An interview with Ron Whittam

Conducted by David Miller and Richard Naftalin on 12 August 2014

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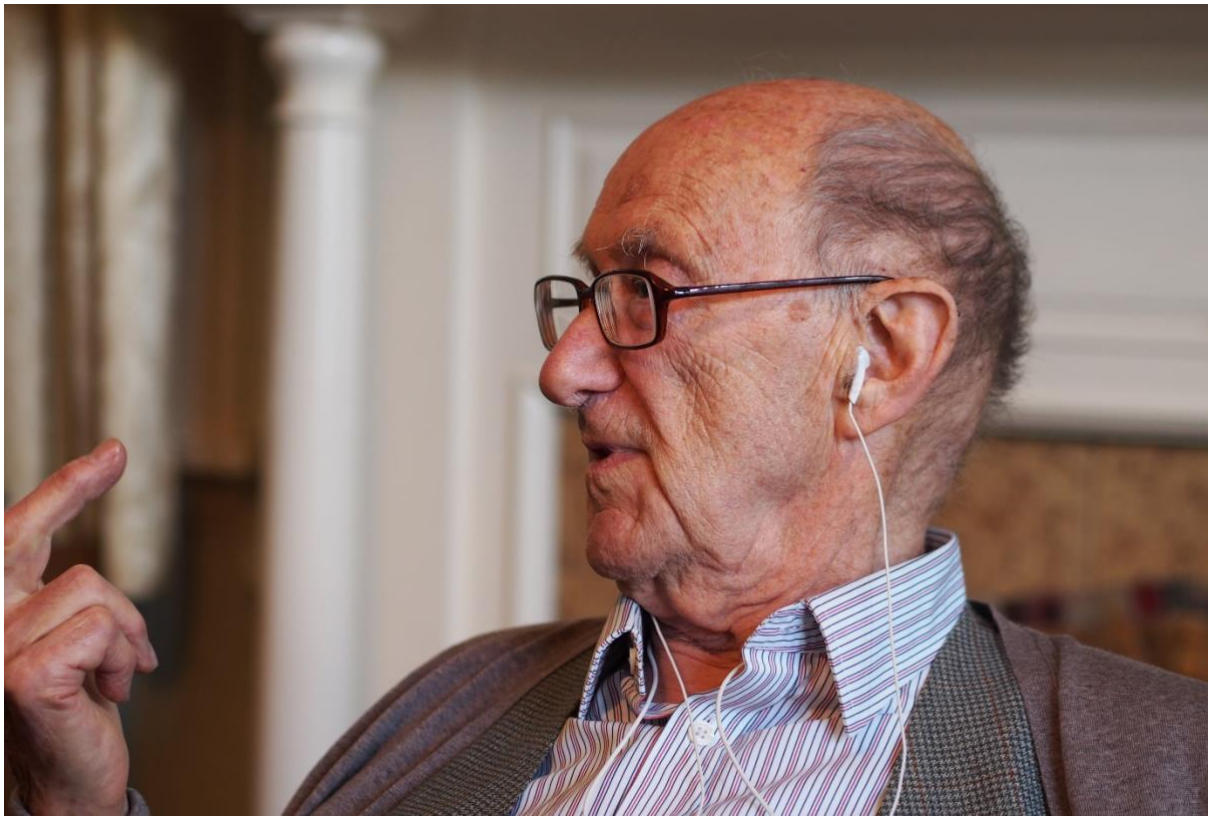
The Physiological Society



Ron Whittam photographed by David Miller

This interview with Ron Whittam (RW) was conducted by David Miller (DM) and Richard Naftalin (RN) on 12 August 2014

DM: **Let's make sure we're starting. Fine. So okay this is David Miller, it's the 12th August 2014 and we're are in Leicester at the home of Professor Ron Whittam and we're here to record for the oral history project of the Physiological Society. So, also here is Richard Naftalin whose voice you'll hear and Ron Whittam himself, of course. So we're going to run through elements of Ron's life and background as he wishes to cover in the usual way. So that's enough from me. Perhaps Richard if you could just say a few words so that it's recognised whose voice is whose.**

RN: **Okay, well I'm Richard Naftalin. I'm currently Emeritus Professor at King's College London but I was, had the privilege of being, in Leicester University and I was appointed lecturer to Ron Whittam's department of General Physiology in 1968. I came to Leicester as newlywed and as a virgin physiologist to Ron's department so that was very exciting –at least for me. We stayed seven years. I was attracted to Leicester as I was fascinated by a lecture that Ron had given at the Biochemical Society, I think about 1967 at Bart's in London.**

RW: Yes.

RN: I remember it was on the sodium pump.

RW: That's back in '67. Well, I should say first of all that I'm giving this interview because of my great respect for the Physiological Society and the recognition of the important part which it played in my life from the mid-50s. And I never expected to finish up in physiology and it's been a very roundabout route.

RN: What did you expect to finish up as?

RW: A baker [laughter]

RN: Okay. I'm not laughing really. No, I thought maybe you meant biochemist or something. [laughter]

RW: No, well there's very, you see this is a point that my background was in Chadderton which is a small place near Oldham, and my family have roots in that area. I can tell you now about my family.

RN: Okay briefly, yes, that would be nice.

RW: I have two brothers and a sister, all younger than me. My father was born in 1899 and he was employed in the cotton trade from the age of 13-14 on a half-time basis. In those days they had half-timers and he worked in the cotton trade until he joined the army in 1917.

RN: Right.

RW: He was on the Western front and taken prisoner by the Germans in the big spring offensive in 1918 and he was in a prison camp in Poland and worked in the salt mines in Silesia, and he had a terrible time all in all and he told me it was the worst possible situation, both on the Western front and in the prison camp. However he safely got back to this country but had had enough of the cotton trade and went into the bakery business, which my grandfather had started. And there's an interesting story to that because he was an under-manager in a cotton mill but got fed up because of the exploitation of the work people by the owners and rather oppressive conditions. So my grandfather's mother had been a baker and my grandfather started a business in 1918 into

which my father went and he prospered, had a model bakery, employed about six people. I was living very close to the shop that my grandfather had. Now I ought to say that I was blessed in other ways, in the family way, because my mother was one of eight children and within a distance of 5 miles of where I lived I had a plethora of relations, aunts and uncles and so forth.

RN: That's nice, and cousins I imagine.

RW: Hm?

RN: And cousins?

RW: No, now that's the interesting… I only had two male cousins.

RN: Okay.

RW: Two male cousins and one girl.

RN: Right.

RW: And the point I was going to make was that out of the uncles that I had, I had two uncles who were in the Great War in Mesopotamia, you know where it is, between the Tigris and the Euphrates. But they met up with a lot of awful things in Mesopotamia, but anyway that's by the way. But the point is that my mother's side of my ancestry are what you would call urban proletariat.

RN: Okay.

RW: My father's side you would call bourgeois in the sense that he had property and land.

RN: A business, yeah.

RW: Now my father had two sisters, one of them married a farmer. He owned his farm and he was bourgeois as well. So in the small area where I went to school at the age of 5, the position was that there was a tiny community where there was very little difference between the bourgeois, solicitors, doctors, people of that kind, and the ordinary working people because they'd all come from the same stock.

RN: It's not a very English sort of story really, is it? Because in England the norm I'm afraid is to have quite rigid class divisions. So you're talking about a rather exceptional sort of situation I think. Certainly about that time, yes? What do you think?

RW: I'm talking about really the fact that after the cotton famine in the mid-19th century the cotton trade, there were great fortunes made and a very large number of cotton mills spinning in Oldham, weaving in Blackburn and Burley and so forth.

DM: And Rochdale.

RW: And they made huge fortunes but the big difference in Oldham was that the people who made the fortunes by and large put it back into the town. Now the house where I lived was adjacent to a big park with two bowling greens, two tennis courts, a big playing field, football ground and it was given to the town by one of the big engineering firms, and the woman who gave the land actually was the first woman mayor of Oldham. But she was a millionaire, very bright. Now I should say this that the house where I lived as a child and where I was born was one of 14 in a terrace and six doors away my mother's father lived, s… he was born in 1863, had worked all his life in the cotton trade, now he's the one with eight children and he had a great big house, only seven houses from where I lived, I was always nipping in and out and he had a maiden daughter, everyone else of his children married except one daughter who looked after him. But I used to…

RN: This was when exactly? What date approximately was that?

RW: It was about 1932? I was born in 25, he died in 35, age 72 and that was the first time I'd experienced the laying out of the corpse and washing and… In those days they didn't have chapels of ease and they just put the body…

RN: In the front room.

RW: On a table in the front room, put it in a box and then people gathered for the funeral. And I had a great affection for this old man because he had so many tales to tell and he was contemporary in Oldham with the man who became Home Secretary, a man called Johnny Clynes [John Robert Clynes, 1869-1949]. No, you wouldn't remember him, you wouldn't know about him but J R Clynes was born in 1869, he worked part-time from the age of 10 and he taught himself, he was the son of an illiterate grave-digger. He taught himself to read, eventually joined the Fabian Society and when he finished, he had really

prospered from being a trade unionist. And he became a member of parliament in 1906, this is Johnny Clynes, 1906, and he was one of the Labour MPs who supported the Great War, in contrast to Ramsay McDonald [James Ramsay MacDonald, 1866-1937] who was a pacifist. And Johnny Clynes became leader of the Labour Party in 1921 and he led to the great increase in MPs from about 40 odd to about 150.

RN: Labour MPs? Okay.

RW: That was Johnny Clynes. He started life in Oldham and in 1924 he just missed becoming leader, he was outvoted and MacDonald became leader of the Labour Party; but anyway Clynes became Home Secretary in 1929-31 and the point I'm making is that from a background like that he became Home Secretary, and was awarded an Oxford honorary doctorate, and when he died in 1949, Atlee thought very highly of him and so did Anthony Eden. Now I'm digressing, you must stop me.

RN: No well, I was trying to, I was going to come to put a stop to that in a minute. So you went to school in Oldham?

RW: Yes.

RN: Okay. I seem to remember you told me that you left school quite early, didn't you?

RW: Well, you see, there was no 11+ in those days.

RN: No, okay.

RW: If you wanted to go to grammar school you sat, that was separate thing.

RN: Okay, yeah.

RW: And I was so integrated that I didn't want to go to grammar school so I left school at Easter in 1939 at the age of 14.

RN: Oh wow.

RW: And I helped my grandfather in the bakery and when I was 14 I went to work in the bake house and there were three generations of us: my grandpa, father and me, and until my grandfather died we got on well together and made a lot of money selling high quality confectionary and so forth.

RN: Right. Eccles cakes. [laughter]

RW: However, I was fascinated by the way the yeast functioned.

RN: Ah…

DM: Good.

RW: And that led me into going to night school and I did science at night school.

RN: Okay.

RW: So that's how I first got into science.

RN: Where was the night school, Prof?

RW: Just nearby, a local school.

RN: Okay, what kind of people would have run the night school? Would that have been college people or school teachers?

RW: Oh, they were school teachers, yes, and we did chemistry, physics, maths.

RN: Right.

RW: And I did that for two years and then I bought this book, which is a science course for bakers, and you see my name in it at the front and that triggered my interest much more.

RN: So you were about 16 at the time?

RN: Fantastic.

RW: It's a very good book that.

RN: I'm sure. Maths, very good. Okay, and a polarimeter. Hmm.

RW: Sorry?

RN: A polarimeter.

RW: Polarimeters, saccharimeters and everything. [laughter] Anyway my dad said, 'You're obviously able to pass exams so you can go..' what they would call day-release we'd call it today.

RN: He was generous, he gave you a day off, yeah?

RW: So if you look in Who's Who 1974 you'll see that I put down my education was council and technical schools, so I went to the technical school, which equipped me to get a good school certificate and there were only two things I had to be careful of, mind. One was a foreign language and I had the benefit there of learning German by the generosity of a Jewish refugee who had been a big noise in Vienna, came as a refugee to Oldham, and he'd been in the cotton trade and a cotton manufacturer let this man and his wife live in the lodge of the mill and his wife used to come into the shop, she knew quality when she saw it. And one day we got talking about Germany and so on, and this man said, "Oh, wouldn't you like to learn German?" So I learned German from this Jewish refugee. So that was school certificate and I got, the other thing was that I'd been involved with the Boys Brigade and the St. John Ambulance Brigade and consequently I was able to get the range of subjects in school certificate. And then the next thing was, do I stay in the business? Well, I stayed in the business until I joined the RAF.

RN: Yes, and how old were you when you were in the RAF?

RW: 18.

RN: Because you were called up?

RW: No, no, I volunteered. I'd been in the ATC.

RN: So what date was that? That must have been about 1945?

RW: ’43. Yeah, I joined the RAF in ’43.

RN: So it was during the war?

RW: Oh yeah.

RN: Definitely. So what did you do in the RAF?

RW: Ah well, you see the thing was from 1941 there were so many air crew dying that they couldn’t get enough air crew from the traditional public school, grammar school people.

RN: Right.

RW: So they got a professor from Cambridge to design aptitude tests [laughter] and the aptitude test allowed people from any background to be recruited for aircrew. And there were two groups for the aircrew.

RN: Air crew is not pilots?

RW: Oh, there were pilots, navigators and bomb aimers on the one hand, and air gunners, wireless operators and flight engineers on the other hand. Now the PNB people, as they were called.

RN: PNB?

RW: Pilots, Navigators and Bomb-aimers. I took this aptitude test and I was picked to become a navigator/bomb aimer, which meant you did both jobs, and I wanted to fly Mosquitos. However, anyway that was it. I was picked to go in the RAF. But, on the records, the RAF some years ago were very good and I wrote to them on headed notepaper and they sent me the whole bang lot of my records, and I’m down in this occupation as apprentice baker.

RN: My father was in the RAF too. He was more or less contemporary with you, a little bit older, but he was a medical officer, around about here.

Wolverhampton and Coventry in a place called Monks Kirby. He was, on the training station.

RW: Over where?

RN: Monks Kirby and Coventry and Wolverhampton. In fact I was in all these places as a kid because they allowed the families to accompany training officers.

RW: Well, I mean joining the RAF actually was one of the best things I ever did because with being in the PNB group we were sort of the…

RN: The elite?

RW: Coddington [former RAF base near Newark-on-Trent], yeah. And you know in those days the ordinary soldiers and the ordinary airmen they slept between blankets, and when I joined on we had sheets on the beds [laughter] and we had a pair of shoes and we had a little white flash in our hats, and so you were regarded as kind of air-crew under training. So I enjoyed that.

RN: So you were treated reasonably well?

RW: Very well.

RN: Yes, and you were fed properly and things like that?

RW: Yes, very. And I started in, we did 6 weeks initial training where we learnt drill and this was in Scarborough, and the drill was by a guard sergeant and we had to learn bayonet practice and we had to do actual training with the rifles and they had us marching through Scarborough to the castle.

RN: I've never been to Scarborough.

RW: They had us running up and down and going with the bayonets. But anyway after that, I quite enjoyed it.

RN: What, running up and down with a bayonet? [laughter] I did that sort of stuff at school, believe it or not. I was at school from 1944 till '54 the school I went to had a corps, they had a compulsory army corps, and boy, did I loathe

it, - and vice versa. But I got into the navy because I thought it was slightly more civilised than the army- but I was wrong. [laughter] Yeah, so anyway I had to do marching up and down too.

RW: And I did some research about these aptitude tests and it was very interesting because I had a friend, my best friend from school was completely uneducated and he was picked to be a pilot.

RN: Okay, okay, weird.

DM: Fascinating. I wonder what kind of things were in the test, it's hard to imagine now because these days, as we appreciate, everyone would have paper qualifications and you would, you'd have to be a long way through the system before anything like an aptitude test. It really would only be an interview now, would it not? Do you think they do aptitude testing?

RN: I don't know. I imagine so, yes. I mean I've had students who've been pilots and they're pretty rigorous because the training is phenomenal.

DM: It's expensive so I guess they choose carefully.

RN: And they have to be psychologically tested too because it's tough.

RW: But anyway you don't want to know about that?

RN: Well, I kind of [do]**, because it's part of the story, it's useful. What drew you into science? I mean we heard about the baking?**

RW: Sorry?

RN: What drew you into, what made you become a scientist? Why be a scientist?

RW: Oh I'll tell you that, I'm coming to that.

RN: Good, tell us.

RW: You see I'd had embarkation leave to go to Canada and then the war finished, so they didn't know what to do with us. So they put you on different courses.

So when I was 16 I bought a Norton motor bike and I was fascinated by motor bikes so I did a dispatch drivers course and then there was no future in that so they said, 'Well, you'd better settle down' so I did the radar course and I had to what they call re-muster, which meant that you had to change. So they didn't want any more aircrew so I got involved in radar and that was very interesting. I did the whole radar course which involved a lot of physics, and I was on the radar chain on the south coast, actually near Little Hampton and I was demobbed from there in 1947 intending to go back to the bakery. And then the squadron leader interviewed me and he must have spotted that I wasn't an apprentice baker or something and he didn't know from the records that I'd got school certificate. So when I told him he said, 'You should go to university'.

DM: Amazing.

RW: And he arranged it.

DM: Fantastic.

RN: So which university, you went to Sheffield, did you?

RW: No, Manchester.

RN: Okay, okay, sorry.

DM: So was this a scheme at the time that ex-servicemen had some sort of financial helping-hand?

RW: Oh yes, it was tremendous.

RN: So that was important otherwise getting through the university life would have been difficult. So you got some sort of grant or…

RW: Oh yes, £163 a year.

DM: Sounds very generous, Prof. I was only on £300 in the 1960s. [laughs]

RW: You see I was able to live at home, being at Manchester.

RN: Yeah, well my university career cost very little and although my father had to pay fees I think it was £40 a year, at Glasgow University, but I lived at home too so it cost very little. Not like now.

RW: You weren't in the armed forces, were you?

RN: No, I was very lucky, I escaped. The year I graduated they decided that medical students had referral, deferral, at the time but most of them up until the year I graduated which was '62, had to go and serve afterwards. Although they had been deferred, deferred meant you had to go back. That was the year they stopped recruiting doctors unless they wanted to join. Some of my friends did in fact join the RAF and they had a good time.

RW: Oh yeah, it was fun.

RN: Especially as a doctor, you got ferried around all over the place. I don't think many people got very sick except those who crashed, who were… My father was in Burma in the RAF and that was a different story altogether. Very different.

RW: Well, I never went out of this country. Not in all this time.

RN: Well, he had anything but a marvellous time in Burma. He said it was horrible and they were very frightened in case the Japanese came through.

RW: But he got out alright?

RN: Yes, but he said it was horrible, really horrible, and apart from anything else, there was nothing that could be done. If the planes crashed, what could you do? They caught fire and people got incinerated, horrible. So anyway, enough of that. So you went, from the RAF you got spotted as being a likely lad and then they sent you off to, or you went to Manchester University. What did you study there, in Manchester, what did you study?

RW: Chemistry.

RN: Very good.

RW: Ah yes, but we were all ex-service and there were six of us and we were interested in the biochemical side of chemistry. And not only that, there were two of us who were also interested in the physiological side and we said to the professor one day, 'We'd like to do some physiology' and Professor Raper [Herbert Stanley Raper 1882-1951] was the professor of physiology [Brackenbury Chair] and without any fuss I was able to do some of the physiology with Raper and Fred Harper [Alfred Alexander Harper, 1907-1996].

RN: Harper of gut hormone fame?

RW: Harper and Raper.

RN: What did they discover? [Discovery of pancreozymin, 1943]

RW: But anyway…

RN: He was… let me think.

RW: He was professor at Newcastle.

RN: Yes, yes, with Tim Scratcherd [1924-1997]**.**

RW: He was the Society secretary [1954-1960].

RN: Yes. Scratcherd worked with him too, didn't he? Yes, Raper and Scratcherd, yeah?

RW: But my degree was really in chemistry.

RN: Yeah, I think they worked in cholecystokinin or something like that. I think he worked on cholecystokinin, you know, peptide hormones, I think. Raper. But I'm not 100%.

RW: No, he did all this work on the pancreas.

RN: Yes, that's right.

RW: Yes, that's it. Raper & Harper. I got to know Harper later, very nice chap.

RN: Good. So you did a bit of physiology and chemistry in Manchester and then anything to say about Manchester, you stayed there for three years?

RW: Yes, I have something to say. I just got down to work in the first year because I was a bit unsure about it and I kept my hand in with baking. But in the second year I got involved with student activities and mainly on the political side because there was a Socialist Society, which I joined, and I quickly realised it was a front for the communists and it was proscribed by the Labour Party. So a few of us ex-service we said, 'Oh, we want something that's in the main stream of Mr Atlee's setup'. So we lead a breakaway movement and I was the first secretary of the labour club in that faculty and the thing is, the thing is that at Glasgow, Liverpool and Cambridge the same situation had arisen with ex-servicemen being fed up with the communist domination of the left wing. And Fred Jarvis [Frederick Frank Jarvis, b. 1924] in Liverpool, Dickson Maybon in Glasgow… [Dr Jesse Dickson "Dick" Mabon, 1925 –2008]

RN: Yes, I knew Dickson Mabon in Glasgow.

RW: And Bill Wedderburn [Kenneth William Wedderburn, Baron Wedderburn of Charlton, 1927 –2012] in Cambridge, we all got to know each other and we met, we were all on the executive of a national organisation. So these people they all prospered in life: Bill Wedderburn became a life peer, QC; Fred Jarvis, he was the main man in the NUT and Dickson Mabon …

RN: He died young.

RW: You know him probably.

RN: Yes, slightly.

RW: He became an MP. I kept in touch with him.

RN: Yes, he was a doctor.

RW: Yes.

RN: Yes, I knew him very slightly. I was a member of the Glasgow University Labour Club 10 years after you were, and I knew a lot of, I knew Ian Smith,

and I knew Donald Dewar [Donald Campbell Dewar, 1937 -2000], because they were contemporaries of mine. They were all part of the Glasgow University Labour Club. And it was run by a fellow called Arthur Houston who was an ex-seaman and quite right wing, quite surprisingly. In fact the whole set up was very much on guard against socialists like public school socialists like me who had wanted to go much further left than these guys because they were middle of the road Labour and they wanted to keep it that way. And it was university, people tended to be a bit lefty. [laughter]

RW: But my other main interest was mountaineering and I was in the MUMC and I got a Norton motorbike, another one, and I went twice to the Isle of Skye and that's a picture of me on the ridge. We did the Cuillin Ridge.

DM: The Cuillins, yeah.

RW: And I also, the professor was very good because when he knew I'd done German he said, "You must spend some time in Germany" he got me a job as an undergraduate to go to Germany for 6 weeks as a *Reiseleiter* [tour guide] showing English tourists, and then…

RN: You speak fluent German?

RW: Three weeks climbing in the Alps. And then I decided I'd played about enough, when I was coming up to the third year, so in the final year I dropped everything and just did science.

DM: That shows tremendous discipline… the attractions of mountaineering and an active political life… many, many people would have gone down that [route]. So you'd really settled on chemistry but with this strand of physiology as well running along. Were you able to formalise that in any way? Did you take any papers in physiology?

RW: You see I was interested in physical chemistry and I could have stayed in Manchester but there was a very good man who'd actually worked with Dr Weizmann [Chaim Weizmann, 1874-1952] in Manchester in the Great War and he said, 'Oh, you ought to get into biochemistry' and he put me onto Sheffield.

RN: Right, so you went, …. you worked in Krebs' [Sir Hans Adolf Krebs, 1900 – 1981] department in Sheffield, didn't you?

RW: Oh yes.

RN: You started with Krebs?

RW: In 1951 I went to Sheffield biochemistry to do a PhD which I did, and…

RN: Who was your PhD supervisor?

RW: Bob Davies. [R E Davies, 1919-1993]

RN: Okay, I get it because I notice there are some papers with Davies in Oxford as well. Did he go to Oxford as well?

RW: Yes, Davies went to Oxford in ’54.

RN: Right, okay and Krebs went to Oxford when?

RW: ’54.

RN: So you all went to Oxford?

RW: No, no, what happened was this. In the first year in Sheffield I got £260, which was nothing, not enough to live on properly and in the Easter of the first year I decided I would pack it in and go back to the business. I was the oldest, I could have taken over the business. And I said that, I made it quite clear that I would have abandoned science. And luckily they put it up to £300. So I stayed on.

RN: Yes, I had the same problem doing my PhD, I was poverty stricken actually.

RW: It was ridiculous.

RN: And I had to do part-time GP work to eke out my very meagre living. But I quite enjoyed that, to be frank, but anyway, so you started in Sheffield in Krebs’ department and did you know David Smyth (Sir David H Smyth FRS secy Phys Soc) at the time?

RW: Ooh yes, I knew David Smyth. You see after two years there was a research fellowship in Sheffield and Hans Kornberg had had it, he was the first one. And

Kornberg went to America in '53 and the fellowship came up and I put in for it and got it. So it put me on the academic salary scale, contributing to FSFU and that was when I decided that I could make a living as a scientist. So I abandoned any thought of baking.

RN: So when did Christine [Christine Whittam] appear?

RW: Oh, in Cambridge.

RN: Okay alright, so we'll come to Christine in a minute.

RW: But you see when I took my PhD, Hodgkin [Sir Alan Lloyd Hodgkin, 1914-1998] was the External Examiner.

RN: Okay.

RW: Krebs was the internal examiner. I was terrified really.

RN: I bet. [laughs]

RW: It was Hodgkin then who raised the possibility of going to Cambridge.

RN: Very good.

RW: So that came off.

RN: So wait a minute, so your PhD was actually examined in Sheffield, or you hadn't gone to Oxford by then.

RW: Oh no, the PhD was from Sheffield and I kept this lucrative research fellowship in Sheffield for a further year, and then Hodgkin arranged that I would go to Cambridge in '55.

RN: Okay, so you didn't go to Oxford straight away?

RW: No.

RN: Okay, right, so you went from Sheffield to Cambridge?

RW: Yes.

RN: But Krebs was in Oxford by then?

RW: Pardon?

RN: When did Krebs go to Oxford.

RW: ’54. He got his Nobel Prize in ’53 and they tempted him to go to Oxford.

RN: Right.

RW: But I was more interested in following physiology, to go to Cambridge. And I liked Hodgkin.

RN: You liked him?

RW: I liked him.

RN: Apparently he was quite a difficult guy to work with.

RW: Oh, nonsense.

RN: Well, some people say that. I can name a few names but we won’t… David, careful here.

RW: Well, I mean the thing…

RN: Okay, what I have heard is that Hodgkin had a bit of a grudge against Americans and they thought that he was favouring the Brits particularly in the lab and particularly down in Plymouth.

RW: Who told you that?

RN: Various folk. [laughter]

RW: Well, we better not say.

RN: But they were in the same time, but a bit younger than you. Peter Baker's generation [Peter Baker 1939-1987].

RW: When I went to Cambridge in '55 it was like a new world opening.

RN: I bet, yeah.

RW: It was absolutely marvellous because I'd got a Beit Memorial Fellowship, very good pay with it, and the people in the Physiology Department or laboratory couldn't have been more friendly.

RN: Really?

RW: Welcoming from top to bottom.

RN: Very good.

RW: And the professor was Bryan Matthews [Sir Bryan Harold Cabot Matthews, 1906-1986] and Lord Adrian [Edgar Douglas Adrian, 1889-1977] was still giving part two lectures in that first year I was there. So I went to those and Joseph Needham [Noel Joseph Terence Montgomery Needham, 1900 – 1995] was lecturing, Mrs Needham [Dorothy Mary Moyle Needham, 1896 –1987] was there, and David Cahalan **? (RJN**) was there. He was lecturing, and I was friendly with Adair. Did you know Adair? [Gilbert Smithson Adair, 1896-1979]

RN: No, but I know his work.

RW: He discovered four-point iron, haemoglobin.

RN: Yes, and Roughton [Francis John Worsley Roughton, 1899-1972] **and all that lot?**

RW: Roughton, yeah, Roughton was lecturing.

RN: Yes, he was a good lecturer, wasn't he?

RW: He used to go up on his feet.

RN: Yes, he was a very entertaining guy, very.

RW: But I took to Cambridge in a big way and mainly Bryan Matthews. Matthews said, because I wasn't married, he said, 'Oh you must be in a college but it means taking a PhD.'

RN: Another one?

RW: Yeah. it was not unheard of, quite a number of people did that. Rod Quell? did that.

RN: Yes, Clive Ellory did it too.

RW: Has he really?

RN: Yeah, one from Oxford and one from Cambridge, I think.

RW: Anyway Matthews said, 'Oh you must, you'll be alright in Kings.'

RN: Right.

RW: So before I knew where I was I was in Kings…

RN: And you got married immediately.

RW: I got married in '57.

RN: Very sensible. Good. Okay let's do a little bit of the science now. What did you do for your PhD? What sort of science was that?

RW: Well, I worked on electrolyte and water in relation to metabolism. But the prof encouraged people if they had interests to do other things as well, so I had a little venture into oxidative phosphorylation and did a nice piece of work on that but it was not part of my PhD. But the PhD work was all to do with the relationship between ion transporting water and respiration and glycolysis.

RN: So it was all interrelated stuff to the great stuff that came along later? So the background.

RW: You see, when I went, in '51 when I went, Krebs only had 4 academic staff. It was a very small, there were only 4 research students and you had to make everything. We had a home-made flame photometer.

RN: But the Warburg's you could buy many off the shelf, even then, I think.

RW: Well, in '51…

RN: The glassblowers blew the glass for you, I imagine.

RW: Well it was a Domingo and Fine published in '49 how to build a flame photometer and we built that flame photometer and I had to learn manometry with Warburg and you had to do everything from scratch really.

RN: I want to ask you about the flame photometry because I have a family interest in that. There's a fellow called Lionel Naftalin [1915-2011] who built his own in Lincoln and he befriended Peter Baker and Lionel apparently was one of the instigators of flame photometry.

RW: Oh, Lionel?

RN: Oh yes, Lionel is my father's first cousin. He was my first cousin once removed. He just died last year actually. He was 98 or 99, yeah. [LN died aged 96 in March 2011]

RW: Well, you had a cylinder of acetylene and a cylinder of oxygen. You had an optical bench, you had filters of light and you had a scent spray and you simply had aspiration of the liquid and you could measure sodium and potassium. Now that's an interesting thing you see because I had to justify to Krebs to get Quickfit glass-stoppered test tubes because when I went they were still using test tubes with corks with a filter paper on the cork. I said, 'This is ridiculous. You can get ground-glass stoppers.'

RN: That's your background in chemistry. But I would have thought Krebs would have known that? No? Apparently not.

RW: But he would argue. He'd say, and he wouldn't pay.

RN: No, I bet, mean old sod. [laughter]

RW: If you wanted to buy ATP he said, 'Oh, you should extract it from the rabbit muscle.'

RN: But Quick Fit was just coming in about that time, I guess. You know, Quick Fit glass joints.

RW: Yes, they were. But it was a good preparation for Cambridge because Hodgkin was the same. You had to make everything.

RN: Pathetic, yeah. [laughter] Yeah, well it certainly isn't like that now, is it? No, we throw everything away.

RW: Well, I mean, I went you see and I remember going to Cambridge and we walked from Physiology to Trinity and the point was that Hodgkin took me and it was discuss what equipment. And you'd never believe it, in 1955 in Physiology in Cambridge, they had no flame photometer.

RN: I believe it. But they had plenty of oscilloscopes they had all built themselves, I'm sure.

RW: I said, "We need a spectrophotometer, we need…" and I gave him a list of what we needed. And he agreed to that.

DM: Prof, you came to Cambridge with a clear idea of the project you were going to do?

RW: Oh no, I followed what he suggested.

DM: But he hadn't a clear idea of the equipment needed because you're saying you had to give him a shopping list.

RW: Well, you see, Hodgkin had the idea that there should be a more chemical slant to the kinetics and everything that Glynn had done. And Peter Caldwell [Peter Christopher Caldwell, 1927-1979], he got a [motor-]bike the same year as me and he went to Plymouth and Hodgkin said, 'They'll do in Plymouth with ATP and such like, and I could look at red cells.'

RN: Okay, let's just talk about the red cells. I mean two aspects of red cells which are interesting from the chronology point of view: the development of the ghost preparation and the use of ATP and ouabain. Now, Schatzman [Hans J Schatzmann b. 1924] was the guy who discovered the ouabain story, am I right?

RW: I can't remember the actual date. [H. J. Schatzman (1953) *Helv. Physiol. Pharmac. Acta* **11**, 356-364]

RN: Yeah, but I mean you were a very early user of cardiac glycosides for the ATPase but contemporaneously Ian Glynn [ian Michael Glynn b. 1928] was doing the same kind of thing, was he not?

RW: No. Glynn had gone into the RAF.

RN: Right.

RW: He attained his PhD.

RN: He was a medically qualified guy.

RW: He was medically qualified, and in '55 he'd gone from Cambridge and I took over his lab.

RN: Okay, right.

RW: And what are you asking, about …ouabain?

RN: Yeah, well I was asking first of all, [J.F.] Hoffman, Tosteson [Daniel C Tosteson, 1925-2009], **you, you know, Glynn, had all developed the ghost**

preposition which is key to understanding the ATPase really and the vectorial transport. Yeah. So just tell us about that and your role in that.

RW: Well, how that came about was the following: I used to demonstrate in chemical physiology with the Adairs. Now Dr Adair discussed with me about 'laked' blood and the fact that Barcroft [Sir Joseph Barcroft CBE, FRS, 1872 – 1947] had once observed that when a drop of saline accidentally went into 'laked' blood under a microscope, the corpuscles could be seen. That was very important, and that was in the twenties.

RN: The membranes, you mean?

RW: No, Barcroft was working with blood and accidentally he was looking at haemolysate and accidentally some saline went…

RN: Okay, so they reconstituted.

RW: They reconstituted but that had been forgotten for 30 years. And in 1956 Hoffman and Tosteson came to Cambridge and we got on well together and Dan Tosteson had the next lab to me and we decided we would try to make red cell ghosts. And we'd had this clue from Dr Adair who was quite an old man then. Well, he'd be 60. [laughter]

RN: Ancient, yeah.

RW: And Robert Conway lent us a centrifuge, and you've no idea, it worked.

RN: What, the ghosts you mean, or the centrifuge?

RW: We got ghosts.

RN: It has to be a reasonably good centrifuge to get ghosts. You need to spin them at…

RW: Yeah, and the Tosteson said, 'Instead of using this old fashioned big centrifuge' he'll get a Sorvall which he did.

RN: That was a bench top one or was it floor?

RW: Yeah, and it never worked. We could never make ghosts again with a Sorvall centrifuge.

RN: Why not?

RW: It was too clever by half, and with the slow centrifuge, with hindsight, it was because it took time…

DM: To come up to speed.

RW: And temperature to allow the membrane to set. And the big part of my thesis in Cambridge was the investigation of temperature, tonicity, … everything like that.

RN: So in other words you had to, I remember you had to incubate for about half an hour at 37 [deg C] to get the thing reconstituted.

RW: Yes, and…

RN: And you discovered that, did you? It was in your thesis?

RW: Yes, we discovered that.

RN: And then putting the ATP into the ghosts, who discovered that?

RW: Well, I guess it was me.

RN: Okay, so do you use that preparation very quickly to work out the stoichiometry of the pump, with the potassium?

RW: No, the big thing in Cambridge, I showed how to make the ghosts but I didn't work out the stoichiometry at that stage. That came in '62. That's the best paper I ever published.

RN: Yeah, okay let's keep on with it.

RW: You see, when it came to 1958, when I was married, I wanted a job and I thought about different places to go and then out of the blue Krebs said, 'You can come to Oxford.'

RN: Right.

RW: So I went straight to Oxford from Cambridge and settled in Oxford for how many years, 58-66. And it was in Oxford that I did the reconstitution with the, and the stoichiometry.

DM: Can I just ask, Prof, did you interact at all with Richard Keynes, who must have been looking at [Na-K]ATPase at the same time but mostly in Plymouth, yeah?

RW: Oh yeah. You see one thing that got me into physiology, apart from just an interest, was the way they enjoyed themselves and I saw that in, I went to the SEB famous meeting in Bangor in North Wales, went on my motor-bike. And Hodgkin and Keynes and Monty Maizels [Montague Maizels, 1899-1976] was there.

RN: And Conway [Edward Joseph Conway, 1894–1968]**, the guy from Dublin.**

RW: Yeah, what's he called? Conway.

RN: Yes, that's what I said. Ronan [*sic*] Conway. And there was another guy, Kernan.

RW: They all enjoyed themselves.

RN: Yes, of course, why not?

RW: I can remember I went climbing on Tryfan [Snowdonia] and Bob Davis would take my climbing rope and he went on my pillion. And then another thing about climbing, in '51 the Sheffield Mountaineering Club had a dinner and I got a free dinner as a representative from the MUNC from Manchester. But then in '52 the famous physiologist, Graham Brown [Thomas Graham Brown, 1882–1965], you've never heard of him?

RN: No.

RW: Well, it was Sherrington [Sir Charles Scott Sherrington, 1857-1952]

RN: Okay.

RW: [Graham Brown was a] Famous FRS, professor in Cardiff and he was independently wealthy. And in '52 he was a guest of honour at the Sheffield dinner. He came to the dinner and I saw how he enjoyed himself and then he went climbing the following day and he'd retired. Anyway, but I saw, and then in Cambridge the physiologists all enjoyed themselves.

DM: So you didn't interact with Richard Keynes [Richard Darwin Keynes, 1919-2010] at all?

RW: Oh, I knew Richard, yeah.

DM: Because he was obviously also working away at the [Na-K] ATPase from a different, slightly different perspective.

RN: I don't think he was working on [Na-K] ATPase at all actually.

DM: Oh, in squid axons. [Keynes RD & Caldwell PC (1957) *J. Physiol.* 137, 12P–13P]

RW: He was a phenomenal supervisor for me. But he didn't do anything on it.

RN: No, he didn't. No I mean there was Skou [Jens Christian Skou b. 1918] **in Denmark who was doing the enzymology but were you influenced by that at all?**

RW: When was Skou's first paper, I've forgotten. [first sodium pump paper; *Biochim Biophys Acta* vol 23, 1957]

RN: Oh about '52, something like that.

RW: You see we should have discovered that in Cambridge and in my '58 paper I actually describe within whole red cells that ouabain had - I call it - a sparing effect on the breakdown of ATP. And it was inhibitory affect, but I never…

RN: **But you didn't measure oxygen or did you? I can't remember whether you did. I've got the papers here. But you looked at the metabolism of some kinds of cells, kidney cells was it? Kidney slices and…**

RW: Oh, that was much earlier.

RN: **Right, but you did look at oxygen measurements there, didn't you?**

RW: I made oxygen measurements.

RN: **And you had looked at the effects of ouabain or digitoxin or something like that?**

RW: Yes, yes.

RN: **And you showed that…**

RW: And that was the other big… yeah, according to Hans Kornberg [Sir Hans Leo Kornberg, b. 1928]… there were four things which I discovered and he said it was much easier in biochemistry and physiology if you just did one thing. But I messed about and I played about…

RN: **Yes, that's been the bane of my life too. Go on.**

RW: Well, I did the biochemistry, then the physiology, and one of the four things I did was show the pacemaker effect of the sodium pump on metabolism.

RN: **Really?**

RW: Oh, didn't you know that?

RN: **Pacemaker, in what sense, sorry? Let's be clear.**

RW: In the sense, it's a bit like the oxygen debt with muscle.

RN: **Yes.**

RW: In the sense that if you impose a demand on cells following a period of excitability in nerve the demand for ATP for the sodium pump elicits an increase of glycolysis or respiration.

RN: Yes, alright okay.

RW: So I proved that.

RN: So loading intracellular sodium put up the level of metabolism.

RW: Yes.

RN: And oxygen consumption.

RW: It was the same with glycolysis.

RN: Yes, well in red cells you had to.

RW: That was in Oxford I did that with red blood cells.

RN: Yes. Now Margaret Agar and you published two or three papers together, didn't you?

RW: Oh, it was, you see, when I got to Oxford the Chemistry Part Two allowed Honours chemists to do a Part Two one year research in biochemistry. And for some reason I attracted several of them, well half a dozen, and there was Margaret Agar, she was very good; David Blond, very good; Ken Wheeler, and they were all Part Two chemists and then they said they'd like to stay and do a DPhil, which they did. But there was a very bright one called Tony Blake, he got a First. Anyway that was in Oxford.

RN: So Ken, okay we'll come to Ken in a minute because he came with you to Leicester, didn't he?

RW: Yes, he did. I'm sorry, I think I'm taking the initiative from you.

RN: No, no, no I'm happy to do that, don't worry.

DM: No, it's going well, we're now into the meat of your main science.

RN: This is about you, not about me, so I'm happy that you should lead because I'll follow and give you a hint. But it sounds, okay, so the clear story of your science was that you've discovered these ghosts or you've got the system working and you could adjust the intracellular concentrations of sodium and potassium, which was a big thing really because the whole of electrolyte physiology depended on this. The only other comparable preparation was the squid axon really and that wasn't as neatly handled really in many ways because you only had one squid axon and you had to do it in time whereas here you could have these test tubes.

RW: I think the asymmetry of an enzyme system was the fascinating thing because any biochemist would say that you couldn't get an action from opposite sides of a membrane.

RN: Yes, well in fact it was anathema. The whole idea of an enzyme is to have a uniform substrate surrounding it whereas here with a transporter you've got two sides, two phases, something which I've been obsessed with too for the whole of my physiological career. But you got there first and were doing this vectorial stuff and you must have been interested in what was going on outside for this particular, rather narrow, field in terms of vectorial transport, in terms of the Katchalsky [Aharon Katchalski-Katzir, 1914-1972] stuff, I know you were interested in.

RW: Well, you see this was part of the wisdom of Krebs because, as I said, he encouraged people to diversify and he said to me one day, we bought a house in Oxford, we hadn't any children then but in 1958/59, no '58, we'd only been there in the first year and he said, 'I think you should spend some time in electro physiology'. And he would like me to go to work with Nachmansohn [David Nachmansohn 1899-1993] at the College of Physicians and Surgeons New York. And I don't know how it came about but he had this idea that I should go to America and I went there for 7 months. Chrissie and I went, we sailed each way and we had a lovely time in Manhattan.

RN: Whereabouts did you live in Manhattan?

RW: 180 something street. It was right north.

RN: Yes, north of Manhattan, in Harlem?

RW: It was up in Columbia near the Washington Heights.

RN: Okay, not a very nice area around there as far as I remember.

RW: It was lovely.

RN: Was it?

RW: You could walk from there and overlook the river.

RN: Right, okay.

DM: And you were working there with David Nachmansohn?

RW: With David Nachmansohn. And we turned up…

RN: Well, when I went there, I visited Columbia the College of Physicians and Surgeons, and they said, 'Be very, very careful because you could get easily mugged on the front' because there was quite a rough area then.

RW: Well, you see it was, West Side Story, there was a lot of shooting [laughter]. We had a little flat, it were very nice.

RN: Yes, I'm sure it was.

DM: You were brought up in Glasgow, Richard it can't have been so intimidating for you, New York.

RN: No, not at all. Well anyway, it was pretty bad too.

RW: I'm very, very proud of this work that I did there because it was real electro physiology. And it was more like, it was with the single cell electroplaques and the electric eel, the cell had an innervating membrane and a non-innervating membrane and you could dissect out a single cell.

RN: The electroplaques.

DM: Single plaques, yes.

RW: We had a system, like the Ussing chamber, and you could put electrodes and you could stimulate, and you could measure the release of potassium with one spark, with one action potential. And I was the first one to show how much potassium is released from a cell of a mammalian…, well, what was it?

DM: An eel. A vertebrate.

RW: And that paper in 1960 is in the *J Gen Physiol*, a very nice paper which shows what you can do with that electroplaques. If you didn't know that, but it's real physiology that.

RN: Absolutely, absolutely, yeah.

DM: Of course, Nachmansohn later became a bit of a heretic with his views about acetylcholine and transmission and so on and slightly lost the plot, I think. But obviously along the way he'd done a lot of really important science.

RN: They used the electroplaques in many ways: they used it for ATPase, they extracted the ATPase out of it too, I think.

RW: Nachmansohn knew everybody … Ernst Chain [1906-1979]…

RN: Well, everybody knew everybody then.

RW: Everybody knew Nachmansohn.

RN: Yeah, well it was a fairly small world. I mean, there were only a few places doing physiology in Britain and there were a few places in the United States doing it.

RW: We travelled all over the place. Art Solomon invited us, A K Solomon, and we went to, we went to see Post in Nashville, we went to see Dan Tosteson in St. Louis, Jo Hoffman in Washington.

RN: He was in Washington, was he?

RW: He was at NIH. And we went to Philadelphia. We had a lovely time.

RN: Yes, yes, it's a good place actually, or it certainly was. It's still quite a good place.

RW: Have you been in America?

RN: Yes.

DM: I've never spent time working there, no.

RN: I had a wonderful time in Yale, when I was at Leicester and we went to Yale in 1972 and it was very impressive. But I also visited NIH and since then I've been several times to a place called Mount Desert Island in Maine, and I've been to San Francisco. I worked with a fellow called Verkman, Alan Verkman, very good he is. And I was very friendly with Arnost Kleinzeller [1915-1997], did you know him? He came…

RW: Kleinzeller, Arnost Kleinzeller, of course I know him.

RN: Yeah, well I think he visited you in Leicester.

RW: He's been to Leicester.

RN: Yes, he came and we got friendly so…

RW: Oh I met him at Mosbach. He came to see the Neckartal in Germany.

DM: It's the town we're twinned with in Lymington, we're twinned with Mosbach so I know it very well.

RW: Are you really?

DM: Yeah, know it very well.

RW: He was so poor, he was allowed from Communist…

RN: Yeah, he had a hell of a time.

RW: Terrible time.

RN: It's amazing he managed to do anything at all but he did, he was great.

RW: He finished up in…

RN: Philadelphia, yeah.

RW: I remember he visited Leicester.

RN: Well we were good friends, he and I, and his wife, Lotte.

RW: But you see I wasn't on the academic staff in Oxford, I was employed by the MRC. Krebs gave me this job and then…

RN: Wanted to keep you for himself, I guess.

RW: Yeah, that's right. But anyway he gave me a lectureship, 1st March 1960, I became a University Lecturer in Oxford.

RN: Right. So you knew, you must have known [D S] Parsons pretty well.

RW: Ooh, I knew Denis, yes. Friendly with him.

RN: Yes, yes, well tell me about Parsons. I mean Parsons had a fairly important role but I don't think his contribution was really recognised properly.

RW: Well, I don't think Fisher was either. [David] R B Fisher. I think both of them were very good.

RN: I mean both of them could have been leaders in the intestinal physiology field but didn't. Some people say it was because they were a bit lazy.

RW: I don't think that at all. Denis Parsons had a tremendous influence on people in Merton, he was an excellent tutor.

RN: Absolutely, absolutely.

RW: And he was a very good…

RN: I mean his school of physiologists, the people who owe their careers to him is quite established.

RW: Ramsay Bronk worked with Fisher.

RN: Yes, and Richard Boyd.

RW: I liked Fisher, Fisher was a nice chap.

RN: Yes, and Chris Cheeseman. But Fisher went to Edinburgh, didn't he?

RW: Yes. I got on with him. I'll tell you a funny thing about Fisher, before I was a member of the Phys Soc, David Fisher introduced me to give a communication in Glasgow, and I got to know [Robert Campbell] Garry [1900-1993].

RN: He was my prof at Glasgow, very good he was too, a very nice guy.

RW: Anyway I'll come back to Garry when come on to Leicester. Are you okay?

[….]

RW: …. We're in America.

RN: Yeah, we're in America. So you had this good time in Physicians and Surgeons in Columbia in New York and then you came back to Oxford?

RW: Yes.

RN: For how long? Till '66? So how long, you were in New York for a year, I guess?

RW: No, I was only 7 months in New York but you see, once I got a lectureship it changed the setup.

RN: Yeah, I bet.

RW: And I had to be in a college.

RN: Which college were you?

RW: New College.

RN: Right.

RW: But New College couldn't even give me full dining rights.

RN: Oh dear. Is that bad? So what?

RW: And then I was friendly with Vaughan Williams.

RN: The composer?

RW: No, Miles Vaughan Williams, the pharmacologist, who worked in…

RN: No relation?

RW: But he was a Fellow of Hertford. And then out of the blue one day I got a letter from Hertford saying they'd made me a member of the Senior Common Room with unlimited rights. So…

RN: So hold on. I've been to Oxford and Cambridge a lot because I've examined there and stuff but I have never really been sure about the pecking order. What did it mean to you not to have dining rights? I can't really believe that meant a lot.

DM: Did you go hungry? That's the question [laughs].

RN: What of it?

RW: I can show you a letter where for instance New College made me a College Lecturer. You have no idea what it was like in Oxford. Before the Franks Commission, when was that, about 1992, they put it into effect, there were a lot of University Lecturers in Oxford who were not college fellows, did you realise that?

RN: Yes, but there were a lot of college Fellows who weren't University Lecturers either.

RW: That was because the science Heads of Department wouldn't have them.

RN: Yes. But I mean there are a lot of people who sort of hang around Oxford and Cambridge getting nowhere all their lives, doing college tuition and things like that. Terrible system I think, awful, if you hang on and hang on.

RW: I knew three people who couldn't get a job with Krebs in the department but they had big college connections.

RN: Yes.

RW: And he wouldn't have them.

RN: Yes.

RW: But you see I was quite happy actually, I didn't rebel against it.

RN: There clearly was some sort of pecking order if you were acceptable to the college hierarchy, I suppose, you got to be a Fellow and that was in their gift, I suppose and if you were in, you were in. And that was you set up for your life, I suppose?

RW: Well, I don't know. John Widdicombe [John Guy Widdicombe, 1925–2011] had the physiology Fellow, Sybil Greenwood [??]. But John Widdicombe he was Phys Soc treasurer.

RN: Yes, he would have been then.

RW: But he was a physiology tutor at New College and he arranged for me to have a College Lectureship. That's lower down in the pecking order.

RN: Oh, I see.

RW: And I've got a letter that said I have one free dinner a week during term time [laughs] and a dinner on the week before, a week before and a week after. I'm sorry if I'm shouting.

DM: No, no, it's fine for us.

RN: No, it's quite interesting. It's a bit archaic frankly now. I mean obviously these things mattered then a great deal.

DM: Was it important academically to sit at the dinners? I mean what those of us who haven't been involved in this scheme fondly imagine is you sit around with these great brains over the dinner table and the world and your discipline are put to rights. Was it not like that? Was it used for small talk?

RW: I mean to be absolutely honest, I got on better with the people at Hertford rather than New college. New College was a very, it was high up in the pecking order, very wealthy. Lots of endowments and so on. Steven Rose was a research Fellow.

RN: I knew him slightly, not well, but I knew him. He's not contemporary with you, though, he's contemporary with me?

RW: Yeah, but he spent some time in Oxford.

RN: He was with McIlwaine.

RW: Yes. That was a non… never mind.

RN: Yes, let's not talk about Stephen Rose.

RW: But Hertford was a very nice setup.

RN: I don't know much about it. So you had a good time as a college Fellow with dinner?

RW: No, I was not a college Fellow.

RN: As a lecturer. But you could have both: Lecturer and Fellow.

DM: So did you have tutorial responsibilities or not?

RW: Oh, I did a lot of tutoring.

DM: Oh, you did have that?

RW: That's how I got the part, I was quite OK there.

RN: The thing of it is you get paid extra.

DM: Yeah, you get double salary basically.

RN: So I mean it's worth a lot of money.

RW: I'll tell you one thing about Oxford. It really encouraged me a lot. The professor of physiology was Liddell. When he retired, Lindor Brown [Sir George Lindor Brown FRS, 1903 – 1971] came. And you'd never believe it, I was just a humble Lecturer, mind you I was in the Phys Soc by then, and Lindor Brown, I guess he was Sir Lindor, he appeared one day at the door of my lab in Biochemistry. And he walked across, instead of sending a letter, something like that, he himself came across to ask if I would be a moderator for some exams in physiology. Lindor Brown, he was so good in looking after people. I mean Denis Noble came to Oxford and Lindor Brown, you see Noble became a Fellow of Balliol straight away.

DM: Otto Hutter, who I know very well from Glasgow of course, and he lives near me on the south coast so I see quite a lot of Otto, and he was a student of Lindor Brown. He tells a nice story of going for his interview to UC and he was finding his way around the corridor and this chap was there in the corridor and he explained, he said, "Can I help you?" And [Otto] said, "I'm

looking for Professor Lindor Brown" you see, and he said, "Well, I'm him." And that's just the way he went. He was very, very friendly, no affectation with him. A remarkable man, Otto speaks very, very highly of him.

RW: He was a very nice man.

DM: Actually I don't know if you know the anecdote about how Lindor Brown supposedly got the Oxford job? He was on the selection panel, apparently, for the Oxford chair and the panel was very dissatisfied with the level of candidates and, apparently he said; "Well, I suppose I could go for it." So effectively [he] proposed himself. That's the story. [laughs]

RW: Whilst we're on about Lindor Brown can I jump ahead and tell you something about…

RN: Sure, go ahead, you're in charge.

RW: When I was offered the chair in Leicester, Krebs and Hodgkin were very helpful, congratulations all that, but they didn't get involved in how you go about coping with the policies. And Lindor Brown heard in Oxford that I'd got…

RN: Just before we get to that, what made you apply to Leicester? You were in Oxford, why come to Leicester?

RW: Well, that's a good question. Well, I think it was three things really. One was money. The other was, it was the first chair of General Physiology since Bayliss [Sir William Maddock Bayliss FRS, 1860 -1924] had one, so that was a distraction. And third, you got hypothecated funds. Do you know what I mean by that?

RN: Where you got research funds.

DM: Earmarked.

RW: Earmarked from the UGC.

RN: And we were well off then, I remember.

RW: And it was Hans Kornberg. You see Hans Kornberg from Sheffield and he was here and he said, 'You'll be alright in Leicester'. But anyway…

RN: So he was here a couple of years before you, I guess?

RW: He came two years before me.

RN: So it was a kind of an incentive that he was here?

RW: I'm sorry…

RN: The fact that Hans Kornberg was here was an incentive?

RW: Yes, it was.

RN: Right.

RW: But I'm sorry, we're jumping ahead.

RN: No, but even so it must have been quite a jump to come from Oxford to a new department, to set it up and all that, and have all…

RW: Well, I think somebody said I was born there. And the other, did you know, you knew David Rosenthal, I guess, in Leicester?

RN: No.

RW: Noel Simpson. They were consultants.

RN: No, I didn't know them.

RW: One of these consultants, he just died about age 92, he said to me in other year, 'You should have stayed in Oxford, you know'. [laughter] Because I had such a rough time in Leicester.

RN: Yes, well we'll talk about that in a minute. I'm just still interested in the motivation. What on earth made you want to leave Oxford which was really one of the premier universities in the world in physiology to come to Leicester which was nowhere? I mean you had to set it up all on your own.

RW: You see Krebs was coming up for retirement and there was no indication of who would take over. And as I said, the money, there was a money incentive, and I think I was a bit annoyed.

RN: Was there much money involved?

RW: No. I think I was annoyed that I hadn't been given a Fellowship.

RN: What, of a college?

RW: Of any college. I mean they should have made me a Fellow and I would have stayed in Oxford. [laughter]

RN: Yeah, okay.

RW: I couldn't understand it. Anyone nowadays, since 1992, anyone who is a University Lecturer would automatically be a college Fellow.

RN: So you felt excluded from the inner circle, so to speak, of the collegiate story.

RW: I was kept out.

RN: You think it was snobbism?

RW: No.

RN: You don't? So why do you think you were kept out?

RW: I think it was logistics, that there was a genuine shortage of places for college Fellows.

RN: Okay, I see, fair enough.

RW: I never, in all the 10 years that I was in Cambridge and Oxford, I never experienced what you might call snobbery.

RN: Fair enough.

DM: Good.

RW: I don't think it was that.

RN: Okay, we'll exclude that. So but you'd still…

RW: But with hindsight, I mean you might say, 'Why did I take early retirement?'

RN: We'll come to that later.

RW: But I mean I think if I'd stayed in Oxford I could have had a comfortable life.

RN: I'm sure, I'm sure, why not?

DM: So when you came to Leicester, Prof, I started in Leicester as an undergraduate in '66 in the Astley Clarke building and the Adrian building was just a pipe dream at that point, although, in fact plans, must have been fairly well advanced. So you must have come with no decent department.

RW: There was nothing.

DM: There was literally nothing.

RW: You see this is where Krebs was so good because in 1964, I don't know if he had an inkling or not but, I'd best look into this, there was a meeting at Colorado, not Denver, where was the other place. Where did they have…?

DM: Boulder?

RN: Was it Boulder?

RW: When I was in Oxford there was a man called Francis Schmidt…

RN: I've heard of him.

DM: F O Schmidt.

RW: And he organised big jamborees, big meetings, and it was a place called Boulder. And everything paid, they invited me, Stephen Rose was there, and it was at Boulder in '63 that I met Aharon Katchalsky [1914-1972]. He was the most inspirational, exciting, gifted person really. He was a visionary, Katchalsky, and I gave my talk about asymmetry and so on and he said, "It'd be nice if you could come to [the Weizmann Institute of Science] Rehovot" and I said, "Yes, it would." Anyway the wheels started turning and before I knew where I was there was a Fellowship from the Royal Society called the Bruno A Mendel Research Fellowship and it was available from funds provided by Mendel who became a rich man from pharmacology. He was an FRS and he left a lot of money for someone from Holland, England or Israel to work in one of the other countries.

RN: Right, very good.

RW: And I was, in 1964, he died young but I was the first Mendel fellow to go to Israel.

RN: So how long were you there?

RW: Seven months.

RN: And what did you do?

RW: Swan around. [laughter]

RN: That was in '64? So there were no wars then.

RW: We went in, when did we go? I got the Chair in Leicester, November '64 I was offered the Chair here but I was lined up to go to Israel about August '65 and we bought this house in May '65, we bought this house. And Oxford were very

good because they said they'd pay my lectureship money for two terms whilst I went to Israel till the end of March…

RN: And you had the Mendel fellowship, so you must have been wealthy. Yes, very good. So it allowed you to spend money on the house.

RW: We left this house, we left it and we went with the children and we had a lovely time in Rehovot.

RN: Very good.

DM: So you spent time in Katchalsky's department but you didn't really do much there you think?

RW: Well, I tried to do some research with Izzy Edelman [Isidore S Edelman, c.1924-2005]. Izzy was there and we, did you know Edelman?

RN: Slightly. I met him with E J Harris, he visited E J Harris for a while in Biophysics at UC and I was doing my MSc then and he was a great guy.

RW: He had a daughter the same age as my daughter and they used to go to the same kindergarten. But that wasn't really with Aharon; that was with his brother, Ephraim.

RN: Yes, he was the President [of Israel].

RW: Ephraim Katchalski?

RN: He became President.

RW: He was a protein chemist. And he suggested to me that we should try to put the protein of the sodium pump on an artificial membrane.

RN: Yes, good idea.

RW: We did a lot of work on it but it just didn't come off.

RN: Well, it had to be in the membrane, didn't it? Yeah, that was, you were 15 years before your time.

DM: Too early.

RN: Because, anyway, interesting. I was in E J Harris' lab when Roger Thomas was the first to show that there was an electrogenic current coming from the sodium pump, I think it was in the snail.

DM: Snail ganglia, that's right.

RN: And I was there when he showed the electrogenic pump. Very impressive. I don't think I was sufficiently appreciative at the time but we're very good friends, actually.

RW: Roger Thomas?

RN: Yeah.

RW: I never really knew him.

RN: That's all right. There's still time. [laughs]

RW: I got so enamoured of Israel that many years later I went on a lecture tour for a month on my own. And it, but we were there in '65-'66 before the '67 war, the Six Day War.

RN: Yeah, well it hasn't really finished.

DM: True. [laughs]

RN: So well let's just round up the story on the sodium pump because you had more or less worked that out before you got to Leicester, hadn't you?

RW: It was yes, except for two things: one was the nature of the protein, but this is where *phosphatidyl*serine came in.

RN: And you did that with Ken?

RW: Yes. With Ken Wheeler. That was important.

RN: Yeah, but the whole vectorial stuff…

RW: That was finished.

RN: That was a sort of combined effort. I mean there were three or four people involved in it, I mean not just you: Glynn and Hoffman.

RW: Yes.

RN: So the world came together really. You had this rivalry, I guess it was. Did you feel a sense of rivalry with Ian Glynn and Hoffman or did you feel you were collaborating on a bigger project than you could cope with on your own?

RW: No, I never really, I never had any…

RN: But there weren't any contradictory results actually. Everybody agreed with everybody which was really good.

RW: I always have a glass of sherry about this time. Would you like one?

RN: Yes, sure, why not?

RW: Christine's made some sandwiches.

DM: Fantastic, that's very kind.

RW: Do you want a sherry or would you rather have a shandy or something?

DM: No, I'm happy with a small sherry, that will be fine as long as it's not too large. I'm driving back to Lincoln later.

RW: I think you've got a good point though about Oxford. I mean I did like Oxford.

RN: Well, it wasn't a point, I was just curious to know. If I had been in your position I doubt very much if I'd have come here myself. But I don't know, maybe I would.

RW: [unclear 1:38:19] had me as his External [Examiner] later.

RN: Yes, yes. Well I suppose the other thing about Leicester was the independence. I mean you could set up your own show really except it didn't quite work out like that.

RW: I think it was money. I think… Have you got enough?

RN: I don't really drink in the middle of the day.

DM: That's fine for me.

RN: Yeah, so okay, let's go to Leicester.

RW: Is that enough?

DM: That's fine, thank you very much, Prof.

RN: We're in Leicester now and you set up the department and there were… you appointed everybody did you? How much discretion did you have in making the appointments here, the first appointments? There was Oliver Holmes [1933-2004] and Reg [Chapman, 1937-1995], and John Tunstall, I guess?

RW: Well before that there was a problem with the terms of the appointment because they offered me the job and I was not prepared to come under the terms.

RN: Right, which were?

RW: And the main thing was—I must be careful what I say—that the professor of general physiology was to be responsible to the Professor of Zoology.

DM: Professor Moon [H Philip Moon, 1910-1982], yes, yes.

RW: And in Oxford when you've been a lecturer for 5 years you were no longer responsible to the Head of Department, you were responsible to the Faculty Board. And I'd been over 5 years so I was not responsible to Professor Krebs. So I would have been crazy to come here to be responsible to some professor…

RN: Well, it depends what they mean by responsible.

RW: Exactly, exactly.

RN: What does it mean? What did it mean?

RW: Well, I didn't go into that because Lindor Brown put me right about what to do. And he came to me in Oxford and he said, "Don't touch Leicester unless you have control of five features: you must have your own budget, your own academic staff, your own space, your own technical staff, and you must be completely independent of responsibility: responsible to Senate.

RN: Very different now.

RW: Well, Lindor Brown told me that and I wrote a letter to the Vice Chancellor and it took them a week to decide that they could agree to this.

RN: Well, a week's not long.

DM: No, that's fast decision-making. So you really did have an autonomous department.

RW: I wanted…they tried to sell the story that the UGC would not finance a separate Department of Physiology here and the only way they could get physiology into the School of Biology was to call it General Physiology. Well I don't think they had any idea really what general physiology was.

RN: Well, that's what you were doing.

RW: That's what I was doing.

RN: That's what General Physiology was then, and still is to a large extent.

RW: Yes, General Physiology appealed to me. You could come from Physics or Medicine or anything. So when they agreed to the terms I then decided, partly with Lindor Brown advising, you must get a bona fide senior physiologist and Oliver Holmes came. And he was a gem.

RN: Yes, okay.

RW: And then Reg.

RN: And then Reg, yes. So Oliver came more or less the same time as you did then?

RW: I appointed them.

RN: You weren't here when they were appointed. You appointed them before you came here? Or did you?

RW: Well, I think Oliver came in… you see there was no lab at all. I mean as David said the old ,Fielding Johnson building.

DM: Oh was it Fielding Johnson, you were there? Oh right, okay.

RN: So the Adrian building was up and coming when I came which was in '68 and it wasn't that new, so it must have been opened in what…

DM: It was opened in '67. It was a building site when I arrived in '66.

RW: Yeah, '67, the Adrian building. The nice thing was, this was the other nice thing, because I'd insisted on separate funding the UGC appointed two assessors to vet the application which I made. And do you know who they were? Two previous Secretaries of the Phys Soc. And there was George Bell and David Witteridge.

RN: Okay.

RW: And they—I was a bit extravagant —I got £200,000 in 1966 to set up the department. That was a lot of money.

RN: Well, it was well equipped - then.

RW: And I also, I applied to the MRC for a research group and they gave me money to set up a research group in membrane transport.

RN: So you had at that time you had Ken Wheeler and Neil Priestland?

RW: Yes, Neil Priestland. And then David Cotterrell and Alan Chipperfield…

RN: No, no, he came later. Sorry, he came later because I was here when Alan came.

RW: He was a winner.

RN: Yes, he was. We'll talk about Alan for a minute but possibly off the record. But yeah, no Alan came in '69.

RW: Well, there were one or two good people here from the very beginning.

RN: But Oliver Holmes left fairly quickly didn't he? He went off to Glasgow.

RW: He went to Glasgow.

DM: Went to Glasgow, yes. He was there when I went.

RN: Yes, I mean my impression was you didn't actually hit it off all that well with Oliver, did you really? I mean when all is said and done.

RW: It was a question of space and so forth.

RN: But he had tons of space, Oliver, for what he was doing.

RW: He had his own lab and so on. I don't know what he said about me.

RN: No, no, no, he said nothing about you and it certainly wouldn't have said it to me, that's for sure. But he had this huge lab, if you remember, on the corner on the upstairs and God knows what he was doing.

RW: There was Andrew Short and Jane Houchin.

RN: That's right, Jane. That was it, I think, and there was a technician, Josie.

DM: She worked with Reg eventually, yeah.

RN: But she worked with Oliver.

DM: Yes, she worked with Oliver first.

RW: Well, they could do what they want.

RN: They could but they weren't happy, were they? No, they were chronically unhappy.

RW: Well you see I took the view when I came to Leicester that I would not publish anything and I would not influence anybody who was on the academic staff. I would not collaborate in research and I never did. I never published a single paper with anyone on the academic staff.

RN: Well, we nearly managed to get one. [laughter]

RW: Well, we chatted.

RN: Lots of chatting, yeah. But that was a mistake actually, I think. It's bad to have people sort of captured but I think collaboration is a good thing in general if it's constructive.

RW: It was absolutely marvellous with Bell and Witteridge because…

RN: Yes, well you were very lucky.

RW: It were marvellous.

RN: That was an almost unique period in British science when there was money in the system and plenty of it for capital needs. And it hasn't been like that ever since, and it's not going to be like that…

DM: Did you get any significant funding from the university itself, or was this simply setting up this new unit at Leicester gave you an opportunity to apply to MRC? Did you get significant funding through the university?

RN: No, there was UGC.

DM: There was the UGC fund, okay.

RW: But the University were parsimonious, they were terrible.

RN: Well, not that terrible. There was basic equipment, there were centrifuges and things like that, as far as I remember. And that was common and there were counters and that sort of thing which these days, these are hard to come by. And the other thing that's hard to come by now is maintenance of equipment which is phenomenal…

RW: Well, they brought it on themselves.

RN: Yeah.

RW: I've no sympathy at all with the plight of the universities now because they…

RN: Go on, we're interested and following, yeah.

RW: If they'd made a stand against Rothschild in the mid-seventies they wouldn't be in the situation they're in today. It all stems back to the disrupting influence of the Rothschild Report.

RN: Can you explain that?

RW: Yes, I can explain. Edward Heath [Prime Minister] set up a think tank because he wanted an explanation of what benefit there was coming from the research money to the Research Councils. The Research Councils, in particular the MRC, didn't have to justify in terms of material benefits from the results, from the work that was being done. Now Rothschild came along and said, 'Oh, we must have a mission statement.'

RN: Yeah. He wanted control, in other words.

RW: It was to put control on the part of the…

RN: Government?

RW: Yes. And I was on the Research Board of the MRC, the Biological Research Board, and you see this is, I had been on it three years and, are we still recording?

DM: Yes, we are.

RN: Do you want to? If you want…

RW: Well, no, I was going to bring somebody in but I won't mention his name.

RN: Well, we can edit it out.

RW: You probably don't know the ins and outs of this.

RN: No, we definitely don't. Well, I don't anyway.

RW: Well, the terms for the MRC, following Rothschild, changed completely and they had to have some indication where there'd be a benefit. Under the previous system, that went back to the beginning of the MRC, there was no such thing as trying to concoct some benefit. Certainly Alan Hodgkin was against it, this new system, and I had been co-Chairman of the research board with Rodney Porter. Rodney Porter was the Chairman. He succeeded Krebs and he asked me to be Chairman with him of the Research Board, so I was for a year. And then when it came to the Rothschild business, John Gray wrote and said, "Would I continue?" And I said, "No."

RN: Yes, I remember you being pretty angry for some reason. You didn't really explain it at the time but go on.

RW: And Doug Wilkie was on the Biological Research Board and quite independently from me, he took the same decision. And Doug Wilkie and I would not continue with the MRC to give advice and take part in what they were doing. And Ian Glynn took over. He took over and they got a different system. Now I'm not criticising Glynn if that's what, but it didn't suit me to have that new system.

RN: Okay, so what was your particular beef about the system? I mean let's be quite clear. Why didn't you like it? Okay, it had to have some sort of mission statement, you say, and justify itself which of course is quite impractical in real terms.

RW: It was part and parcel of concentration of power in Whitehall rather than being subsidiary control of your money. And the same thing was happening all different walks of life. (I'm sorry if I'm shouting.)

RN: No, no, no, it's alright.

RW: It's the same with the police. You get a chief superintendent with 600 a year to run his set up. [Sir John] Harvey Jones was the man who was a trouble-shooter and he pointed out the same phenomenon with the armed forces, that they hadn't got the confidence in the people at the lower level, was eroded.

RN: Okay, so basically it was a takeover by the Civil Service of the Executive, of the scientific councils, of the universities, of the armed forces, of the Law too, to some extent, I'm afraid, particularly the police force, and in education. We see the same problem these days that in fact the central government is controlling how education is being run which is probably wrong. So I mean I agree that this is a problem and it stems, I'm not sure it necessarily stems from Rothschild but…

DM: I think some of us would see, in Margaret Thatcher's period, the big change that occurred was this same strand that you're describing, Prof, what is being delivered for money. I remember particularly she was incensed that molecular biology at Cambridge hadn't patented things like cloning and sequencing, techniques which we all know were, a huge amount of the developmental work was done there at Mol Biol Cambridge, and she couldn't believe that nobody had any eye on this. So this is some years after, or just a few, one term of office after the Rothschild report but it's interesting, very interesting that you put the origin to this back there.

RW: I put that as one of the seminal…

DM: Yeah.

RN: Yeah, well I think it's related to party politics frankly. I also think it's related to the nature of centralised control of government. Certainly Labour hasn't done anything different.

RW: Well, you see, I had no idea when I came to Leicester I'd be caught up in this sort of national business because I was on the UGC Committee as well. The Biological Sciences Committee of the UGC, I was on that for many years, 10 years I think. And I think the reason was, they wanted, there was a sort of tokenism of having people in places, I mean, in committees. It used to be the same with the Phys Soc committee before they had elections. They'd say, 'Oh we want someone from Northern Ireland or…'

RN: Sure, but that's not tokenism, that's regional representation, and quite sensible.

RW: Well, but you could see the same thing happen with the UGC.

RN: Well, also if you want to find a distribution of views from Scotland, England, Ireland, Wales, where else do you go? Otherwise you have a centralisation in London probably or in Oxford and Cambridge, which is what's happening anyway. But…

DM: I've always thought that another strand that I saw, I did two terms on MRC on grants panels, and the time I was there was under George Radda, he was the big boss. And the big change he wrought with MRC policy was based on his own experience of research which is very much large scale, big group collaborative research. And he was the person who really made this, I think, a really big shift in the funding system the MRC was using, that particularly badly affected physiology because the idea of a small group doing really sharp work. The project grants were abolished, it became programme grants and so on. And there were no career grants. Wellcome at least had career grants for promising people, there was a continuity of funding that Wellcome persisted. But MRC became… they followed that. So for me, which is later than the period you're talking about, Prof, but that for me was a big change in centralised funding, MRC, that had a tremendous knock on effect in the way the subject was played out. So it played into the hands of people who ran large centres of excellent with huge teams of people who demanded long

term continuity of funding and it also led to short-termism in the science that was being done because a large group can always churn papers out.

RN: **Well, the other thing was the shift between in the balance between the MRC and Wellcome funded institutes and the Universities, just a redistribution of funds.**

DM: **Exactly. All of that happened.**

RN: **So in fact ultimately because of inflation and failure to increase the science budget in proportion to the growth in universities that too is something which nobody had control of. Sorry, go on.**

RW: No, I was going to say that it was my disenchantment with what was going on from about '76, I won't swear…

RN: **No, no it was earlier than that, Prof, because I left you here in '74 and you'd already resigned from the MRC.**

RW: I never resigned. I didn't accept reappointment.

RN: **Yes, same thing. But I remember you were pretty angry with the MRC at the time for various reasons, which I can't remember. So it was in '74 that you left.**

RW: The MRC gave me a research group and they reviewed it every 10 years. So that, I don't complain about that.

RN: **No, no, no but you were on the Central Board. So you left that with some acrimony, I suspect, because you didn't like the central policy-making of the MRC, I think. We've gone on with that so we won't go back over it but anyway… so it was earlier than '76. You may have had more problems in '76 as well. Okay, so alright. So let's go back, let's talk about the dynamics of the Physiology Department, the General Physiology Department. It changed its name didn't it?**

RW: It changed its name after 18 months because I realised to be in the mainstream of physiology there shouldn't be some qualification saying 'General Physiology'. And by that time I'd been on, I was on the committee, you see.

DM: Of the Society?

RW: Of the Society. They were very good. Eric Denton was the Secretary and I think we had the first meeting in Leicester, you remember it.

DM: I remember it, yes.

RN: I remember.

RW: I think it was formalising what the actual position was and also I was looking ahead to when we get a Medical School because Lord Todd had been to Leicester and I gave evidence about how physiology could provide all the essentials for teaching medical students because partly with people like you, Richard, there would have been sufficient spread of interest.

RN: A nucleus, yes there would. Yes. Well, I remember there was, I remember it differently. I remember Reg, for example, was a bit hostile to the Medical School because he felt it would be disruptive of the influence that the biological science would have. And I think he was apprehensive frankly that the medics would ultimately take over, as in fact, they did.

DM: They did. [laughs]

RN: And so he saw what was coming very clearly. I, on the other hand, being a medic, I didn't welcome it but I could see that it wasn't going to be as desperately harmful as you all feared. But then [Tim] Scratcherd came, if you remember, and he was supposed to bring in the Medical School, wasn't he?

RW: Yes. You see, we got money to have another chair and I offered the chair to one or two people, very eminent in physiology, but they didn't want to do it at Leicester, not surprisingly.

RN: No, not with you and me. [laughter]

RW: But I don't know how Scratcherd came to come but he was okay and I mean he could run the Department. I had no interest now, by that time. Once we got the Medical School I was doing loads of examining.

RN: But why did you lose interest actually?

RW: Well, you see…

RN: The Medical School was an important development in the University. I mean as you see perfectly well.

RW: Yeah, well Scratcherd didn't get the chair when Fred Harper finished. Eric Blair got it. Now I got on well with Blair, he had me as his External at Newcastle. Scratcherd left, he was a Senior Lecturer at Newcastle, he went to some clinical job in Glasgow, I think.

RN: I think not, but never mind.

RW: Well, wherever it was. But he wanted to get back to mainstream physiology and he was okay to come here because he'd got all the right background. And then, he'd only been here six months, and Robert Kilpatrick poached him to go to Sheffield.

RN: Yes, well he'd had this work with Maynard Case as well, if you remember.

RW: Yes.

RN: Yeah, anyway, so yes he only was here for a very short time and that must have been very annoying altogether.

RW: Well, it put me in a bad position.

RN: Yes, but it wasn't your fault.

RW: And then Asa Blakely came. Well he was okay.

RN: Well, he came after I left and he took over the Medical School teaching, I guess?

RW: Yeah, but I had no axe to grind, I mean…

RN: But I thought he was a pharmacologist?

RW: Yes, but he worked with…

DM: John Gillespie, he came from Glasgow, didn't he?

RW: Yes.

DM: He came down from Glasgow, I think he was with, wasn't he with Gillespie?

RW: He was with John Gillespie.

RN: Yes, right, anyway… by that time things were getting out of control.

RW: What I was concerned with was the confidence of people at the top in physiology. I had no illusions that Leicester was ever in the top rank.

RN: But it was a mistake to think that we would be.

RW: I never thought we were.

RN: I mean excellence in individuals but not as a major teaching power.

RW: Exactly, you couldn't be, you couldn't be. You couldn't.

RN: But in fact the department that you ran was an excellent research department in comparison with most or similar size…

RW: Well, I mean Richard Adrian [1927-1995] once said, 'We're near the top of the Second Division.' But we were never in the First Division. I knew that.

DM: But you were never big enough principally. You had, as Richard said, I mean without all flattering yourselves or ourselves, I mean there were a number of very successful research groups but only a small number. You needed to be three times the size before you could expect to have a constant turnover of really good quality people. So it was unreasonable to be anything other than top of the Second Division.

RN: Yes, well the snag I imagine as far as you were concerned was that there was Kornberg in the top division just beside you?

RW: No, you see, I've got letters, I've got handwritten letters from Lord Adrian, A V Hill, Krebs, Sir Rudolph Peters, Andrew Huxley and they all thought well of Leicester, but no illusions that we were other than provincial. And what encouraged me was that people like Rod Gregory had me as their External, I was the external for Witteridge, Wilfred Widdas had me as his external, I quite enjoyed going to…

RN: Bedford [College, London].

RW: Wilfred?

RN: Yes, yeah, yeah.

RW: And I used to go to the other two as well. The kidney man at Chelsea.

RN: Dick.

RW: Dicker.

RN: Dicker.

RW: And Alan…

RN: Howe?

RW: Knox, at Queen Elizabeth [College, London].

RN: Yes. I knew him.

RW: So, but the thing about all that was that I could not have done the External Examining without the discipline of the Phys Soc communications as they used to be. Time to finish now, I'm hungry.

RN: Yeah, okay, fine good.

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RN: **…there would have been quite a useful future for me there. And also I got on well with members of the King's department: Venetia France (Franglin) and Mike Bradbury and to a lesser extent, Joan Abbott. So I felt that I would have a, you know, congenial life there. And I wasn't getting on terribly well with some of my colleagues here, if you remember? …one way or another. So it was a good time to leave, I think.**

RW: Yeah.

RN: **Also it was, I could see with the Medical School coming there was going to be tremendous changes and there were. And I must say, I don't think, I think it was a mistake for you to withdraw as you did then. It was a pity and you should have stayed on.**

RW: What, when I retired, you mean?

RN: **Yeah. Was there a reason for it apart from ill health? You weren't well, were you particularly at that time?**

RW: Well, you see, I think once Asa Blakeley came I had no interest in being Head of Department. He took over.

RN: **Right. But you had some reasonably good research still going then, didn't you?**

RW: Yeah.

RN: **Alan had left. Alan Chipperfield. And David Cotterrell had left.**

DM: **David Cotterrell will have left by then, yes.**

RN: **Where did David go?**

DM: **Well, David, did David Cotterrell, he had a post-doc period with you, did he?**

RW: Yes, he did.

DM: And then from there he went…

RW: He went to Ken Cross at the East London.

RN: Oh yes, so he did.

RW: And then he went to Leeds.

DM: Yes, then he was in Leeds for quite a while.

RN: Yes.

RW: And I think the other thing was, I think when it got to the mid to late seventies, I think I was quite tired.

RN: Well, the other thing was the nature of studying the sodium pump had become a different business altogether. It was no longer a business of looking at the ion fluxes so much as looking at the intra-protein partial reactions. And that was becoming relatively big science actually in terms of, I mean, it required a much more biophysical approach.

[Comments by Christine Whittam regarding refreshments]

RW: I think part of the trouble was the Vice-Chancellor.

RN: Here? What, Noble? What was the matter with him?

RW: Too much old guard, too much… You see he was very good before there was a Medical School but a vice chancellor running a place with a Medical School, you need a different sort of vision and you can't run it… It's like if you have a little business, if you have say a business with five people.

RN: Yeah, everybody knows everything.

RW: And then if you go to 20 people, it's different.

RN: Yeah, absolutely.

RW: And I think that he hadn't really adapted to what was required with the medical school. That was my opinion.

RN: Well, I have never been at that higher level in university politics to know how things are actually run but I can tell you at King's College they are run on a very, very tight inner cabal, it's all the accountants and the people who run King's are the people who run the property. It's actually shocking. Is this still on?

DM: Yes, it's still on…

RN: I mean we have a crisis at the moment in King's College in terms of restructuring of the College. There's, the finances have run out of control and although we have, we're a relatively rich university with a budget of about £600 million per annum, there's a shortfall and most of that shortfall is due to the fact that the hospital is spending a lot of money. And so the restructuring has been focused on the Medical School and this is causing a lot of trouble at the moment. And this is happening all over Britain at the moment. The universities are retrenching, and yet they're trying to expand at the same time. It's extraordinary. I just don't get it.

RW: Well, you see you left before Maurice Shock [b.1926] came. He came in '77. He got more idea about what was required but you see there were a lot of problems and one of the insulting things that happened was when there was a pay pause and anyone in academic life who was on a salary of more than £8,500 didn't get a penny increase for two years, and yet the technicians and non-professorial staff they all got £6 a week. And I had technicians, and this was the other thing that happened as well, the technicians used to be run on a, it was on a different basis, and then they got the 'Blue Book' where jobs were defined irrespective of the quality of the work that was done. And you, it was, the setup changed really. Anyway it's all, you think it was a mistake that I finished?

RN: Yeah. Yes, because…

RW: Well, you may be right there.

RN: Well except Peter Stanfield did a good job for a while.

RW: Who?

RN: Peter Stanfield.

RW: Yeah, yeah. Oh, I got on well with him.

RN: Yes, nice guy, I liked him a lot. Sane.

DM: A rare quality.

RN: Yes.

RW: You know dandelion and burdock?

DM: No, it's not one I've tried, I must say.

RW: Non-alcoholic.

DM: I know it well.

RN: I don't do sweet drinks except tea. But I suppose it comes from Lancashire.

RW: It comes, I don't where they make it.

RN: Yes, Sainsbury's but I mean it's got a Lancashire feel about it. Yeah, whereas IronBru is the Scottish equivalent. [laughs]

RW: I think, I went on a tour of the Soviet Union. The Royal Society paid me to go and I went to, did I ever tell you?

RN: No, go on.

RW: I flew to Moscow and then I went to Kiev, Tblisi, Leningrad and they could hardly believe in Kiev for instance, Kostyuk, have you heard of Kostyuk?

RN: Yes.

RW: He couldn't believe that we were treated in the appalling way that eminent scientists in this country were treated like dirt compared with the way they were treated.

RN: Except that we aren't executed. [laughter] Actually…

DM: Your funding is cut off but nothing else.

RN: That's not true anymore. We can be sacked and many of my colleagues are being sacked as we speak at King's College. The pay is the thing. If you can't justify your salary anymore with grants, you're out, irrespective of your record, irrespective of your responsibilities to teaching, to PhD students. If you don't bring in £150K per annum, you're out. It's extraordinary. Christine has overestimated our appetites by a factor of three. [laughter] You'll have to have these for lunch tomorrow.

RW: Yeah, I made this bread yesterday.

DM: Oh, you made the bread?

RW: It's homemade, yeah.

RN: Very good. We haven't succumbed to one of these.

RW: What they should have done with me, actually, they should have said, 'You've had a tough time, set up the department, have a year off and see what you feel after a year.'

RN: Why didn't you take sabbatical? Why didn't you ask for a sabbatical?

DM: Did they exist still?

RW: I'm not sure they would have… No, but if they'd said that to me, who was it in Oxford who had a year off and learned to play the piano? Something like that. But they never thought of that.

DM: To give you a chance to recharge the batteries.

RN: I think you're thinking of C P Snow's book, what's it called? *The Corridors of Power* or something. Was that what it was called? C P Snow wrote a book about some, about the science things going on in Oxford. One of them played the piano most of the time, yeah. There's nothing wrong with playing the piano, I wish I could do it. Actually I don't. It's a terrible, time-consuming occupation when you can listen to the best in the world just by putting on a CD.

DM: You mentioned, just in passing, Prof, you mentioned earlier on, Robert Campbell Garry and you said you might come back to it. What do you want to say about Garry?

RW: Garry! When he heard I was coming, I mean coming to Leicester, I don't know how it came about, I must have seen him somewhere, but he said, 'You know, they all call you by your surname.' Letters were all written… Whittam, my dear Whittam

RN: Yes, yes.

RW: Garry said, "You'll be expected to do four things and you can only do three, and you've got to decide which one not to do." Very good advice.

RN: Well, Gary wrote a little book on his sort of autobiography, it's available from Glasgow University.

DM: Yeah, I've got it.

RN: Quite nice. It was, Glasgow University physiology department started as a sort of Nutrition Department more or less, as far as I could make out.

DM: Well, it didn't start as, it had a much more … but under him.

RN: Yes, but he had been, he'd done nutrition studies after the First World War and there was a national need to see how much—or how little—the workers

could eat and still survive, so they were looking at the calorific requirements of various jobs. I think that's one of the things that…

DM: **Yeah, it's one of the strands there. But he had Noel Paton who was also there, so there were a number of strands of physiology there. But when I first, well Garry had retired, when I went to Glasgow Otto Hutter had set up, changed the department but I was hearing these stories about Campbell Garry's view of it which, his lectures were 'to start with the cow in the field'. That was the way he used to teach physiology so he started with the whole organism in its environment as it were, and worked his way through to membrane transport from there.**

RN: **But I mean there's the big conflict. I don't know if you've read Noble's**

DM: **'*The Music of Life*' …?**

RN: **on, which came out a couple of months ago in Phys Soc [*J.Physiol*], about reductionism versus holism, I'm not really sure what you call it. I think they are being a bit over optimistic, frankly, but that's by the by. But there is a problem that over the time period when you started in physiology up until now we've concentrated on reductionism to the detriment of understanding what's, where these things fit in. And I mean the problems arise when you have knock-out mice for example and you knock out one gene or one structure and nothing happens. And it's quite clear there are too many, it's really too complicated to explain on the basis of single gene defects and…**

RW: I very much agree, I've got the book of Denis Noble and I very much agree with it.

RN: **Well, I think we all do.**

RW: And incidentally I think I got across to Vice Chancellor Noble because I followed one of the four things that Gary said and I would not do the donkeywork that Noble wanted me to do. He wanted me to be Chairman of the Departmental Assistance Board, of all things.

RN: **What's that? What was that expecting you to do?**

RW: Overseeing all the technical and clerical staff.

RN: **Oh my God.**

RW: It's the last thing I'd want to do. He wanted me to be Chairman of the School of Biology. Who'd want to do that? Ridiculous.

RN: Again, somebody has to do these things. I agree, it would probably be better that you didn't, but somebody has to do these things and clearly if he wants somebody to do it and you don't then he's not going to be happy.

RW: Yeah but you've got to, I was ready to, if they'd said, "You can have a year off" I might have gone to the Lake District for a year because I've got a brother who's retired up there, or I could have done something else. And then if I'd got my second wind…but they never thought of that.

RN: Yes.

RW: And they didn't value what they had. And there were quite a number of professors took early retirement for this sort of reason. I mean look at Sidney Hilton [1921-2011], Stan Thomas [b. 1925?].

DM: Yes, that's true, that's true.

RN: Well, you've got to keep at it. Yeah, well, anyway, that's…

DM: What about, Prof, maybe for the last few minutes you may like to say something about your time with Phys Soc because it's always changing. It's changed dramatically in the last 10-15 years, but it changed in your time.

RN: When was the last time you've been to a Phys Soc meeting?

RW: I haven't been to one, oh it must be, when they last met in Leicester.

RN: Oh my God, that's a very long time. My God, that must be about 30 years.

RW: Must be.

RN: Why didn't you… have you lost interest in physiology or what?

RW: Well, I haven't set foot in the department in Leicester for 24 years and I got a letter from the present Head and someone was retiring, oh it was Nick Standen was retiring and they wrote and asked would I go to the retirement place. I said, 'I've never met the man.' [laughter] And what happened next was that Peter Stanfield said that he'd take me by car because I stopped driving. And it was 24 years since I'd been in the department. Do you know, nobody spoke to me?

RN: Oh dear. Well, that's not very nice.

RW: Nobody came up to talk to me.

RN: Well, they didn't know who you were, I'm afraid. That's the other problem. Yeah.

RW: You'd hardly believe it.

RN: Yes, well retirement dos are very difficult unless they are properly stage-managed, it becomes a very difficult event.

RW: Oh, Richard Boyd came. He came. He's very nice.

RN: Yes, he is. And he presumably did speak to you?

RW: He spoke to me.

RN: Yes, he's a real gentleman.

RW: And [Robert] Meech spoke to me.

RN: Who?

RW: Bob Meech from Bristol.

DM: Oh, Bob Meech.

RW: He came, and Alan North, he's a nice chap. Do you know North?

DM: Yes, I know him a little bit, he was Secretary of the Society of course for…

RW: President?

DM: That's right. He was one of our…

RN: About five years ago.

RW: But I was quite pleased that the department, it's given quite a number of people who have been active in the Society.

DM: It has, and I think that's important.

RW: I mean it might be a little offshoot to putting value on the Society.

RN: No, I think it has.

DM: No, I think so. I mean it's one of the things, so we have this little project in History and Archives Committee just now where, amongst other things, we're trying to establish people's family trees, their scientific family trees. This exists online for a number of subjects but it doesn't exist for physiology. We overlap a lot with the Neuroscience tree and I had been very keen for about a year that we get this going. Anyway we're doing it now and what you just said is an important point. I think, as you were saying earlier about certain people run—or have had an effect on the subject—not so much themselves directly but through their influence on a large number of people. And if you follow it back you realise the common denominator with certain schools of physiology is a single person with whom all of these people studied or collaborated. And you realise that the roots of their discipline were back there, and this core, this trunk of the tree, if you like, is unknown. It's almost like the roots, it literally is underground, is unknown. So this is one of the reasons why we're interested in this family tree idea to see where people came from. So it goes back to what you just said, Prof. Leicester, if you look at it, has been remarkably successful in generating a whole lot of folk who have done really rather well in physiology.

RN: With all due respect I think it's due to your eminence that it did. I'm not saying that just to flatter. I mean your influence over a large number of physiologists, and it's not unique obviously because you and Ian Glyn and internationally, the work on ATPase would have gone on whether you were

doing it or not. But, none-the-less, you were right in at the beginning of it and you made a substantial contribution and we wouldn't, without it, well British physiology wouldn't have been in the state that it was over the past 30 years. I think practically the impetus for that sort of stuff is gone now from physiology.

DM: **That's the worry, that's the worry.**

RN: **I mean there isn't, I don't think the key nodes or growth points in physiology coming, and I can't see anything much in British physiology that is unique.**

DM: **No.**

RN: **Whereas this bit was. And it was important and useful. And so, Leicester clearly benefitted from that to a very large extent and we're all very grateful. What Reg did was important too but to a large extent it came out of the biophysics department at UC. But your stuff came, obviously from Oxford, but it was yours really. And so you brought it on your own. So that was an important growth point in physiology, I think.**

RW: I still think that there's a lot going for the subject and it's important not to be watered down by linking up with other subjects. You see they've got it linked up with pharmacology and if you're not careful, I helped to fight this battle with the early days in Leicester because biochemistry was extremely powerful and they tried to make out, in some ways, although I got on with Kornberg, they actually were the very strong department in the School of Biology.

RN: **Well, it was called molecular fascism. [laughter]**

RW: And I think physiology is such a, you would call it a 'broad church', but you get people really contributing say from physics or from chemistry or whatever.

DM: **Mathematics, and from medicine. Everything comes together.**

RW: And medicine, yes.

DM: **I was very struck, Prof, you sent me, when we first started corresponding, you sent me a copy of your Inaugural Lecture here and the way, I mean there it is documented what you said in '66 when you actually gave the lecture, or '65 was it?**

RW: Yeah, '66.

DM: What you had to say about the nature of General Physiology then, of course when you read it now it sounds absolutely up-to-date in the way Denis is talking about it, and you were saying these things. And it's just knowing that that was an undercurrent that was running through your thinking and informed your thinking then, and obviously you setting the department up with that ethos. And, as Richard was saying, that's what came out, that people had this… we were each working on our specialised little areas but we saw them all as examples of, or exemplars of what physiology in general is about. And that comes right through in that lecture of yours and it's a story that deserves to be retold.

RW: I'm glad I sent you that because I was taken with what you had said.

DM: Yes, it was a pleasure to see it, a pleasure to see it.

RW: I'll give you a copy.

RN: Yes, I'd like a copy. If you send me a copy that will be fine.

RW: No, I've got one, I can give it you. That's something that I agitated about in Sheffield. I was against anti-Semitism and I was against racism and I lived in a Hall of Residence and some of us went to a public dance at the Cutlers' Hall. And there was a man from Pakistan and a man from India and they were refused admission.

DM: No!

RW: 1952, February. I was outraged and I mobilised all my political skills and I got the President of the Students' Union to write a letter to the Master Cutler, the Lord Mayor, the bishop, the Vice Chancellor, everybody, and it all got in the local newspaper.

RN: Very good, very good. Well, you should be at it now.

RW: That's a picture from the local newspaper. 'Colour bar'.

DM: Oh yes, that's a phrase you don't see.

RW: 'Colour bar in Sheffield'. 1952. It was outrageous.

DM: February 29th, it was a leap year.

RW: There's another piece of paper somewhere. Oh yeah, that describes it.

RN: Very good.

DM: Excellent.

RN: So, you were a bit of a Commie, yeah? [laughs]

DM: A bit of a Commie. [laughs]

RW: Krebs had caricatures of people, of Hogarth prints, and he had a caricature and he put me as the preacher pointing like that. [laughter] I think there's a lot of moral fibre required with these departments and so forth.

RN: Yes, you've got to stand up. But the odd thing is if you do people will cave in eventually. You know because most people don't have the energy to stand up. Unfortunately there are a few who do.

RW: You can't keep, I mean, I thought in Leicester it had reached the point of sort of diminishing returns because of…

RN: Well, the thing is, I mean, to carry on doing just coming into the department without having a need to want to do research is crazy actually. I mean if it wasn't something that you actually wanted to do. But presumably there were things that you would like to continue doing in research? I mean Ernesto Cavieres was doing stuff. Why didn't you carry on with him? I mean he would have been…

RW: Well, I think it was the last meeting when you said, "Well, I'd like to come and work for you." [laughter]

RN: Well, okay. For, I don't think 'for', 'with' I think. [laughter] I couldn't have afforded you.

RW: If you want a good idea that's not been worked on it stems right [back] to Claude Bernard.

RN: I'm a big admirer of Claude, yeah.

RW: The regulation of pH is grossly underestimated and the effects of it. You see, I've got severe osteoporosis. Now I attribute that to the fact that I've eaten too much protein over many years. And protein is, no, there's been no epidemiological study…

RN: Oh, excuse me, there's been plenty.

RW: No, not on the effect of pH on osteoporosis.

DM: You mean, you're suggesting a chronic effect of, low pH of extracellular fluid?

RW: Yeah.

RN: Yes.

DM: But isn't that so tightly regulated – or that's what we all believe…

RW: Somebody could do a research project on that.

RN: Okay, yeah.

RW: And it's to do with obesity as well. And the other thing, and I've thought this for many years, in the regulation of acid base balance there's one thing that has not really been looked at and that's the redox potential.

RN: Ah, go on. Well, this is something that I've been involved in quite heavily over the past eight or nine years. One of the things that keeps me busy is, we're looking at iron redox, which is the key to understanding redox, and I'm very interested in ferritin and mobilisation of iron from ferritin, and we've been looked at ascorbic acid in relation to that. And it's really quite

interesting. As we speak I have a couple of people in the lab doing that. But I mean redox is important, very important, but we're a long way from understanding…

RW: This is one thing [Ronan] Conway was keen about and everyone laughed at him.

RN: Well, his ideas sort of faded. I mean one of the things I was going to talk about, I'll ask you now, is when you were studying stoichiometry of the sodium potassium ATP relationships, you had a fairly strong idea that there was some sort of fixity in the relationship. But I suspect that it became embedded in the whole business of chemical stoichiometry, which is not the same as transport stoichiometry. And that's been something which has interested me. So, I mean the way that stoichiometry was looked upon as the three sodiums, two potassiums, one ATP, very important. But at the edges of this relationship, as you showed for example with the reversal of the sodium pump which was a great idea to do that, it's quite clear that the stoichiometry isn't 3:2:1 and it breaks down. And yet people have this kind of fixed idea and it is partially embedded in the way that we look at structures of these things. You know we think there must be three sodiums and two potassiums, which is completely wrong really. But when you think about it in terms of these vectorial movements that we are looking at, did you actually have a concept of strict stoichiometry or did you think that…

RW: No, I didn't, no. No, I think it can be modified but I don't know how.

RN: Well, you have a model in terms of the irreversible thermodynamic approach that Aharon Katchalsky had these equations which were…

RW: Well, I just hadn't a clue about that.

RN: Okay, fair enough.

RW: But I think it's like Denis Noble is saying now: you can't have the reductionist view, which is what the view would be, about a fixed stoichiometry.

RN: Yes, exactly. Well, I agree with that.

RW: You see, I've been out of it for so long now.

RN: Fair enough...

RW: I mean I got to the stage where I've parted and given away all the books on science and philosophy and religion, apart from the ones in this room. And the only book I've kept on science if the famous third edition of Bayliss' *General Physiology*.

RN: Excellent book. I'm just curious.

RW: A wonderful book.

RN: It is a wonderful book. How do you know about the [Denis] Noble stuff then? I mean if you don't keep up, how do you know about that?

DM: Yes, how do you know about that?

RN: Because it's only, it's quite current.

RW: Well, I still use a laptop.

DM: Good.

RW: And I still keep tabs on…

RN: So you have a subscription to *The Journal*, do you?

RW: No, no.

DM: You're an Honorary Member so you have free access through that, yes.

RW: How did I hear about it? Well it was publicised in all the literature about the International Congress.

DM: So you were following that?

RW: I was following that.

DM: Good.

RN: But there's an edition of *The Journal* that is full of this stuff.

DM: Yeah, I had it in my bag, I've left it in Lincoln. I didn't want to lug it with me.

RW: I don't get *The Journal.*

RN: Yeah, but you could get it on the internet.

DM: There was a special edition edited by Denis about six months—no, not as many as that—before the last Phys Soc meeting.

RN: It would be about March, no April.

DM: …which brought together a series of paper right across the board in this area of, for want of a better word, integrative physiology on the big scale as opposed to reductionist. And I think it's going to be a very powerful element. I think it's one of those things, we will point at it, I hope we will find in 10-20 years' time that it signalled a turning point. It's recognising it.

RW: I think they had a symposium, the Royal [Society] arranged something at, this place in Buckinghamshire. He was there at that.

DM: He's been to any number of such things. I know he went to quite an important conference in Sweden on this topic, I didn't know he'd done one in Britain but he really is very, very busy on this agenda. And good for him.

RN: Yes, I agree with that.

DM: I mean hark back to … there was a very nice book that he and Richard Boyd co-edited for the '93 International Congress [in Glasgow] called '*The Logic of Life*' and they brought a lot of these ideas forward there in one place with a number of invited reviews and that's carried on now for nearly 20 years, or more than 20 years.

RW: I've just been toying over my response to your question about the Phys Soc because I don't think in the present time you could have the same system as it was 40 years ago.

DM: No.

RN: Well, we haven't.

DM: We have a very different system…

RW: You'd have 8 or 9 meetings a year, communications were not streamlined and people sat through the whole…

DM: We sat through everything, didn't we? The only unusual ones were the big ones like Oxford and Cambridge and UC, where they had two, or sometimes three, parallel sessions. And that was already a little bit too much but that was the closest. Otherwise you saw everything. And that was tremendous because you did hear things and you met people and became exposed to the whole subject, but also things that were useful in your own lab. And that doesn't happen now.

RN: No, and also the presentations were encouraging the younger members of the Society to present properly.

DM: True.

RW: I mean I think of all the societies, compared with the Biochemical Society, I did actually resign from that one [laughter], there was no point keeping up with that.

RN: I remember.

RW: But I think there's something about the Phys Soc and it will appeal to another generation. I mean I can see that with my grandchildren.

RN: Well, I think it's changed dramatically, I'm afraid, if you haven't been there for 30 years, I'm afraid you would get a rude awakening if you actually came to an Annual Meeting now because you would think you were attending the Biochemical Society, I'm afraid. Not because of the content, which you would think was the medical…

DM: Medical Biochemistry Society. [laughs]

RN: Medical Biochemistry Society, but it's really quite different.

RW: I don't think in the interview, I don't think I've done enough justice to science, scientific contribution.

RN: Of whom? By whom?

RW: You see, I was making the point of how wise Krebs was in letting people do what they wanted. And one thing I never mentioned is the work on seminal mucosa. Now you've never heard of that, but I did that work.

RN: Seminal mucosa?

RW: Seminal vesicle mucosa.

RN: Yes, okay.

RW: I did some work on that, and it was just a whim.

RN: Yes, why not?

RW: I showed an important point and Hans Breuer from Bonn, he developed this, he made a big thing of it. And the other thing I did, which I never, a very important thing this, was from travelling from Cambridge to Oxford with Geoffrey Dawes (Geoffrey Sharman Dawes, 1918 –1996), do you remember Dawes?

RN: No.

RW: He was head of the Nuffield place that did all of the neonatal work.

RN: Okay, yes I did meet him.

RW: And I did some work in Oxford on the new-born rats, rabbits and guinea pigs, and the difference in reaction to anoxia and ouabain…

RN: Yes, hypoxia.

RW: …and so on. In other words it was possible in my time, you didn't have to concentrate only on the sodium pump, you could have these little other interests.

RN: Well, you still can but you have to…

RW: Perhaps it's not a good idea nowadays.

RN: I don't know. I think one should have hobbies in physiology, you know? Because you can become too concentrated and too specialised and people are, and that, you know…

RW: I think that's the case.

RN: You think what?

RW: That they're too specialised.

RN: And too compartmentalised as well in the sense that they don't…

RW: I mean when I was doing all the External Examining I would sometimes say in a viva, "What would you like me to ask you about?" And see what they were strong in. And I think it was only through sitting through all the sessions that you get the know-how.

RN: But that's the big strength of a medical degree, actually. You get the wide knowledge of lots and lots of problems so that you can home in on different things. Whereas the science degree that we teach at King's is too narrow and you don't get nearly enough exposure to different problems and different techniques, that's the other thing. There's a lot to be said for broad approach plus a narrow. I mean you need both.

RW: I think David Smyth [1908-1979] took this point very strongly. He used to say, although I mean I know there's all that difference of opinion about what Gerry Wiseman [Gerald Wiseman 1923-2000] did compared with what Smyth did.

RN: **Well, I was going to ask you, again, there was a Wellcome meeting [London, 1999] where this was actually discussed at some length. I mean Gerald Wiseman was under-rated actually, I think. He made a tremendously important contribution in terms of the gut sac technique which David took over and I think that was…**

RW: You see Gerry Wiseman got it from Bob Davies because Bob Davis did the everted frog stomach.

RN: **Okay, right.**

RW: But I was there [Sheffield], I knew Gerry quite well.

RN: **Yes, yes, yes. Well he worked with a guy from Boston, microbiologist…**

RW: Oh, Tom…

RN: **Tom Wilson** [Thomas Hastings Wilson]

RW: Tom Wilson, I remember him.

RN: **Well, in fact Wilson wrote a little biography of himself and of Wiseman and he gives a lot of credit.**

RW: But Gerry was, he was very sort of retiring and he didn't push himself.

RN: **But he was a bit resentful I think of the…**

RW: Was he?

RN: **…that he was in fact pushed out of the limelight possibly or of any attention at all. Because I mean there was David Smyth getting the plaudits for stuff that he didn't really do, to be frank.**

RW: Tony Angel thought well of Wiseman.

RN: **Yes, yes. Well I think everybody did who knew him.**

RW: Yes, he was a nice chap.

RN: Roy Levin also things a lot of him. Yes.

RW: You see when I was in Sheffield, I lived in a Hall of Residence and I was in the senior common room as a junior research fellow, and Gerry came back I think from America. He came into the same hall.

RN: Okay.

RW: And I can remember him.

RN: Okay.

RW: I think now the hand of the Society's on the lines of Denis Noble's get it back to the holistic…

RN: Yeah, well if only, if only, if only that could come about. But it needs a whole set of skills that are missing, in my view.

DM: Exactly.

RN: …from the armamentarium of most physiologists.

RW: You see I read your stuff about 3 years ago when you had that article about physiology not being…

RN: What it was. [laughter]

RW: Yeah. But I thought, 'Well, you're underestimating the political skills required'. And that came about, this is where Lindor Brown were so good you see.

RN: Yes, I agree. But it's all very well talking about political skills but you've got to have, you've got to have the power to, I mean you can't exercise any political skills without the kind of power in a Medical School that allows you to handle money. If you don't handle the salaries, you don't handle anything.

And I mean you've got to get, I'm afraid you've got to get these guys by their salaries [laughter]

RW: I absolutely agree with you. I quite agree. If the salary isn't there, people will not respect you.

RN: No.

RW: Do you agree, David?

DM: Exactly. But it's this business about the way you shaped the department. You had an idea for a department, you had a whole ethos that you chose to bring in. But you had what you described, which was underwritten because you had posts at your disposal. More or less you could appoint in a way that you chose to, the disciplines you brought into the department, or didn't bring in, that was in your choice; and also that these people could be set up and get started. But now it is [not], and for many years as you well know, this hasn't been the case at all. So what Richard's saying I think the politics of it, I think there are plenty of people around with great ideas who should be supported and given their head in a sense to do it. But that doesn't happen anymore. It's very, very small, it's this concentration of power that you spoke about politically, is also true in the university sector. It's concentrated in very few hands.

RN: We had a debate about this in Oxford three years ago I think. I mean my view is that the power was handed away primarily by people like Maynard Case who very much favoured the idea of a multi-skilled department or multi-departments, and primarily with him at the head of it. And it was a disaster.

RW: I didn't know about that.

RN: Oh yes, well Manchester, they amalgamated the physiology department with molecular biology, cell biology and pharmacology so there is no department of physiology anywhere in Britain without…

DM: Bristol.

RN: Bristol.

RW: It's ridiculous. It is ridiculous.

RN: Well yes, and it's not very successful either and physiology have been the losers by a long way because, frankly, if we do molecular biology, it's amateur molecular biology.

DM: Exactly.

RN: And until we can bring in people who have skills and the insight into physiology, I don't see that happening.

RW: Have you had your ginger beer?

DM: I have.

RW: Do you want another?

DM: No, that's absolutely fine for me.

RN: I think we should let you…

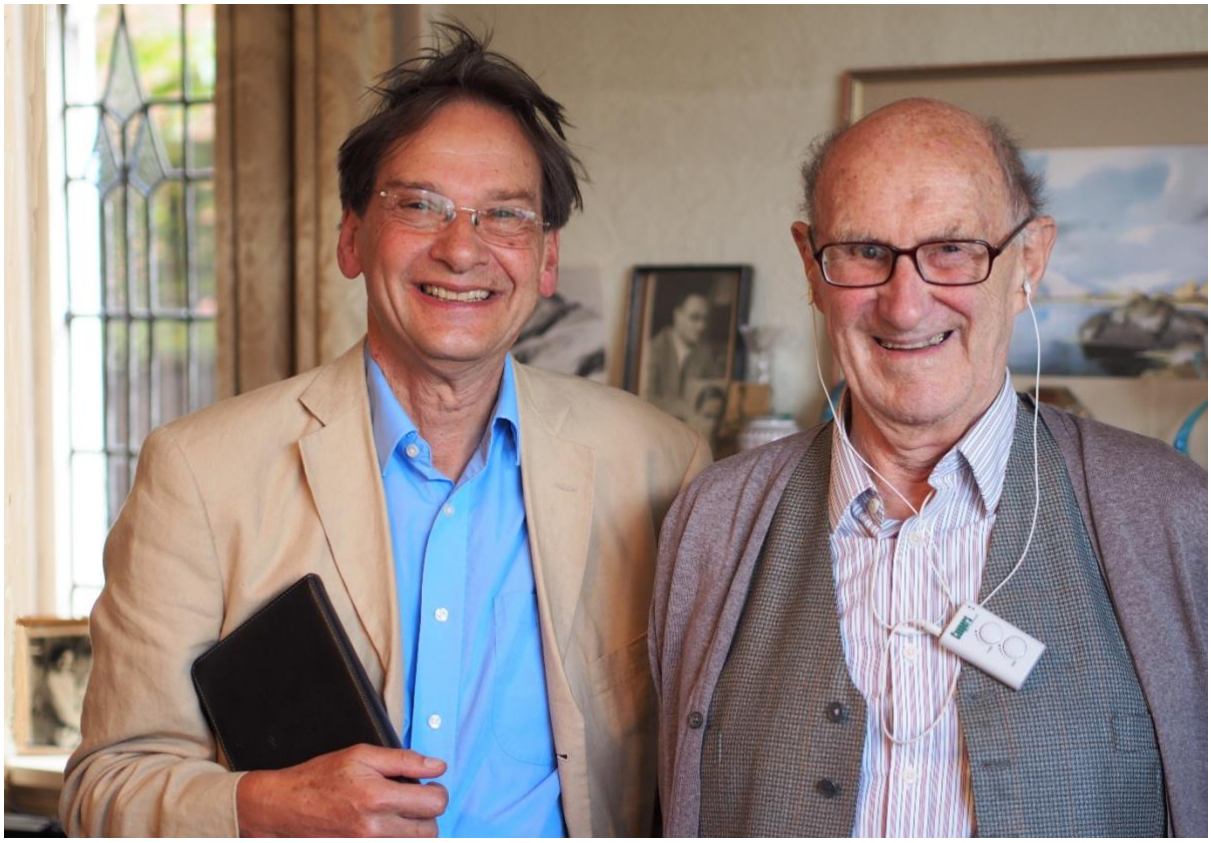
DM: We should wind up. I mean formally to get it on the record: thanks, tremendous time you've given us here, Ron. We've touched on 1,001 things, we've only managed to explore in detail a few of them.

RN: Yes, we'll have to do strong editing.

DM: We'll do our strong editing later on.

RN: Thank you very much. Maybe we'll come back to you if we have to ask for some details and things like that.

DM: We'll do that.



Richard Naftalin (left) and Ron Whittam photographed by David Miller

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