chemists] establishing nomenclature for mass spec?

BEYNON: Yes. I was on an ASMS committee for a while and also on an IUPAC mass spectrometry committee.

GRAYSON: And did that result in anything?

BEYNON: No, nothing really noteworthy; though the IUPAC recommendations were published.

GRAYSON: It was just an attempt to try and standardize the literature.

BEYNON: Try to get people to be completely logical, but people never are.

GRAYSON: I meant to bring a book with me published by David Sparkman. He called it *Mass Spec Desk Reference*, and it's devoted to the appropriate way to refer to things in the mass spec literature, but I'd forgot to bring it with me. I think you would've enjoyed seeing it.

I think your interest in that area actually was fruitful in the long run, but this book really covers quite a bit of those details that you were dealing with back then,

When we left off before lunch, you had started at Swansea University, and you were establishing yourself with a group. What were the initial topics that you did research on at that time?

BEYNON: Well, we were starting with the ZAB. I had three students. One was Christopher Proctor. One was Stuart Howells.

GRAYSON: Then there was Gareth.

BEYNON: Gareth Brenton from memory, half-way through doing research for a Ph.D. in physics. He remained in my Unit right until I retired. He was my right-hand man and I depended greatly on him.

GRAYSON: You had three students and a postdoc, and what topics did you guys get started on?

BEYNON: Well, they were all on the ZAB, and MIKES topics, and what's become of them now is that Chris Proctor, I think he's the managing director of the British American Tobacco Company.

GRAYSON: Okay.

BEYNON: Stuart Howells is making lasers somewhere and Janet Harris is a housewife, she has a family.

GRAYSON: Yes, but Gareth is still involved.

BEYNON: Gareth is still in the university here [Swansea]. There was also F.M. Harris Frank Harris [another physicist]. He came to work with me. He had the idea of exciting ions by shining a laser on them. He did a lot of work on that, and it was very fruitful.

GRAYSON: So you created the ions and then did the laser exposure. How did you get the ions to sit still for the laser.

BEYNON: Well, we drilled holes in the flight tube so we can shine the laser light either along the ion beam or perpendicular to it.

GRAYSON: So you did a modification of the instrument in order to get the laser beam into the instrument.

BEYNON: Yeah, we had an argon ion laser, a very powerful one. I put in a request for funds. I was told by the Science Research Council that it wouldn't work, that the laser wasn't powerful enough. Well, I reckoned it was. They said a referee said it wasn't, so I said, "All right then. I'll resubmit asking for a laser ten times as powerful," and they said, "In that case, you'll be able to excite H_2^+ , but nothing else." They were completely wrong, and our original calculations were right. We had a laser that was plenty, plenty strong enough to excite the ions.

GRAYSON: So somebody was doing a miscalculation on their end, and what was the outcome of doing this laser excitation of the ions?

BEYNON: Well, various things. One thing I can remember was looking at toluene as a spectrum that starts off with a 92^+ molecular ion, and a 91^+ ion, and to see how the ratio of 92 to 91 is altered as a function of excitation of the beam.

GRAYSON: So the 92 would be the molecular ion, and the 91 would be the tropylium ion, so as you changed the laser energy, the relevant amounts of these ions shifted?

BEYNON: Then you could say from the metastables how these two fragmented, and how the energy in them affected the fragmentation. I think we used an argon ion laser with 18 watts output, the final one that we got. It was strong enough that if you walked past, you had to be careful, because your clothes would start burning. The ions would absorb extra energy equal to the quantum energy emitted by the laser, so as the laser was scanned, you could study fragmentation patterns as a function of the ion internal energy.

GRAYSON: Yeah, well, of course if it's focused down to a narrow beam, then 18 watts can be dangerous. So how long did you stay at Swansea?

BEYNON: For 12 years. I retired in '86 for the second time.

GRAYSON: By that time you had a fairly large group, I would imagine.

BEYNON: Well, not all that large, but I think in one of these publications, it shows it. I used to have people from all over the world. [Looking at picture]

GRAYSON: Well, that's a pretty good sized group. Did you have more than one postdoc?

BEYNON: Well, I had Gareth Brenton and [Looking at picture.] Frank Harris They both rose up to professorial status, ultimately. I can't really see very well. Oh, that's Frank Harris and there's an electronics man who was seconded to me from British Petroleum. There's my secretary and me. The next man's from Israel; the next from Australia; the next was English. Some other of my students, there's Chris Proctor. There's Emad Mukhtar the next man was from Venezuela. One was from Iowa; one was from Iran. I used to have an Iranian and an Iraqi together while the Iran/Iraq war was on. They were the best of friends.

GRAYSON: Yes; crazy. And who are these gentlemen and people up here?

BEYNON: She was from Purdue, Joyce Wiebers. We managed to attract academics from chemistry, physics, electrical engineering and mathematics to spend sabbaticals with us.

GRAYSON: So it was a fairly decent sized operation. You were able to fund all these people out of your Royal Society money?

BEYNON: And I had a grant from the Science Research Council.

GRAYSON: Okay and that also provided funds for purchasing equipment.

BEYNON: Well, except that I always found that it was far, far easier to get funds for doing the fairly mundane things. If you proposed anything that was a little bit out of the ordinary, then they were very conservative, and opposed it very strongly.

GRAYSON: So you learned to propose things that weren't too far out in left field?

BEYNON: Yes.

GRAYSON: But could you then do some left field stuff anyway?

BEYNON: Well, once you've got the equipment ...I found that when I was at Purdue, I used to have to spend a lot more time writing reports for NSF and so on, on the progress of a research project and proving that you weren't deviating from what you said you were going to do. In this country, they're more free and easy. Once you've got the money, what they wanted was to see some results and not to have you spending too much time writing reports about it, but getting on with the work. In any case, your ideas [expand] as the research progresses.

GRAYSON: So you had a chance to experience the grant bureaucracy on both sides of the Atlantic.

BEYNON: The very last thing, I should say incidentally is about publications....you don't generally get much chance to publish a lot when you're in industry, and I spent most of my life in industry except for my year at Purdue plus some extra bits, and 12 years at Swansea, so my academic publishing time was really 13 or 14 years.

GRAYSON: But you did publish a few things.

BEYNON: I published, I think, 350 or so.

GRAYSON: But, you did published some while at ICI.

BEYNON: I published some, yes. I published 30 or 40, I think.

GRAYSON: The motivation for these other books was just because you felt that a book was needed?

BEYNON: Well, I did a book on accurate masses for choosing formulae, a book on metastables, 'cause I felt that they needed writing up. Half the book was written [with G. R. Lester, a mathematician] before I went to Purdue. It was finished at Purdue with R. G. Cooks

and R. M. Caprioli who was my post-doc. The last thing that was done at Swansea was to make a novel 'energy' spectrometer. This was a spectrometer where the mass resolution was zero; but it had very, very high energy resolution. And the idea was that you'd take an ion selected by a small magnetic sector preceding the energy spectrometer, collide it with something, say, helium so that you'd have an inelastic collision, but the helium wouldn't get excited, because it's first excited state is 20 odd volts above the ground state; but the ion itself would become excited. So with high energy resolution you could pick out the individual excitation states formed. You could pick out the electronic state and the vibrational levels on it, so that's why we called it our energy spectrometer.

GRAYSON: And your research unit designed and built that instrument.

BEYNON: Yes, it was built at Swansea. It consisted of a small magnet for getting mass separation to choose your ion, and then two very large electric sectors with a collision region in between, and when I say very large, I think they were about 30 inch radius. They were huge.

GRAYSON: Yeah. What did the machine shop say when you came in with that?

BEYNON: Well, the sectors had to be made in a shipyard somewhere. I didn't have much chance to work on that. That was about a year before I retired. Gareth will tell you about that. He did most of the work on it.

GRAYSON: Okay. Did you have any patents?

BEYNON: No.

GRAYSON: Okay, so you didn't have to deal with the patent office or any of that kind of thing. I guess ICI patented things, but they didn't figure anything you were doing was patentable.

BEYNON: Some of the things were..

GRAYSON: And so when you left ICI, they still had a fairly benign attitude towards basic research?

BEYNON: Indeed, yes.

GRAYSON: Okay. I was just wondering if that attitude still exists today. It seems like some of these companies have moved over to the money-is-all-that-counts position

BEYNON: It was a long time ago and I don't know what changed attitudes now prevail.

GRAYSON: This whole business with accurate mass, when did you get the inspiration or how did that come about, knowing that the accurate mass would provide you the elemental composition?

BEYNON: Well, as I was saying, we got the resolution of our first instrument up to about 600 or 700, we could separate CH_4^+ and O^+ , and that showed us that if we measured the mass, we could get the formula and then it's a question of how accurately could we measure the accelerating voltage as we scanned from mass to mass. We could do it on the single focusing machine to about 20 parts per million, on the double focusing machine to about one part per million.

GRAYSON: And that whole idea of the elemental composition from accurate mass derived from your work? It wasn't something that anyone else did?

BEYNON: I don't think so. I think we started it.

GRAYSON: Okay, as an application in organic chemistry?

BEYNON: Yes.

GRAYSON: I interviewed Al Nier a long time ago. He said that when he was giving a paper or talking about this issue at one time or another, you had the intuition for the application of accurate mass in organic chemistry. Nier was doing it because he was interested in the physics side of things, because they needed to know the exact mass.

BEYNON: Yeah, Al was interested in accurate mass determinations of the isotopes of all the elements, following on the work of Aston in the 1920's. I remember going to Nier's lab and

suggesting organic doublets that he could use for getting the accurate masses of ^{12}C , relative to ^{16}O which was, at that time, the mass standard. I can't remember which ones they were now.

GRAYSON: So when did you visit his lab, do you recall?

BEYNON: I can't remember exactly when.

GRAYSON: But you did spend some time at Minnesota?

BEYNON: Well, I visited only a day or two on my first visit, but one of the things you can see in my CV is that I was a 3M Boomer Memorial Fellow in the 1950's and I spent, I think it was three months in Minneapolis.

GRAYSON: At 3M?

BEYNON: No, at the university. And I got to know Al Nier pretty well.

GRAYSON: So you spent some time with him, but essentially, you spent a couple days visiting with him in his lab; and seen what all he was doing.

BEYNON: Alfred Otto Carl was his name.

GRAYSON: So do you recall the time?

BEYNON: It was earlier than 1955.

GRAYSON: So that was a pretty useful experience, to have visited his lab. What was your attitude of his contribution to mass spec?

BEYNON: Well, he's got the gift of being able to do things right the first time. His work and mine never really overlapped, because he was on atomic mass measurement and did no organic chemically oriented work.

GRAYSON: Sure.

BEYNON: I think in my book in 1960, there was some unease about the oxygen standard of mass, that oxygen 16 should be exactly 16.0000, and they were going to change. IUPAC and everybody were discussing it, and in my book, I state what I believe should be the standard of mass, that fluorine should be 19.0000. Because fluorine is mono-isotopic and combines with more elements than any other suggested standard. But it wasn't accepted. They chose carbon, and I think carbon is not a very good choice because it combines so readily with hydrogen which has a very big mass excess so that the doublets you're measuring are very wide doublets. Then mass discrimination and so on comes in, so you don't get a very accurate measure. Anyway, I said in my book that I supported fluorine as the standard, but carbon was chosen, and ^{12}C is still the standard of mass.

GRAYSON: Yes, but either one is better than oxygen, do you think? Oxygen had some issues with regard to isotopes. Of course, carbon has an isotope too, but oxygen I think had some other difficulties.

BEYNON: It wasn't so flexible organic chemically I don't think.

GRAYSON: Now there were a number of developments in the field that I didn't see reflected in your literature references. For instance, did you do much FAB [Fast Atom Bombardment] work?

BEYNON: No.

GRAYSON: So FAB was something that really had no interest for you?

BEYNON: Well, it was developed more or less on ICI's doorstep by Mickey Barber. The sources I worked with were electron bombardment and a photo source, a gas discharge, photo ionization. I think there are a lot of advantages to these and, in any case, one should not try to work on everything possible.

GRAYSON: When FAB hit the world and took off, it wasn't something that particularly interested you; in applying it to the problems you were working with?

BEYNON: No.

GRAYSON: Okay, and neither MALDI? That was a later thing.

BEYNON: The world had gotten beyond me then, and was going up to high molecular weights. I think I would've developed some of the things there quite differently than the ones that were developed, just because of my particular interests.

GRAYSON: Can you expand on that a little bit?

BEYNON: Well, you've got isotopes, and isotopes are very, very important as an analytical way of distinguishing things. Now when you get up to masses of thousands and tens of thousands, then the ion with only carbon 12 in it, becomes an insignificant one. But the isotope pattern is still extremely important in telling what you've got there, and nobody now seems to bother with the isotopic pattern. They're all concerned with the speed with which they can get through the spectrum, and the group of molecular ions is just a spike. Well, I think I would do a lot more analysis of the detail of some of these peaks.

GRAYSON: I see, well the isotopic pattern of the molecular ion does get very complex quite quickly.

BEYNON: You've got mathematical aids to help you. The patterns. I first worked on them when I wrote my book in 1960 specifically working on the patterns of the halogens; things with two, three, four, five, and so on, chlorines or bromines. That became very popular as a way of sorting out the halogen content of compounds. Klaus Biemann repeated my patterns in his book, and I think found them useful.

GRAYSON: Yeah, I think they show up in McLafferty's *Interpretation of Mass Spectra* book too.

BEYNON: Yes.

GRAYSON: So you're saying that you were the originator for those.

BEYNON: Well, anyway at the time, with publication and so on. It's all a matter of record. I don't want to squabble about who was the first. As I say, if you were around at the time you're bound to be involved. You're bound to have an influence.

GRAYSON: That's true.

BEYNON: 1960, as I say, it was a good time to write a book on mass spectrometry, because it could cover all of mass spectrometry as it had been developed up until then. I was able to put in references and Elsevier, who published it, insisted on doing this in a very particular way. All the authors I think were listed alphabetically in numerical order. If you looked up, for example, Abramovitch, say, reference number 76, it would tell you in addition to where it was published, on which page[s] of the book that reference was mentioned. I don't think that's done very often, but it's very useful if you wanted to know something about, say, Nier's work, you see? You just look up Nier in the alphabetical list of references and you could take down all the pages on which various reference to Nier's work were made.

GRAYSON: The publisher wanted it that way.

BEYNON: It also had a mass table in it. It was the first time that the mass spectrum of heptacosatributyl amine, the commonly used mass reference, was published; its mass spectrum was given. And the accurate masses of formulae up to mass 500, I think were given as an appendix. All of these were worked out on a hand calculator, turning mechanically this hand calculator, and reading off, and writing down the results.

GRAYSON: Now did you do that?

BEYNON: Yes, I did.

GRAYSON: Oh, you did all that calculation? You didn't hire someone to do that calculation?

BEYNON: No. I did it at home and Yvonne checked it with me.

GRAYSON: Oh my, that was quite an effort.

BEYNON: Yeah, it was a hell of a job.

GRAYSON: Now you were telling me at lunch that you had some help with the figures in for the book? What was that about?

BEYNON: Well, Yvonne did all the diagrams for me.

GRAYSON: So that was a significant contribution for which she wouldn't get paid, right?

BEYNON: Yeah. There is a copy of the book here somewhere. But I don't know where it would be. It's here. I saw it a few weeks ago.

GRAYSON: Well, it's certainly a very famous book.

BEYNON: ASMS had it reprinted.

GRAYSON: Do you have any idea how many of them are actually sold?

BEYNON: Well, there was the first printing. They printed about two and a half thousand, I think. They used to calculate that each library would buy one, and that there were about 600 libraries that buy these. Anyway, the first printing sold out, so they said they wanted a revised second edition. I said there was no way I was going to go through that sort of thing again [laughter]. They could reprint it, but I wasn't going to revise it, so they reprinted it, and the reprint sold out.

GRAYSON: Was that the same run, 2,500?

BEYNON: I don't know how many it was, and then they reprinted it again, and that sold out, and I think they sold somewhere between 7,000 and 10,000 altogether.

GRAYSON: From which you did derive income, right?

BEYNON: I derived an income, which considering the time I had spent on it, repaid me approximately \$0.10 an hour.

GRAYSON: You did the calculation.

BEYNON: Yeah, I did [laughter].

GRAYSON: But think of all the notoriety you got. Your name became synonymous with mass spectrometry after all the various printings of that book.

BEYNON: The only other money that I made was I had a deal with VG. I was a consultant of theirs and one day I was talking to Robert Craig, their managing director. We were discussing my remuneration as a consultant. He said “I don’t know what to suggest. You keep bringing us ideas for expensive Rolls Royces [e.g., the ZAB] and what we really want is ideas for reliable little Toyotas. Pay me a royalty on sales,” so for a few years, I got a royalty on every ZAB that was sold. That was pretty lucrative.

GRAYSON: Do you have any idea how many they sold?

BEYNON: For quite a while, 30% of their total production was ZAB’s.

GRAYSON: So basically, ideas that you originated with instrument companies started the MS9 and the ZAB, both of which were very successful instruments for the companies that built them. You put the ideas into them and probably didn’t get as much out of them from a financial perspective as you could have.

BEYNON: For a long, long time, most mass spectroscopists were working on quantitative analysis of mixtures, and very few were working on qualitative identification. McLafferty was one of the very few. I visited all these people in the States. I visited McLafferty when he was with Dow at Midland, in Michigan.

GRAYSON: Do you recall when that was?

BEYNON: That was when I went to my first ASTM Committee E-14 that was in San Francisco in the early '50s, I think.⁵

GRAYSON: So then after, you just stopped at Fred's lab.

BEYNON: And ICI used to allow me to go to the ASTM E-14 meetings. Not every year, but every other year.

GRAYSON: And ICI, they were okay with you visiting a guy that worked for Dow?

BEYNON: Yes, sure.

GRAYSON: Okay and that was when he was in Midland.

BEYNON: Yes.

GRAYSON: Because he did move to the east coast eventually with a research group.

BEYNON: He went to Framingham, Massachusetts.

GRAYSON: Yes. Okay, so what do you consider to be your most significant publications, other than the books?

BEYNON: Well, I am fond of the publication about He_2^{++} . I don't know what journal that was in.⁶

⁵ The first mass spec meeting in San Francisco was in 1955. McLafferty's oral history makes note of Beynon's visit to Dow Midland in 1955. See Fred W. McLafferty, interview by Michael A. Grayson at Cornell University, Ithaca, New York, 22 and 23 January 2007 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0352).

⁶ M. Guilhaus, A. G. Brenton, J. H. Beynon, M. Rabrenovic, and Paul R. Schleyer, "First observation of diatomic helium(2+): charge stripping of diatomic helium(1+) using a double-focusing mass spectrometer," *J. Phys. B* 17 (1984): L605-L10; Michael Guilhaus, A. Gareth Brenton, John H. Beynon, Mila Rabrenovic, and Paul von Rague Schleyer, "Dihelium(2+), the experimental detection of a remarkable molecule," *J. Chem. Soc., Chem. Commun.* (1985): 210-11.

GRAYSON: Okay, I think I can probably find that.

BEYNON: In *Chemical Communications* or something like that.

GRAYSON: So this is a fairly esoteric exercise. What prompted you to, look at He_2^{++} ? Just to prove that it could be done?

BEYNON: It had been predicted theoretically to be stable. It has only two electrons, similar to H_2^+ and has the shortest known covalent bond between the two helium nuclei. Something a little bit closer to physics than chemistry.

GRAYSON: Yeah, here it is. He_2^{++} , *J. Chem. Soc., Chemical Communications*, '85, by Guilhaus, Brenton, Beynon, Rabrenovic, and von Rague Schleyer. Okay, I'll see if I can get a copy of that. This is probably the most esoteric piece of work that you've probably done, or most anybody in mass spectrometry has ever done, I would think.

BEYNON: The ion was also predicted to have a stable excited state, from memory, 50-60eV above the ground state. We also succeeded in making this excited state. Interestingly, to do this work we had to add an extra electric sector and an extra collision region in the ZAB; i.e., we needed a three sector machine.

There was another general topic, the structure of doubly charged ions, like the structure of doubly charged benzene.

GRAYSON: Yes.

BEYNON: Which I believe is a linear species, with a formula, $\text{CH}_3\text{C}^+\text{=C=C}^+\text{-CH}_3$. Or, $\text{H}_3\text{C-C}^+=\text{C}=\text{C}^+-\text{CH}_3$.

GRAYSON: So a tri-olefin opening up the ring.

BEYNON: Yes.

GRAYSON: And what evidence did you have to support that?

BEYNON: When two charges separate, coulombic repulsion energy is released and when $C_6H_6^{++}$ fragments, the energy release suggests that the charges in the ion are about four angstroms apart, whereas the diameter of the benzene ring is only about two angstroms, so the ring's been opened.

GRAYSON: Okay.

BEYNON: Also, that the only fragments found contain 3 hydrogens. It always loses three hydrogens whenever it breaks. CH_3 , C_2H_3 , or C_3H_3 .

GRAYSON: That's interesting.

BEYNON: Which I think is evidence that there's CH_3 on the end. So I did a lot of doubly-charged ion fragmentation studies on other ions after that.

GRAYSON: From a basic gas phase ion chemistry kind of a viewpoint.

BEYNON: Yes.

GRAYSON: I think that's one of Gross' special interests, gas phase ion chemistry. All right, so you had some topics that you had highlighted on my list that you wanted to discuss in more detail?

BEYNON: I think we've covered them.

GRAYSON: Okay, I did write down some notes of things I wanted to follow up on. Yes, there's this big discussion that is in the Sparkman's *Desk Reference* about accurate mass, exact mass, and precise mass, and the subtlety of trying to say what you mean when you make a mass measurement. When you measure a mass with sufficient accuracy or number of significant figures that you can use it to do an elemental composition, what do you think is the appropriate nomenclature for that type of a measurement? What do you call it? Do you call it an accurate mass measurement, a precise mass measurement?

BEYNON: I call it accurate mass measurement.

GRAYSON: Okay. I think that's the sense which the *Desk Reference* tries to convey, but people are not always as careful with these things as they should be in publications and I think it does lead to confusion.

You elected not to stay at Purdue because the Professorship showed up here with the Royal Society?

BEYNON: Yes.

GRAYSON: If that hadn't shown up, would you have gone to Purdue?

BEYNON: I might be in Purdue, I think. But I couldn't accept the Royal Society Chair and stay at Purdue. The Royal Society wouldn't allow me to have any other post whilst holding that chair. It was all or nothing.

GRAYSON: Well, it was a pretty nice offer. [Phone ringing.]

[END OF AUDIO, FILE 1.2]

[...] ⁷

BEYNON: There's a mass spectrometric picture.

GRAYSON: Yeah, what's that about?

BEYNON: There was a mass spec conference in Switzerland, and at the end of the conference, there was the conference dinner. We all came down from the Rigi mountaintop where the conference had been held to, I think it was Lac Lemman. We're going on a boat over to the Castle of Chillon where Byron was a prisoner and I was the speaker at the dinner. And on the boat I'm roped in by a French jazz band to play the washboard with them [laughter].

⁷ At the beginning of this recording, there was a brief off-topic conversation regarding the use of personal computers by John Beynon and his interest in photography. The transcript picks up with a discussion of a picture of Beynon playing a musical instrument with a band.

GRAYSON: So those are thimbles on your fingers?

BEYNON: Yes.

GRAYSON: So this represents the intake of a certain amount of alcoholic beverage prior to this?

BEYNON: Oh, I think probably it does.

GRAYSON: Can I borrow this to make a copy of it and bring it back?

BEYNON: I haven't got another copy.

GRAYSON: If I can borrow it, what I'll do is I'll have copies made and bring it back and just leave it with you tomorrow.

BEYNON: Sure.

GRAYSON: Because that would be a nice piece to put into the oral history.



BEYNON: There are some nice things that you might borrow right here.

GRAYSON: I have downloaded these. I have them as PDF files.

BEYNON: These are some of my photographic things.

GRAYSON: I don't know if I have these.

BEYNON: I worked out how to make repeating circular patterns with any chosen number of repeat units in the circle and with an exact fit.

GRAYSON: I don't think I've seen them. This is like a kaleidoscope.

BEYNON: Yes, PhotoShop[®] used in a way that nobody else has described, I think.

GRAYSON: Very neat. That's in RCM.

BEYNON: That's in RCM, and it is volume 18, number one. That's January 2004.

GRAYSON: Okay, I can download PDF files of these. Now I'd like go over interactions with the Society. You went to the ASTM E-14 meetings fairly early on.

BEYNON: Yes. The first one, San Francisco....I think it was meeting number three.⁸

GRAYSON: And so you got wind of these meetings from just knowing people who had gone?

BEYNON: I don't know how I did, but it was jolly interesting, because there were no parallel sessions. Everybody went to every meeting, so you met people. I met people that I would not otherwise have probably gone to their lectures, like Mark Ingram in Chicago, who was very big on photoionization. Fred McLafferty was there. I think from memory there were about 20 odd papers, and maybe 30 or 40 people.

GRAYSON: Well, it turns out one of the projects that the present President of ASMS wants to have done is to have all the papers and abstracts scanned electronically so that they can be accessed over the internet. I think the last seven or eight years are already published as a CD.

BEYNON: Yes.

GRAYSON: So you don't get a hardcopy bound volume that weighs ten pounds anymore, but she's going to have all the previous hardcopy paper put into electronic form.

BEYNON: I think if you get an abstract of that meeting, there is a paper that discusses the structure of an organic compound, the formulae and, detailed fragmentation pattern. It's pretty early. Somewhere or other in one of these things, Fred McLafferty's written an article in which he says he remembers that paper. It was his first inkling that that sort of work could be done at that level.

GRAYSON: So who presented this structured paper?

⁸ The San Francisco meeting was the third meeting in 1955.

BEYNON: I did. I don't know where the reprint is. It's in one of these former issues.

GRAYSON: I have the program booklets from year one, so I'll be able to research that.

BEYNON: It was San Francisco. Now from memory, I think it was 1955, and it was ASTM E-14 meeting number three, I think.

GRAYSON: I will certainly research that when I get back home.

BEYNON: [Continuing to peruse papers Beynon comes across a publication list.] There's my publications list. It got a bit frantic at the end. There are nearly 400 papers here, but I only ever spent 12 years, in academia, so I was publishing about 30 a year.

GRAYSON: My, my.

BEYNON: A lot of effort was publishing.

GRAYSON: Yes, and actually the first couple of years, you weren't really able to do too much because you didn't have any equipment.

BEYNON: True.

GRAYSON: So it's compressed even more.

BEYNON: When papers were coming out, we used to have a paper about every ten days.

GRAYSON: So these went mostly to chemistry journals. When did the *Mass Spec* journal per se start up? There's *MS and Ion Processes*. Was it then?

BEYNON: *Journal of Mass Spectrometry*, [*Organic Mass Spectrometry*, as it was called at that time] and *The International Journal* started up simultaneously.

GRAYSON: Oh, okay.

BEYNON: And I don't know what year that was, but it was the year that the triennial international meeting was held in Berlin. I had the honor of being invited to write the first paper in *The International Journal*, so I have one reference, volume one, page one.

GRAYSON: I'm going to explore the developments in mass spec both in the U.K. and the U.S., and it seems like at different times, the development of instrumentation seemed to be dominate in the U.S., and then at other times, dominate in the U.K.

BEYNON: The U.K. used to be very good in mass spectrometry. It dwindled as the years have gone on. In the early days, they were quite big.

GRAYSON: So you feel that today, the influence of the U.K. is considered less than before.

BEYNON: Indeed. .

GRAYSON: And do you have any idea how that came about?

BEYNON: I don't know. I've marked a couple of things here. Now reference number seven, derivative mass spectrometry.⁹ [Phone ringing.]

[END OF AUDIO, FILE 1.3]

GRAYSON: Okay, reference seven, derivative mass spectrometry, 1958.

BEYNON: To scan a mass spectrum you alter the accelerating voltage okay? You scan the accelerating voltage. Now if on the accelerating voltage, you put a small sine wave so the accelerating voltage becomes $V + v \cdot \sin(\omega t)$, where v is a small voltage much, much less than V . Then you tune the detector to only accept the signal at an angular frequency, ω , you get the first

⁹ J. H. Beynon, S. Clough, and A. E. Williams, "Derivative mass spectrometry," *Journal of Scientific Instruments* (1958): 164.

derivative of the mass peaks and it turns out that the smaller you make the amplitude, v , the better the resolution you get, and that reducing v is equivalent to closing the exit slit. [Phone ringing.]

[END OF AUDIO, FILE 1.4]

GRAYSON: So by adding this frequency signal on the accelerating voltage you are able to synthesize greater resolving power?

BEYNON: Well, I can't describe it, but the mathematics is simple and is given in the paper.

GRAYSON: Okay, I can get a copy of that.

BEYNON: One advantage is that exactly at the top of a peak, the derivative signal goes through zero so from a positive to a negative value and the zero point can be detected very accurately. But if instead of tuning your detector to the frequency ω , you tune it to the frequency 2ω , you get the second derivative of the spectrum, and reducing ω is now equivalent to closing the inlet slit as well as the exit slit.

GRAYSON: Oh, I'll have to read that.

BEYNON: It works. It's sort of complicated. If you've got a peak like that, and you take it's first derivative, it starts at zero, then goes up to a maximum slope about here somewhere, and then it goes down to a zero slope at the top, and then it goes the other way. That's the first derivative.

Now that peak is narrower than the conventional peak, and you can alter the width of this peak by altering the amplitude and the AC that you put on the accelerating voltage, and its equivalent to closing the slit. Well, I can't draw the second derivative. It's even more complicated, but it's like closing the inlet slit as well as the exit slit, so you have an electronic control over resolving power.

GRAYSON: And that was published in '58.

BEYNON: I don't think that anyone followed that up as far as I know.

GRAYSON: Yeah. I actually had circled that in my list of references, and I've got a copy similar to yours and it was one of the ones I had circled.

BEYNON: Well, I think it was one of the more different sort of things that I did.

GRAYSON: Well, I imagine there's probably a lot of stuff in the literature that's got some neat ideas that never get followed up on, but I'll get a copy of the paper.

BEYNON: The other thing I did was set up a zone melt for zone refining organic chemicals.

GRAYSON: Is that published?

BEYNON: Yeah, sure it's published. It's in the list there somewhere.¹⁰ It's amazing, you buy these chemicals, which are sold as analytical reagent quality and especially pure. You zone refine them, and you get so much filth out of them.

GRAYSON: So this would be like you'd zone refine a piece of silicon?

BEYNON: Like you do if using silicon, except that you'd be doing benzoic acid or something like that. You can grow great single crystals of chemicals. What's more, you concentrate the impurities, so if it was your aim to find the impurities and identify them, then this was a good way.

GRAYSON: So did you actually do this?

BEYNON: Yes, indeed we did. We did it on quite a large scale, a kilogram scale. And we used to take samples of all competitors' products and we used to heat them in the sample inlet system and heat them violently and see what we could drive off. See if we can drive any intermediates off to guess how these things had been made. We were doing this one day, and the sample exploded in the mass spectrometer, and it blew our eardrums out, knackered the sample handling system.

¹⁰ J. H. Beynon and R. A. Saunders, "Purification of organic materials by zone refining," *British Journal of Applied Physics* (1960): 128.

GRAYSON: I guess [laughter]. Is this what you did on that early instrument that you described?

BEYNON: Yeah.

GRAYSON: So it was like an instrument of abuse.

BEYNON: Yeah.

GRAYSON: Okay, but there were some other issues that you were gonna mention to me that you had highlighted on that list of things that we were gonna talk about. You say we covered most of those?

BEYNON: Pretty well covered.

GRAYSON: Who was involved in starting up the British Mass Spec Society?

BEYNON: Well, that's an interesting thing. I believe that I was the first chairman of that.

GRAYSON: Okay.

BEYNON: And that John Waldron whom I mentioned somewhere, the director of Metro Vic or AEI, he was the second, but the records of the British Mass Spectrometry Society show that Allan Maccoll was the first chairman, but five years after I thought I was the first chairman. They recorded his chairmanship as the first meeting. He was at University College, London.

GRAYSON: So you suppose that there was a misunderstanding or just a mistake?

BEYNON: Some misunderstanding or something's funny somewhere. My recollection's wrong, or their recollection's wrong, or I don't know.

GRAYSON: So this would've been started in what year in the early '60s?

BEYNON: I guess. I think so. You've got my CV there?

GRAYSON: Yeah.

BEYNON: Maybe I can tell you from that. Well, I received in 1988 a Gold Medal of the British Mass Spectrometry Society, and I believe that was their silver jubilee meeting. I have a paper in this Silver Jubilee issue of *Organic Mass Spectrometry* [OMS] and in it, I talk about the start of the British Mass Spectrometry Society. My recollections would have been much sharper at that time. In the paper, I thank Allan Maccoll for his help in establishing the facts so I guess he must have agreed at that time with the story in that issue of *OMS*.

GRAYSON: Okay.

BEYNON: So 1953 would be when they reckoned the BMSS started.

GRAYSON: So that would've been at the same time that the ASTM E-14 meetings were getting organized.

BEYNON: Yeah, I reckon it was a bit earlier than that.

GRAYSON: Okay.

BEYNON: Anyway, I don't have any records or anything to back up my recollection except that *OMS* issue.¹¹ It's not important anyway.

GRAYSON: And so this is an annual meeting, the BMSS, and they are still active today?

BEYNON: They certainly are, yes. They have a very good magazine called *Mass Matters*, which is very informative, newsy, and covers in detail the ASMS meeting and things of that kind. You want to perhaps ask Gareth, who I think has something to do with the committee. Ask him if he can give you a copy of it. I get them, but I throw them away after I've read them.

¹¹ J. H. Beynon, "Mass spectrometry in retrospect: 25 years and more," *Org. Mass Spectrom.* 26 (1991): 353-8.

GRAYSON: Sure, and what about the international meeting? Were you involved in the tri-annual conference?

BEYNON: Overwhelmingly, the International Mass Spectrometry Conference was British. Yes, I was very much involved in that, and one of them was held in Swansea in 1985.

GRAYSON: Oh, okay, so how did that originate? There are people going to the ASMS meetings, there are people going to the BMSS meetings.

BEYNON: These were before either of those. The British Hydrocarbon Group, the Oil Companies Group, started organizing mass spec meetings, and there was some very early ones. They're mentioned in some of my publications, and the way these were organized was the chairman was always somebody from the British Hydrocarbon Group, maybe somebody from Royal Dutch Shell in Holland, the U.K., or somebody from France. I don't think there was anyone from Germany. Sometimes there was somebody from the United States, and it was the sort of the attitude that the rest of the world can go hang.

GRAYSON: Okay, but even though this was a British Hydrocarbon Group, it had representation from France and occasionally the United States?

BEYNON: Overwhelmingly it was a British organized thing, and this went on for many, many years, and then when this meeting was held in Swansea in 1988, I thought, "Isn't it about time that we have a proper international committee?" So I wrote 'round to every country that had a mass spectrometry society and said, "Would you like to nominate one member to represent your country on an international committee?" and everybody wrote back and said yes, except the United States, who wrote back and said, "But we're bigger than anybody else. We ought to have three representatives."

So I wrote back and said, "No, we're trying to sort something out, and we're starting off one from each country," and they said, "Well, in that case, we won't take part," which would've been a great shame, but what do you do, so I said, "Well, all right then, don't take part." Well, then they decided that perhaps after all they would take part, but I forget who finally was their representative. It could've been Fred McLafferty or it could've been Catherine Fenselau. And in the event, at the meeting, we had one representative for the United States and one extra observer from the United States who couldn't actually take part in the meeting, but who observed, and that was Graham Cooks. After 1985, from then on there's been an international committee, so everything's gone all right.

GRAYSON: That happened in 1985 at Swansea.

BEYNON: We had a lot of trouble. I remember organizing some of the social events; the local council here decided they'd like to give a big dinner for the international conference, and they'd like to invite all the representatives of the international committee. From memory, 42 countries were represented. Could I send them a list of all the members? So I did, and on the list was the South African member, so they said they were sorry, they could not accept a member from South Africa. I said, well, we were a nonpolitical organization, and if they couldn't accept our member from South Africa, then I was afraid we couldn't come to their dinner. They were very put out about that, because they really wanted us to come, so finally we agreed that the South African member could go to the dinner, but that he wouldn't wear his badge.

GRAYSON: So it was a political issue.

BEYNON: Yes.

GRAYSON: And this would've been at the time because there was tension....

BEYNON: Anti-apartheid.

GRAYSON: So the dinner did go forward?

BEYNON: Sure. The South African representative was there. I mean he wasn't political in any way.

GRAYSON: Didn't somebody have to ask him not to wear his badge?

BEYNON: Yes, I had to explain the situation to him and tell him that I hoped that he would go and enjoy it with everybody else, but that he couldn't wear his badge. Sam van Rensburg he was.

GRAYSON: And this would've been in 19?

BEYNON: '85. I think he worked for the South African Atomic Energy Commission

GRAYSON: Okay and then you decided to do a tri-annual conference instead of every year?

BEYNON: Well, the International Meetings were always tri-annual, because we felt this gave time between meetings for the subject to progress. You could then have papers that survey advances. Then the British Mass Spectrometry Society decided to have an annual meeting, but not to hold the meeting in the year when there was an international meeting, so there'd be a meeting one year, a meeting the next year, and then a blank.

GRAYSON: That makes some sense.

BEYNON: And that's continued to this day.

GRAYSON: Okay, well, I don't know if there's any other topics that you want to discuss.

BEYNON: I don't think so. If you take with you that other interview, and that Wiley research, and you say you've got a copy of this RCM.

GRAYSON: Yeah.

BEYNON: And that international journal. Did I give you the records for that as well?

GRAYSON: Yes, that'd be a good idea.

BEYNON: And there are other ones I believe for my 65th birthday, so that would be 1989.

GRAYSON: Okay, so on this paper that you say, "Why do research?" you actually gave this paper in Welsh?

BEYNON: Yes.

GRAYSON: So the FRS is the Fellow of the Royal Society. So when did that happen?

BEYNON: It's in my CV. I think 1971.

GRAYSON: That's a fairly prestigious honor. Yes, somebody has to nominate you?

BEYNON: Well, you're nominated by your fellows, and the Royal Society's got committees for mathematics, physics, different chemistries, all sciences, and each committee is allowed every year to nominate, I think, one person.

GRAYSON: And so what were you nominated in, physics, or chemistry, or?

BEYNON: I really don't know. I was just told that I'd been elected.

GRAYSON: Okay, and so you don't even know that you're nominated.

BEYNON: You don't know anything about it; until the day that you are inducted. And on that day you get the thrill of signing your name in the Fellows Book that contains the names of all Fellows elected since 1660. People like Sir Isaac Newton.

GRAYSON: This then makes you eligible to receive the type of grant that you did get.

BEYNON: Yes.

GRAYSON: Without being a member of the Royal Society, you wouldn't have gotten the grant.

BEYNON: No, I could've been selected without being a Fellow, but it would've been very, very unusual, I think.

GRAYSON: Where did you deliver the lecture in Welsh?

BEYNON: I did that in Welsh at the annual literature festival, they call it. The Royal National Eisteddfod in 1980.

GRAYSON: Okay, so when you do something like that, can you deliver it in Welsh from reading English, or do you have to write it out in Welsh?

BEYNON: I have to write it out in Welsh. I had to make sure to get the grammar correctly.

GRAYSON: How does the Welsh grammar compare to English?

BEYNON: It's very different.

GRAYSON: Does it compare to a romance language grammar?

BEYNON: No. I don't know how to put it. If you say something like, "It's a nice day," in English, in Welsh you'd say, "There is a nice day it is."

GRAYSON: Oh, okay, so verbs are at the end?

BEYNON: Sometimes, but it's very complicated.

GRAYSON: Okay, well unfortunately I suppose it would be fun to have a little bit of this interview in Welsh, but it would drive the transcriptionist crazy, and I don't think we....

BEYNON: You can look it up, see if you can get it on the internet. It was published in a magazine called....what is the magazine called? I may even have a reprint of it. Graham Cooks once sent me a list of publications, and I wrote back to him and said, "I bet you haven't got this one" [laughter]. I think he knows of it. But if I find the original publication, I'll send it right to you.

GRAYSON: Very good. What I'd like to do is I'd like to make copies of all this material and then return it to you for you to keep.

BEYNON: Oh, sure, I don't mind.

GRAYSON: And then I will probably try and do that either tonight or tomorrow.

BEYNON: And you can take it back to the states if you'd like. I'm not in a hurry for them as long as I do get them back.

GRAYSON: But the thing about my functioning is that if I want to do something like this, I...

BEYNON: You have to do it now?

GRAYSON: I've got to do it now, because if I wait until I get home, then I've got a ton of other things that will get in the way.

BEYNON: Better do it now than wish you'd have done it.

GRAYSON: Yes, and the other thing I'd like to do, and I forgot to bring it up. Are you familiar with this book called *Measuring Mass* that was edited and given out to everybody for the 50th anniversary of the conference on mass spectrometry?

BEYNON: No, I'm not.

GRAYSON: Okay. Well, I was given the job of editing that book, so I have a copy that I brought with me, except I didn't bring it. It's at my guest house instead of here, so I'll drop that by tomorrow as well.

BEYNON: Okay, thank you.

GRAYSON: So I'm maybe being presumptuous to give you this book.

BEYNON: No, not at all.

GRAYSON: I think you might find it interesting.

BEYNON: I'm interested to see it. I would thank you.

[END OF AUDIO, FILE 1.5]

[END OF INTERVIEW]

**APPENDIX
AN INTERVIEW WITH:**

GARETH BRENTON

Transcript of an Interview
Conducted by

Michael A. Grayson

at

Swansea, Wales
United Kingdom

on

23 April 2008

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
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(Date) 1 Oct 2008

INTERVIEWEE: Gareth Brenton

INTERVIEWER: Michael A. Grayson

LOCATION: Swansea, Wales, United Kingdom

DATE: 23 April 2008

GRAYSON: Is it University of Swansea, or Swansea University?

BRENTON: It was called then University College of Swansea; because University of Wales had five constituent members. So it was one of the larger universities, well, at least within the U.K., and possibly within the world, because it was structured as a collegiate system.

GRAYSON: Yes, and Beynon came here on a Royal Society Professorship that he had been awarded.

BRENTON: The Royal Society has a number of members, several hundred, and they started a scheme of Royal Society Professorships and would actually fund the salary and give money for equipment and research. I think six or eight new Professorships were created in the '70s, and he was offered one, so this has a proven role in research.

GRAYSON: Well, I was thinking this was a rather substantial amount of money because he was able to pay his own salary and some doctoral students and post-docs; as well as buy equipment.

BRENTON: I think it was actually the reverse; that it actually just paid his salary and some money, if I remember what it was, I think it was something of the order of 18,000 pounds, which at that time was worth just over \$30,000.00. Actually what it gave him, it gave him a letter, and this opened many doors, so within the University, I seem to remember there was a letter that the Registrar of the University signed that they would give him any assistance he needed. This was the power of the Royal Society post. It opened all these other doors, but in terms of raw money, the answer was there wasn't a great deal.

GRAYSON: Okay, that's interesting, because I was under the impression that it had to be a substantial amount of money. But it was the prestige of having the Professorship that was valuable.

BRENTON: Correct, so within the university, it allowed him to buy in technical staff, secretarial staff, support staff, but postdoctoral workers would then have to be got via funding, and that's the mechanism he used.

GRAYSON: Okay, and so you came on board. You were the first postdoc in the group, as I understand it.

BRENTON: No, second. The first was a gentleman called Roger Morgan. He wanted to be the managing director of Shell by the time he was 39. Because he was a public schoolboy, and all public schoolboys were taught that they should run British industries, so he's still with Shell. He hasn't made it to the top.

GRAYSON: Hasn't made it to the top yet, so that was his first postdoctoral student, and then you came shortly after.

BRENTON: Yes, I was the second. Shortly after he [Roger] started.

GRAYSON: So the ZAB equipment then arrived after a few years so for a while, he basically had no equipment, right?

BRENTON: Well, he had a post in Purdue. Then he went into an interregnum, and his old friend Keith Jennings he used to go to Warwick, so he had his network of scientific friends to use for support. But his first machine actually was a prototype machine that was purchased off Varian. It was the SM1 machine developed by Karl Maurer, and it was a very high resolution, normal geometry sector of mass spectrometer with a claimed resolution of 200,000. And Curt Bruneé and Karl Maurer had developed this as the next generation of super mass spectrometer. But Varian cut the line under it when the budget was exceeded for some management decision.

GRAYSON: So this is the time when Varian owned MAT or was running MAT in Germany. I think there was a period when it was called Varian MAT.

BRENTON: Yes, they were called something else before that, which I don't exactly remember [laughter]. It'll come back to me.

GRAYSON: Yes, they've all been called something else; and they've all been in a variety of incestuous relationships; VG, Micromass, Kratos, what have you.

BRENTON: It might be Krupps or something even at that time. I don't recall.

GRAYSON: So he had that equipment here?

BRENTON: Yes, he had that. That was his first equipment on which he did IKES.

GRAYSON: Okay, now was this the reverse geometry?

BRENTON: No, it was normal geometry.

GRAYSON: I think when he was working with Brian Green on the ZAB, he wanted a reverse geometry instrument, and that was part of his desire for setting up that interaction with Micromass at that time. Is that what they were?

BRENTON: Yes, Micromass had...it might've been a 70/70 machine at that time. They wanted a high performance machine, and they went for the Hinterberger and Konig design. It was published in '57 and that design was basically taken from the textbook.¹²

GRAYSON: Yes.

BRENTON: And so it turned out to be a normal geometry machine, and I understand customer number one was Eli Lilly, and John persuaded Brian Green who was an old friend of his, to swap it 'round, and I think he succeeded, but that instrument, I understand, came from a giant grant with another of his friends, Dudley Williams at Cambridge, and it was meant to be a shared machine. But I think John persuaded the powers to be to deliver it to Swansea rather than Cambridge. And the rest is history, as they say.

GRAYSON: Oh yeah, definitely.

¹² H. Hintenberger and L. A. Konig, "Mass spectrometer with complete double focussing in second order," *Zeitschrift fuer Naturforschung* 12a (1957): 773-85; H. Hintenberger and L. A. Konig, "Mass spectrometer with the complete double focusing in second order," *Zeitschrift fuer Naturforschung* 12a (1957): 443.

BRENTON: Dudley still smiles about it. His students and postdocs came down initially for the first few years, but then progress....

GRAYSON: Yes, so then he did a lot of this IKES and MIKES work that was really the beginning of the MS/MS thing where people were starting to use the magnet as a mass selection device. Then the electrostatic analyzer could be used for a number of different things, amongst which was the ion kinetic energy measurement and so on. It lent itself to the whole concept of selection of an ion of interest with the magnet, and then leading onto the tandem mass spec operation. The MS50 I know was modified with an ESA on the back end to do a lot of this type of work, and that was a machine that Mike Gross had in Nebraska. I actually used it when I was at Washington University up until the end of last 2007. I didn't use it for that particular application though. So other than IKES and MIKES, what other research projects were going on in his lab at that time?

BRENTON: Yes, there's some we had angle resolved mass spectrometry. There was also laser photo dissociation.

GRAYSON: Okay. I think he started some of that Purdue, didn't he?

BRENTON: No, no, it was definitely started here at Swansea in about 1982.

GRAYSON: Oh, okay.

BRENTON: I'm absolutely certain, the summer of 1982, because I can remember the start of the project, and the mathematics to work it out to make sure it was feasible, because there was some question over the feasibility of the work that was carefully worked out by....

GRAYSON: Okay, because he mentioned that he had proposed something like that to the NSF when he was at Purdue doing something with a laser dissociation or photo dissociation work at that time and he had difficulty getting the granting body to buy into it.

BRENTON: That I don't know, but I know that John had certainly talked a number of times about photo dissociation within the source, which is a slightly easier experiment. The experiment within the flight tube is difficult, because the density of ions is so low; six orders less per cubic centimeter.

GRAYSON: Yes, the typical problem. The further down the flight tube you go, the fewer ions you have to work with.

BRENTON: Yes, this isn't Avogadro's number!

GRAYSON: Yes, so in addition to the photo dissociation, were there any other research activities beyond that? He only was here for about 12 years, right?

BRENTON: Yes, I think John arrived in '76, and I arrived in late '77, and he announced his retirement just after the '85 conference. He retired two years, before the normal age, in '86.

GRAYSON: I understand he that the number of publications that came out of the group was measured in per days. They were rolling out of here, on average . . .

BRENTON: Yes, that's true. There was at least one a month or one a fortnight.

GRAYSON: That's what he was saying; it was close to one a fortnight.

BRENTON: I would think so, yes, 'cause I remember at one time, he was involved with a lot. Yes, it was a lot of work. He wasn't aware of the work and didn't want to be aware of it. I knew that. He just got on with things.

GRAYSON: So people really were burning you out.

BRENTON: Oh god, yes.

GRAYSON: Fairly intense.

BRENTON: Oh yes. I think I was the main driver or whip cracker [laughter].

GRAYSON: You were the slave master type, major domo, beat ‘em up guy. [laughter]. So how many students did he have at this time? People coming into this group must’ve understood that they were going to have to work hard.

BRENTON: Yes, it’s difficult to say. I would say there were more visitors than students at times during the year. On average, there would be six Ph.D. students, a couple of postdocs. I went to industry, then he persuaded me to come back, and I became a staff member, so I spent two years at Kratos.

GRAYSON: Okay.

BRENTON: Learning sectors methodology; so I would say there were probably between six and twelve visitors a year, and they were the most productive. They were mature academics or young academics who had lots of energy and ideas.

GRAYSON: Yes, they had the ideas.

BRENTON: He would supervise students and then direct. It was an ideal environment.

GRAYSON: Sure; the equipment’s there, it’s ready to be used. Yes, well, that’s an interesting way to approach it.

BRENTON: It was very productive. He had a minimum of hands-on at all times.

GRAYSON: But obviously he was managing it from the sense that he was aware of what was going on in the various research projects.

BRENTON: He was very aware of everything.

GRAYSON: And he was lending ideas, and support, and challenges. That’s a fairly substantial volume of work. His publications were up in the vicinity of 400, and he mentioned that most of those were in his years when he was here at Swansea, which is ten or twelve years, and so you’ve got to be really doing some work to get that kind of thing done.

BRENTON: It's true.

GRAYSON: So how big of a stable of instruments did he have?

BRENTON: Really, the major instrument was the ZAB that most of this work was done on. The Royal Society also has another source of funding, which is called the Paul Instrument Fund, which is named after a Mr. Paul, and he donated a substantial amount of money to the Royal Society in 1947.

GRAYSON: He mentioned that one.

BRENTON: For the development of unique instrumentation, and he developed, with Frank Harris, a quadrupole ESA system. And then the last thing he did with me was develop a double focusing energy system; which was developed between about '85 and '88.

GRAYSON: Okay, so I believe this was a system that had a double ESA. Was this a reverse geometry ESA after this first ESA.

BRENTON: Yes, but in a very special arrangement.

GRAYSON: Okay, and so the purpose of this was to resolve the energy of the ions as a result of any interaction they had in a collision, which would've been between B1 and E1?

BRENTON: It would be between the two E's, because the two ESA's themselves were non-energy dispersive, but if you tickle them [ions] in the middle, it would disperse that, but disperse it to a really high resolution, phenomenal resolution, way beyond ten to the five.

GRAYSON: So you were able to get these energies down very precisely.

BRENTON: And at high resolution. There were also another one or two little ESA's tucked at the end [laughter]. They just cleaned up neutrals, and funny ions, and things like that.

GRAYSON: Well yes, that is the problem. You have to get rid of those things, because they still impinge on the detector and make life a little bit messy if you don't get rid of them.

BRENTON: And when you were talking about Mike Gross developing the multi sector, I actually designed [one] for Mike in '81. I tried to convert his MS9 or MS50 into a multi sector machine. But he either didn't have the money or something, because he had a slick and very cheap way of converting his machine.

GRAYSON: Well, what they ended up doing is putting another ESA, which I think is almost identical to the one that was in the 9 at the tail end, and in Nebraska, the machine was placed out in an area where there was a loading dock, so you could hang all that stuff underneath. Now at Washington University, they built a stand for it. It's an interesting machine to run, because you feel like you're the captain of the ship up there; you climb up onto this little platform where the console and the main part of the instrument is. It feels like you're running the instrument from this command post kind of situation.

BRENTON: Yes, I just seem to remember, that Sid was the leading light on that, Sid Evans. But Kratos really were opposed to reverse sector instrumentation. They had sort of a hostile feeling about it. They came in probably a little bit late to take full advantage of the market.

GRAYSON: Yes. Now was Green at Kratos at one time, or was AEI, or whatever they were, GEC or Metropolitan Vickers?

BRENTON: AEI. Metropolitan Vic's [was] before AEI.

GRAYSON: Right, and so he jumped ship from AEI when...

BRENTON: Yes.

GRAYSON: Who fired up Micromass?

BRENTON: Well, Micromass was a vacuum company to start off, and that gentleman's name...looking back, Bernard Eastwood, and that was set up in the southeast of England. John Beynon's apocryphal story is of one of Bernard Eastwood's employees, Robert Craig, was hawking around a non-glass UHV [Ultra High Vacuum] valve made out of stainless steel and tried to persuade John Beynon to buy one. Robert Craig then went on to establish the VG mass spectrometry side. Of course Brian Green would know all of this. Robert Craig was in Kratos, Brian Green, Bob Bateman, et al where there, and half of them stayed, and half of them went.

Now the half that stayed, I went to work with them. Some of them were at times, bitter maybe at the success of these guys who took that knowledge away, and made a bit of cash from their business.

GRAYSON: It was a very dynamic business for a while until the big boys came in, and bought 'em out, and started putting all those overarching rules on things; making sure that everybody made money instead of instruments.

BRENTON: They were doing it naturally. Brian's original story of how he learned his engineering, he and his brother had a toy train set that went right 'round the garden, and I think his dad encouraged him to do that, just as much as in Britain, boy's mums buy them a Meccano.

GRAYSON: Well, I wish I had the time to sit with Brian while I'm in the U.K., but unfortunately I don't, and I think he needs to be interviewed at one time, because the contributions he's made are significant, and I'm not sure very many people are aware of how significant his contributions are. They really are important, so then you went away to industry and came back. Now when Beynon retired, he essentially retired from the professorship, so there was no more of that funding coming in.

BRENTON: Correct.

GRAYSON: My understanding is that there was a little bit of a love/hate relationship between the University and John because of the fact that the Royal Society gave the money to John rather than to the University, and then they had to supply him space, and he was competing for students and so on and so forth. I don't know if that's your perception of how things were back then.

BRENTON: I think that's true, but because John was a powerful personality and had the position and authority, he essentially cut an independent line, and while that might seem arrogant, he didn't do it from an arrogant perspective. He actually did it from a very pragmatic point of view, because he understood how people worked, and that was something I learned from him, but not always applied. That was an efficient way to do his research, actually. He was very productive. They would have had him teaching otherwise; and doing admin.

GRAYSON: Yes, so then when he retired, this facility existed here.

BRENTON: Yes.

GRAYSON: And so the University took it on then as part of its operation?

BRENTON: Yes, the University continued to pay salaries, but then funding had to be got from Research Councils.

GRAYSON: Sure.

BRENTON: And after the international conference in '85, I think John had cut a deal with Gunter Heyden to start up RCM, Rapid Communications. I think Gunter even had to fund the setting up of that conference initially. I think he loaned us 10,000 pounds in 1984 to have funds to underwrite the costs of organizing the international conference. John had this theme of work [publishing] that he wanted to go into.

GRAYSON: Okay.

BRENTON: But I think his retirement partly was prompted by funding issues. He knew the American system. When you had a good track record, then the funding stream would continue, but the U.K. system is somewhat different. This may be more of a personal remark, but I knew that John suffered because his proposals sometimes weren't always accepted by the Research Council because they thought it was someone else's turn.

GRAYSON: That is a factor that fits into the selection of people to receive grants. It's appropriate to a degree, I guess, because you've got to be a little more inclusive. But he was particularly productive; I think they had to recognize that. It's nice to see a situation in which, as he mentioned when he was at Purdue, he felt like he was spending more time writing reports indicating that he was getting work done, whereas in the U.K., with this arrangement with the Royal Society, he just did the work, and he didn't have to spend time writing about it. The literature spoke for itself and he didn't need to be reporting all the time that he did this, or he did that, or what have you.

BRENTON: Reports were required for the Royal Society, but they were more efficient. You didn't have to write 100 words to get one dollar or whatever the conversion rate is [laughter].

GRAYSON: So did he write any books during this period? I know he had nine books altogether.

BRENTON: Yes, we did write one title together, which was the first and only scientific book for the University of Wales Press. They actually invited John to write a book so he collared me and we wrote a book.

GRAYSON: On?

BRENTON: Mass spectrometry. Because mass spectrometry in 1981 was new to schools, schoolteachers were teaching it, so we wrote a volume, about 120 pages, that was aimed somewhere between high school, we call it sixth school, and University.

GRAYSON: I see.

BRENTON: It could well be a very good introduction even for a startup post graduate student today because of standards.

GRAYSON: Well, one of the things I'd like to do as part of this whole exercise, is to actually see if I can gather together those books to make a contribution to CHF to put those in their library, because Beynon was just totally prolific. We talked about his first book, and how diligently he worked on this thing, and I'm familiar with the tables in the back and I had assumed they were farmed out to some person who was handy with a Marchant calculator or whatnot. But he ground through every one of those and apparently his wife checked out his numbers, so the guy was just so productive, and it didn't matter what level the work was, he'd do it. If it was grunt work, he'd do it. If it was high powered work, he'd do it, so that's a unique characteristic. I think a lot of people would have said, "I'm not going to mess with this,"

BRENTON: I think it's a bit of a Welsh characteristic as well. I see it within the mindset of Welsh people. They'll grunt [work through a difficult problem] at any level, what is appropriate for the day.

GRAYSON: I see, so he's apparently fluent in Welsh and he said he gave a lecture once in Welsh. That must've been interesting. He said a few things for me in Welsh and it's very interesting.

BRENTON: I have a copy on our website, because I retrieved it from John recently. I found a copy in a file and actually posted it.

GRAYSON: Oh, okay, in the Welsh?

BRENTON: It could be in Welsh. I'm not sure though.

GRAYSON: He gave me a copy of the English version, which I ended up making a copy to take with me. I've got to actually go by there and drop off some stuff.

BRENTON: I've got it on the website, so you can just download it when you go.

GRAYSON: That would be great, having it in electronic form. So what happens with all this business, this is a joint effort of the American Society for Mass Spectrometry and the Chemical Heritage Foundation in Philadelphia to record oral histories of important people in mass spectrometry. So for we've got Al Nier, Biemann, McLafferty, Sy Meyerson and Charlie Judson, and a couple others. Basically when it's all said and done, then the oral history is transcribed, and it'll be kept at the library at the Chemical Heritage Foundation, and be made available for scholarly study in the future years, and having this type of firsthand, personal information about what was going on then is invaluable. I remember once when I saw Biemann at a conference probably 20 years ago, I'd been involved in the history side of mass spec for quite some time, and he just pooh poohed the idea of this sort of history because everything's in the literature. Just go to the literature and you see everything that's done. But he was a very willing subject for an oral history interview when I contacted him a couple years ago and said, "I'd like to do an oral history." I think he's mellowed a little bit in his attitude towards these things, because the things that go on at a personal level and the interactions that occur that are not known from looking at the literature are what makes this type of a activity valuable. So I don't know if you have any other concepts or ideas about John's work that you wanted to tell me about at this point, but if so, go ahead, otherwise I'll use this, and I'll shoot you a permission form so you can see what we're doing, and you'll get a transcript so you can see what we've talked about, and make sure that it's been recorded and transcribed correctly, and we haven't put words in your mouth.

BRENTON: From what you've said, I think you've been fairly perceptive in the way in which you've analyzed his character, so I'd agree with what was said. Yes, I think you've more or less captured him. Well, the way I see it, it's the spirit of one or two individuals who blaze a trail, and then whatever comes out at the back creates all this industry behind it, and one of the people who created that....he certainly was one of those persons.

GRAYSON: It's interesting to me too that apparently he never had any formal chemistry education.

BRENTON: I don't [laughter].

GRAYSON: Well, it's all right. I think probably we may be the last of the breed of people that came into mass spectrometry wandering in the door, not knowing what we're getting involved in.

BRENTON: He was a mathematician and physicist [laughter].

GRAYSON: Yes, right, and his final degree was a bachelor's in physics, but it's clear that he thinks like a chemist. The literature speaks for itself; he's widely respected throughout the world for the work he's done, so it's the old story that it's not the education that you have, but what you do with the education you've got, and he certainly has done a tremendous amount.

BRENTON: Yes, there's another aspect as well. He had a remarkable way of managing people, but particularly at the level of large organizations to get things done efficiently and quickly without too much fuss. He managed the international meeting and actually drove each part well, as anything, he was pretty remarkable, so there's somebody who could run a major company.

GRAYSON: Yes, I think that his skill in managing people has to be obvious to get that much work out of them. Even if people are industrious and interested, they can certainly be turned off by someone very quickly if they feel like they're being co-opted, or if they're being pushed.

BRENTON: No, it was reciprocated. I think that's the only way you can work

GRAYSON: Yes.

BRENTON: He certainly reciprocated hard work. On a Friday night, he'd be fixing something; he could well be in the lab at 9:00 at night. And he loved Friday nights because they coincided with drinking at Swansea Rugby Club. Friday nights in Wales were sacrosanct many years ago because of the Rugby drinking culture on Friday night. It was the end of the working week for the miners and the tin workers.

GRAYSON: Okay, so yes he said he grew up in a mining town.

BRENTON: It was a mining town not tin in South Wales.

GRAYSON: That was the industry. So you had an evening of imbibing alcohol prior to the weekend's games?

BRENTON: Weekends game on a Saturday, and yes, 30 years ago, you would go to church or chapel.

GRAYSON: Okay.

BRENTON: I don't know whether John did or not. I don't remember. I would diligently do so.

GRAYSON: Now one thing I didn't touch base with him on was whether or not he had any children.

BRENTON: No, I don't think so.

GRAYSON: Okay, because I know he was married, what, in '47. They have been together for quite a long time, and I met his wife. They get along very slowly, but they do get along.

BRENTON: Yes. On a personal side, he had his own friends; but I always had a work relationship. It was just instinctive. He didn't suffer fools.

GRAYSON: No, I can see definitely not [laughter].

BRENTON: In fact, you'd know very, very quickly if he was irritated.

GRAYSON: Good.

BRENTON: Which I quite liked, actually, because there was a nice, easy method of communication.

GRAYSON: Well, I think that we've recorded enough information to kind of seal the deal, so to speak. It's too bad, I really would like to talk to some Biemann's students, and McLafferty's students, and people that worked with them; postdocs and so on, to get a little bit of a spin from their side, because obviously they see the character, and the work, and what was done from a completely different light, and I think that tends to obtain a different perception of what's going on that'll be valuable later. Because it's nice to know how other people thought about this individual and what they were doing, and see how that's reflected in their own thoughts, and sometimes things mesh, and sometimes they don't. Sometimes perceptions are different, depending on which side they're on.

BRENTON: I didn't go too deep into that.

GRAYSON: Very good, I think we can go ahead and put this guy to sleep then.

[END OF AUDIO, FILE 1.1]

[END OF INTERVIEW]

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