

# An interview with Sally Page

Conducted by Tony Gardner-Medwin,  
Roger Woledge and Martin Rosenberg  
12 June 2007

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 The Society's archive at The Wellcome Library.



Sally Page photographed by Martin Rosenberg on 12 June 2007.

This interview with Sally Page took place on 12 June 2007 in room 122 in the Physiology Department at University College London. Those present were Professor Tony Gardner-Medwin (TG), Professor Roger Woledge (RW), Martin Rosenberg (MR) and Sally Page (SP).

TG: Ok, so Sally we all know that you are from New Zealand; tell us when you came to this country and what you did in terms of just the development of your career and how long you stayed here and all that sort of thing.

SP: Well basically I came as a PhD student at the end of 1959 and I've stayed here. I came to the Biophysics Department and stayed on until the department was dissolved. I became a member of the Physiology Department therefore in 1984, I think it was, stayed here until I reached 65 in 2001 and have continued in a part time category doing the odd bit of teaching and chatting to colleagues and expressing opinions on changes in the department occasionally.

TG: So does that mean you have been here as long as or longer than Roger Woledge?

RW: About the same isn't it?

SP: It is yes.

RW: I came here in 1956 as an undergraduate, but I don't hang around quite so much as you do.

MR: So can you tell us about New Zealand; where were you born?

SP: So I was born in Christchurch in the south island in New Zealand. It was a wonderful place to grow up and also a very good time particularly the late '40s and '50s when New Zealand was a very prosperous country. Christchurch was

on the sea with miles of beaches but the mountains were only two hours away, where we went for frequent family holidays.

MR: Were your family in science?

SP: Yes my father was an industrial chemist worked in a tannery. His father before him was a Demonstrator in the Chemistry Department at the university, taught and was presumably one of the first calibrators of Ernest Rutherford in fact. My father might have been an academic scientist himself I suspect, but he was, in the family tradition, a conscientious objector in the First World War. I suspect that was when he was a student that affected his future career and instead he went into industrial science.

MR: So your family goes back a long way in New Zealand then?

SP: Yes 1844 might be the first.

MR: And they came from?

SP: Scotland, Ireland and England.

RW: Was chemistry a tradition? Did your father have a chemistry degree?

SP: My father had a chemistry degree yes like his father before him.

RW: And the same that you have, yes?

SP: And the same that I have, yes.

TG: Where did you do your chemistry degree?

SP: In Christchurch. It was then part of the University of New Zealand, an umbrella organisation in the same way as the University of London is. I left school thinking I would do a maths degree at university but my brother followed a year behind me and showed me very quickly that he was the mathematician in the family and I was not. So as much, I'm afraid, like many things by default I instead did chemistry. I wasn't sure between physics and chemistry at one stage but then decided on chemistry. I was almost the last generation for whom the MSc was the honours degree, so we did a three year BSc and then a two year MSc in laboratory subject. In maths my brother did only one year for an MSc.

TG: Were you interested in biology at all at that time?

SP: No not at all! I did very little at school. Most of my biology was because of my parents' interest in botany and the native flora of New Zealand. I used to get rather irritated when we were walking in the mountains at the time my parents would spend identifying or looking at plants.

MR: I've heard that before!

RW: Yes my parents were very similar to that.

SP: Right yes.

- TG: So did you think at the time when you chose to do your degree, did you think much about what kind of career you were looking forward to or did you just do things because you enjoyed them?
- SP: No I just did things because I enjoyed them, yes.
- RW: Well, you were in a sense influenced by the family tradition a bit because you were sticking with chemistry, which was the family default.
- SP: Yes, well maybe, but my father was very interested in education. He was actually very active in many scientific circles and such like in New Zealand. He was very careful not to take over or interfere with the teaching we got from our schools and I think it was partly by example but partly in a very indirect and peripheral way he stimulated our interest in things.
- TG: So was New Zealand at that time quite an emancipated environment in the sense that there were no special pressures on you and your brother that were different?
- SP: I would say so yes.
- TG: That's good, yeah.
- SP: I was at... my high school was a purely girls school and my brother was at a boys school but those were chosen partly for family history, our parents were at those schools, and partly because they were the strongest academically at the time.
- MR: Was religion important?
- SP: Yes in that my father's family had strong Quaker influences and they were strongly active in peace movements, but by the time he married there was no affiliation with any church or with the Quakers. My mother came from a strongly Anglican family, in fact her half-brother was a missionary, younger half-brother, but neither of my parents had a religious affiliation. We were not baptised, my brother or myself. We did in fact later attend Sunday school and so on, I suspect because that's what all our peers were doing and so we did.
- MR: The war was not long over; was it very much felt in New Zealand? I mean the Far East?
- SP: For me the war was the blackout curtains and blinds, the quite considerable rationing, my parents listening anxiously to the six o'clock news, and of course it restricted use of the car because petrol was very heavily rationed so we travelled by train up to the mountains rather than by car in those early days.
- TG: Did you have a place in the mountains?
- SP: Yes my grandparents first bought a sort of shack that had been lived in by the men who were drilling the eight mile long railway tunnel through the backbone of the South Island. When that was finished these buildings were made available, in the late 1920s I think, and that was the core of our fairly primitive holiday place up in the mountains.

TG: Maybe this was at Arthur's Pass?

SP: It was indeed, yes.

TG: Had you ever been out of New Zealand before you came to England?

SP: No.

TG: Had your parents?

SP: My father had once, about 1950/51, on a professional visit, to visit leather chemists in this country, Sweden and the States. But even Australia was two or three days by boat in those days to get away. My father did fly on that trip but when I came to England in '59 we came by boat, all four weeks of it.

TG: So when you started out as a child was New Zealand the centre of the universe?

SP: I suppose so but we always were aware of the link with Britain and I certainly still spoke of England as home.

TG: Really?

SP: In a different sense from New Zealand which I still regard as my home but Britain was the home country.

MR: Was there more of a focus on Scotland than England?

SP: No because if I lived in Dunedin, the Scottish settlement, yes there would have been.

MR: Of course yes.

RW: I thought it would be nice to hear something about your undergraduate education because chemistry can mean lots of different things so...

SP: So it was very broad for the first three years. I took a BSc and still then I did some physics. I remember I worked some of the university vacations for people who were researching in radio astronomy I think it was in the Physics Department and I worked on calculations for them... very routine stuff.

The first year of the MSc was still equal emphasis on physical chemistry, organic and inorganic. I am being a little vague because my memory of it is very slight. Then the second year was a thesis project. I was most interested in physical chemistry and had a project which was measuring the lifetime of fluorescent molecules, anthracene and so on. The reason for doing this was because my supervisor was interested in the kinetics of interaction of molecules in solution. He hoped to follow quenching of fluorescent molecules as a tool and one wanted to know the half-time of the fluorescent molecule. He'd got largely assembled before I started the equipment for measuring the half-time which was of the order I think of  $10^{-9}$  seconds. I don't remember very much of the detail or even the principle, but I got some values at any rate.

MR: Were the text books mainly UK text books or American?

- SP: Entirely, entirely, yes.
- TG: And the exams you'd done at school were they English exams?
- SP: No they were the equivalent of GCSE roughly. It was a nationwide exam. University entrance was by accreditation by schools; it was a first year of the sixth form. Then those of us who thought we might go on to university full time usually stayed for another year. In my case there were about a dozen of us in that class and some of us sat scholarships, which were national exams. The university exams had been marked out of the country for a long time but by the time I was a student they were all marked internally.
- TG: So were you narrowed down to about the same extent as students were in this country at that time sort of doing three subjects, just science subjects?
- SP: Not as much, no. At sixth form level I did English, pure maths, applied maths, physics and chemistry. My school could not offer applied maths so I was sent by the school to somebody who tutored university students who were having difficulty in passing their maths. That was, you see, part of the thing that I saw my future direction in maths.
- MR: Were there any particularly inspirational teachers or personalities in your academic year?
- SP: I had a maths teacher – that probably again was an important factor – who I was very fond of and I thought was very good.
- MR: Do you remember his name?
- SP: Irene Milne.
- I'm afraid my chemistry science teacher was not so good because she was overawed by my father. She was one of these bad teachers who holds up one pupil as an example to others in front of them, which I think is shocking. I was horrified 30 years later when one of my closest friends confessed that she was very upset that she came second to me in a particular science prize and she was sure that was because of my father.
- MR: What was your father's name?
- SP: Robert Owen Page. Robert Owen probably the link to the social... what one calls them... hardly a reformer, but a social thinker.
- MR: And your mother's name?
- SP: Nancy Grace Glen.
- MR: And you have a sister?
- SP: No I had a brother, that was all. So at school and university I was much more than just somebody who achieved fairly well academically; I played a lot of sport, our family did. We had a very good tennis four; we all skied, spent a lot of time in the country and so on, walking. I got very active in student politics;

I'd been something of an organiser at school and that seemed to carry on at university.

TG: Did a lot of your peers think about going somewhere abroad at that stage?

SP: I don't think so, no.

TG: When did you first start to think about that possibility?

SP: Again I was almost the last year or last but one of students who if they were fairly sure you were going to get a good degree, you automatically applied for overseas postgraduate scholarships. The government funding for example provided overseas postgraduate scholarships. There were the things like the Rhodes scholarships; Shell offered a very prestigious scholarship, the 1851 exhibitions which is the one I came on. I think very wisely almost the next year or two the government changed the funding to scholarships for doing PhDs in the New Zealand universities to help strengthen the research done in the universities and offered overseas funding for postdoctoral visits. So in my case there was the usual wait when decisions were made over here about postgraduate scholarships and in the meantime I took up a post in the Chemistry Department. Then when I knew I had a scholarship to come over here I'm afraid I started to think about what I might do.

RW: So you applied for a scholarship but that scholarship was not for a particular project?

SP: No.

RW: It was a scholarship that gave you funding then it was up to you to find a place?

SP: Indeed.

RW: Is that how it worked?

SP: Indeed yes.

TG: Anywhere in Britain?

SP: No, I was in New Zealand, so our exams, we knew our results in November the end of the academic year and I think it was probably, perhaps, I'm not sure, March, April that sort of time in the next year that we knew the outcome of the scholarships which would be for starting in the September, October, i.e., the English academic year.

TG: But you applied for the scholarship on the basis of your record to date as it were and there was no issue when you applied about which university you would go to or what subject you would study and who you would study with?

SP: Exactly.

TG: Oh, that's interesting yes.

SP: Yes.

- TG: So you must have had some ideas.
- MR: Well we had big delays, I mean the telephone was enormously expensive and mail took so long.
- SP: Well I'm not exactly proud of this part of the story. My father had died two years previously so I wasn't able to discuss all of this with him. I knew that I didn't want to go on with what I had done for my MSc project; it didn't seem an area that was going to go anywhere very interesting. My MSc supervisor took me in hand and he said, 'Well, I think given your background...', he mentioned two or three lines where he thought they were going to be interesting in the next period. Two of them were straight chemistry and I don't remember them. The third was the role of calcium in biological tissues and not knowing enough about it; that one interested me. Then came the question, well, where would I go for this and because they had been on research councils together and as my father had known John Eccles, and my supervisor knew of him and wrote to Eccles and said could he recommend anywhere. So he produced two names that didn't mean a thing to me and I wrote to each of them saying would they accept a postgraduate student for this sort of area? Alan Hodgkin from Cambridge wrote back and said he had no room for overseas PhD students this year. Bernard Katz wrote back and said he would accept me in the department to do a PhD under Hugh Huxley on muscle. I didn't feel I was in a position to write back and say well that isn't what I said I wanted to do! So I arrived in this country to join Hugh Huxley.
- MR: After a month at sea.
- SP: After a month at sea, yes.
- MR: And your mother was still in New Zealand?
- SP: No this is the sad part of my early life. I had a very happy supportive childhood apart from my father's early death. But my brother also got a postgraduate scholarship to do maths and he and I and my mother were all coming on the same boat to Britain. My brother and I had free passages because the shipping company offered free passages to students with scholarships. My brother had an accident on the first night out on the ship and the ship turned back but he never recovered from that. So that is why you found on the web a prize at our University of Canterbury. My mother and I gave them some money to create a prize in the Maths Department in honour of him. So it finished up that my mother and I came on together.
- TG: Was that quite a long time later? Or how long did you stay?
- SP: In fact the next sailing... it went every month so we went one month later.
- TG: So your brother died fairly soon after the accident did he?
- SP: A matter of days, yes.
- TG: You had got back to shore by then?

- SP: Oh yes we were back to shore and he was in hospital for four or five days perhaps. Heroic, inappropriately heroic efforts to try and keep him alive, but the brain damage was considerable.
- TG: What sort of accident was this?
- SP: He was a climber he had always enjoyed challenges, physical challenges and he been seen climbing somewhere, I don't know where he was. It was thought he probably fell from a beam, I don't know what, nobody saw him.
- MR: So your mother and you arrived in Southampton?
- SP: Yes, that's right, yes.
- MR: What was your impression of the UK climate? New Zealand can be a bit wet as well can't it?
- SP: Yes, but it comes not in dribbles, it comes in a real good downpour and then beautiful blue skies and warm afterwards. I experienced some of London's smogs.
- MR: Was it winter you arrived?
- SP: Yes it was October.
- TG: So this was 1959?
- SP: That's right, yes.
- TG: So was it the idea that your mother would stay for some time in Britain?
- SP: Yes she and I shared a flat in Swiss cottage. She didn't tell me at the time but I think she felt that it wasn't fair to me, that I didn't have an independent life. In very much in quotation marks 'independent' isn't the right word, but any rate so she went back to New Zealand after two and half years I think.
- TG: I see, so you lived in this flat for that time in London?
- SP: Yes.
- TG: So she must have been devastated by her husband having died a couple of years before, I think you said.
- SP: Yes, then her father and then her son.
- TG: Oh gracious, yeah.
- SP: She was 15 years younger than my father and so she was still quite a young woman, so she went back and made a life of her own.
- TG: So she was almost without family when she went back was that right?
- SP: Yes.

- TG: So after your brother had died she was making a life in this country really, a new life as it were. I suppose your brother would have lived far away even if he hadn't died?
- SP: He would have been in Cambridge, yes.
- MR: So what happened here? You were in Swiss Cottage and you had a mission to see Bernard Katz?
- SP: Yes and feeling still not aware of who Bernard Katz was really and so came in and met him. Feeling very aware that I was arriving late because of my brother's accident, term had started and I felt I really should get on with things. Recently I chanced upon a letter I had written back to family and friends in New Zealand saying what a charming man Bernard Katz was. I think also because you know Bernard's wife, Rita, was Australian there was something of a link and she and my mother liked each other, and Rita and I liked each other. We were actually invited to Sunday lunch at the Katz's family home once; the boys were very young at that stage.
- MR: In Kenton
- SP: That's right, but I didn't see that much of Bernard because Hugh's laboratory was here in the Physiology Department – what later became Andrew Huxley's electron microscope area. Some of Biophysics was still up on the double A floor here and the Darwin Building was partly complete.
- TG: It was being built then was it?
- SP: Yes, being built, yes.
- MR: You met Hugh Huxley after Bernard Katz?
- SP: Yes.
- Mr: And how did he receive you?
- SP: Very friendly I think, a little diffident; I was his first student and I think almost his only student.
- TG: Did you have any correspondence with him before you came at all?
- SP: Not that I remember very much.
- RW: How old would he be at that time? He strikes me as being not much older than yourself.
- SP: Yes I suppose early thirties; I could work it out but I don't really remember. It was known that once the MRC Laboratory of Molecular Biology in Cambridge was built, completed, that he would be moving back to Cambridge and he did before I completed my PhD, but I was well on the way before he went.
- TG: So how long had he been in UCL before you arrived?
- SP: Two or three years probably.

- TG: Working on his own or had he been working with...?
- SP: Yes, in fact when I came he was not working on muscle at all. It was the period when he was working partly with Aaron Klug who was at Birkbeck at the time and a post doc, I think with Aaron, and they were looking to see whether they could develop methods to specifically stain nucleic acids in tissues to detect in the electron microscope. But also it was when he was beginning to develop the technique of negative staining which is useful for, at that time, for something like a virus particle. You surround the virus particle in a background of a heavy metal salt that looks dark in the electron microscope and the three dimensional structure of the surface structure of the virus can be picked up that way. A method that of course Hugh used enormously powerfully to look at muscle filaments and proteins later but that was after he had gone back to Cambridge.
- MR: So this is your introduction to the electron microscope or any microscope?
- SP: Indeed.
- TG: Had Hugh Huxley's earlier work with Jean Hanson, was he in Cambridge at that time? Or was he in King's?
- SP: No, he was in Cambridge Massachusetts.
- TG: Oh they worked in America on that did they? So they were both there together, I see.
- SP: Yes that's right and they continued a close contact when they came back to England but not the actual collaboration that they had in Boston.
- TG: So he had done that work on muscle. Had he done other work on muscle since then? That was 1954 was it?
- SP: And '55, yes, and then he did more electron microscopy on muscle and the classic paper that shows the two sets of filaments are separate filaments overlapping.
- TG: And that was shortly before you came wasn't it?
- SP: I think it was '57 published if I remember rightly.
- TG: So what sort of discussions did you have with him when you were starting out? You felt you were committed to working with him at that stage?
- SP: Yes
- TG: There wasn't a discussion to be made basically?
- SP: No, there wasn't.
- MR: And you were happy with the project – you had no misgivings?
- SP: Yes, he sent me initially to do a comparative study of slow muscles, so I looked at the tortoise muscle; I looked at toad for example, initially. I think I realised only afterwards that he treated me almost like a colleague rather than a

student so he opened lots of doors for me, taking me with him to talk with people at King's, to be involved in as we used to call it 'the Muscle Club', a group which started with about 20 people perhaps who had a dinner every so often and talked on muscle. The King's Biophysics people and Hugh and Doug Wilkie I think were the core of that.

TG: This was in London presumably?

SP: This was in London but also then Cambridge as well.

MR: Was Andrew Huxley in your life much then?

SP: No Andrew came to London very shortly after Hugh went back to Cambridge so they didn't overlap here.

TG: So that was in about '62 or something like that?

SP: Yes and so I became close friends with people like Jean Hanson and others at King's because they too were very welcoming and easy; made one feel part of that group. The muscle world felt a very small world at that time and it was enormously exciting period wasn't it? Because the sliding filament was really still being challenged.

MR: Were you convinced?

SP: Oh of course, but I remember there was a paper published by a Scandinavian group that was measuring filament lengths or band patterns really in the electron microscope and they were saying that filament lengths did not stay constant, that when the sarcomere shortened the muscle filaments shortened. Hugh felt that this needed to be looked at and challenged and so he said that this was more important than your tortoise work and so on. I was a bit peeved, I didn't want to just check on other people's work, I thought I had my own project, but still I took it up. I think Hugh very much let me make that my own project and it turned into looking at all these sources of error that there can be in making measurements of dimensions in the electron microscope. The direction in which you cut sections, the shrinkage and how you can possibly calibrate both the microscope and your tissue. That was my first published work and I was very pleased with that. I remember when I came finally to write it and Hugh said well that's your paper he wouldn't have his name on it and I thought well it would be to my benefit to have Hugh's name on it as well. He had suggested it; he had supported it along the way.

MR: Was that this one? [Showing a copy.]

SP: That's right yes.

RW: It was rather the tradition in those days...

SP: It was.

RW: To allow people to publish papers on their own, it was not so unusual.

,

- MR: Who taught you microscopy? Did he teach you?
- SP: Indeed yes.
- TG: So your interest in quantitative methods, does that come from undergraduate education or was it a spontaneous interest you had?
- SP: I suppose a bit of both... I think, going ahead a long way to working with Rolf, I think it's always been one of his strengths and he has reinforced that, always looking very carefully at things, are they real? Is that an artefact there? How can one quantitate this and, yes, it's been a thread there.
- TG: You said that of course you believed in sliding filament theory from the beginning, but tell us a little bit about what the alternative was. What people were claiming was a plausible mechanism and how that disappeared.
- SP: I think probably still, that filaments were elastic.
- TG: That they folded and unfolded in a calcium dependent way perhaps...
- SP: No calcium wasn't yet really very much...
- TG: But it could change whatever controls them, of the folding of the proteins in some way. OK.
- SP: Yes.
- TG: And what made that go away, as it were, that idea? Given that it was still alive after the work of the two Huxley's.
- SP: I think Hugh's paper showing that there were the two sets of filaments, the x-ray work which perhaps was not appreciated by people who were not working with x-ray that showed that there were repeats within the filaments that were not changing when the length of muscle was changing. I suppose showing that with the level of detail that you could get in the electron microscope, that the filaments were not changing length.
- TG: So that was your own work basically?
- SP: Yes.
- TG: Did you go to meetings where this was actively challenged?
- SP: Not so much at my first meeting and again I was naïve. I didn't realise, here still as a PhD student, and I was asked to present this at the Royal Society discussion meeting in '63. I think the challenge really came a little bit later with people who if you like were a little bit iconoclastic – the Gerald Pollacks of this world. People, who felt too much that this has been accepted as mainstream and there are all these queries and so on. I remember reading in one of, I think, one of the books of one of the Sugi/Pollack conferences. Pollack had collected a lot of papers which claimed to show that filaments changed lengths under different conditions and he said, 'Look, here are all these and there is only one paper that shows they don't change lengths.' The discussion was published in the book in those days and I can't remember ... may have been

Anne Marie Weber or someone who said, 'But look, once a piece of work has been thoroughly done and all the problems involved have been made clear and sorted out people are not going to need to repeat that work.'

MR: In those days there was no pressure to produce multiple papers was there?

SP: No.

RW: There is an issue about comparative physiology here because part of the resistance might have been because people thought that's all very well for frogs but it might not apply for other animals.

SP: I think you are right and particularly with some invertebrate muscles, yes.

TG: Are there in fact, and this is a rather naive question, but are there any muscles in any kind of situations which do work by protein folding and unfolding changing the length, they are expressed in the outer hair cells of the cochlea maybe.

MR: You call them muscles?

TG: Well their muscular cells

RW: What's your answer to that Sally?

SP: There is some uncertainty over the horseshoe crab muscle, wasn't there for a while, *Limulus* isn't it?

RW: Yes.

SP: And that's a catch muscle is it? I'm not sure. But I have been out of the field for too long.

RW: I think there are still unresolved things in invertebrates where no one has clearly shown another mechanism but there is evidence from which it is difficult to exclude other possibilities so I think it is still a fairly open question, but nothing...

TG: Maybe or may not work by cross-bridges basically.

RW: Yes, may or may not work by cross-bridges certainly but cross-bridges and sliding filaments are often confused. So what we are really discussing from that period is not cross-bridges but sliding filaments. The issue then is whether the filaments change length or do not change length as part of the contraction mechanism. It certainly changed length in response to this and that: in smooth muscle there is no doubt about that, but with a contraction mechanism that's the issue. Was there any smooth muscle that you were involved in?

SP: I wasn't involved in any, no. That was a tissue to keep away from. Now we touch on Andrew Huxley and contacts there shortly after he came. Clara Franzini Armstrong and Clay came to spend their required 24 months outside the States for Clara before she could go back in on an immigrant visa. The thing that she did whilst she was with Andrew here was to look at filament lengths in

various crustacean muscles and show that they have different lengths even in different sarcomeres within the same muscle, so it's a complicated story.

MR: Do we want to talk about the Biophysics Department in general?

All: Do we!

MR: I mean we have spoken about the people who you were associated with in your research but how about the other people around in general the atmosphere and so on and so forth?

SP: In my first year or two while Hugh was still here I didn't have much interaction with the rest of Biophysics partly because I was in a different building and of course doing very different things from there. It was a very interesting environment to be in and you know what a Mecca it was for people to come and work. I missed the first great wave of people like Furchspan and Potter, John Nicholls, Bernard Ginsborg and so on. Then there was a period, the early '60s when there were a lot of Americans, a lot of visitors in the department and they made it a very friendly place. There was a group of us who used to eat together frequently, do things together.

MR: Who did you associate with mostly?

SP: There was Dick Orkand and then Clarke Slater, Bob Eisenberg came at the same time, Cuy Hunt I think was that period, René Couteaux [1909–99] from Paris had a sabbatical here and I think thoroughly enjoyed... instead of being the grand old man as he was in Parisian science, instead being one of the group of youngsters. Oh Alan Grinnell, Radan Beránek from Czechoslovakia. Fred Dodge from the States, many names from all round the world.

MR: Paul Fatt?

SP: Paul yes! He had been there from, of course, very early days from the beginning of the department in '52. Gertrude [Falk, 1929–2008] came 1969.

MR: Were they welcoming, did you see much of them or did everybody stay in their rooms?

Sp: Paul and Gertrude tended to be their own nucleus in their group as small as it was. E. J. Harris was very much a man on his own. He simply stuck out like a sore thumb I think in the department. After a while particularly in the later '60s it was rather the sixth floor and the fifth floor of the Darwin building. The fifth floor was those who came very much to Katz and [Ricardo] Miledi, increasingly Miledi taking over the running of the research in that part of the department. Then the sixth floor E.J., Paul and Gertrude, Rolf [Niedergerke, 1921–2011] and myself because we were not working on neuromuscular junction so our interests didn't overlap. But this period in the '60s particularly when there were a lot of these visitors, particularly with a good core of Americans, there was very much common friendliness within the department.

MR: Did you know about the Physiology Department?

- SP: I did and my first year here having had no biology background and doing a degree in biophysics I was required by the University of London to do a course in physiology which Jack Diamond ran superbly. I am afraid though I didn't give that the care and attention that I should have done. I think particularly coloured by arriving late and feeling I had to get on with research and so on. But Jack had not only run a very good course and amazing amount of practical work in it but he also gathered me up in a group who played tennis at Shenley and so on, so Don Grieve and Alan Ness and Jack. But then that contact went and then there was a period when... I think there was a feeling wasn't there, amongst physiologists that Biophysics had a wonderful life? We were very lucky, we didn't have any teaching and so on. We were not invited to the Starling room; we were not allowed to go to the Starling room Friday seminars.
- TG: Surely that cannot be true?
- RW: It's true.
- SP: I remember actually being asked, and I don't remember by whom now, but to give a seminar on Friday lunchtime series and feeling how this must be quite something that someone from outside the department had been asked to go and give a seminar there.
- MR: But who agreed with this tradition?
- RW: Well, there was a very strong group identity in the Physiology Department in G. L. Brown's days and I can quite understand and sort of remember what Sally is saying really.
- TG: Well there was a strong tradition when I came, which I suppose was the late '60s or '70s, something that people in the Physiology Department would give those Friday seminars or at least most of them. Indeed that's rather sadly disappeared I think in the Physiology Department these days that very few people from the staff do give seminars in the department but what you are saying is that people were not welcome to come in the audience. That's what astonishes me.
- SP: Yes.
- RW: The original idea of these things was that they were strictly internal, the idea of this was to make it unthreatening.
- TG: So work in progress workshop sort of atmosphere?
- RW: Yes, it was meant to be an informal discussion and in order to make it less threatening it was external people that weren't specifically invited. I don't know the thing how they guarded the door exactly.
- MR: But BSc students were there weren't they? Which years are we talking about?
- RW: Well we are talking about the 1959, '61, '62 those kind of years, early sixties.
- TG: So part of '62 was when Andrew Huxley became head of department?

- RW: Yes I think he would be somebody who would have probably changed that.
- TG: Did he do anything about it?
- RW: Well I'm sure he would have done, I think he would have been somebody who would have opened that up quite a bit.
- MR: But Bernard and Rolf used to teach on the BSc course.
- SP: Indeed yes, and Ricardo.
- TG: As did a whole lot of people.
- RW: Yes, but it was a 'biophysics component'; it was so called because it was largely taught by the Biophysics Department, so no doubt the content was up to them really.
- SP: Yes.
- RW: You were talking just then about the people you associated within the Biophysics Department and I wasn't quite sure which were scientific influences and which was the general social milieu that you moved in. I wanted to say, well, if you were discussing science and your results and you don't know what to make of them who are the people you were discussing that with in those days? Perhaps even going back to when you were a PhD student. Who beyond Hugh would be the people you would talk to about your own science?
- SP: Probably no one other than Rolf and people in Andrew Huxley's group and yourself probably.
- RW: Jean [Hanson]?
- SP: Oh yes, I was thinking within the college. Jean, very much, yes.
- RW: Because she would have the expertise which would be very relevant to what you were doing I suppose.
- SP: Yes indeed.
- TG: So was Rolf here when you were doing your PhD?
- SP: Yes.
- TG: But you weren't working with him at all then?
- SP: No, not at all.
- TG: But he was interested in what you were doing?
- SP: Yes.
- TG: So it was at that time you were saying he was one of the people who you would discuss your science with?
- SP: Yes. I think the first thing I remember and I don't know how it came about but preparing the paper for the Royal Society I am afraid I treated it in too cavalier

a fashion. I must have given a rehearsal and Rolf was horrified and he took me to task and worked with me as a supervisor would have, to, instead of just using the material as I had for the other publication, to present it with the figures, redraw them, present the material differently and got me to present that in a much more appropriate way. Then the work I was doing with muscle particularly, as it was comparative studies, the differences were not there on a contractile protein filament side but in the SR and so my interest on the striated muscle structure became very much on the sarcoplasmic reticulum.

- RW: So that was after you finished your PhD was it?
- SP: That was, but some of it was in my PhD.
- RW: So let's just say, so what was in your PhD?
- SP: The filament lengths, tortoise muscle structure and frog slow fibres and twitch fibre structure I think.
- RW: Right so not SR?
- SP: No, it was there in the frog SR and the tortoise.
- RW: Oh right, and did you have a ferritin experiment?
- Sp: The ferritin was the first thing I did that wasn't in my PhD. I had my blocks there in the oven when I got a letter from Hugh telling me the exciting results he had just got with ferritin. This is showing that the T-tubules, the transverse tubules of muscle, must have their lumen open to the outside because a big molecule like ferritin will go in and because we haven't been able to see in the electron microscope the openings of the T-tubules.
- TG: That was very important because people were starting to realise calcium was critical and they couldn't understand how calcium influenced the middle of the muscle so quickly as it did.
- SP: Yes.
- RW: It was also known that from local stimulation experiments I think that muscles could be stimulated at very specific points which seemed to be where the T-tubules were. Is that right?
- SP: That's right indeed.
- RW: And then what was the mechanism of that? That was the question and current in the teaching...
- TG: There [was] also the business of the large capacitance of the muscle membrane which was due to the connection, but that came later, that people showed that disappeared...
- RW: That was Paul and...
- SP: Paul and Gertrude that did that.
- TG: Oh was it?

- SP: Yes.
- RW: So there is a point of interest that you might have shared with them.
- RW: So when you finished your PhD how come you stayed here?
- SP: Well in fact before I finished B.K. [Bernard Katz] offered me a post of junior lectureship in the department. I think he wanted somebody to be doing structural work to run the electron microscope. A little bit of a service facility probably I think. Well, I know he hoped that I might collaborate, well a bit later with somebody like Lincoln Potter and looking at receptors or looking for receptors in muscle, but that didn't work out. At that stage life was interesting here, science was interesting and I thought, yes, I will stay a few more years, but I took out the sort of pension scheme that when I went back to New Zealand would transfer because the insurance company I took out my scheme with also worked in New Zealand. I started getting offers of jobs in New Zealand, but it never seemed quite the right moment to go. Then, well, offers stopped coming and I stayed here happily.
- TG: So the idea of possibly leaving UCL was one and the same as going back to New Zealand?
- SP: Yes and I had a couple of offers, one in Oxford and one in Cambridge; serious considerations, but they weren't sufficient to tempt me away from London.
- MR: Have you paid many return visits?
- SP: Whilst my mother was alive I went home every about three or four years but when she died in the early eighties I didn't have the excuse, as it were, to go all that way. It was only when she became terminally ill that after... no I'm sorry... after she died, I didn't go for 19 years. It was only after I retired that I had time to go back.
- MR: But you have other family there?
- SP: I have a half-uncle, but he is more like a cousin than an uncle, but otherwise rather distant connections.
- TG: So by the time you took this offer of a job, a lectureship in the department, in about 1962, you actually hadn't been back to New Zealand since you left?
- SP: No.
- TG: Did you like London as a place to live?
- SP: Yes. I wouldn't have accepted that job within my first 18 months here; not having the mountains on the horizons and the grey skies. I hadn't yet sufficiently adjusted but otherwise yes. I always said that if I had children I wouldn't want to bring them up in London, or England for that matter. I think New Zealand was much better place for family growing up, but that issue didn't arise.
- MR: Did you have many contacts outside of the college?

- SP: Very few. Largely New Zealanders living over here, either my contemporaries or family connections, and that was in a way a disadvantage of the Biophysics Department because it was a changing moving population. People were here for a year or two and then went back to their home countries. So my close friends are those I made at school and through my undergraduate years and not so many from my late 20 and 30s. Two of the closest friends I've made here were both in Physiology here, i.e. Clara Franzini-Armstrong and then later Jean-Marie Gillis. Later Jean-Marie and I collaborated, I think because what he [had] come to do with Doug [Wilkie, 1922–1998] was not working out so he came to me with a suggestion about possibly doing the histochemistry at the EM level...
- RW: Can you tell us about that; it would be interesting to know about that work. When was this?
- SP: I think '67 we published, so '66-ish I suppose. Jean-Marie had the idea that we might localise the ATPase site by using lead to precipitate a lead phosphate as a result of hydrolysis of ATP. The lead deposit one could see in the electron microscope and certainly we got lead deposits; we could see them and they were where you might expect against the cross-bridges, but we did some more controls than people very often did in those days. One of the things we did was just to precipitate a lead phosphate within muscle not from ATPase at all and take it through the preparative stages and just see where the lead phosphate finished up. It was exactly ... it washed, presumably washed up, against the cross-bridges like marbles in a box. We tried various ways of getting around this and concluded that there may have been some preferential binding I don't know but there was just not a feasible method so there was a paper with a negative story.
- MR: How did your work with Rolf develop?
- SP: That was because of my interest in the role of calcium with the structure of the sarcoplasmic reticulum. He had done such fundamental things in showing the role of calcium in activation, the calcium injection and then calcium movements. Then at that stage I felt that the techniques that were available with the electron microscope were not ones that would give us any more information on muscle; I was not one for developing techniques. I had the choice of either following calcium interests, moving away from the EM, or moving away from muscle and looking at another tissue. Not being a biologist the latter, moving into a different tissue, didn't interest me at all and so following the calcium was something that was attractive. Rolf was doing it, he had already helped me and so when he suggested that I might join him with some of his calcium measurements that was attractive. So I continued...
- RW: ... what you come to London to do? ... I don't know if that was still in your mind at that time?
- SP: No, it was pure coincidence like so much.
- TG: When it wasn't coincidence, you picked up on the right idea at the beginning.

- SP: My supervisor had yes, yes.
- RW: It's quite far sighted!
- SP: Indeed yes.
- TG: I was actually interested when you mentioned that about how much of the biology he actually knew and whether he was interested in that sort of thing. Do you remember that at all?
- Sp: He wasn't interested in muscle at all and I think he later went on himself to look at the way calcium is handled in the body, its absorption and general calcium balance.
- RW: So what was the role of yourself and Rolf in working together on calcium questions, how did you collaborate?
- SP: Well the first project was using calcium-47. He had already done measurements with calcium isotopes looking for movements of calcium between the cell and the environment. He worked... one of the attractions of working with frog heart was that it was thought that it had a negligible saroplasmic reticulum so that the bulk of the calcium probably came from the extracellular fluid and therefore if you could measure that you could be measuring the movement of the calcium involved in activation. We actually did look at the SR and found that there was a bit more than we thought there had been and it was a little bit more organised.
- RW: So that was you doing EM on the tissues which...
- SP: We were then using the techniques together.
- RW: Which is the particular aspect of your collaboration you were bringing, your EM skills, which Rolf wouldn't have done that if it hadn't been for the collaboration with you.
- SP: That's right yes. Of course frog heart's cells are very small so you can't do measurements of calcium fluxes on single cells and so the problem is how to cope with movements in the extracellular space, and Rolf's idea had been to use calcium-47, a gamma emitter, and sulphate. The sulphate as the label of the extracellular space because its diffusion coefficient was similar to calcium and it was a beta emitter so you can measure the two simultaneously and pick up a difference in the calcium movement. Those were heroic experiments because calcium-47 had a half-life of four or five days; it was available every two or three weeks on a Thursday or Friday. So we would work over the weekend doing two experiments a day while the calcium was still sufficiently active. Again we found sources of artefacts because its daughter isotope scandium clung to everything in sight and in the end we didn't get anything out of that. But it did lead us on to a way of going back to use calcium-45 and measuring net movements, not over the time scale of a single beat, which is what we hoped, but over a longer time scale in which we were able to show there were net movements over 30 seconds or a minute or so.

- RW: So what sort of years are we talking about now?
- SP: So that was I think the late '60s; I joined Rolf early '70s.
- TG: So what was Rolf's history before? He had been working with Andrew Huxley in Cambridge was that right?
- SP: Yes he'd worked in Bern in Switzerland dissecting single nerve fibres, working with them then when he came from Göttingen. It was thought, therefore, that he should be able to dissect single muscle fibres and so he was recruited to join Andrew in Cambridge to dissect the single muscle fibres for the work with the interference microscope.
- MR: He has a biological background or a...?
- SP: Medical.
- TG: And did he do a PhD on that?
- SP: No.
- TG: But he was at that sort of stage, it was one of his first sort...
- SP: Well his career is really affected because of the war and I doubt he'd have been a scientist if it hadn't been for the war. He did medicine which was a family tradition and which was something that meant he didn't have to be holding a gun. So he went through the war doing his medical degree in spells of return to university, finally qualified MD and then sort of came into science, decided that physiology would be what he was interested in.
- TG: Did he spend several years with Andrew in Cambridge?
- SP: Two years.
- TG: Two years, I see and how did he come to UCL?
- SP: It was clear there wasn't something for him to do in Cambridge I think. Alan Hodgkin was at that stage doing the work on single muscle fibres with potassium contractures and various things and they were active and busy there. Andrew was busy working on his model wasn't he? So that the full paper with Rolf didn't get published for quite a long time.
- RW: I didn't know that.
- SP: I think Rolf met Bernard at a physiological congress and Bernard offered him a position here and apparently it was Bernard who suggested that he might find the frog heart interesting to look at. Rolf had done quite a lot of reading around muscle before he went to Cambridge even and so followed that up.
- TG: So that was a couple of years before you arrived? Something like that, 1955 or something around about then.
- SP: Indeed yes.
- TG: And so he worked on frog heart from the beginning at UCL?

- SP: That's right yes.
- TG: And indeed specifically on calcium in relation to that.
- SP: Yes.
- TG: So you got more and more into a series of ideas that he had already been developing when you joined him basically?
- SP: Indeed yes.
- RW: How hands-on was it and how you did calculations which you sharpened with a pen knife in order to plot some points on the graphs or whatever it is?
- MR: And your association with computers or computing?
- RW: Yes that kind of thing... I mean the sort of a picture of the laboratory. I don't know how possible that is but a picture of being a scientist in the laboratory and doing some experiments might be interesting.
- TG: You and your Rotring pen sort of thing.
- RW: That's right, yes, I used to have Rotring pens and what is that stuff that you kind of make a graph by rubbing the stuff?
- SP: Letraset.
- RW: Letraset was very important.
- MR: They are still in business but I don't think they sell much of that now.
- TG: I used to love that because the amount of thinking I did while plotting graphs onto graph paper, it's something you never do anymore.
- RW: Yes well, I still plot graphs on graph paper. I mean it is a bit of treat but I do it when I'm doing experiments.
- MR: That because you feel like you have achieved something.
- SP: Indeed, so that you know how you are going.
- RW: Yes I plot curves as I go and of course I can easily program a computer to do it but I don't because I like doing that.
- RW: Give us a picture of the time you and Rolf were doing these experiments.
- MR: Did you build any equipment?
- SP: Rolf was the person who built the equipment.
- MR: What he was good with his...
- SP: He was good with his hands, he had spent... coming back... starting in physiology after the war he was at Göttingen and they had a superb workshop there with staff who were very prepared to train people who were interested. It was actually something about Biophysics which wasn't terribly good. We did not have very good support staff. I think Katz was not very good at managing

people and Ricardo wasn't either. So Rolf did quite a lot of building of chambers and also constructing his own electronic equipment, the differentiator that we used for quite a lot of work for example, filters for recording and so on.

MR: So he studied that afterwards, of course, that one.

SP: He taught himself yes.

TG: So you did have an electronic workshop in Biophysics which a staff member who in principle would build things for you?

SP: Yes.

MR: Who was that?

SP: There was Keith Copeland and then later Peter deVilliers who had been a technician with Paul for a period. Then I think B.K. was looking for someone to take over from Keith, and Paul recommended Peter. Peter was a nice guy but if he came into your lab, you didn't see the back of him for half an hour or more; he talked with everyone. So experiments by Rolf and myself, our collaboration, Rolf did those sorts of things. I did the dissection and preparation of solutions and so on.

MR: But you had to learn that because that wasn't part of your training?

SP: I had to learn that yes.

RW: But you learnt how to dissect when you were doing your PhD surely?

SP: I had yes.

TG: As a chemist you knew how to mix solutions.

SP: Indeed! Yes!

TG: So just going back to the workshops there wasn't a mechanical workshop?

SP: There was.

TG: Oh there was?

RW: A manned one? Or unmanned one?

SP: A manned one and then we did get an unmanned one which Rolf was unhappy about because of the way it wasn't properly overseen. There was always trouble A, with equipment from the drawers disappearing and so on but also the lathe and the drill which we had there not being properly used, people being irresponsible, and so that was a source of a lot of friction. It was useful to have it for people who knew how to use it but when your colleague had used it for building kites or whatever it wasn't so good.

MR: So a lot of your time was spent actually making equipment?

SP: Indeed yes.

- RW: How did you record data?
- SP: We didn't ever get on to recording onto computers. Frog twitch is slow enough that we could record it with a chart recorder, I think advised by you. The type of experiment that we were doing for the last several years was when we'd follow the time course of changes in the twitch after we applied say adrenalin or caffeine or something like this. We needed to be able to follow many hours of an event which say happened every three seconds and with the chart recorder we could mechanically make the paper speed fast for the twitch and then slow for the interval. We looked into possibly doing that with a computer but it didn't look as though it was going to be straightforward.
- RW: When did chart recorders start? I mean when you started with Rolf did you use an oscilloscope and a camera?
- SP: He was using a chart recorder when he was [collaborating] with [Reg] Chapman [1937–95], which was in '64, I think.
- RW: Right so when you started with him this was the technique that was...
- SP: Yes the little bit of work that we did that was recording action potentials with intracellular electrodes, we did that with oscilloscopes.
- RW: You mentioned differentiation just now, what were you differentiating?
- SP: We were differentiating the twitch because that was a simple way to give us immediately what the rate of rise of tension was and that as well as the peak tension was valuable because some of the changes in peak tension were related to the changes in duration of the twitch and by following the rate of rise as well as the peak tension simultaneously and doing just as you say with plotting a few things on graph paper as the experiment goes on just to have an idea of the general properties of the particular preparation.
- RW: So you had loads of charts?
- SP: Indeed.
- SP: So we started I suppose in the '70s perhaps using a single trabeculum from the frog atria. This was something that was perhaps 50–100 microns in diameter and if we were lucky a millimetre in length. We didn't get a good preparation every time I dissected it and it would take me two or three hours to do the dissection actually. That was a disadvantage when I started to have heavy tutorial issues because I realised if I was not doing the dissection day in and day out I became much less efficient. When just doing the experiments during the three or four months of the summer, our proportion of failed experiments was quite a lot higher than if we could do them through the year. It wasn't the sort of experiment that we could pick up on the days in the week when I wasn't teaching or doing something similar, and just one day the next week and so on. So we'd prepare solutions the day before, I'd start off with the dissection in the morning and if we were lucky we got a preparation mounted before we went to lunch. It would be equilibrating and we would start recording seriously, testing out the properties by getting what its maximal

tension would be, what its response to adrenaline would be, and then start whatever it was we were particularly interested in, following it through to about – we were probably exhausted before the preparation was – nine o'clock at night. Then the next day making with a ruler the measurements on the twitches and indeed plotting them on graph paper. Then we used computers really only to fit those data or look at those data with the model that we had for the quantities of calcium that might be in the SR, that were released from SR, that were moving across the surface membrane.

TG: So were these the sort of experiments where you really knew pretty well how the experiments had gone by the time you finished and obviously you need to measure things up and plot the graphs accurately but...

SP: Yes.

MR: Who were your competitors in this field?

SP: Very few other people worked with the frog ventricle or frog cardiac muscle. The atrial muscle has more SR and also, as it turns out, has the type of ryanodine receptors that are probably involved in the release which the frog ventricular muscle doesn't have...

MR: Were there any difficulties with publications?

SP: The difficulties were now being slow in writing it up. Difficulties in grants because grant giving bodies like the British Heart Foundation only wanted work with mammalian cardiac muscle but we built up such a body of understanding of the frog heart that we didn't want to move from that.

TG: You had people working with you from time to time didn't you? Were they part of the experimental group or were they doing their own projects or how did that work out?

SP: We had a post doc for a short period, Marianne Talbot, who came from Peter Caldwell, and she was involved with us in the calcium movements using calcium-45. There was a German chap, Ernst Lammel; he'd worked with smooth muscle and he wanted to learn the techniques that we developed for following calcium movement, but he wasn't really very interested in those and he got more involved, partly under Rolf's guidance, with modelling; he had a maths background.

TG: They were those people that came for just a few months, that sort of scale?

SP: Lammel came for a year, Marianne had come on a post-doc but she became ill and so her period finished. The other two were on research assistanceships, David Gadsby first then David Ogden, and they did a little bit with us on the heart but mainly they were working on the frog skeletal muscle and the sodium pump.

TG: But were they funded by the grants that you got?

SP: They were funded by the grants Rolf had yes from the MRC and the Wellcome if I remember rightly, yes.

- TG: So they didn't interrupt too much the experiments you and Rolf did, the two of you, they didn't interact?
- SP: Yes. We had enough space up there that we worked in one room and they worked in another and we had separate equipment. No, they both got a very thorough, even if not always a very comfortable, training from Rolf and both flourished wonderfully.
- TG: You say from Rolf what about from you?
- SP: I didn't get involved very much in their science.
- MR: Was he a hard task master?
- SP: I think so yes.
- MR He wanted things to be exact, precise?
- SP He was extremely thorough in looking at everything, every detail. Anything that didn't quite come out as expected he would want to get to the bottom of it. Sometimes that was very profitable because it turned out to be something that was very interesting to follow up.
- MR: Were there tears?
- SP: Oh no.
- MR: Was there voices raised?
- SP: No, but I remember David Colquhoun who told me he met David Ogden who said he had just had a session with Rolf, I think it was over his thesis writing; he felt quite depolarized.
- MR: That's David Ogden who is at Mill Hill now?
- SP: He has certainly been at Mill Hill.
- MR: Oh, he is not there now?
- SP: He is in Paris now I think, yes.
- TG: And David Gadsby who is at the Rockefeller isn't he?
- SP Indeed yes.
- MR: And Jonathan Ashmore was there at the same time?
- SP: Jonathan indeed was a postdoc with Paul... well Gertrude particularly, and he and Gertrude had the lab next door to ours and yes he was an enjoyable neighbour.
- TG: He had done an MSc with me, in fact, before he went to Gertrude, when I was in charge of the postgraduate students.
- RW: You supervised MSc student projects quite a bit. I think I can remember one or two.

- SP: Yes, less projects more... I took over from Tony as postgraduate tutor in the department and like Tony I did both the MSc and the PhD students and then at some stage I think we increased the number of PhD students; it was felt that was a rather heavy load and the MSc student responsibility was given to somebody else.
- RW: I was remembering Malcolm Irving, think he did a...
- SP: With Rolf, yes on the sodium pump and the skeletal muscle.
- RW: Was that unusual that you were supervising a PhD student or Rolf or both of you?
- SP: It was the only one I remember yes; that was arranged by Bob Simmons.
- TG: Malcolm did an MSc with you and he did a PhD with you, Roger?
- RW: Yes he did his PhD with me later on which is why I am remembering this actually, and I thought he might have been one of many but it seems that he was one of one.
- SP: Yes, I think one of the disadvantages of Biophysics was that one didn't have contacts with undergraduates really except those whose who were teaching on that Biophysics course.
- RW: Were you one of those? Did you teach on that course?
- SP: In the last years of its existence I was the course organiser but I didn't actually do any teaching.
- MR: You had editorial responsibilities on *J Cell Science*?
- SP: For quite a while yes.
- MR: Did that take up a lot of time?
- SP: Not a lot, no. I didn't have a great deal... nothing like the number of papers that people do on *Journal of Physiology*; they were not getting enough on muscle.
- MR: You said that Bernard Katz and Ricardo Miledi were not very good at managing the department is there anything of interest there?
- SP: No, I think the department was very much arranged around Bernard and then Bernard and Miledi and their science, which was understandable. I don't think Bernard really... perhaps he even hadn't had experience of a good workshop. I had the feeling he didn't really know how to interact with the mechanical workshop chap to give feedback or to quite know what to expect from them. For all that, he built his own oscilloscope and so on. I don't think he again interacted well on the design of equipment with the electronic people. He knew that he wanted and needed these sort of people but he didn't know really how to encourage them and they became very slack. He had left the running of the department initially I think very much really to his first lieutenant or whatever you call them, Parkinson, Parkie as we call him who

was a... if you were a favourite of Parkie that was fine, if you were not you were in deep trouble.

RW: Were you a favourite of Parkie's?

SP: I wasn't but I was neutral, I think, but Paul and Rolf couldn't get anything from Parkie. I got on with Audrey [his assistant], I think because I was a woman and that helped. Audrey had been trained by Parkie and I also think her bark was worse than her bite, but they were difficult.

RW: This relates to the fact that a lot of your funding was, in those days, would be departmental funding as everybody's was because the basic funding of your research came through the department. Therefore it would come through the hands of Parkie and Audrey and whether you could get some money to buy something might have been in their gift.

SP: Exactly that, yes.

MR: What was the feeling in the department when Hodgkin and Huxley got their Nobel Prize?

SP: I think we were disappointed that Bernard wasn't part of the trio, wasn't one of the trio at that time and so of course there was delight when his came not so very long afterwards.

MR: But was there any change in him when that happened, it didn't happen rather?

SP: Oh, I didn't know him well enough to know, I would be surprised.

TG: Did people talk about that sort of thing over tea in the Biophysics Department? Was it quite sort of sociable and interactive in that kind of set up?

RW: Was there tea in the Biophysics Department?

SP: Yes there was at one stage. We did have a common room at some stage but it did later become very much the fifth floor and there... I think it's well enough known that when Katz came to retire all the members of staff apart from Ricardo told the powers that be that we didn't think Ricardo would be the right head of department to succeed Katz. We would have liked someone from outside.

MR: Why was that?

SP: His management... he was very much his people had to have priority and he wasn't a biophysicist really I think. The noise work with Katz you can say was biophysical but that was Katz and Katz had discussed it with Paul Fatt before they started the work. Ricardo was more biological but I think there was a feeling that there was quite a lot of antagonism between Ricardo and particularly Paul and Gertrude so Rolf and I were more on the sidelines.

MR: Neutral.

SP: Yes.

- RW: So would there be squabbles over allocation of resources essentially...
- SP: Yes
- RW: Not about scientific matters?
- SP: No, and time on work. Resources, both of physical resources, time of support staff and so on, yes. I don't think he was appreciated by other heads of department; he wasn't regarded as a very good colleague I understand.
- RW: No, he was regarded as an extremely treacherous I think...
- MR: Were Bernard Katz and Ricardo, were they interested in your work?
- SP: Not terribly.
- MR: Did the head of department explain to visitors what people in the department were doing other than their own work?
- SP: Very rarely.
- TG: But you would meet them at tea or did the sixth floor not go to tea in the fifth floor in practice?
- SP: I'm hazy. There was a period when we did. The period when Jean Rosenthal, Dale Purves, Bert Sakmann, Bill Betz that sort of period, there was more interaction, friendly but later not so.
- RW: Did you feel that the label Biophysics fitted you? Did you see yourself as a biophysics person? Not because of the department but because that was the field of science in which you worked?
- SP: I think initially I felt if you can't... One form of biophysics is using physical equipment to study biological tissue and yes, whilst I was doing electron microscopy that was certainly the case. I would think later no, I wouldn't, I don't describe myself as a biophysicist.
- RW: What label would you put on yourself?
- SP: It is a difficulty because I don't really think I'm a physiologist either.
- TG: Why not?
- SP: I'm too narrow in my understanding.
- MR: There are very few people in physiology who...
- SP: These days but...
- TG: You would fit right in!
- RW: Do you think you're a physiologist Tony?
- TG: Yes, but I think Sally is a physiologist too actually.

- RW: Yes so do I; I think I'm a physiologist but I kind of took it in an earlier stage but I certainly would have labelled Sally as a physiologist. I mean if you have a frog heart then that's...
- SP: OK, but ask me about the kidney!
- RW: Yes but ask me about the brain! I mean you know, we all have things we don't know about, it's really all about your attitude to the things you do know about.
- SP: I would accept being a physiologist with respect to muscle.
- RW: Rather than a biophysicist?
- SP: Yes.
- RW: Because you would record force for example as where a biophysicist might be more interested in the diffraction of x-rays from the tissue.
- SP: Or the membrane properties Paul and Gertrude worked on.
- RW: Yes but if you are interested in functional things and then you want to build that up.
- SP: Yes, I don't think in fact... Bernard Katz I think himself once said to David Colquhoun that he thought that he BK was more of a pharmacologist and David the biophysicist.
- MR: Were there many or any physicists who became interested in the biological problems?
- SP: Within Biophysics no, because Paul was a biochemist; within Physiology, clearly people like Tony and Bob Simmons and Malcolm Irving...
- TG: Had come from physics degrees...
- SP: Exactly.
- TG: But what about E. J. Harris, was he in Biophysics?
- SP: E.J. was a chemist; oh Bernard Ginsborg of course was a physicist originally.
- TG: I remember in those days when people talked about biophysics it was almost a sort of word that meant the physiology of neuromuscular junction from outside as it were. Then when I came to see King's for example the x-ray crystallography going on that seemed to be much more what I would have called biophysics in a sense.
- SP: I think that's true. It was a useful term wasn't it for... also of course it led on from A.V. [Hill] and I think it would be fair to call A.V. a biophysicist? No?
- RW: Well he is hard to classify I think that but he certainly is the one who stuck the label of biophysics on the Biophysics Department as he is the one who created it and so he called himself a biophysicist, he thought that's what he was.
- SP: Yes.

- TG: But with him the Holy Grail was almost accurate measurement in itself, I mean it's biophysics in that sort of sense.
- RW: Yes his definitions were the application of quantitative physical measurements in biological tissues; he would have used that definition which fits you in perfectly.
- SP: Yes.
- TG: But fits most physiologists in fact.
- RW: Yes quite, although I guess as those days... or even then I think there was a lot of emphasis on quantitative measurements really. I mean people who were measuring blood pressure were very concerned with getting accurate measurements and blood pressure and understanding.
- TG: But he and Helmholtz I think are often credited with sort of really seeing that measuring things above and beyond the call of duty as it were as it was generally perceived was actually potentially worthwhile and you could actually distinguish hypothesis by making much more careful studies than otherwise people had been inclined to do and of course developing theories. You were never really a theoretical person in that sort of sense?
- SP: No indeed.
- TG: That's often also a part in part of biophysics?
- SP: Yes.
- RW: But you did a lot of modelling of flows and diffusions and compartmental dynamics, which fits as being physical kind of stuff doesn't it?
- SP: Yes.
- RW: It's the kind of thing biochemists do quite a lot.
- TG: How did you interact with Rolf in that kind of context? Was that primarily you or primarily him developing those ideas?
- SP: It was very joint input from both of us. I mastered using a spreadsheet and a computer, which Rolf didn't need to and so didn't. So I did the mechanics of it as it were, but very much the ideas put into the modelling I think from both of us, probably on the whole things started with Rolf and then I would perhaps fill them in.
- TG: And push them right through to the point that there was a bottom line?
- SP: Yes.
- TG: I was interested in you saying earlier on that one of problems I think you said in Biophysics was that there wasn't much contact with undergraduates. Is that something that other people in the Biophysics Department would have said do you think?

- SP: Yes, Rolf was disappointed I think that he wasn't having the opportunity to teach the areas that he felt he was master of. Rolf before he came to England I think he interacted a lot with people. I think Cambridge must have been pretty devastating: to be German so soon after the war, probably not speaking very much English and the importance of being articulate I suspect in Cambridge was very important. He was living on the breadline and so I think he probably was not thought of as a person who would be an obvious good lecturer but I think he had done and enjoyed teaching in Göttingen for example and interacting with the students. I don't know what he was like as a teacher on the biophysics course.
- MR: He was good; he was good on the BSc course.
- RW: He was very good at encouraging student's discussion in the biophysics course.
- SP: Yes, in contrast with BK?
- RW: Yes quite so, and so I do remember him interacting very well with the students on that course.
- TG: Was that partly because the students were intimidated by BK do you think?
- RW: No, I think from our point of view BK and Rolf were pretty much of an equal intimidation.
- TG: OK.
- RW: Rolf made more effort to get in touch with what we were thinking and well had more instinctive idea of what might be difficult for us than Bernard Katz did. Certainly Paul Fatt was terrible at understanding why other people didn't understand what he said.
- SP: And Bernard Katz had little patience with equipment. I gather he'd come down if something wasn't right and he would just pull all the cables out and Ricardo would be heard to mutter 'I wish he would go away!' Actually it was something of that I saw when BK was starting to use a computer in late retirement and Michael Wilson started him off and then I sort of found myself taking over at being BK's tutor on a word processor and he had no patience at all. For somebody who had such an analytical mind he didn't apply it if something was wrong on the screen. He would be up at my door rather than working out: now, if it does this, it means this, is what I have done wrong?
- MR: It's frustration.
- SP: Yes it was fascinating in a way and quite unexpected for me just how difficult he found it.
- TG: Did he enjoy teaching though do you think?
- MR: Who? Bernard Katz?
- TG: Yes

- MR: Oh, he was very... I mean he used to give these BSc talks without any notes, he used to just come in and start talking, he was very fluent and very easy...
- SP: But I suspect that had all been very well worked out beforehand, hadn't it, as that was his...
- MR: Yes, probably one didn't see the feet working under the water sort of thing but he certainly impressed us anyway.
- TG: Whereas Ricardo, I don't think he did much enjoy teaching did he? He didn't volunteer much anyway! In those days and the story was that sometimes if you signed Ricardo to do some teaching you found that BK turned up on the day, kind of thing.
- RW: What have we not touched on that we should have discussed, anything?
- TG: A shift to Physiology, of course, when the Biophysics Department wound up.
- SP: The closure of the Biophysics Department I think is something that wasn't handled well by the College because we were not told what was happening and particularly certainly not told officially. The technical and support staff in the department, for them it was even worse and so the loss of morale was enormous. The academic staff were reasonably confident from what Tim Biscoe had said to us. We knew we were going to be absorbed into Physiology but the support staff didn't feel confident about that. It was actually only about three weeks after we officially become presumably officially part of Physiology that we got anything in writing to tell us what was happening, so it was a difficult period. [...] After a while, Tim Biscoe said I had the choice of running a first year course or becoming a tutor to the postgraduate students taking over from Tony and he thought I would prefer the latter and I did because I didn't feel I could run a physiology course for first years; third years yes, I did on muscle and we enjoyed that, but not first year. Then gradually that took over more and more of my time, partly I enjoyed it, partly I felt an obligation to pull my weight in that way. I became gradually more involved at the college level in the running of graduate schools and research degrees, and so that's how I rounded off my time in full-time employment.
- TG: How many staff transferred with you at the same time?
- SP: Paul, Gertrude, Rolf and myself. Paul retired fairly soon afterwards, very soon afterwards, Rolf a little bit later and then Gertrude. Tim had said to me that the other three were regarded as... they were seeing out their time within Physiology but my being a different age group I would be expected to become a full teaching member of the department.
- TG: And what about the support staff that you said people were worried about? Were there some there?
- SP: One of them joined the electron microscope group here for a period. I'm not sure, oh the mechanical workshop chap had actually gone off a while before the department actually closed and we had a chap who had been in the

biochemistry workshop, he was under short term contract. The electronics chap I don't know whether he stayed in Physiology; no I think he left, yes.

MR: Keith Copeland. Oh no, he'd gone.

RW: He retired.

TG: Still seen around occasionally I think.

RW: Yes he was but I think he had retired by then.

TG: So, if you had been Provost how would you handle this better? Over the years before, as it were.

SP: I would have established a better line of communication. Actually when Lighthill visited the department Rolf and I told him what we thought should be done with the technical staff, which was actually change quite a lot of it, but it was too late then.

RW: The key decision was probably not having Ricardo as head of department when Bernard Katz retired because Ricardo wasn't interested in integrating the Biophysics Department into the college. At that time the politics was that if you wanted to survive as an independent entity you had to be seen to be doing teaching, undergraduate teaching and seen to be productive across the board and Ricardo wasn't interested in doing that I think, is that fair?

SP: Yes.

RW: And if someone else had been brought in with different, more integrationist ideas at that time then he was more able to see the politics of the institution and then the department might have survived as an identity for a while, perhaps until about now when everything gets pushed around but there are always these pressures aren't there against small groups...

SP: Yes indeed.

RW: And have been for many years but that would have been the way to defend against them then.

MR: What year did the transition happen?

SP: Bernard Katz retired in '78 and Ricardo left in '83 I think it was.

RW: About five years there.

MR: And that's when the departments merged?

RW: Yes in '83 or '84.

SP: Yes.

TG: And Ricardo took on the Royal Society Chair, was it the Fullerton Chair?

SP: Yes.

TG: And that was the time BK retired wasn't it?

SP: Right, it was actually that he took it a year or two before BK retired and I'd been a non-professorial member of the professorial board and some of the other people I knew from that warned us that the College was asking the Royal Society whether the terms or the Fullerton could be changed so that holder of the Fullerton could be Head of Department. There was already the warning that Bernard Katz and people were lining up that Ricardo would take over when Bernard Katz retired.

TG: I know from interaction with Peter Baker that he was very strongly of the view that this ought not to happen because the Biophysics Department was one of the major educational institutions and it wasn't satisfactory to have that run by somebody whose primary responsibility was to research, as a Fullerton Professor. I think there was quite a lot of sympathy for that view that it didn't work out.

MR: Where is Ricardo now?

SP: California, Irvine.

MR: Is he still active?

SP: I don't know.

SP: Yes it seems we have come to a natural end.

MR: Thank you very much.

SP: I feel we've sort of gone here, there and everywhere.



Tony Gardner-Medwin, Sally Page and Roger Woledge photographed by Martin Rosenberg.



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](http://www.physoc.org)

