# An interview with Otto Hutter

Conducted by Tilli Tansey and Martin Rosenberg in 1996

Published November 2014

This is the transcript of an interview of the Oral Histories Project for The Society's History & Archives Committee. The original digital sound recording has been placed in its archive at The Wellcome Library.

The Physiological Society

Otto Hutter photographed by Martin Rosenberg
at the Wellcome Library in 1996.

This interview with Otto F. Hutter, then the Emeritus Regius Professor of Physiology, University of Glasgow was conducted at the Wellcome Library, Euston Road, London in 1996. Those participating were Otto Hutter (OH), Tilli Tansey, Hon Archivist of The Physiological Society (TT), and Martin Rosenberg (MR).

TT: How did you become a scientist, did you want to become a scientist as a child?

OH: How did I become a scientist? Well, I put the origin in my mother's kitchen. In Vienna one still went to the market and chose a chicken, not live, but whole, feeling its breast, looking at its legs to see whether they were young or not. Then it was part plucked for you and you took it home and the housewife herself took out the entrails, and, being a horrid little boy, I used to like playing with the entrails of the chicken, also with carp eyes. Fortunately, instead of being aghast and discouraging me my mother explained to me as much as she could, certainly how to dissect out the gall bladder from the liver, or to open up the gizzards and take out the stones; and she explained to me that this worked as a mill for grinding. Then she would take out the little bits of very dark red flesh and put them into the chicken soup. Oddly enough, she knew that these bits of gizzards were good for women. The only bit of the chicken, which she reserved for herself, was the bits of gizzard. Nobody knew at the time about vitamin B12 or that the gizzard was a very rich source of vitamin B12, but it was an old wife's tale, that the chicken gizzard was good for you. I like to think, for anecdotal purposes, that that was the beginning. It could of course have been that my mother in a far-sighted way, like all Jewish mothers, thought that she was creating 'her son – the doctor'.

MR: Was there a tradition in the family of science at any time at all?

OH: No there was no science tradition in the family. My father was a student of law, was to be a solicitor in Vienna. In misplaced enthusiasm, like the rest of his generation, he rushed to join the colours of the Austro-Hungarian Empire in 1914, served all through the war, met my mother whilst he had typhoid. She nursed him and they married in 1919. My father never returned to complete his studies at Vienna University. He continued as a sort of estate agent. In Vienna at the time, all property was transacted by lawyers, but he never had his own firm.

MR: But was he good with his hands or did he have hobbies?

OH: No not in particular, I mean he had no manual hobbies. We could go into this for ages. Business in Vienna was conducted in Kaffee [coffee]-houses and the office was really a 'Stammtisch' ['regulars' table] in a Kaffeehaus. That's where you met your clients and you then went back to whatever office you had.

MR: The same table?

OH: Oh, you always had the same table and to move from one Kaffeehaus to another was a major event when it happened.

TT: When you were a little boy, did you have hobbies? Did you make things?

OH: No. It was a middle-class home in a flat. Oh, I had the usual toys and things, but there was nothing outstanding there. I had a love for history and my father was very keen on Latin – that was in addition to Hebrew – and he coached me in Latin for a couple of years. So my upbringing leaned more towards classics and languages than science, really.

MR: Were your family in Vienna for generations, or did you come from further east?

OH: No, my father was born in Lemberg [Lwow or Lviv] in the north of the Austro-Hungarian Empire [present-day Ukraine]. My mother was born in Vienna, but her father emigrated from Kattowitz [Katovice, Poland] then also part of Austro-Hungary. He was a haberdasher.

MR: Were they Yiddish speakers?

OH: It was a very Austrian culture really – it was traditionally Jewish, but not orthodox in any sense. I didn't speak Yiddish. My father had a good education at University – he spoke and wrote very good German and he was fond of language and its presentation. If there's any outstanding memory, then it is [of] him helping me in presenting a kind of paper, as it might have been termed, on 'The Emancipation of the Jews in France'. In a way, my historical memory goes back to the French Revolution, because you see my father was born in 1891 and for anybody who was educated at the end of the 19th century, before the First World War, the great event which shaped Europe was the French Revolution and the Napoleonic Wars. That was the way he had been educated and some of it has rubbed off on me. I know very little history before the French Revolution. In England also, I learned 19th century history for school certificate. So I feel myself often a 19th century man!

MR: But you were bar mitzva in Vienna?

OH: Oh yes.

MR: Have you returned to Vienna?

OH: Just once, after the Prague International Pharmacological Congress, I can't remember when that was, in the '60s I think. Yes, I was already at Mill Hill and I returned to Vienna just to visit the graves of my grandparents – to find them and visit them. I walked around the part of Vienna where I lived, which used to be Jewish, but which was completely dead as far as Jewish existence goes. There was no sign of any kind of memorial. The only part of Vienna which was paradoxically alive from the Jewish point of view was the Friedhof [graveyard]. Obviously, I was not alone in my sentiments. There were many people, visitors from America and other countries, who were walking around the graveyard trying to identify the graves of relations. So there were Jewish people there. It's an odd contradiction to find that the graveyard was the only place where there was Jewish life.

But what did strike me, despite the fact that I spent a very short day in Vienna and I didn't even have a meal there, was what a beautiful proportioned town it was; the proportions of the roads, the buildings, the background of the mountain drop.

TT: Let's get back to how you became interested in physiology.

OH: I think that that has all happened, after I came to England. I came as Kind [child] number 359 out of 360, which was jolly lucky I might say, on a Kindertransport from Vienna to Harwich in December 1938. We were taken to Butlin's Dovercourt holiday camp in mid-winter. It was a bit cold, but never mind. Our first meal in England was grilled kipper. We had never seen anything like it, either in colour, or strength of flavour. Not one child touched them – this was best Lowestoft kipper. I think our hosts were deeply offended that these graceless, refugee kids wouldn't eat grilled kipper when offered it!

TT: Did you have any relatives in Britain?

OH: No, I had no relatives in Britain. My sister came six months later, but I had no relatives at the time. Well, we didn't stay at Dovercourt for very long. We were taken in small groups and boarded in boarding houses in Ramsgate and Broadstairs. We were mostly 14-, 15-, 16-year-olds, because that was the group considered most in danger of early conscription to forced labour in the Reich. At the same time, the under 16s did not swell British unemployment figures. We were middle-class children. We had expected to stay in education until 18 and possibly go to university. But we were put under pressure to take up apprenticeships and other kinds of jobs, because of course the organisers of the Kindertransport couldn't afford us further education. But my father, bless him, kept on writing to me that I must try to continue going to school. So I turned down all offers to become a tailor, cobbler, electrician. I kept on saying: 'no I want to go to school'. I was probably helped by being quite young for my age. Eventually a couple of gentlemen from the Old Boys Association of Bishop

Stortford College, a small home counties, Hertfordshire, public school whose old boys had magnificently collected funds for two refugee boys, came along and interviewed me; and I was one of the lucky boys to be chosen. And so, in April 1939, I appeared at Bishop Stortford College as a new boarder, duly fitted out with grey flannels, blue blazer, and straw hat boater, tuck box and all! Like a real English public schoolboy! And I was afforded an English public school education, which was very good.

MR: How was your language at that time?

OH: Well, I had learned English at school from the age of eleven onwards. I had done four years of English and so I think I picked up on lessons fairly quickly. The physical rigours of English public school education were quite another matter. I never managed to cope with cricket: I instead became the scorer.

MR: How was the Jewish side of things?

OH: Oh, there just wasn't any, but when it came to Passover and so on, a Jewish family was found.

MR: They were sympathetic?

OH: Oh yes and I didn't attend assembly. They knew who I was and I knew who I was. I had been in the Chajes Gymnasium [Grammar School, founded 1919] in Vienna.

MR: Chajes, that's like Chaim?

OH: No, Chajes was the name of a chief rabbi of some years past [Zwi Perez Chajes, 1876–1927]. It was very much a Zionist-orientated school, where we learnt a lot of Hebrew and other things. That is also why the school taught English rather than French, because, Palestine was under English mandate. Many of my contemporaries did emigrate to Palestine. I learnt a good Hebrew there. Let me just – I didn't premeditate at all – you notice I have still got an Austrian accent. There was one person who could immediately place it, a taxi driver in New York, going through Hoboken tunnel 30 or 40 years ago, in the days when you still sat in the front with the taxi driver, when they were all PhDs. He engaged me in conversation and after two or three sentences, he turned around and to my astonishment he told me 'sir, you were born in Vienna and educated in an English university'. I said, 'how on earth do you know?' He said 'in New York that is easy!' But anyway, I speak English with an accent. Then 10 or 15 years ago I very exceptionally visited Germany – I think it was the 65th birthday celebration of Wolfgang Trautwein, with whom I had worked and who was a very good friend of mine. I visited him and I tried to speak a few words in German when I had the chance to make a little speech. Afterwards, Wolfgang told me that I now speak German with an English accent! There's only one language that I can speak without an accent and that is Hebrew. When in Israel, when I've given seminars there and practised some introductory sentences in Hebrew, I was paid the compliment that I speak Hebrew with a perfect accent. The Chajes Gymnasium Hebrew accent was like Queen's English, you see! So there are my linguistic problems.

TT: When you got to the school, were there any particularly influential teachers?

OH: Yes, I had a very good zoology teacher, who was a visiting teacher from another school. Also a very good chemistry teacher, and a very conscientious physics teacher. It was a good education – we had to write up our practical books and they were then looked at and annotated. We had to write up our notes and everything was looked at. Chemistry I enjoyed – there was a lot of practical chemistry in those days for 'O' levels and 'A' levels, i.e. School Certificate and Higher School Certificate – volumetric and gravimetric analysis and qualitative analysis, hydrogen sulphide all over the place. These days it would be all forbidden! [laughter.] Hydrogen sulphide coming out of the Kipps apparatus. We also did a lot of dissection – dogfish and embryology too – the smell of formaldehyde dissecting dogfish aortic arches! Cockroach, dogfish, rabbit, frog, they were all dissected according to practical books. This has gone by the board now, hasn't it? I was obviously doing well, especially in chemistry and biology. I also set my heart on winning the English essay prize – just to show off! And I duly did – I also won the science prize. My sponsors were absolutely delighted that the foreign boy should have made good. The essay was on something to do with philosophy of science. I had laboured away over Christmas to stick it together.

When it came to leaving school, it was war-time, 1942. I had been a lance-corporal in the Cadet Corps. I even knew how to dismantle and reassemble a Bren gun, but I realised that I wasn't Pioneer Corps material. That was the only corps into which an enemy alien, as I still was then, could join. I had helped with the potato harvest and whilst all the boys could take a sack of potatoes, pick it up, and sling it onto the back of a lorry, mine always fell down. I just didn't have much physical strength, you see. So the sensible thing to do was to seek some other kind of war work. I also then still hoped that my parents might still be alive and that I might have to look after them after the war. I was given one or two choices. The one that appealed most was to take a job as a laboratory assistant at the Wellcome Physiological Research Laboratories. It happened that one of the old boys of Bishop's Stortford College, probably one of the contributors of the funds on which I was educated, was John Moore, who at the time was vice-chairman of Burroughs Wellcome. He had his office in this building [183 Euston Road] on the ground floor. So in the spring term of 1942 I came down by myself from Bishop's Stortford as a schoolboy, walked in through these hallowed portals of 183 Euston Road to be interviewed by Mr Moore. I was probably the only schoolboy ever to be interviewed here for a job.

MR: What was Mr Moore's position?

OH: I think he was vice-chairman, or vice-president, of Burroughs Wellcome, the commercial side. I don't know whether Burroughs Wellcome was the Wellcome Foundation. I think the Wellcome Foundation was already separate. I cannot be sure.

MR: Burroughs Wellcome existed in Dartford.

OH: Burroughs Wellcome had a factory in Dartford, but that was the manufacturing side.

TT: The manufacturing company became the Wellcome Foundation. It's very confusing, because the charity is the Wellcome Trust. The Foundation is the company and was created from Burroughs Wellcome in 1924.

OH: The WPRL [The Wellcome Physiological Research Laboratories] were part of the Wellcome Foundation I think, but we had close liaison with Dartford of course. For instance, the insulin, which I was engaged to help standardise in my first year and a half there, was manufactured at Dartford. When I arrived, I was given a brief interview by J. W. Trevan who then assigned me to the biological standards division, which was probably closest to his own heart. He had been a statistician; LD50 and all that was J. W. Trevan's doing. We tested protamine-zinc insulin, a long-acting insulin, which was then a relatively new product. It had to be standardised in experiments that involved taking blood samples from rabbit ears for eight hours after the injection of insulin, to plot out the blood sugar profile after the injection. There was a team of two or three assistants who did the bleeding. I still can cope with a rabbit's ear. It's quite an art to take samples for eight hours without getting it all nastily inflamed. Some of the tests were crossover tests where 24 rabbits, that is four groups of six, were switched over for experiments for four days over two weeks. Quite an elaborate standardisation.

MR: That was in this building?

OH: No, that was in a shed – in a sort of prefabricated shed.

MR: Where was the shed?

OH: The shed was in the lovely grounds of Langley Park, Beckenham. There was the mansion in Langley Park and our division, which was the division of pharmacology, had its main part and laboratories in the mansion, but biological standards was 50 yards away in a prefabricated building. That's where I spent 18 months of my youth and met my future wife. So some good came of it!

TT: What did your wife do?

OH: My wife was a Beckenham girl and she was also interested in biology and doing work in the laboratory. Later on she did nursing. But we met at WPRL. Before I left there was another now notable physiologist who joined us. That was the young Tom Sears who came at the age of 16, a very tender age, to start bleeding rabbits with us.

MR: He worked on the bench with you?

OH: Yes, yes, and we were all under the strict eyes of a lady called Hertha Müller who had been a laboratory technician of some calibre in Frankfurt. Everything had to be washed, cleaned – well for good reason – because blood glucose was then assayed by the Hagerdon–Jensen method which was really a method for measuring reducing substances.

TT: Could you say a little bit about that method.

OH: Blood had to be precipitated with zinc hydroxide – a well known method for precipitating whole blood – and the final back-titrations for excess oxidizing agent substance were done with the famous Wellcome titration syringe [the Trevan syringe] because quite small quantities were involved. The plunger of the syringe had to be held against the micrometer head with a bit of rubber band. It was all done carefully and professionally. It was repetitive work but we were young and quite happy.

Tea was tuppence halfpenny and you could have as much bread and butter for that as you wanted. A good dinner in the dining room was sevenpence. My [weekly] salary was 35 shillings and 6 pence, but greatly increased by an extra five shillings for fire watching. Without those five shillings for fire watching, life would have been very dull.

MR: What were the hours of work then?

OH: Normal sort of hours. Nine to half-past five.

TT: Did you work on Saturdays?

OH: I wouldn't think so. No I'm pretty sure not, because after all I did a good deal of study and was given every possible facility. I was a perfectly ordinary employee, but there was a little asterisk against my name and everybody knew that I was trying to carry on with studies. I had left school with enough to carry on. I did chemistry during the war at Birkbeck, where the lectures were excellent. Physical chemistry was by Professor Ives; I forget the organic chemistry lecturer. The practical facilities were appalling. This was when Birkbeck was still a rickety building in Chancery Lane, before the new building. I just took the schedules and went back to Beckenham, where I had quick-fit glass-ware and good fume cupboards at my disposal.

In London during the war two institutions never closed. In both one could study the human body. One was the Windmill, the other one was Chelsea Polytechnic where you could study physiology. Well I chose to attend Chelsea Polytechnic although I have been to the Windmill once.

MR: They did evening and day classes?

OH: I think it was entirely evening classes during the war.

TT: So you were working in the lab at Beckenham, and you were coming up to London to study.

OH: I was coming up to London twice a week in the evenings.

TT: In the blackout?

OH: Oh yes, one took this in one's stride.

TT: And at the weekends you were doing?

OH: At the weekends, at least for the first 18 months or two years I took chemistry ancillary and finished with chemistry from Birkbeck College, I mean for degree purposes. So these were Saturday/Sunday courses. Tuesday and Thursday evenings were at Chelsea. The one really inspiring teacher at Chelsea who did influence my career was R. A. Gregory [Roderic Alfred Gregory CBE FRS, 1913–1990] who then was lecturing at Leatherhead. The medical students of University College had been evacuated to Leatherhead and Gregory had come back from America where he had worked with Babkin. He was one of the few people who was retained to keep the medical course going. In the evenings he came from Leatherhead to lecture at Chelsea. He was an inspiring lecturer and I remember learning all about Babkin pouches and Pavlov pouches. He was the first eminent physiologist that I met.

I ceased with the biological assay after about 18 months and moved on into the Mansion to assist Dr Gertrude Glock who was then doing research on the mode of action of thiouracil, a new antithyroid agent. That was in the main lab in the Mansion. There, figures from the original WPRL had left their mark: in one cupboard one could still find little jars of ergot extract labelled H. H. Dale [Sir Henry Hallett Dale, OM, GBE, PRS, 1875–1968] and G. Barger [George Barger, FRS, 1878–1939]. Quite early when I came to WPRL, I was told that Dale was giving a course of lectures in the Royal Institution – I think the first one was in 1943. I was given time off and encouraged to go up to London to hear these lectures. They started at four o'clock in the magnificent theatre of the Royal Institution – the first time I had ever been in such splendid surroundings – it was tremendously impressive. Then this upright, white-haired man, still youthful in those days, came out with one minute to go and he stood there silent for a minute, waiting until the second hand said exactly four. Then he started to lecture. They were wonderful lectures, but in the first year I went I couldn't understand a thing. I was enthralled by the surroundings, by Sir Henry's sonorous voice and everything else, but I had far too little physiology to understand it. This was a course of four lectures which he gave four years running and I was there for each of them. The second time round, I could already understand some of it. I think the third time round, I really relished it and the fourth time it was just pure entertainment and to hear the jokes again. He used to rail against David Nachmanson [1899–1993] and tell tales about how the censor crossed out the word 'Torpedo' in the correspondence between Feldberg [Wilhelm Siegmund Feldberg, CBE, FRS, 1900–1993] and Fessard [Alfred Fessard, 1900–1982] who had worked together in France early during the war on electric fish.

TT: So the lectures were on acetylcholine?

OH: Yes, the lectures were on chemical transmission. They covered the whole field of chemical transmission as Dale saw it between '43 and '46. He didn't change them, I am sure of that.

MR: Did he give the same lecture – you heard it four times.

OH: I think he gave the same lecture, because I knew which jokes were going to come and I still enjoyed them.

MR: It was a performance of a piece. He didn't sort of work things out as he went along?

OH: Oh, no. They were set formal lectures. Lectures were still lectures in those days, both in the choice of language, resonance, and everything else.

MR: He gave them without notes?

OH: I can't remember whether he gave them without notes or not. I wasn't sufficiently critical at that stage to study the actual art of giving lectures. They were illustrated. I can't remember whether he did experiments or not. Royal Institution lectures are always extremely well-presented and prepared. They were one of the few events of their kind in wartime Britain. They certainly were very informative.

So I had got as far as I could by studying externally and when it came to VE day I had two main quests – one was of course to search whether my parents had survived. I spent many days helping with listing survivors and one thing and another. The other search was for a place to do a proper honours course in physiology. I was advised to write to Lovatt Evans [Sir Charles Arthur Lovatt Evans, FRS, 1884–1968], whose 1942 edition of Starling [Ernest Henry Starling, FRS, 1866–1927] had been my bible. I still have that treasured copy, tattered copy, in my books. And that's how I arrived, I suppose it would have been in the autumn of 1945, October or thereabouts, at University College.

The physiology building was then still occupied by the Admiralty who had its use. Lovatt Evans had only two rooms – they eventually became my laboratories – on the corridor leading to Pharmacology at the top of the stairs. It was the old operating theatre next to the demonstrating theatre. I was half an hour early in my anxiety, I think it was a Monday morning. Lovatt Evans was coming up to London from Porton [Down] where he was still working; I was waiting in this corridor. Eventually a gentleman in a trench coat topped with an Anthony Eden Homburg [hat], an unusual combination, came up the back pharmacology staircase. I asked him – didn't think it could be Lovatt Evans – did he know where Professor Lovatt Evans was? – and he said 'well that's I' and he took me into his office. He treated me with utmost courtesy, as if I was already one of his colleagues. I had never, up to then, received such courteous gentlemanly treatment. I was only a boy, a laboratory assistant coming along. He listened to me with great sympathy, we had a very friendly chat. What I did not realise at the time is that Lovatt Evans himself had made his way up through being an assistant in chemistry in Birmingham and then came to work for Starling and took medicine. He obviously was quite genuinely sympathetic. It also turned out that I was the first applicant seeking BSc Physiology course. Like Rabbi Akiba [Akiva ben Joseph (Hebrew: עקיבא בן יוסף) *ca* 40–137 CE] – who is quoted in Samson Wright's textbook [Samson Wright's *Applied Physiology*, OUP] to have said 'more than the calf wishes to suck, does the cow wish to suckle'. Lovatt Evans was just as pleased to find a student as I was to find a teacher. He told me that they hoped to start the intercalated physiology honours course in April 1946. By then some of the medical students would join. The second MB in those days was taken in March and the intercalated BSc course was a four-term course, namely the summer term and the subsequent session. I had already qualified for one of the FET grants, the Further Education and Training Grants, which were one of the wisest acts of the Attlee government. It made funds available for anybody who had been in the services, or who had done essential war work, to go to university, if they had the necessary qualifications. They extended the eligibility to all people in the Allied Forces, Poles and others, who had done war service. The astonishing thing then was that many of the refugee boys, who had initially been sidetracked into various apprenticeships, had through the war somehow or other studied and served. Many of these refugee boys found their way back into education through the FET grants. Many others like myself have had similar experiences.

TT: Were you a British citizen by then?

OH: I became a British citizen in 1947.

TT: Had there ever been any question of you being interned?

OH: Oh yes, in 1940, when I was still at Bishop's Stortford, I had turned 16, so I was an enemy alien. How the British Civil Service was unable to distinguish between refugees and enemy agents beats me. But anyway, I was an enemy alien. Others in my situation were interned or they were transported to Canada or Australia. Some of them, unfortunately, were torpedoed and lost. On the other hand, some fell on their feet, established themselves in Canada or Australia and made good careers there. My headmaster accompanied me to a tribunal in Hertford in the summer of 1940 and vouched for me. So my education was not in any way interrupted.

MR: Was it the 18B Act?

OH: The 18B was also the Act under which Mosley [Sir Oswald Mosley, 1896–1980] was interned. Oddly, no difference was made between him and refugees.

MR: Did you come across Elizabeth Ullman [later renal physiologist and lecturer in Physiology at St Bart's, London].

OH: Oh yes, of course, Elizabeth Ullman was at Chelsea Polytechnic at the same time and in the same class as I was.

MR: She was a bookbinder wasn't she, before she became a physiologist? You were a contemporary of hers?

OH: Yes, and Tris Roberts [Tristan D. M. Roberts, d. 2009] was the other person at Chelsea who became a physiologist.

TT: When you were at WPRL you were living in digs were you?

OH: I was living in digs and we were there all dodging the doodlebugs. Actually I was a voluntary stretcher-bearer in the hospital, most of the evenings. The digs I lived in, the house was bombed and the people evacuated. I lived in an old bomb shelter behind the house. My possessions all went into a very small suitcase. Most evenings were spent in the hospital.

MR: When were you married? You were single in those days? At what point did you marry? At what point in your career?

OH: I was single in those days. We married in 1948, a year after I graduated. The course we had at University College, which we were mentioning, the main tutor was Len Bayliss [Leonard E. Bayliss, 1900–1964]. That was the very first course after the war. My two colleagues, one of them was then called June Hill. She became Mrs Doug Wilkie [Douglas Robert Wilkie, FRS, 1922–1998]. The other was David Fryer, a very bright pleasant man who went into the Air Force Pathological Service and unfortunately was killed in a car accident.

TT: So there were just three of you?

OH: There were just three of us on that first course. Len Bayliss built amplifiers out of bits and pieces. Mr Parkinson, A. V. Hill's assistant, had bought up an immense store of army surplus electronic equipment and Bayliss built the amplifiers. They were always in National Milk tins. They really ought to have been preserved. They were wonderful things. They worked – just. Bayliss's design philosophy was that he designed an amplifier and then he pulled out the valves one at a time to see how many were really needed. You will know more about this than I, but apparently this is a perfectly good way of designing electronic equipment: to reduce the original design once you had got it built.

MR: Was Grace Eggleton [Marion Grace Eggleton, 1901–1970] there then?

OH: Grace Eggleton was there.

MR: We have these pictures of the staff of those days

OH: A picture on the stairs of the department. That was taken I think at Lovatt Evans' retirement wasn't it? I have that picture too.

MR: Did we identify everybody in that picture?

OH: If you can't I think I can, not necessarily here, because I have also the menu of the occasion, with all the people's signatures. So with these signatures I could identify the odd person such as the Brazilian Mendez, and various people of whom you can see only half a head. I can help you with that one. There was that smart Davson [Hugh Davson, 1909–1996] standing at the bottom of the stairs.

What else did we have? We had a Lucas pendulum [Keith Lucas, FRS, 1879–1916]. It wasn't an ideal time to learn electrophysiology, because the modern work wasn't yet available in 1946, certainly not for the students. The only bright point was Hodgkin's [Sir Alan Lloyd Hodgkin, OM, KBE, PRS, 1914–1998] local circuit theory. But for most of the time, as far as electrophysiology goes, we spent our time with the Rushton–Lapicque literature [William Albert Hugh Rushton, FRS, 1901–1980; Louis Lapique, 1886–1952] and with A. V. Hill's [Archibald Vivian 'AV' Hill, OM, FRS, 1886–1977] theory of excitation, of u and v. That was a very formal mathematical theory of excitation. That part wasn't inspiring at that time and my own interests [were] more towards the mammalian physiology. We had an ample supply of decerebrate cats and we worked our way through Sherrington's Handbook on Physiology [Sir Charles Sherrington, FRS, 1857–1952] from cover to cover, doing some experiments many times over until they worked in our hands.

MR: Charlie Evans [Head Technician, Physiology Department UCL] was active in this?

OH: Charlie Evans was active in doing the decerebrations, and in helping altogether. He had of course worked with Lovatt Evans.

MR: Len Shaw?

OH: No. I did see Lovatt Evans do a heart–lung preparation once. My elevated role in it was to defibrinate the blood, whipping the blood with a wire brush. Towards the end of the course Bernard Katz [Sir Bernard Katz, FRS, 1911–2003] had come back from Australia and we had some lectures from him.

TT: Did A. V. Hill lecture?

OH: No. AV wasn't around a lot. I don't know whether he had already started his work again or whether he was still engaged with the Royal Society and other business of winding up after the war. Katz taught us a little and I remember an exam question which Katz set us in the finals was 'discuss the gaps in our knowledge on the origins of the nerve impulse' or something of that kind. It was a very clever question. It was not on what you knew, but on what was not yet known.

MR: What kind of a man was he in those days? The same as now?

OH: I can't tell you. Pretty busy and he gave us his time willingly, but he had lots of other things on as you might very well imagine. He was making gains on many fronts. I had the good fortune of meeting Cyril Rashbass [1927–1982]. Did you know him? He was a contemporary of mine who was a student in Cambridge and who subsequently worked with Rushton on the function of the sheath of mammalian nerve which acts as an ion permeability barrier. Towards the end of the year Rashbass was in London swotting in the Thane library, like I was. I knew him and we had several chats. He told me all about the most recent work going on in Cambridge – that was already in 1947 you see. So when it came to the finals I had a flying start, knowing about things which had not yet been published. It was perfectly fair – the search for knowledge – and I rather suspect that this helped me get a good degree. I was in fact placed second – I got a first class degree, but I was placed second after a medical student from St Mary's. The medical student did not want the University Postgraduate Studentship which the university offers to the top candidate in each subject. So it fell to me. I stayed on at University College, and as the first graduate I automatically became a demonstrator for the subsequent years. I was in the fortunate position of never having to look for a job as people at that stage now have to do.

MR: Do we know who this other benefactor was?

OH: Who the person who beat me was? No, I suppose there are records. Do you think I should write to him now?

MR: It would be interesting to know what became of him.

OH: So I stayed on and demonstrated to the students in subsequent years. Almost everybody who came back to College had either been in the army or in some service or another or had some experience. So one's so-called students were often older than oneself. I can remember, in particular two streams in those days. First the people whom A. V. Hill brought from operational research into physiology. Amongst these were Murdoch Ritchie [Joseph Murdoch Ritchie, FRS, 1925–2008], Eric Denton [Sir Eric James Denton, FRS, 1923–2007], Bud Abbott and other now well-known physiologists. They were all put through the physiology mill before they started biophysics research. So you can see I learnt as much from the people who followed me as I had learned during my course. The other group were pharmacists who had taken a degree in pharmacy at Bloomsbury, at the School of Pharmacy, and who wanted to do research and to take a PhD in physiology, with a BSc first. Amongst those was Jim Pascoe [James (Jim) Edward Pascoe, 1924–2011], Liam Burke [b. 1922], and W. E. Brocklehurst. Then there was John Bligh who made a career in temperature regulation and later became director of the Arctic Research Institute in Anchorage. They were often a year or two older than me and more experienced. There was plenty of give and take. There was a wonderful atmosphere in those years as far as the teaching went. On Monday mornings I came in to decerebrate three cats with Charlie Evans helping. Even when I got to the stage of doing three cats in 45 minutes he still would not give me the accolade of being as good as Stella (of Bronk and [G] Stella). Stella was at University College pre-war and, according to Charlie Evans, he was the best decerebrator ever. But he would give me second place.

TT: What about your research?

OH: My first two attempts at starting on research were failures in the sense that they gave negative results, but I am extremely proud of the negatives results which I obtained and insisted upon as them being negative, because had I by any chance obtained a positive result I would look terribly foolish now.

OH: Lovatt Evans knew he was soon to retire. He told me that he was not returning to research, he was harbouring the Department's resources for his successor. So I had to find some other person to stimulate me. One was Andrew Wilson [CBE, 1909–1974], who was then lecturer in clinical pharmacology. He was interested in myasthenia gravis. With all that I had learned about acetylcholine from Dale's lectures, that was also a topic that interested me. Wilson had a theory that the thymus gland produces a curare-like substance, because (i) thymoma was often associated with myasthenia gravis, and (ii) thymectomy was demonstrably helpful in myasthenia gravis. Wilson had collected thymus glands from thymectomies in the hospital and he wanted me to extract and assay these glands to demonstrate a curare-like substance. So it was a question of setting up frog rectus preparations, standardising them for acetylcholine and testing these different thymus extracts. Try as I may and convinced as Andrew Wilson was, I could demonstrate no curare-like action. The only action which I could demonstrate could be shown to be due to the potassium in the extract. Of course, today we know that myasthenia gravis is an autoimmune disease and that the thymus is involved in a very elaborate way. If under pressure I had somehow produced a positive result it would now be embarrassing.

MR: Your training at Burroughs Wellcome must have made you much more strict about controls, etc.

OH: Oh yes, I got better. Towards the end of my time at WPRL, I was involved in the pyrogen testing of penicillin. That involved a dozen rabbits each with a thermo-couple in their back-side, a cold junction, a galvanometer and a manual switchboard. My job was to fit all of the couples, etc. and then sit during the day working that switchboard and keeping notes on the temperature of the rabbits. I remember that on several occasions the batch was pyrogenic. Then A. C. White [Adam Cairns White, 1901–1962], who was the head of Pharmacology, and Johnny Trevan [John William Trevan, FRS, 1887–1956], used to come along with very long faces and look through my notebook at every figure. I myself didn't realise what it really meant at the time. Penicillin was in terribly short supply. It was being produced […] at WPRL by surface-culture in half-filled milk bottles. A large dairy-like factor was built in the grounds of WPRL for the purpose. It was closed down after a short run once Glaxo had introduced the higher yielding deep culture method. When a batch had to be rejected that was loss of life-saving material – it was in terribly short supply. So I was used to pressure and having my work looked at.

MR: So if you hadn't had that training, you might have... it must have helped.

OH: I dare say it helped. I have come across spurious positive results more recently. Muscular dystrophy full of positive effects, which don't seem to be possible in the light of recent development. Well, anyway that was my first go at research. Lovatt Evans then advised me that the best way to get started in research is to pick up somebody else's work and try to repeat it, and then to pursue it in some direction or other. So my next project was based on a paper I had come across by McGhee Harvey [Abner McGehee Harvey, 1911–1998], who worked at Hampstead under G. L. Brown [Sir George Lindor Brown, FRS, 1903–1971]. Harvey had come from Baltimore, the home of J. J. Abel [John Jacob Abel, 1887–1938] of tetanus toxin fame. In a paper in *The Journal of Physiology* in 1939, Harvey claimed that local tetanus was of peripheral origin. It was a complicated experiment which depended on the timing when the tibialis anterior was injected with the toxin in relation to when its denervation was carried out. Harvey claimed that with appropriate timing the denervated tibialis anterior showed local tetanus. I had been interested in tetanus toxin since the Wellcome days. A lot of toxin, antitoxin and the toxoid was produced down in Beckenham. Old army horses were used because they possessed a high initial level of immunity. When they were killed, we got horsemeat, which we took home, though not for human consumption. I took it home to my landlady for her dogs – she was delighted. I think that's why she kept me!

MR: Where did you live then?

OH: In two or three different places in Beckenham. One of the landladies didn't like my name, Otto. She felt that if her boys, who were seven or eight, were to say at school that somebody named Otto lived at home, people would think that they housed a German spy. So for a few years I was called Fred in that house.

TT: What about when you were at University College, did you live in the students' accommodation there?

OH: Ah, when I was at University College, I lived, the first year, in Tavistock Square in a room which was part of the International Students House. It's now one of the Halls of Residence. It was a slip of a room, it took a bed and about three feet, at the most, at the side of it, which I fitted out with a little electric ring. I had a little kitchen corner. I was self-sufficient there. There was a United Dairies in a street off Tavistock Square and if you were on good terms with the ladies there, they would tell you which days they had some cake delivered. Food was very short. Marchmont Street – that's right – I did my shopping in Marchmont Street, and one really had to have one's wits about one. It was also the winter 1947 when we had the coal shortages. In 1948 we got married and lived in a bed-sit in Belsize Park. When our first child was on the way – we didn't dilly dally – we were pretty desperate. I put up notices everywhere, at University College, seeking accommodation, etc., etc., etc. One day I had a phone call at Belsize Park – 'this is Mrs A. V. Hill'. 'Oh!' 'Well, I have seen your notice at University College and you say that you are in physiology, right? Would you be interested in a flat I have?' Now it turned out that Mrs A. V. Hill [Margaret Neville Hill, née Keynes] was one of the very first people who was interested in gerontology and who had a series of Hill Houses – as they were called – in which she provided accommodation for elderly people. That is now very common, but in 1949 it was quite unfashionable. It was youth everyone was concerned with. But Mrs Hill was the leader in that new movement. One of her houses was no longer suitable for old people. It had a basement flat, euphemistically called a garden flat nowadays. The house was in Crouch End and she offered the flat to me. We were, of course, absolutely delighted. We lived in different flats in that house for some 10 years. So physiology came in very handy.

TT: Did you have any contact with the Jewish religion during this period? Did you manage to retain your contacts?

OH: Oh yes. As soon as I had left Stortford and was by myself again, I made contact. I don't know about belonging to a synagogue, because to belong to a synagogue you had to pay and I had no money. Even during the days in Stortford, once the war started people from the East End of London were evacuated to Bishop's Stortford. A school with quite a few Jewish children came to Bishop's Stortford. My sister, who had been in various parts of the country – she came to England on a domestic permit for which work she was quite unsuited – because I was in Stortford, she came to Bishop's Stortford to look after that group of children. So already during the war, the process of reintegration began. It was only a relatively short period in 1939/40 (about 18 months) that I was entirely by myself.

TT: I was just going to ask you about A. V. Hill, because one of the things that he was very important in doing between the 1930s and 1940s was the Academic Assistance Council, and actually helping refugee scholars from Europe. Did you have any involvement in that?

OH: No, because you see I am not really of the generation of scientific refugees. I was younger than Feldberg [loc. Cit.], than Blaschko [Hermann Karl Felix (Hugh) Blaschko, FRS, 1900–1993], Edith Bülbring [FRS, 1903–1990], than Katz [loc. cit.], Kosterlitz [Hans Kosterlitz, FRS, 1903–1996] and Marthe Vogt [Marthe Louise Vogt, FRS, 1903–2003]. All these were older.

TT: Did you know about this role of A. V. Hill's when you were at University College?

OH: Not really, at the time. But one becomes more aware of it and one saw some of his published correspondence and letters and other things, and one realised that he was one of the giants of righteousness. In years when many people vacillated, A. V. Hill was always on the side of justice and righteousness and freedom and liberty. I had a little contact with Mrs A. V. Hill, because we lived in one of her houses. She was a fairy godmother to us.

MR: Hannah Steinberg [b. 1926] was around?

OH: Oh Hannah, yes. Hannah was a contemporary and she was the senior girl or president of the women's union when I first came to University College, a very much looked-up-to person. When I first came she was already wearing a gown up there on the platform at the induction of new students. She had a brilliant career as a woman student. Later on we got to know each other very well.

MR: Did she come from Europe in those days?

OH: Well, Hannah and I worked out that we must have known each other in our prams, because our parents pushed us in the same park in Vienna. We are almost exactly the same age, and we must have waved to each other.

MR: How is Hannah? I haven't seen her for years.

TT: She's fine. She came to a meeting I organised in November. Yes, she's in good form, still living in Kentish Town I think.

MR: You have some of her crockery?

OH: Oh, I was not going to speak about this. Yes, I still have some of her beautiful handmade dinner set, which Hannah brought from Vienna with her, very similar to the set which we had in Vienna. She never married and it was much too large for her. I bought the larger part of it from her. It is specially kept for Passover and it comes out on those occasions and she is always remembered. It's a lovely set. The individual plates are not even the same shape, because they are handmade. Unfortunately, it's got absolutely no name on the back.

TT: Does Hannah know? Had it been in Hannah's family for a long time?

OH: Oh, yes, it had been in Hannah's family, She still has some pieces of it. It was at least 12 or 18 place-settings.

MR: I have eaten from it.

OH: You must come again. Come to Scotland! Come again!

TT: Well, right, shall we get back to tetanus? It's up to you Otto – you have complete control over us.

OH: Back to tetanus. McGehee Harvey and tetanus. Again, try as I may, and I devised some kind of spring balance to measure the tension in the tibialis anterior, I could not confirm McGehee Harvey's work. I remember going to see G. L. Brown about it, because he was in the lab at Hampstead at the time, but he didn't particularly help me. If Harvey's work has been confirmable, I would have gone on to measure the to-be-expected spontaneous endplate potentials. I had made a plan and given it to Lovatt Evans. It sounded good to me and it sounded good to him. He gave me the Sharpey Studentship on the strength of it. But I just could never confirm McGeehe Harvey's experiment. Of course today we know that not only is the toxin transported up-stage (i.e. centrally) initially, but also that such local or peripheral action on nerve endings as it has is a botulinum-like action, which prevents the release of transmitter. So on all counts McGehee Harvey's findings are implausible. As far as I was concerned they were not confirmable.

There is an abstract in *The Journal of Physiology*, 1951, vol. 112, 1*P*, where this is published belatedly, all by itself, because I failed to send the proofs back at the proper time. What had happened was that in the course of these experiments I got bitten by a cat, an Irish farmhouse cat, I believe, a little tiny nip in this finger. I took hardly any notice of it and put TCP on it. After some weeks there was a tiny irritating spot here. I went to the finger clinic at UCH and after another month my trochlear gland started to swell up, first like a little pea, and then like a chestnut and it spread all up the glands of my arm. I was in hospital, having biopsies, I still have the scars up here, and receiving all manner of treatment: penicillin by intramuscular injection three-hourly, arsenic by intravenous injection. They never found out what it was. Aspergillosis was suspected at the time. Cat scratch fever and subsequent glandular involvement has since been recognised as a clinical entity. I was in a dire state in the surgical ward. Eventually, Dr Alfred Schweitzer [then Reader in Physiology at UCL and a family friend] managed to get Max Rosenheim [Max Leonard Rosenheim, Baron Rosenheim, KBE, FRCP, FRS, 1908–1972] to come from Medicine into Surgery to take a look at me. Max Rosenheim prescribed potassium iodide, which is a very ancient remedy against gammatous lesions, fungoid infections, and so on. It worked like a dream. I was off everything in about three or four weeks. But when I stopped taking potassium iodide, it all came back. I then took potassium iodide 10 g a day, in five 2 g lots, day and night, for two years. That did the trick. It's worth mentioning, because cat bites are a hazard of the physiologist. Dale was bitten by a cat – do you know about this? When I was later introduced by G.L. as 'the boy who had the cat bite', Dale mentioned his similar experience. But he didn't have any biopsies. He was sent to Devon by his family doctor for a holiday and the swellings resolved spontaneously. The surgeon told me: 'Next time you get bitten put your finger into your mouth and suck.'

TT: So you must have lost quite a lot of time?

OH: Yes, I was ill on and off for about six months. That picture taken on the occasion of Lovatt Evan's retirement you were referring to, I was not completely well yet by then. I came out of UCH to join in on this! By the time I had recovered G. L. Brown had arrived at University College. So that was then the beginning of a new chapter.

... Quite apart from giving me the Sharpey Scholarship, shortly afterwards he [Lovatt Evans] managed to increase the standing of the Sharpey Scholarship and put it on a par with an assistant lectureship, both as regards salary and as regards duties. I was then required to do demonstrating and this type of thing. So in my second year as the Sharpey Scholar I received an assistant lecturer's salary, plus 50 pounds for the first child, Elisabeth. Then in subsequent years, we got fifty pounds for each child born, paid on the salary plus additional child tax allowance. Historians who look at the birth pattern in Britain will wonder why up to a certain year there was a boost in births before 5th of April – how come? All this was due to getting your child allowances before the old tax year ended. If you played the game correctly, and if you were a physiologist, you'd know how, then your children were born before 5th April. Well Elisabeth was born in January. That was because we didn't have much confidence. But Jonathan and Mickey were both born in March, just in time to collect the additional allowance. These child allowances for academics were introduced in the 1930s, I believe, because it was discovered that academic families were not having children, they couldn't afford it. Academics were never wealthy. And some sensible person wanted to preserve the genes. So they tried to encourage academic families to have children by offering extra salary. This has all come to an end. It came to an end in the '50s or thereabouts.

MR: Extra salary potential?

OH: It was something like – the salary then was about £600 and if you had four children, you had an extra £200.

TT: That's a lot, isn't it?

MR: That post-war grant was I think I remember £360?

OH: I think mine was £275. I am sure mine was £275, but that was still more than I was earning in my last year at WPRL. It was over £5 a week. It was about £1 more than I was earning at WPRL. One could live as a single person on that – it was quite an acceptable sum. It would probably be equivalent to what a postgraduate student gets now at the bottom of the scale, probably the equivalent to about £5000.

MR: Well an annual salary was less than £500 – that was quite a good salary – £5 a week. People lived on £5 a week.

TT: We were talking at lunch about technicians. Did you have technical help when you were a Sharpey student? Were there technicians in the lab that you could call on?

OH: Once I started the experiment I am just coming to there was a fellow called Ray Featherstone, a young man who was assigned to me as a technician. You needed an extra pair of hands when you were working on cats. He was an intelligent boy and he left after a few years to become a salesman for typewriter copy paper. Two or three years later he came back to visit me in a large car, very well dressed. Obviously he had done very well for himself. That's why I say he was an intelligent boy. He left research, for technicians a worse career, sad to say, than that for academics. Good technicians are much undervalued.

[MR starts taking photographs.]

MR: We are going to do the whole thing – we are going to create a waxworks of you.

OH: This is the second time this has happened to me. Recently the technician at Medical Illustration [in Glasgow], Ian [Ramsden], took a set of photographs of me for a portrait!

TT: Has it appeared – has he done the portrait?

OH: No he hasn't done the portrait, but he has done the studies. I haven't seen the actual photographs. [The portrait hangs on the first floor landing in the West Medical Building, Glasgow University.]

TT: When did you first get involved with The Physiological Society?

OH: Oh I gate-crashed Physiological Society meetings ever since I was at University College, since 1946, and I gate-crashed the Oxford Congress in 1947 when I was still a student. Never was one to ask by your leave when it came to gaining access to an auditorium. When you say my involvement with The Physiological Society, there were various capacities that started rather later.

TT: What about your first Communication?

OH: My first Communication was the one on local tetanus; my inability to repeat A. McGehee Harvey's [loc. cit.] work.

TT: Where did you give that?

OH: At University College in March 1949.

TT: Was it an awesome experience? People talk about their first Communication with some trepidation.

OH: I know that it pleased. I have always taken great care in preparing formal communications, sentence structure and understandability and so on, because

I have always had problems when listening to difficult communications, if they are not well done. I am sure everything can be broken down in such a way that you take the audience with you. I think it's thanks to a little bit of the training that I had from my father that I could do that. I would like to think that I made a decent job of giving my first Communication. I gave it after G. L. Brown had taken the chair [at UCL] and without any of his or anybody else's help. I think partly on the strength of that G.L. kept me on. He then helped me with my next problem by suggesting that I should analyse post-tetanic decurarisation. That is a facilitation of neuromuscular transmission after tetanus, an old phenomenon which was first discovered by Boehm at the end of the 19th century. Brown and Von Euler [Ulf Svante von Euler, ForMemRS, 1905–1983, Nobel Prize winner] had come across it and had left it unanalysed. G. L. Brown suggested that I should analyse it with close-arterial injection of acetylcholine. He took me into his lab once, showed me how the preparation was done and then, in his typical way, left me alone.

TT: Which animal preparation did you use?

OH: Cat. Cat tibialis anterior muscle, a method which G. L. Brown used and invented to great advantage to demonstrate that acetylcholine was excitatory not only on rectus, tonic skeletal muscle, but also on fast twitch muscle. His demonstration, way back in '36 or '37 that acetylcholine when injected quickly into mammalian muscle produced a twitch, was crucial in extending the chemical theory of transmission.

TT: Did you do those [close arterial injections] – I have seen him on film with a string in his mouth.

OH: I am not sure whether I put it in my mouth, but there is a crucially placed string to restrict the blood supply, which you have to pull at at the appropriate time. The injecting cannula is peripheral and you inject acetylcholine retrograde through the arterial tree. To prevent the blood coming down from the heart, you occlude the artery by pulling on the string. It's a nice technique, not over difficult, but it was difficult enough for me as a beginner. But the real trick and difficulty lay in maintaining a steady state of curarisation. Now in these days you just go out and buy a slow infusion [pump]. But the device that I used was an oil-driven burette. Burns and Dale in 1924 described this slow infusion system. The critical part is a capillary tube through which oil flows from a raised reservoir; the flow of oil is quite slow and it drives the infusion solution out of the burette. Very readily controllable – ever so simple, ever so cheap, ever so easy to use and adjust. So once I had learned how to maintain steady-state curarisation it wasn't so bad, and I got a very nice result: the response to acetylcholine remained the same before, during and after a tetanus, whereas the response to nerve stimulation varied during the tetanus and afterwards. So, one could conclude that these changes were pre-junctional rather than post-junctional in origin. Unfortunately, I have regretted it since, I was pedantic in the title – I called the paper 'Post-tetanic restoration of neuromuscular transmission blocked by D-tubocurarine', *J Physiol* 118, 216–227. Nowadays, in the modern style, it should have been 'Pre-junctional origin of post-tetanic facilitation' and the paper would have lived much longer than it did. Unfortunately its pedantic title hides a result which has always pleased me, and which really was quite an important one at the time.

MR: This was the style of titles wasn't it?

OH: It was the style of the time to be correct.

… [My] next move really did originate from going to [International Physiological] Congresses. I remember the Danish Congress, the Copenhagen Congress in 1950, I attended that legitimately. It was my first trip abroad with a British passport and I was very proud of it. I think everybody was there. It was an emotive occasion in 1950 and we received a wonderful reception. Only the other day I met somebody who was there and we had fond memories of it. I also met Liz [Lise] Engbaek whose work I had followed. She wrote a thesis on magnesium anaesthesia and I asked her to send me a copy. I was interested because we had used magnesium sulphate at WPRL to dispatch rabbits in a cheap way. It was non-poisonous by mouth, so you could eat the rabbit meat afterwards. On one occasion, I am not quite sure how this came about, somebody demonstrated to me the classical experiment of Auer [John Auer, 1875–1948] and Meltzer [Samuel James Meltzer, 1851–1920] on magnesium anaesthesia and its reversal by calcium chloride. If you inject magnesium sulphate into a rabbit intravenously, the rabbit collapses, stops to breathe and is to all intents and purposes dead. The heart keeps on going because it is not affected as much as is the respiration. If you leave the needle in the vein and inject calcium chloride down it within a short enough time, the animal revives instantly, shakes itself, sits up and continues eating. It is absolutely fantastic. It's a well-known experiment. I've always remembered it. So I was interested in the mode of action of magnesium and my next work was on the effect of magnesium on acetylcholine release. Again G. L. Brown demonstrated the perfusion of the superior cervical ganglion. This was not his own technique, but Gaddum [Sir John Henry Gaddum, FRS, FRSE, 1900–1965] and Feldberg's [loc. cit.]. But G.L. had used it. He demonstrated it to me once, a long and fairly complicated dissection, gave me a pair of Collison forceps – you know the ones which fed out the cotton for the multiple ligatures which you had to make – and again left me alone. I was joined by Krista Kostial [Krista Kostial-Šimonović, b. 1923] from Zagreb who came over as a visiting foreign worker. Together we did experiments on perfused ganglia with magnesium, and we found that magnesium prevented the release of acetylcholine and that this was reversible by calcium.

MR: This was your original finding?

OH: Yes, that was a nice finding. At the same time Castillo [José del Castillo, 1920–2002] and Engbaek did quite similar experiments electrophysiologically. So this all came together, but the origins were separate. There was an interaction immediately afterwards. Fatt [Paul Fatt, 1924–2014] and Katz [Sir Bernard Katz, FRS, 1911–2003] had suggested that the release of acetylcholine from the nerve ending was by an ion exchange process in exchange for sodium. So we put this to the test by trying the effect of perfusing ganglia with solutions deficient in sodium, but leaving enough sodium to get the nerve impulse to propagate. We also tested the effect of abnormal sodium on acetylcholine release produced by potassium. In neither case was sodium essential. Again this was a negative result, but Fatt and Katz themselves, within a year, produced other data which rejected their original hypothesis.

MR: Were you in friendly competition with them?

OH: No, no, I don't think that I was ever the calibre to compete with Katz. It just happened – maybe it was just a bit of opportunism. One had a method, somebody had put forward a hypothesis, one had the method to test it, and one went ahead and was encouraged to do so. The result turned out negative, but that was the right result. Fatt and Katz themselves later produced other evidence.

MR: They used it with great strength in their...

OH: No. I cannot remember the details to tell you the truth. I was already in America when I brought out that paper and when I sent it to G. L. Brown. He wrote back to say he was quite happy with it because Fatt and Katz themselves had changed their minds. Ours was an independent conclusion. It was sound work and it was the right result with a different method. The point I really want to make is that by 1953 I had at last broken through and had done a few good experiments and published a few nice papers. But the methods which I was using were the methods of the 1930s, they were the methods of Feldberg and Brown. The methods with which I had more or less established myself as a physiologist were close arterial injection and superior cervical perfusion. I had done a lot of mammalian physiology, as I explained; I demonstrated a lot of mammalian physiology; I was by then quite a competent dissector and operator. But I became more and more aware that I was becoming outdated, because by 1953 we had Fatt and Katz (1951) on endplate potential with the intracellular electrodes. I had done a little bit of extracellular recording of endplate potential, but I was falling way behind modern work. So through G. L. Brown – he was a very powerful person and sat on all the right committees – thanks to his patronage I was awarded the Rockefeller Travelling Fellowship.

TT: You had your PhD by then?

OH: Yes. The desire and the need to travel and to get the Rockefeller Fellowship finally persuaded me and put the pressure on me to write up a PhD. You know, in those days, you didn't have to get your PhD in three years or at all for that matter.

TT: Like Jim Pascoe.

OH: Yes, and what mattered in the PhD was not that you had gone through some phoney training programme, but that you had made a contribution to knowledge. It was only when one felt that one had made a contribution to knowledge that one put it together and submitted it. So, yes, I think I submitted in '52 or '53, I can't remember exactly when, and I had my PhD when G.L. put me forward for the Rockefeller Travelling Fellowship. Both on his suggestion and my inclination I asked Stephen Kuffler [Stephen William Kuffler, ForMemRS, 1913–1980] in Baltimore to take me and he very kindly agreed. So sometime after Labour day in 1953, September 1953, a bedraggled Hutter family, looking for all the world like poor immigrants, with second-hand cases and boxes, because we were still quite penniless, arrived on Baltimore station after a nine-day stormy crossing in a bow cabin of a 6000 ton Holland-America line ship. We arrived in Baltimore to be received by Stephen Kuffler with all kindliness. He himself had had plenty of his own migrations. He was born in [Tap,] Hungary, studied in Vienna, graduated in medicine in Vienna. Then he went to Australia.

TT: He came to this country for one year.

OH: He came to this country? What year?

TT: 1938. Then he went to Australia. I didn't know until recently.

OH: Still doing medicine in this country presumably.

TT: Yes, then he went to Australia. Katz was there at the same time.

OH: Yes, and he met – this is nothing to do with my interview – but he met Eccles through playing tennis. Stephen was a very good tennis player and somehow or other he used to play Eccles at tennis. Eccles took a liking to him, saw what sort of chap he was, and put to him that he should stop doing medicine and come and join him in the laboratory. On the first day when Stephen was in Eccles' laboratory, Eccles asked him to join up the battery to the amplifier. It was a large car battery. Stephen said he connected one pole of the battery to the other and everything went bang. Stephen used to tell this story to illustrate how innocent and ignorant he was of anything to do with electricity or physiology when he first joined Eccles. Stephen was wonderful in telling jokes against himself, you see. Just in the same way as Feldberg used to insist that he doesn't know the formula of acetylcholine. You have heard him say that?

TT: When your family went to America, how many children did you have?

OH: We had two, aged two and four, when we arrived. We came with two and we left with three. I had always wanted to have an American citizen in the family you see. Being born in the 1920s, to have somebody to send you an affidavit could be a lifesaver, so I took great care that my daughter should preserve her American citizenship.

MR: Elisabeth is an American?

OH: No, no, no. That's the second. Judith is American. It came in quite handy. Well now, Stephen [Kuffler] settled us in a flat on Broadway. In 1953 meat rationing was still on in Britain. In America, of course, it was luxury. Even if you were on a poorish grant for a large family, as we were then, you could live well in Maryland, in particular on turkey wings and chicken livers, which were dirt cheap. I remember having many meals at home on turkey wings which my wife had bought across the road in the supermarket. In the laboratory, Stephen took me into a largely empty room, except for a microscope, one Bunsen burner, a box full of capillary glass and a bucket. He adjusted the burner to a sharp blue-edged flame, and showed me how to pull a microelectrode by hand and how to view it under a x100 water-immersion objective. He just took the microelectrode and put it under the microscope and he said 'here you are' and walked out. He left me in the room; there was this box of glass; there was this bucket. It took me about two days before I even learned to locate a broken tip under the microscope by gradually working down onto the shadow. There was no other lens on the microscope other than the x100 objective! Stephen did it in one second; he never told me how to do it. In the subsequent days, the bucket got full of glass and the box got lower and lower. I think it must have been unhappiest week of my life. I was really tempted to pack up and go home. I thought I would never be able to pull microelectrodes. Then suddenly the spell was broken and I got the feel of it, could judge the colour of the glass and so on, and I could produce microelectrodes. I was then joined by Werner Loewenstein, who has since made a name for himself. We started to work, on Stephen's suggestion, on the sympathetic nerve facilitation of neuromuscular transmission. That is the Orbeli phenomenon, as it is known from the Russian physiologist who first discovered it [Leon Orbeli, 1882–1958]. And sure it is there. But it is a very small and fickle phenomenon. We did complete the job, but I made up my mind then never again to work on a small effect. Even if these small modulating effects are physiologically important. What you want is a nice large phenomenon.

MR: There aren't many around.

TT: What preparation were you using?

OH: The frog's sartorius muscle dissected together with the motor (sciatic) nerves and the sympathetic chain both being available for stimulation. Not too difficult. That occupied most of the first year, most of the working season anyway. The habit in Stephen's laboratory was that he and his entourage would move to Woods Hole in May. Baltimore becomes very hot and unpleasant. So we all packed up and went to Woods Hole in the summer of 1954. That was a wonderful experience because Woods Hole was a Mecca for biologists from all the east coast. All the eminent people were there. It was the summer in which Jim Watson [James Dewey Watson, KBE (hon.), ForMemRS, b. 1928] came back to America from Cambridge. His were amongst the many lectures that were held there in the mornings for postgraduates who came to Woods Hole. I remember hearing him. I remember hearing Heilbrunn [Lewis Victor Heilbrunn, 1892–1959], who was alone in prophesying that calcium is involved in almost everything within the cell. He was considered as rather cranky. And I was warned off him – a sad fate he had. I don't think he ever saw the success of his theories. The other person there, whom I remember, was Harry Grundfest [1904–1983], who early foresaw the complexity and diversity of ion channels. For instance, that there would be some channels which would be turned on by hyperpolarisation. Again he was considered as rather iconoclastic at the time, because at that time the Hodgkin [loc. cit.] and Huxley [Sir Andrew Fielding Huxley, OM, PRS, 1917–2012] system, involving an increase in permeability with depolarisation, was paramount. But most important for me was the presence of Otto Loewi [1873–1961] at Woods Hole. You know how the Americans are inclined to adore Nobel Prize Winners. Loewi wasn't their Nobel Prize winner, but he was a Nobel Prize winner in America. After lunch on the veranda outside the refectory Otto Loewi used to hold court and I was duly introduced to him by Stephen. Otto Loewi spoke a curious mish-mash of German and English – he had never learned English that well. He was an old man. When he discovered that I could understand his mish-mash, he beckoned me over repeatedly after lunch to come and sit at his feet and keep him company. He quizzed me on all sorts of things in physiology. He asked me what work I had been doing and I described to him those perfusion experiments with low sodium which I had just written up. He then drew my attention to the sodium–calcium antagonism – he still had his head well screwed on. But, in particular, he asked me what I knew about cardio-vascular physiology and the heart. He was a great admirer of W. H. Gaskell [Walter Holbrook Gaskell, FRS, 1847–1914]. When he found that I knew nothing about Gaskell, he was horrified and he sent me off into the library, which was open 24 hours a day, to read up on Gaskell. Then he called me along to see whether I had done it or not. He would have been cross if I hadn't. I was so afraid that the old man would have a stroke or heart attack whilst I was with him that I obliged.

MR: How old was he?

OH: At that time he looked old to me. He must have been in his 80s by then – this was 1954. [Loewi was 81 in 1954.]

TT: He died in 1963 [25 December 1961 – loc. cit.] didn't he?

OH: Yes, his wife was with him in Woods Hole. So he would have been in his late 70s. Not so much older than I am now!

MR: But I remember in 1963 you gave a talk at Mill Hill about meeting Otto Loewi. Was this the same?

OH: That was the only time.

MR: I had a funny idea it was in South America. Maybe you were talking about South America at the same time.

OH: By '63 I would have been to South America yes.

TT: Was Otto Loewi actually doing anything, any experiments in Woods Hole? Or was he just holding court?

OH: No, no. He was just there as an Emeritus on the American scene and, as I say, I think he brought his experience to bear in the discussions which he had with people. He was a little bit idiosyncratic, but that was understandable. Anyway, he made me learn all about W. H. Gaskell – I did it reasonably willingly, but I really had no particular interest then in cardio-vascular physiology. I was committed to the neuromuscular junction and the various factors that influenced its behaviour. I think I should have done some work at Woods Hole, but I can only remember going to lectures. I can remember going boating across to Martha's Vineyard and sailing; I can remember a hurricane, but I can't remember what work I did, if I did do any work. I had a jolly good holiday with my family. I don't regret it, it was one of the relatively few occasions. I didn't see much of them in the year before.

MR: It was a couple of weeks was it?

OH: Oh no. It was a longer period of time. It was possibly something like six weeks or two months. It was quite an extended period. It ended up with the experience of one of the hurricanes which comes along and swept up Cape Cod. It did a fair amount of havoc to the Woods Hole laboratories, a large percentage of which were housed in basements; they were all flooded.

Well, we went back to Baltimore and I was teamed up with Wolfgang Trautwein [1922–2011] whom Stephen had accepted into the lab. It could have proved a disaster, but it didn't. Wolfgang had served in the German Army during the war in Russia. I was, and so was Stephen, a refugee. Stephen knew who he'd got. Wolfgang was fortunate that he was born and brought up in Konstanz. Konstanz is on the Swiss border and I think the Nazis took care that the rigours of Nazism didn't come to be seen from across the border; Konstanz was overlooked by the Swiss and the international community. I think Wolfgang's father was a schoolteacher. I think Wolfgang wasn't much touched by Nazism and he was invalided out of the Army fairly soon during the Russian campaign. I don't know exactly why. He became a medical student and then worked under Hans Schäfer [1906–2000] who was interested in cardio-vascular physiology in Heidelberg and that is what brought him to Baltimore.

Now by the time Wolfgang came, Stephen had already suggested that I carry on with experiments on the neuromuscular junction, in particular the effect of stretch on the neuromuscular junction. Stephen had observed that in some jaw muscles the safety margin of transmission was very low, unless the muscle was stretched, and he suggested that we might analyse that phenomenon which fitted into the pattern of my past work very nicely. Wolfgang hadn't worked on the neuromuscular junction and was quite willing to have a go at it, but he also wanted to carry on working on the heart. So we agreed that we would try and do both things in the season. By then Wolfgang was already quite an experienced experimenter and it was the first time that I was working together with another person who was quite experienced. I enjoyed it very much and we became very good friends. We dispatched the stretch work within a few months. So we had enough time left to go on to the cardiac problem on the action of vagus which Wolfgang had wanted to tackle. It was, by this chance contact that I moved in this direction. My contribution was to bring the tortoise into these experiments. I knew, through reading Gaskell, what advantages it had. It worked very nicely and we demonstrated hyperpolarisation, and got various other nice pictures, which were original for their time. We worked very, very hard. Because it was moving preparation, you had to have slender, flexible electrodes and we got a record that lasted for 20 seconds once every few days. There were many, many, many failures for the few successes.

Our third child, Judith, was born during that time. I saw very little of her in the first three months of her life.

We left the laboratory when the preparation gave up. It was always a question of which heart, ours or the one under the microscope, would give up first! Late at night in Baltimore, near the hospital, was not the best district. Wolfgang used to wear a very wide-brimmed hat. He looked like a gangster. We had to walk across a bridge to the bus stop. It was a little dangerous narrow detour. I suggested to him that we two should walk together so that other people should think that we were the gangsters and cross over the road away from us, rather than us having to do it. So we pretended to be tough guys in Baltimore! It worked, we didn't get mugged even near midnight. That was a very productive period. It also brought me to a new interest, which I then imported back to this country when we returned.

TT: Could you describe the differences in American and British labs at that time.

OH: Oh yes. I was a bit staggered by the lack of formality, because in Britain one still addressed people, well not so much G.L., as 'Professor' and so on; one didn't do what in German one called 'Dozent' – to use the second person – so much. The habit of calling everybody by their first name had not yet become established in the 1950s in Britain. If anything, one used the surname or the initials, something like A.V. or G.L., if one wanted to be at all familiar. So this business of 'Stephen' and 'Horace' [Horace B. Barlow, b. 1921, worked with Stephen Kuffler at the time] was a bit strange to me. Being a shy and slightly formal youngster, I found it difficult to get used to it. I will give you one illustration. The head of physiology, not of Stephen's unit, but the physiology unit, was Philip Bard [Archibald Philip Bard, 1898–1997] and I could not avoid calling him 'Professor Bard' and saying 'Yes Professor'. This went on for a week or two, until one day Bard turned round and said 'Otto, will you stop calling me Professor? In this country that's what they call the fellow who plays the piano in the whore house!' I think that cured me of it! I tell that story regularly to newly appointed professors, of which there are plenty nowadays, just so that they shouldn't get too swollen headed. And I have always remembered myself not to let that title go to my head.

TT: What about the equipment in the lab or the numbers of people working, technical assistance and so on?

OH: Well, I wouldn't like to judge. I don't know what things were like generally. I think, generally, in 1953–54 when I went to various local meetings, they seemed to me parochial and backwards compared with British physiology. At that time I went to Federation meetings and so on and the standard didn't strike me as very high. This, of course, couldn't be said about Stephen Kuffler's laboratory in the Wilmer Institute. There was Horace Barlow there from Britain working at the time. Eyzaguirre [Carlos Eyzaguirre, 1923–2009] and Kuffler doing wonderful work on the crustacean stretch receptor and, generally, Stephen had gathered good people and good equipment and funds around him. I remember, now you come to mention it, with particular fondness, Bob

Bozler, who was Stephen's main technician. Had you heard of him? Not E. Bozler, that was another Bozler, Emil Bozler [1901–1995].

TT: Yes, I am thinking of Emil Bozler.

OH: No Bob Bozler was a extremely skilled and knowledgeable electronic technician and engineer, and I learned more about the mysteries of earth loops and impedance matching and all that sort of thing from Bob Bozler than from anybody else. He was really a tutor in electrophysiology. I dare say he even taught Stephen quite a bit. He was very able and he also was one of the first people to design a mechanical electrode puller. I remember arguing the point with him whether it was really worth it now that I had accomplished this wonderful art. And he said, 'Well in America we set about to make a machine for everything. It's always possible to make a machine – if you can do it, then a machine can do it.' He was a very typical American, a first-class engineer who knew that things could be mechanised. Production could be mechanised. That was certainly one of the times when one felt that one had come in contact with the real America. But you see most of the other people down there in the basement of the Wilmer Institute were from Chile and from England – it was pretty international. We didn't have all that much contact with the real America.

TT: So then you came back to UCL, bringing your apparatus with you?

OH: I came back to UCL in 1955 and found the place much changed. The generation of experienced students had passed through. The students were now straight out of school. Everything was a great deal more ordinary, or at least so it seemed to me.

TT: Did you come back to a job?

OH: Oh, yes. I was appointed to an assistant lectureship before I went to America, because with Rockefeller Fellows it was always a condition that you had a job to come back to. You never got the Rockefeller Travelling Fellowship unless you were in an appointment and had an appointment to come back to. They didn't want you to be at a loose end. I might say now that you mention it that at the end of the time with Stephen I had the opportunity to stay in America. Otto Krayer [1899–1992] at Harvard, who was a cardiophysiologist then, offered me a job. But I felt considerable loyalty for this country. I'd moved once, I didn't feel that I wanted to move again. There may be people in this country who might say that I should have stayed – that the loyalty was misplaced! But never mind, I did come back. When I got back I was given the job of lecturing, on the heart and circulation, on the strength of the little bit of work I'd done on the heart. That's the way it goes. I had an awful lot to learn.

MR: Was G. L. Brown still there?

OH: Yes, he was still there. I tried to follow in the tradition of Lovatt Evans, whom I had heard give lectures. Lovatt used to start by saying 'don't take any notes, just listen to the noise which physiologists make'. And he gave plenty of demonstrations as part of the lectures. I did a good deal of that. There was one demonstration that was memorable, because it went wrong. It was the usual experiment on the factors influencing the blood pressure like sharp pulling on the carotid and cutting the vagi. On that one occasion I had clamped the carotid arteries and cut one vagus. I then cut the second vagus and the blood pressure shot up so much that it blew the mercury out of the manometer. Blood spurted all over my face. I realised in a flash that I had forgotten first to take the clips off the carotids. So I whipped the clips off the carotids and the blood pressure came down. There was just enough mercury left in the manometer to keep down the blood pressure after the animal had lost all that blood. I wiped my face and turned around to the class and started to explain what had happened, how I had effectively denervated both the aortic arch and carotid sinuses. For years afterwards I met people in Gower Street, medical students who stopped me, and who told me, 'do you know I learnt more about the control of blood pressure that day, when you got blood all over your face, than at any other time'. One can use this to illustrate the value of live demonstrations, especially when they go wrong!

TT: You were giving lectures on heart and circulation. Were you demonstrating, practical classes, or just demonstrations?

OH: Not so much practical classes then. I think I had become that little bit more senior. I also participated in the teaching of the BSc Honours Class. That would have been comparatively easy, straightforward going. I know that the lectures on heart and circulation were, initially at least, quite a new thing for me. Even though I had learned a little bit of cardiac physiology, there was a vast field of circulation and many other things of which I had only some practical experience. It was one of the core parts of the curriculum. So one had to deliver them properly. It took me a year or two before I worked myself into this. Research wise, shortly after coming back I made contact with E. J. Harris [Eric James Harris, b. 1915], who was up in Biophysics. He was one of the people at University College who was *au fait* with ion flux methods. I asked him to show me how to do ion flux measurements on heart muscle and the sinus, because the obvious corollary of the hyperpolarisation which we had found was that there would be an increase in potassium permeability. I wanted to demonstrate this by ion flux measurement in the same way as in the classical work on nerve – electrical work was balanced by ion flux measurements. And E.J. was only too willing to show me, and he was a remarkably active man, a nervous man. The counts in those days were still taken manually; you had to wait a minute after setting the clock. E.J. had a bit of jade which he used to polish. He bought broken bits of jade and polished them up in the minutes when the counts were taken. He could never stand still. Never. Always on the move.

TT: He was in Biophysics? Was that a separate department?

OH: Yes, Biophysics. I can't remember exactly what its status was. In the days of A. V. Hill – I can't remember whether A.V. was still there – in the '50s he was there.

MR: You mean there physically or there in charge.

OH: Well, no, there in charge. I mean he did experiments on the active state of muscle in the middle ’50s. He probably was still in charge. But there was also Bernard Katz. I think it was called the Biophysics Unit. You know its history – its connection with the Rockefeller Foundation. But I don’t know whether it had been adopted by then as a University department. It was never part of Physiology. It was a sort of independent unit which was set up by the Rockefeller Foundation. Eventually, it became a university department under Bernard Katz and then moved into other quarters. At that time, I can’t tell you what it’s status was. But E.J. was the doyenne of ion flux measurement. We got nice results. An experiment which I did in my room pleased me most. It was on a preparation of the sinus with the vagus nerve attached. The sinus alone was in the bath and I demonstrated the increase in potassium permeability not by adding acetylcholine, but actually on vagal stimulation. It worked very nicely and was controlled by atropine, to make sure that the release of potassium wasn’t from the nerve endings.

MR: Were you working alone then?

OH: Well, I did that experiment alone, but my learning of the counting technique and how to present these results, I owe that to E.J. We published an abstract giving the essential results together. Unfortunately, E.J. went off in an odd direction; he became enamoured with Gilbert Ling’s [b.1919] views that the cell consisted of a fixed internal charges and that the cell membrane didn’t really play such an important role as the permeability theory suggested. This really was an iconoclastic, idiosyncratic view which I don’t think anybody else really followed.

MR: A bit like [Harold] Hillman today.

TT: Which Ling?

OH: Ling, Gilbert, is the same Gilbert Ling who first made microelectrodes. The same Ling as Ling [loc. cit.] and Gerard [Ralph Waldo Gerard, 1900–1974]. But he developed strange ideas. It’s one of these episodes, where people put forward new ideas, which is something which needs to be done. Sometimes it is the seed of new things to come, sometimes it turns out to be a mistake. E.J. had a very open mind and he was attracted to Ling’s view. He tried to interpret these experiments in that way. I wasn’t prepared to go along that way. So what happened eventually to that work was that I published most of it under my own name, but in a variety of review articles, other than the joint abstract [*J Physiol* 133, 58–59*P*] which was fully quantitative. It hasn’t been written up as much as it should be. But it’s all there.

There were plenty of foreign visitors, at University College when I got back. I had two of them in the immediate years after returning from America. One was John Fales, a contact made in Baltimore, from Philip Bard’s department, who came over to work with me. The other one was S. M. Padsha [b. 1921] a young Pakistani medical who had come to University College for postgraduate education. With Fales I tried to carry on on the cardiac side and to record action potentials from the AV node and the effects of acetylcholine on it. It took us a long time to sort out the anatomy of the AV node and we did get a few records of action potentials with slow rates of rise. But one could never be sure that the slow rate of rise was physiological, because we couldn't hyperpolarise the preparation. One couldn't, or at least we never succeeded in that. So one couldn't really conclude that the slow conduction in the AV node which we'd noticed was due to a deficit of sodium channels the only inward current path known at that time. The alternative hypothesis was reduced inter-cellular connectivity. I was trying to be 100% right, and I let it slip through my fingers. I have been sorry for it since. This is one of my regrets. It was the beginning of several other things which later on I didn't properly finish off as I should have done. But the other work with Padsha went rather well.

A. V. Hill and his colleagues had then just shown that replacement of chloride by nitrate produced an increase in the muscle twitch which was ascribable to an action on the membrane. I thought it would be interesting to see what anion-replacement did to neuromuscular transmission – whether it did any facilitation along my old theme. So I set Padsha to record extracellular end plate potentials to see if anything happens when you replace the chloride and, lo and behold, the end-plate potentials rose and they greatly increased in length. It looked very much like an increase in membrane resistance. So then, instead of releasing acetylcholine as a means of depolarising the membrane, producing current through the membrane, we used a Wheatstone Bridge system to produce electrotonic potentials. This was a simple way of measuring changes in membrane resistance which Katz had used years earlier and which he drew to my attention when he heard about the work we were doing. We set up a nice way of measuring membrane electrotonic potentials. It requires very large non-polarisable electrodes, huge, silver–silver chloride plates, because very large currents were passed. Once we used large enough electrodes, we got nice evidence that replacement of chloride with other anions produced sizeable changes in membrane resistance. That was the first indication that the chloride conductance of muscle was very much larger than people had hitherto thought.

After that I had the chance to pick a student from our home-grown BSc classes. The outstanding person was Denis Noble [CBE, FRS, FRCP, b. 1936], who had done extremely well. He was a medical student intercalating a BSc and obviously very good. I can't quite remember by what negotiations I managed to get him to come to my laboratory. But, by then I had intercellular recording electrodes, and we set-to to measure quantitatively of the chloride conductance in skeletal muscle, and then in cardiac muscle – all which give nice results. The work involved a little bit of cable theory and constant-field theory. Denis took all this in his stride. There were plenty of people up in biophysics who could give advice on these sort of things, but I myself was not able to give it. He worked with extreme intensity. I must admit that for the first couple of years it was really no more than I expected from a home-grown good student. I expected full devotion and 100% effort. It wasn't until the third year he was with me that I started to realise I was dealing with someone quite unusual. In America I had joined, because everybody can, the New York

Academy of Sciences and chosen one of their volumes to receive regularly. It must have been in 1959, in a volume of the New York Academy of Sciences there was a paper by Andrew Huxley, where for the first time he was using a computer to show the effect of temperature and other factors on the nerve action potential, including how one can get from the Hodgkin–Huxley model repetitive discharges which as far as the pacemaker potential goes, looked like cardiac pacemakers. I remember the thing arriving on my desk and my perusing it then going to the back of the room where Denis had his desk and saying 'look here's something for you to do, i.e. on cardiac muscle'. I kind of half meant it, I won't say in jest, but I just sort of threw it out as something new. Well, I didn't see much of the fellow for the next month. He took himself off to somewhere in Bloomsbury where Mercury, the university computer, a very low-powered thing, a very slow thing at the time, was located and, picking other peoples' brains, he taught himself how to use the computer. Another month later he came up with something that had a semblance of a cardiac action potential. The thing was refined through experiments with an anomalous rectification which we'd done and which we showed was present also in cardiac muscle. A visit to me by Dick FitzHugh [Richard FitzHugh, 1922–2007], who was in Steve Kuffler's lab, a mathematician interested in that kind of problem, also helped Denis. He took off from that point and [has] made a wonderful career.

TT: I was going to ask you whether you could say something about the equipment you had in your lab. Were they still home-made?

OH: The equipment I used in my earlier experiments was all home-made and put together. I was completely familiar with it. I had done all the soldering on it. After I came back from America, I must have had some grant or some money from somewhere or other, I can't recall exactly from what source, but I then had a moderately good set-up, typical of the biophysical set-ups of those days – Zeiss sliding micromanipulator, Cossor, oscilloscope and a hand-operated camera.

TT: Were there averagers?

OH: No, not yet. That would have been a few years later, I think. This was still, '55, '56, '57.

MR: No the Biomac was the first commercial one, wasn't it?

OH: It was before any kind of digital technique. It was before transistors of any kind. Amplifiers were still in separate stages and had to be balanced.

MR: You still averaged by superimposing traces?

OH: Yes, yes. Every stage of a DC amplifier had to be balanced and that sort of thing.

MR: You made your average by superimposing traces – you did it graphically?

OH: Yes, film. At least one knew what one was doing.

MR: Wet physiology.

OH: Yes, it was wet physiology. But I was tolerably well equipped by then. Then in 1960 I was invited to join Feldberg's division.

TT: Who invited you?

OH: I can't remember quite how the word came, but what I learnt later, to my astonishment, was that I was recommended to Feldberg, who was looking for an electrophysiologist because there had always been an electrophysiologist there; I was recommended to him by Hodgkin. So I think that my interview with Sir Charles Harington [1897–1972] at the time was probably a formality, I certainly can't remember it lasting very long, and I took the job with glee. I had hoped to go to Mill Hill. When I was eventually able to put down the deposit for a mortgage it was a house in Mill Hill which I bought, whilst still at University College, just on the off-chance that I might perhaps get the opportunity to work at Mill Hill. So that opportunity came and I grasped it and started there in 1961, after a trip to South America which I remember enjoying.

TT: What were you doing in South America?

OH: I was invited by [Walmor] Carlos de Mello who was interested in cardiac muscle. But the problem he was interested in when I arrived, I thought, had already largely been solved. But they had that wonderful electric eel on which I had never worked. So I agreed with Carlos that I would work with him for two weeks on the heart, I can't remember what it was on, and for two weeks on the electric eel. And, in fact, we demonstrated the presence of anomalous rectification in the eel electroplaque, which was fair enough. When I came to demonstrate this in Mill Hill after my arrival, I got a bit of electric eel sent to me; one notable, very notable, physiologist at the demonstration wondered whether I had removed all the sodium from the jelly, which was difficult to remove from the fish. There was a lot of jelly on one of the faces. Because if sodium had been present, the inflow of sodium would have changed the current–voltage curve in such a way as to resemble inward ratification. I couldn't answer this properly and again, for the second time in my life, I let something drop, because it was open to criticism, which is so stupid because it was quite right. Nowadays I would not worry about such criticism. I would take the risk. But at the time I wanted to be sure that I was absolutely right, and if I wasn't sure that I was right I used to drop things – and it was always a big mistake.

TT: Who was the physiologist?

OH: I am not going to tell.

TT: It's confidential?

OH: No, no I am not going to tell.

MR: When did Anne Brooks [Anne Warner, 1940–2012] come into the story?

OH: Ah, yes thank you for bringing her into this. I brought Anne with me from University College as a new postgraduate student. A slip of a girl, a Twiggy sort. I chose her on what turned out to be very good criteria. She worked for the operatic society at University College as stage lighting expert. She did all the lighting effects for the UCL Operatic Society. And I argued that if the girl could work behind stage with wires then she ought to make a good electrophysiologist. She was a very good student in other ways also. So I offered her the opportunity of coming to Mill Hill as a postgraduate student and she accepted it. So that's how Anne came to work with me and she stayed with me for four or five years.

TT: She became a member of the staff herself, didn't she?

OH: I cannot quite recall whether she did or didn't – quite possibly yes, but eventually she came to the conclusion that she wanted to branch out into a new field and was advised by Ricardo Miledi, who she had consulted, to go into developmental physiology. Again, she made a very good career for herself there. I had by then become ambitious, wanted to understand things fundamentally. I had read up Michaelis, and learnt about the effect of charged membranes on ion permeability and set up the hypothesis that at an acid pH, when a membrane is positively charged, it should be more permeable to chloride than at an alkaline pH. We went to test this and found exactly the opposite position, i.e. muscle membrane proved more permeable to chloride ions in alkaline than in acid solution. Now it was such a clear result and it was so nice. In a sense it both perplexed and pleased me. This was the first time that I had set up a physico-chemically based hypothesis and it completely let me down. But it was a nice large effect and we analysed it, both electro-physiologically and, following on what I had learned from E. J. Harris, with isotopes. We went on to study the effect of foreign metal ions and so on, and got quite a nice series of papers out of it, which also then provided material for a PhD for Anne before she eventually left.

I was very happy in those first few years at Mill Hill. I had inherited a wonderful laboratory, the laboratory which G. L. Brown had designed for himself when the Mill Hill Institute was built, when he was still at Hampstead. It was a basement laboratory which was fully screened and with an absolutely solid floor – you could walk around whilst you had three microelectrodes and a muscle fibre without these coming out; and absolutely no noise whatsoever. The only trouble was that it was the lowest point in the building. As happens in London from time to time in the summer, there were storms. Water would then come up from the drains and you get a local flood. I remember this happening once when my main concern was to salvage records, films, which were in bottom drawers, because I was afraid of losing them. So I was collecting those films and putting them high up whilst other people were collecting the water and running upstairs with it. But this flood never got any better. So I went upstairs myself to see what was going on up there. They were taking the buckets from downstairs and emptying them into the sinks upstairs. So the water came up again on my floor!

MR: How was the general atmosphere? Feldberg was in charge?

OH: The general atmosphere in those years was wonderful. Just before that, let me finish. I had a wonderful lab. I also had as sole assistant, Jack Spratt who was one of the really skilled electronic engineers and mechanical engineers at Mill Hill and presumably earlier at Hampstead. He gave me all the benefit of his skill and within a year or two I had a really very good set-up. It was a joy to work. Any improvements, any bit of mechanics necessary, he made for me, and made for me quickly. I was very happy and on top of that was the wonderful atmosphere which Feldberg created around himself. He was larger than life. He was wit and wisdom.

MR: Did he take an interest in your actual research problems?

OH: I can't say that he did. He was interested whether or not we were making progress. No doubt he sounded out other people to find out what their view was. But his own interests were then on temperature regulation. He was still perfusing brain from the inner surface as he used to. His own interests were very different from mine. He was broad-minded enough to have an electrophysiologist in the unit, but we were largely independent. But coffee time was always an event. People did foregather in a small preparation room and Feldberg was always ready with either an anecdote or joke or some advice. I can't bring them all to mind, but one of them which comes to mind is: if you have children at university who won't write letters, just send them a short note and say 'I enclose five pounds, have a nice time, or spend it as you like' – and don't enclose the five pounds. Then you can be completely sure that you will get a nice letter back to say that you forgot to enclose the money. Also you should never give any explanation of why you are refusing to attend somewhere, because if you say it's too far, they will send you an air ticket; if you want to take your wife, they will send you a ticket for your wife. The moment you offer an explanation, they will argue with you.

MR: He could either have been a scientist or a rabbi. I think he would have made a good rabbi.

OH: You mean like Lionel Blue [b. 1930]

TT: Who else were your colleagues there [at Mill Hill]?

OH: Sidney Hilton [1921–2011] was still around. Vic Abrahams, who later went to Hamilton, Canada, or Toronto. Graham Lewis was there for some years. These were the early names I can remember. There were others. It wasn't all that long before Feldberg formally retired from the headship of the division in 1965; there may have been a little interval before Ben Burns was appointed. Tim Bliss [Timothy V. P. Bliss, FRS] came, I am not sure with or after Burns.

TT: Ben Burns had been at Hampstead.

OH: Ben Burns had been at Hampstead with G. L. Brown and [F. C.] Macintosh and he had then been in Montreal.

MR: Tim Bliss was with him in Montreal.

OH: I was never as happy there after he [Burns] came as I had been before. For one thing he snatched Jack Spratt away from me…

Feldberg had an absolutely immense self-discipline. You will know of his obituary notice of Henry Dale in the *Proceedings of the Royal Society*? Well, I was next door when he wrote this and he never missed a day's experiment. He wrote this wonderful piece of literature between 8 o'clock and 10 o'clock in the mornings. He came in an hour and a half later than usual, did his normal day's work. You would not know that he was composing and writing all this. He had this perfect discipline of productiveness, of being able to do one job for a period of time, put it down and do something else. Whilst I was there, when you did go and speak to him or he read your papers, because in those days you gave your papers to your head of division, never mind whether they were in his field or not, he could always make improvements and suggestions. He could put down the pen in the middle of his paper, which he wrote in handwriting, beautiful gothic script, ready for typing, with hardly any alterations, and pay attention to your work, go back to his work. He could switch from one topic to another. He would have made a wonderful businessman, or magnate, or bank manager. In fact, he did have quite large business interests in Germany, I believe. He used to go across to Hamburg, once in six months for a day or two, make the necessary decisions. He used to come back and say, you've got to make decisions, you are going to be wrong half the time anyway, so you might as well make them quickly.

TT: That was where the money for the Feldberg Foundation came from – I think it was shops or something.

OH: I don't know exactly where the money came from, but I think he had some ongoing interests. Wasn't his family wine-merchants or something? I really don't know enough about it.

TT: They were merchants of some kind.

MR: I thought they had a department store. It was the Selfridges' of Hamburg, I think.

OH: Or was it a department store. But he certainly had some ongoing business interest with which a lesser man could occupy all his time. He despatched it in two days in the year, once every six months.

MR: The best thing about him was his humour. The thing one remembers is his humour.

TT: What kind of differences did you find moving from a university environment at UCL to Mill Hill?

OH: What I felt was important and eventually decided me to go back to a university was that at Mill Hill you could not attract to you the bright young students. I had been lucky to be able to do this once or twice at UCL. Sure, you could get students, but the best students had always been creamed off, and held back in the universities themselves. The development of a Post-graduate School at Mill

Hill has now changed this. So I persuaded myself that that was a good enough reason for going back to university and I started to keep an eye open.

This may be a good moment to start talking about my involvement in The Physiological Society, because it was the spare energies during my Mill Hill years, when I did not have teaching responsibilities, which made me seek some other outlet, perhaps mistakenly, to give me a little variety.

Perhaps the first, was acting as referee and then as a member of the editorial board. My early papers were refereed by people like Bryan Matthews [Sir Bryan Harold Cabot Matthews, CBE, FRS, 1906–1986], Feldberg and Huxley. They always signed their reports and when, later on, I was myself called upon to act as referee and then as a member of the editorial board, I followed that salutary example and always signed. I think it's a good thing to do. It makes sure that you are civil. It makes you reasonably humble. After all you might do better to ask a question, than to throw out a nasty remark, and I wish that it were still done these days. I served on the Editorial Board for five years, mostly at the beginning of my time in Mill Hill. That was the early 1960s and it was always time-consuming, but always rewarding. Then later in the '60s, as my children went to middle school, I became aware of the struggles of biology teachers to keep up with the advances in physiology, especially nerve and muscle physiology, which had been made in the '50s and '60s. These were just about coming into the school curriculum. Together with a small band of enthusiasts, I started courses for school teachers in various polytechnics, further education colleges, in the home counties.

TT: Was this on behalf of The Physiological Society or was this the Education & Information Committee [of The Society]?

OH: This was how the Information & Education Committee came about. I got the support of The Physiological Society to launch these things and then shortly afterwards was nominated to join the [Society's governing] Committee and the Committee then agreed to set up the Education & Information Sub-Committee. The first Chairman was O. Collier who was in the pharmaceutical industry, and he had an interest only on the information side. But it soon became the Education Sub-Committee, with myself as chairman from 1969 to 1972. Really I think this was the beginning of a change of culture in The Society, because up until then The Society was only devoted to the oral and written presentation of scientific work and to maintain the conditions necessary for research such as animal legislation – countering the anti-vivisection lobby. But it [The Society] felt it had no responsibility for physiology as a whole, in other spheres. I felt that as The Physiological Society was the only organisation of physiologists, it should take a wider interest in the subject and that we needed to attract good students into our courses. It was sailing with the wind, but somebody had to hoist the sail.

Afterwards, I think thanks to Eric Neil's [1916–1990] patronage – he was then chairman of IUPS – I was asked to take on the chairmanship of the Commission of Medical Physiology of IUPS, which was pretty defunct at that time. That was the beginning of the first of the teaching workshops after congresses. The first one we had was after the New Delhi congress, a fairly spontaneous affair, but it went quite well. They have now become a regular feature of the congresses. The argument has always been that one needs to create a reason for physiologists from developing countries to come and attend the congress, get support from their government to attend the congress, and through attending the congress to become stimulated to do research. The intention was always to promote physiological research by new routes, but since those groups' prime responsibility was teaching, one had to make some provision on the programme to justify their attendance. This has now become a regular feature. The other event which we organised was that exhibition in Paris in 1976. By that time Laurence Smaje was the Chairman of the Education Committee and I was the Chairman of the IUPS Committee. We worked together very closely and had a very enjoyable time. The British contribution was the strongest, not surprisingly as we were the nearest. We produced a very nice catalogue. I don't think that has been repeated, that type of exhibition. Maybe it would be out of place now because in the electronic age rather different things could be arranged. Somebody needs to look at this again and see what is the appropriate thing to do these days. From there, I went on to become a member of the Council of IUPS, which was a pleasant type of job. There you get a lot of travel and meet a lot of people. This was both in the later years at Mill Hill and in the early years in Glasgow. An enjoyable episode, I've always enjoyed travel. I don't do much of it now, but nice to have done it whilst one had the chance.

TT: How long were you associated with the IUPS Council?

OH: I was on the IUPS Council for six years and I'd been Chairman of the Commission for six years previously, so altogether it was a 12-year stint for IUPS. I have substantial piles of archives, both of The Physiological Society period and of the IUPS period, which I don't know whether you want me to dump them on you at all or not. I mean The Physiological Society ones perhaps?

TT: Yes, yes. Don't throw things away. Send them to me rather than throw them away.

OH: I've kept quite a few things. I've put a lot of work into this, perhaps more than one should have done. But I have enjoyed it. I did it because I enjoyed it.

TT: Talking about the Society and IUPS, we've bridged your move to Glasgow.

OH: Yes. Well, that came in early 1971. As I said largely from the desire again to have students and to have a chance to attract postgraduate students. I can't quite remember how now, I think John Gillespie [1926–2009] had a little to do with it, whispered into my ear that I should apply for the job in Glasgow. There were last-minute alarms after my interview. Was I a British subject? Had I not been a British subject, Her Majesty could not have signed the Royal Warrant, this being a royal appointment. But what one has to remember is that the Regius Chairs were imposed upon an unwilling Scottish university by Queen Victoria. Originally there were the 13 professors of Glasgow University, five in divinity. Sometime in the middle of the 19th century Prince Albert persuaded Queen Victoria that Britain was falling behind in science, that she should herself appoint additional professors and impose them upon the reluctant universities. There's nothing Scottish about it at all. Nor are Regius Chairs in any way Scottish. There are some Regius Chairs in Oxford and other places.

MR: Are Regius Chairs always the Head of the Department?

OH: Well, actually, when I took on the job there was already Ian Boyd [1927–1987] in the Buchanan Chair and we then shared the headship of the department in a kind of alternating way. Ian was a remarkably able and competent man and our partnership, I like to think, was a little bit like the Hapsburg eagle – two heads but only one pair of executive hands (or claws). We divided up the job. We were good and sincere and very open partners. We never misled each other and we worked well together and appreciated one another's very different strengths. We were never close friends, we were very different in background and upbringing and everything else, but we were good partners. There was a lot of work to be done. Garry [Robert Campbell Garry, 1900–1993, Otto Hutter's predecessor as Regius Professor at Glasgow] was an excellent teacher in the school of Scottish dominies, but he was a great traditionalist and there was not an oscilloscope in the practical class. It was still all kymographs.

MR: Was Garry still alive then?

OH: Oh yes, he was alive and stayed alive for another 20-odd years. He was the perfect gentleman and he did what he thought was right. After he retired he would never set foot in the department again, at least not until his 80th birthday, when I had to prevail upon him to come back for some festivities. Very different to what I have done after I retired, but never mind. In 1970, I took a holiday in Israel before I went to Glasgow and I was really rather disappointed that Glasgow at that time had no inaugural lecture for its professors. So I decided to make my first lecture to the medical students, it was scheduled as the first lecture after Christmas, to make this something like an inaugural lecture. I was to lecture on nerve and muscle and I put together a lecture entitled 'Cables and Nerves' in which I discussed Kelvin's work on the submarine cable and its significance and relation to nerve transmission. I still have the text of this lecture and it's a very good lecture, but I had absolutely no idea of what Glasgow students were like, especially the first lecture after Christmas, when boy meets girl after an interval. I had great difficulty to bring the class to quiet. There was no question of standing there like Sir Henry Dale for a minute, until there was a hush. The hush just never happened. At the end of the lecture one girl came up and asked me, 'Sir, was this for our benefit or was it for the exam?' Only gradually did I realise that teaching in Glasgow to students who had been used to the Scottish schoolmasters was inevitably spoon-feeding, which was anathema at University College. It took me a long time to gradually get used to it. I tried to wean them away from it, I don't think ever with any very great success. I started off by giving lecture–demonstrations. Times had changed – I no longer wheeled in the dog to show the blood pressure, but I still showed the tortoise heart and things like that.

But even that I gave up. [Some of ] my staff rebelled against the spinal frog as a practical class, because the students were abhorred by the movements of the preparation. It was such a general rebellion that I had to give way, but I regretted it bitterly because it is in experiments where students can see how much the spinal cord can do by itself, without any complicated apparatus. The very fact that coordinated movement of that leg could take a bit of acid paper away… but there you are; times had changed. We are largely losing the battle against the 'Animal Rights' lobby.

MR: You were very concerned about the equipment of the laboratories at Glasgow, weren't you? You were very proud of your Polish microscope.

OH: Oh, yes, the [Polski] dissecting microscope. But they were very cheap and good value at the time.

TT: These were for the teaching labs?

OH: Yes. By the middle '70s, and the end of the '70s, Ian and I ran very good courses in practical physiology. The BSc class had come up to the best of standards and we produced quite a number of physiologists who are now stalwarts in The Physiological Society – a dozen or so of our students of those years are now doing good work. Sadly, I could not dissuade Ian from taking early retirement in the early '80s – I tried hard. And, of course, his early death was a tragedy. What has happened in the last 10 years, and especially more recent years, fills me with sadness. I think there is very little left of the work that I have done for the department. Even the department has disappeared. Even the name physiology has been taken off the door to the building! I recently read the minutes of a meeting of the Institute of Biological and Life Sciences, which has been created and in which physiology has now become a minor unidentified part. Some of my past colleagues asked to use the title of 'Laboratory of Physiology'. This was denied to them by the present head of the Institute, Fewson [Charles Arthur Fewson, 1937–2005], who is a biochemist. More sadly, my immediate successor is minuted to have said as regards the title Laboratory of Physiology, 'let us go forward with the minimum of historical baggage'. So I am afraid physiology in Glasgow now is no more than 'historical baggage'. This brings me to the end of what I want to say!

TT: Can I just ask you about when you went to Glasgow in 1971. Your own research.

OH: Well, I did continue with some work, of course. This is what Martin [MR] asked me earlier about formaldehyde and inward and delayed rectification. I told you that I have spent the weekend rolling my career through my memory. But I came to the conclusion that I'm not yet ready to pass judgement or comment on my Glasgow years. First, the outlook would need to be a bit different. My job in a university was not only working for physiology but for the university, for the students, for the staff and the development of the staff, etc. So one's work isn't just one's personal work, but also what one managed to promote, one hopes selflessly, by way of other people. And I haven't assessed this really. I have kept my papers. My activities were so multifarious there that one really needs to look through one's papers to remember much of it. I know that when I do this I find things that I cannot even remember to have done. Of course, one never remembers one's later activities as well as one's earlier ones. As regards research, I think that in the '80s, when I was for four or five years in sole charge of the department, it certainly slipped behind. It's recovered again, I hope, during the last four or five years. I became interested in new topics, mechanical properties of muscle membrane and lipid chemistry, which were new challenges and I enjoyed having to learn new things. The methods I am using nowadays are again very simple methods, and nothing is more enjoyable than good results with simple methods.

TT: It's almost as if you have come full circle then?

OH: That's right.

MR: Who are the people who you remember in the department, apart from the ones you've mentioned.

OH: I told you that this interview would concentrate on the early part. I wouldn't like to ... there were many good things too. I haven't reviewed this period yet. I am too close to it still. I am still there, I am still involved in it. I've kept my papers. I can do the job, maybe, but it would be looking at it as a Glaswegian and as a contribution to Glasgow. There would also be the personal contribution and work, but one would not expect that to be as large a part of the total. So this is something I haven't yet thought through.

At Otto Hutter's 90th birthday party, Denis Noble and Otto Hutter, photographed by David Miller.

The Physiological Society
Hodgkin Huxley House
30 Farringdon Lane
London EC1R 3AW
United Kingdom

Registered Charity No. 211585.
Registered company in England
and Wales No. 323575
020 7269 5710
www.physoc.org

The Physiological Society