

An interview with Lynn Bindman

conducted by Laurence Smaje, Alex Cooper & David Miller on

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



Further interview photos on the last page

This interview with Lynn Bindman (LB) was conducted by Laurence Smaje (LS), Alex Cooper (AC) and David Miller (DM) at Lynn's home in London on 30th March 2015. The transcript has been edited and annotated [*explanatory details given in square brackets*].

- DM: Let me introduce what's happening here. It's the 30th March 2015, and we're at the home of Lynn Bindman in Highgate and we're here to record an oral history interview for the Physiological Society. I'm David Miller and the next voice you hear will be Laurence Smaje, who will then pass you along until eventually we reach Lynn.
- LS: I'm Laurence Smaje. Lynn and I met at University College London in 1962, I think, for the first time, and we remained colleagues and friends ever since.
- AC: I'm Alex Cooper, as Alex Milne I was Lynn's PhD student from 1973 to 77 and have remained a friend since.
- LB: And I'm Lynn Bindman [laughs]. I was born on the 14th July 1938, so I'm 76, nearly 77.
- LS: Lynn, although we've known each other for a long time, I don't know very much about your background. I know snippets as we have chatted over the years but perhaps you could take us back to your parents and grandparents, maybe?
- LB: Sure, well all four of my grandparents were immigrants. Two had come from Poland and two from Russia. And although they were Jewish, they didn't run away because of pogroms, they were economic migrants. My maternal grandfather, Jack Prevezer, was a very, very dynamic person, he ran away from home because he was 17 and engaged to be married, and his father died and he was responsible for his mother and seven siblings. And then his fiancée's father died and he had another eight siblings and mother in law to look after, and at 17 he thought this was too much and he ran away. And he was on his way to America and he got off in London [by mistake] and he stayed. And he would tell us how [he] did three jobs at once to earn money. One of them was being a presser and he burnt the trousers and lost that job almost immediately. [laughter] He was a really terrific

guy. And he had a great gratitude for the way in which England had taken him in and he always said to us, “You’ve got to be very careful to give back to the community and to be honest and not do anything criminal” and was very keen on education. So he was very proud of the fact that I went to university and that I married Geoffrey [*Geoffrey Lionel Bindman QC, b. 1933*] who was a university graduate and professional too.

He was married to a very sweet woman who had come from the same town but he met her in England. He felt very guilty about having deserted his fiancée and went back and gave her money. He did visit his family, and he went with an uncle of mine in 1938, before war was started, to the Konin area [in Poland] and tried to persuade his family to come over to England. He persuaded three brothers to come but all the others were wiped out by the Nazis. So that was the end of that family. My father’s father came over with a man called Ralph from Russia, and they were housepainters. My grandfather was Jewish, Ralph was not. And I met Ralph when I was a child, and when I was first married he painted Geoff’s and my first flat and he talked about my grandfather. But my grandfather died of cancer when my dad was 21, so I never knew him. My father had to look after his mother, a younger sister, the older one had already got married, and two younger brothers. So he was very aware of the importance of being able to earn a living, which is relevant to my story.

Dad had left school, at 14 but went to Regent Street Polytechnic and he studied to become a licentiate of the Institute of Builders, eventually, so he did full time study for two years, left Regent Street Polytechnic at 16 and carried on at night school there to get his building diploma. And he took over his father’s building firm and ran it with advice from his father until my grandfather died. And so when he married my mother, who came from a slightly wealthier family because the Polish grandfather was very entrepreneurial, she had to accept that he had a lot of family responsibility still. And they were both highly intelligent people. My Dad was well-read. My mother was very angry with her parents for taking her away from the school she loved, which was called Skinners in East London. And she was sent to South Hampstead High School in Hampstead; I think she was 14 at the time. She was so angry at missing all her friends and teachers.

LB: My mother, Nettie, didn’t study at school and she left at 16, her parents sent her off to Switzerland, to a finishing school where she did French and commercial subjects, and German, and met some good friends who remained friendly for life. But she was very keen on me continuing to study. And when my father made comments like, “Well, yes, of course you can go to university and I’ll help you all I can, but don’t get too much of a blue stocking because men don’t like it.” [laughter] my mother said, “What rubbish!” She said, “You go as far as you can.”

I went to South Hampstead High School. Oh, I had an older brother as well, by the way, I do have an older brother, who is a very lovely chap, very sensible and very kind to me, and always has been. But we’ve led very separate lives and we normally see one another a couple of times a year and phone one another once every couple of months, but we aren’t close.

LS: You said your mother was angry or irritated about being taken away from Skinners. Why did they do that?

LB: Because they moved house.

LS: Oh, because they moved, I see.

LB: And it was too far to travel. Sorry, I didn’t make that clear.

LS: Well, I started coughing so it wouldn’t have helped us anyway.

LB: I went to South Hampstead High School when we came back from being evacuated during the war, when I was six. And I stayed there until I was nearly 18, and I absolutely loved it. And the junior school was private but in the senior school we had county council scholarships and it was 80% funded from the State via the local councils, and there were very few private pupils. And it was a GPDST [*Girls' Public Day School Trust*] school, which meant it was part of a series of schools that were set up for educating women in the 19th century. And it was peopled by the widows of the First World War, so there were lots of elderly spinsters. One of them proudly wore an engagement ring but her fiancé had been killed. The others were all single and it wasn't until I was about 14 or 15 that we got the first married woman teacher, which was interesting. And I remember when I was thinking of going to university, one of the women, a Miss Brecke said, "Now, Lynn, don't go to an all-women's college." She said, "I made that mistake. My sister and I went to an all-girls school, we went to Bedford College London and then we started teaching in girls' schools and we never had any opportunity to meet men and get married. So don't you make the same mistake that we did," which was very sweet.

LS: Do you think she was right?

LB: Probably. No, not actually. Because of my brother and the fact that we belonged to a tennis club, I had a very full social life, but I can see the point she was making, which was interesting. So it was a good education but science was not that well taught and chemistry was pretty appalling. Biology was adequate. The physics was well taught but I wouldn't do A level physics for a sad reason and that's that the nice teacher, who encouraged me to do science essay prize things, she had the most appalling BO. [laughter] Isn't it awful? She had polio and she was small and a bit deformed and it was hard work walking, I think, and no one ever told her about deodorants and antiperspirants and so I couldn't bear being with her. And so I did botany, zoology and chemistry for A level, and then I discovered I had to have physics and so I went to a tutorial college and studied alongside young men. And I got some good advice about going to university from the head of the tutorial college. I'd never heard of physiology and I didn't know anything about university. One of my mother's cousins had gone to Cambridge to read law and he ended up as a law professor and was very able but none of the rest of my family had gone to university.

I had never heard of the word physiology but one day I was asked by some American friends to take out a young man of about 18 or 19 who was coming over to England, and he phoned me up and I arranged to meet him outside Foyle's bookshop because he knew where that was and it was a convenient sort of place. And Foyle's was a wonderful second-hand bookshop that was totally badly organised. I know this because my daughter worked there when she was 18, in her gap year. And on the outside shelves of Foyle's bookshop, among the novels, I saw a book called *Bainbridge and Menzies 'Essentials of Physiology'* and I picked it up, it was quite a big textbook. And I leafed through it and I thought, "Hm, this is the aspect of zoology I really enjoy, or biology." And I bought it for some trivial sum and so then I realised, yeah, I'd like to study physiology. And the head of Borlands Tutorial College, when I got my A Level physics said, well, it was probably before that, "what are you going to do and where are you going to apply?" And I said, "Well, I rather like the idea of studying physiology" and he looked it up for me and he said, "Well, mostly it's studied together with medicine and in London there's University College and King's College but King's only do it as an intercalated BSc subject. So you could go to Oxford or Cambridge or you could try University College London." So I applied to UCL not knowing that it was either the best (or the second best to Cambridge) place in the country. I didn't apply to Oxford or Cambridge - although my Head Teacher had wanted me to - because I couldn't bear the thought of going to an all-women's college under circumstances that I thought were like a women's boarding school, where you had to be

in by a certain time at night. Because my parents had left me very free to live my life with a lot of responsibility and I had a good social life and I came in when I wanted to. And I think I was completely wrong; I would have had a great time at Oxford or Cambridge if I had got in, but never mind, I went to UCL and found it was terrific. So I was lucky.

Now, I didn't actually get into UCL right away, I was put on the waiting list and I found out later that one of the other applicants got a place at Cambridge and chose that, and so I was top of the waiting list and I got in.

LS: Just one small question, Lynn, you said earlier on something about you needed physics and you, I don't think you explained quite why.

LB: No science department would take me without A level physics.

LS: I see, you were quite clear that was the route you were going? But it wasn't that you were going for medicine?

LB: No, well that goes back to my parents and the fact that I was very happy at home and they were lovely parents and very sensible and I probably put too much weight on their advice. And I was quite keen on doing Art, English and French at A level, and Dad said, "Well, look, you can always learn French and you can always read, and you can always paint as a hobby," as he did, "And you won a science prize, so why don't you do science in the Sixth Form and you will always get a job wherever you go in the world, as a technician." And there was that feeling of immigrants, knowing that many immigrants have had to flee, even though my own family hadn't, that you were taking something with you that you could use to earn a living.

LS: The comment you made there showed that the advice you got from your father but also the career advice that you got from the physics tuition outside school in the tutorial college, was clearly quite pivotal in moving you in that direction?

LB: Yes, yes, my father's advice, but that from the tutorial college was after I was already doing A Level science.

LS: Sure.

LB: And what else? I would love to have done medicine and I read a wonderful book about Albert Schweitzer and I thought, "Yeah, I'd love to do medicine, it would be terrific." And my parents put me off and they said, "Look, you'll want to get married and have kids and it's an appalling career for women and living in hospital for a year, it just isn't compatible." And, of course, when I went to UCL and I worked with Olof [*Lippold*] and Joe [*Redfearn*] as a PhD student, Olof said, "You'd be really good at doing medicine, why don't you do it now?" But to jump a bit, by that time I was getting married and I knew we'd want a family and so I decided I really liked physiology enough that I didn't want to do medicine after all. But I think for the sort of teaching of physiology that I did, I read a lot about medicine, but that's jumping the gun rather.

LS: It's quite interesting, you came across this book...

LB: Bainbridge and Menzies.

LS: Yes, and I did something rather similar. I'd never heard of physiology, at least I had studied, it was plant physiology we studied. At school at A Level, I had to do botany, zoology, physics, chemistry, that's right. But I came across this book by Winton and Bayliss [*Human Physiology*] in the loft of my parents. Why on earth it was there, I don't know, my parents had nothing to do with science or medicine but I read it and I thought it was fascinating. But by then I had already committed myself to do a medical career but we'll come back to that later on.

LB: You've reminded me of being interviewed at UCL, and one of the people who interviewed me was John Gray [*John Archibald Browne Gray, 1918-2011*], who was very straightforward and easy to get on with. But the other couple were Sir George Lindor Brown [*1903-1971*] who was Head of Department, and I don't know who he was interviewing with, in retrospect, and he asked me the question, "Why are you applying for physiology, not medicine?" And naively I trotted out this advice from my parents about it not being a suitable career for women, and he said, "Oh." He said, "Well, both my wife and daughter are doctors and married with children." [laughter] So I felt somewhat sheepish about that.

LS: Well, let's move to your time at UCL now, you've just got there.

LB: Well, I think there were eight of us in our year, something like that, doing physiology. And it was an absolutely wonderful place. My first tutor was Otto Hutter, and physiology tutorials - we had an hour a week. I think we were divided into two groups so there were four or five of us, and Jim Pascoe [*James Edward Pascoe, 1924-2011*] was the other tutor, and we had them alternate terms. And Otto's first essay he set us was, 'I want you to go to the library and find out something about a famous physiologist.' So I went to the library and spoke to Mr Marmot, the librarian, and I said, "Where will I find biographies of physiologists?" and he showed me the shelf and the cupboard and I picked up one about Claude Bernard [*1813-1878*]. and one about Pavlov [*Ivan Petrovich Pavlov, 1849-1936*]. And I chose the one on Pavlov and read it and thoroughly enjoyed it. But the amusing aspect of that is, I had no idea what was expected for a university essay as opposed to a school essay, you know, four sides of foolscap. And so I wrote this long screed about Pavlov and his work with Babkin [*Boris Petrovich Babkin, 1877-1950*] on gastric physiology and then the conditioned reflex and all that kind of stuff, and I also noted how awful it must have been for his wife and children because he was always working and always in the lab. And so that was quite an early lesson to me about doing research and how one should perhaps combine it in a slightly different way with family life.

And then I wrote a summary and handed in this typed single-spaced 12 sides of foolscap paper to Otto and he wrote at the bottom "It took you 11 pages to get to the core of the essay," which was the summary. [laughter] So I learned another important lesson about UCL.

But I didn't enjoy the first year all that much in that I had only just scraped through my A level chemistry with 40% so I was lucky to get it, and the chemistry teaching or the course at UCL was dire. We had to do physical chemistry and we had to do organic chemistry and the physical chemistry was weighing substances on these dreadful scales which took forever to settle down and give a reading, and was incredibly boring. And the organic chemistry was given as a series of dictations of material written on the blackboard, and we had to go to lectures, it was compulsory at that time. I took in my knitting and the lecture theatres seats had quite high fronts to them, so I kept my knitting down in my lap so it couldn't be seen, but I did discover afterwards, the lecturers were aware that I was knitting - presumably it was the click of the knitting needles. And after a term I thought, "Stuff this" and I got my mates to sign my name for me, and I gave up going to chemistry lectures altogether. And then of course there was a little problem: the exams and having to pass them.

I think I've given away the textbook [*Fieser and Fieser*]. So I went across to Dillon's bookshop, was it Dillon's in those days? There was Lewis' next door, oh I think it was Dillon's. And I hunted around and found something called *Introduction to Organic Chemistry* by Fieser and Fieser. It was an American textbook and it had these wonderful summary sections and they had test questions to make sure you'd really understood what

you'd read. And I thought, "This is a really good way of learning" and I read that textbook and I remember spending most of my Easter holidays revision time on the chemistry reading that, and I did pass the chemistry. Again, I think I got something like 44% in physical chemistry but I got 80% in the organic chemistry because I just loved what I was reading, about sugars and things that were relevant to biology. And so the chemistry staff said, "Well, you can obviously do chemistry" and I was allowed to pass into the second year. And I scraped through the physics, and of course I loved the physiology, and that got better and better.

And I enjoyed the social life.

AC: Were you living at home?

LB: I was, yes, but as I said, my parents were very free and easy and so if I decided to stay at college to do something, I gave them a ring to say, "Sorry, won't be home for dinner" and otherwise I did go home. And I never felt that I lacked independence by not living away from home.

LS: So where were you living?

LB: We lived in Childs Hill, so how did I get to UCL? I got a bus along the Finchley Road to Finchley Road station and then I got the tube for three stops to Euston Square. And so it was very quick and easy. And I joined the Jewish Society in the first year and then that transmuted into a membership of the Humanist Society by the second and third year. And I also played tennis, and I played tennis for UCL and I was in the first couple of the first six, and that's not because I was that brilliant at tennis but I enjoyed it very much and I belonged to a tennis club, but the person I was partnered with was actually Kent junior champion. I was useful because I could always get my serve in, and she would win the points after I got my serve in. [laughter] So that was tennis and Jewish Society. And I can't remember when it happened (maybe when I was a graduate student) but at one point there was a wonderful Staff-Student Science Society which Denis Noble started and I think that arose out of the Humanist Society but he was two, I think Denis was two years ahead of me or would he have been three?

LS: Two.

LB: Two, same year as you? And Denis was going out with one of my girlfriends studying Philosophy at UCL. He didn't marry her, that was Susan [Kahn] Wright, she became. And so we all became friendly and Denis set this up. And we went to, we had, a wonderful weekend at which Shirley Williams came in because she was married to Professor Bernard Williams [Bernard Arthur Owen Williams, 1929-2003]. He was a UCL philosopher and Head of Susan's department. I think the weekend was about religion versus humanism and philosophy. And we all, no actually that was later and the reason I know it was later is that I went with Geoffrey and a baby in a carrycot. [laughs] So anyway that Staff-Student Science Society started at some point and with Denis, so that was all opening my eyes to other things.

Another very important thing that happened as an undergraduate was that we were invited to work in labs, and in the Easter holidays I went to work with Alan Ness [Alan R Ness, 1923-1989]. And I reckoned that holiday period with Alan Ness was the first time I'd ever been asked to think. That sounds crazy but at my girls' school, you were instructed and you wrote down notes, you learnt your notes and you regurgitated them for exams. And of course we were taught to think in tutorials but not in the way that Alan Ness made us do.

And the first thing he did, he sat me down and he said, "Right," he said, "this is a rat's jaw and look, the lower incisors can move sideways like this." And I said, "Oh, have they got a cutting edge in the mid line?" And he said, "Good." He said, "No, they haven't, they're flat." "I'd like you to find out why they are able to move their jaw in the midline." And so for the two weeks of my lab. work we looked at lots of rats and I gave them cheese to eat and they scraped it up, you know, through the bars of their teeth, and I wasn't able to find the answer. The interesting answer is they use the lower incisors for crushing insects in their fur when they're grooming. Alan Ness came to me some years later and said, "There's a paper published that will interest you." [laughter] And what they'd done is they'd put a collar around the rats so they couldn't reach bits of their body and the fleas and the insects multiplied in the fur. And when their collar came off they groomed. So they observed the rats and found out.] We just didn't observe them doing the right thing! So that was great fun.

And Alan taught a third year course which was Interpretation of Data and , we would have a research paper and he taught us to pull it to pieces in a way that some members of staff, like Jim Pascoe, said he thought was possibly counter-productive, because it made us so critical of our own work that we were unable to write it up, because everything was open to question and criticism. So that was another aspect to the teaching there. I don't remember all that much about the lectures but we had very good lectures and I did attend all of them. I remember very good histology lectures. Do you remember the name of...?

LS: [Keith] Richardson.

LB: Richardson, thank you. So he was in the Anatomy department and he gave nine o'clock in the morning histology lectures.

LS: Starting exactly at nine and finishing exactly at five minutes to ten.

LB: And I suppose they were dry but they were just fascinating because actually he gave the physiological function of every structure he talked about. So for the skin you knew why there were hairs and sweat glands and everything else.

DM: These were large room lectures together with the medics, or together with other science...?

LB: Yes. Science, medics and dental students. Well, the Richardson ones I think were in a smaller theatre, there were about 100 odd.

LS: Oh dear.

LB: I don't know how...

LS: Well, they were given in the anatomy theatre, I think.

LB: Were they? I remember...

LS: Well at least they were when I was there.

LB: I remember Embryology theatre, actually, that's my memory.

LS: But he left, didn't he, to go to the States.

LB: Right.

LS: Because they wouldn't give him an electron microscope.

LB: Right, how fascinating. But he was such a brilliant teacher.

LS: He was, excellent.

LB: And of course what was really remarkable about UCL was the way the third year students were treated because we did animal experiments four days a week sometimes and we were invited into the Starling Room, which was the Staff Room, so we had our tea and coffee there and we were mingling with the postgrads and the staff. And everyone was teaching on first name terms while we were round the rabbits and the cat and I remember one evening we'd finished -and of course we had to be supervised by staff with anaesthetised animals and they had to be killed at the end of the experiment- and I said, "Could we practice doing some cannulations on the dead rabbit?" or cat or whatever it was, and so I was given permission and three of us stayed on. And G L Brown who was the Head of the Department was wandering along the corridors, as the staff often did, and stuck his head round the door and said, "What are you doing?" And I explained that we were practising cannulations because it was quite difficult and would be a good idea, I thought, and he came along and watched us doing it and then he showed us how you could pull a thread under a vessel and then, just with one hand if you had the standing thread, with the forceps make a loop in the standing part and pick up the end so you could tie the half tie and then the full tie with just one hand, which was quite useful. You could use your other hand for something else.

And so I really enjoyed my time at UCL as an undergraduate and I didn't think about what I was going to do that much but I think I mentioned I had a very good social life and I had a number of boyfriends and by the end of the second year one of them, Geoffrey Bindman, my husband later on, sent me a card and it said, "I must see you on Thursday because I'm going off to America on Friday, and I'm not mad, just lovesick. Yours, Geoffrey." So I rang him up and we arranged to meet in Green Park.

What Geoffrey wanted to say was, "Please don't marry anyone else while I'm away." Anyway we corresponded weekly and Geoffrey was teaching as a law fellow in Chicago. And he wrote to me and said, "What about coming over to America in the summer? I've got a car I can borrow and we'll travel across the States and back."

So Geoffrey met me off the boat and I stayed with my parents' friend, a woman in New York. After five days she said, "Would you and Geoff like to come and stay with us in our little cottage at Tanglewood, and you can come to the music festival." And so she invited Geoffrey to join us and there the two of us were having our picnic breakfast on the lawn and I made some comment about the milk in the milk bottle, saying my mother wouldn't approve of us having breakfast with the milk in the milk bottle, and Geoffrey said, "Well, I won't mind when we have milk bottles on our breakfast table" and I just looked at him and said, "What do you mean?" and he said, "I'm asking you to marry me." [laughter] [Then we drove via Canada to Chicago] By the time I'd travelled for a few days with Geoff I knew I trusted this guy and of course I'd marry him.

Geoffrey had in fact been offered a job to stay in America after his year in Chicago And I thought, "Well, how am I going to carry on with physiology if I'm going to be in America?" And I noticed on the lower corridor in the Physiology department there was an advert where a professor at a Boston university, which is where Geoff's prospective job was, called Stephen Kuffler was offering a scholarship.

I decided I'd apply for the scholarship and I thought, "Well, this is biophysics and Bernard Katz [1911-2003] had taught us a bit in the biophysics course in the third year, so I'll go to Bernard and ask him if he'll give me a reference." So I did, and he said, "No, no, no, I wouldn't dream of giving you a reference to go and work with Stephen Kuffler [*Stephen William Kuffler, 1913-1980*]." He said, "He takes the pick of the country and they wouldn't just take a BSc student." So I was very crestfallen. But about a day or two later I was summoned to see Sir George Lindor Brown, my professor, and he said, "I gather you want

to go to the States to do graduate work?" And I said, "Yes, that's right because the chap I think I'm going to marry is going to be out there and he's invited me to join him. And Sir George Lindor Brown said, "It would be the most terrible mistake." He said, "Go to the States after you've got your PhD but if you go now you'll be treated as a technician because they don't rate their BScs so highly. We have trained you to be able to do research work, and you should stay here and do your PhD at UCL. We will find a place for you provided you get an upper second class degree, we will give you an MRC research scholarship." So that was how my future got decided. [laughter] Isn't it bizarre?

AC: And pretty nearly Geoff's as well?

LB: And Geoff's as well because Geoffrey was a little bit worried about staying on; he said, "I'm not very happy about the political way in which they appoint judges in the States and I think maybe I'll come back to England with you and start work..." So he came back and lived in our house with my parents until we got married, in April 1961 That was nine months after I got my first degree, so it was in my first year of my PhD. I got my PhD in 1964 although I'd submitted in '63. Jonathan our elder son was born in May '63 and he interrupted my PhD. I'd written it all up but I really felt a bit soggy in the brain, and after I had him, and my brain felt much clearer again, when all those excess female hormones had gone, I rewrote the thesis. All the work was the same but I rewrote it from beginning to end and submitted it in the autumn. And so I have jumped ahead to doing the PhD with Olof.

AC: Your brain does go to mush when you're pregnant, doesn't it?

LB: It really does.

LS: Well, how are we doing for time?

LB: It's quarter past twelve.

LS: I think we could carry on a bit?

AC: Can I pull you back a bit because you gave us that lovely story about being shown how to tie the cannula to the artery with one hand. It reminded me of this wonderful atmosphere at UCL, the tremendous amount of help there was and it seemed to me, you know, the guys with the Nobel prizes had all the time in the world for the students.

LB: Well, that the Nobel prizes were a bit later when Andrew [*Andrew Fielding Huxley, 1917-2012*] came, and Bernard [*Katz*] hadn't got his Nobel prize yet, but you're right, it was the eminent people.

AC: It was just this feeling that there were all these tremendously eminent people and they always made time for the students.

LB: And there was a lot of joshing around but what I was always aware of, was that the moment you wanted to be serious, everyone would be absolutely serious and take you as seriously as you wanted.

LS: I agree entirely. I think, I did an intercalated BSc and I really, I had enjoyed the physiology as an undergraduate but after I completed my medical course I went back to do a PhD too and it was largely because of that atmosphere, that very positive atmosphere. For the first time in my life, as an undergraduate medical student, indeed in the clinical course even more so, you were treated as some sort of idiot, certainly a lesser being. Whereas doing the intercalated BSc, as you say, your opinion was listened to seriously and obviously sometimes it was rather naïve but you were listened to seriously and they treated you as another human being.

- LB: And you were giving seminars based on research papers to the students and the other staff in your third year and you were really being trained to be a PhD student.
- LS: Well in the third year certainly we had no lectures. I don't know about you but when I did mine we had no lectures, it was all done from reading the original literature or experimental work. As you say, I can't remember how many days a week.
- LB: And sometimes we were sent to hear external lectures. I remember being sent to one by Woolsey [*Clinton Nathan Woolsey, 1904-1993*] who did all the mapping the brain on the cortex and me saying to Jim Pascoe the next day, "You sent us to a dreadful lecture last night." And he said, "Why?" And I said, "Well, because it was just anatomy, it was nothing to do with physiology." And he laughed and said, "You've got to make your own mind up about people." He said, "He's actually very eminent." [laughter]

... RECORDING BREAK

- LB: Okay, so Laurence has just said that I'd mentioned Pavlov and Laurence said...
- LS: I was his great grand PhD student.
- LB: Yes.
- LS: Taught by Babkin.
- LB: I remember Babkin was ... was he also Russian or was he German?
- LS: Russian as far as I know.
- LB: And Mel Schachter [*Melville Schachter, 1920-2000*] had gone to work with him- when you did your PhD with Mel Schachter.
- LS: Babkin lived in the States or Canada, I can't remember, where Mel Schachter was educated and then he was my supervisor at UCL. Anyway.
- LB: Well anyway, I was asked to go and do a PhD with Olof.
- LS: You got your upper second.
- LB: I got my Upper Second, yes. And apparently I was viva-ed for a first. Jack Mongar [*John Lawrence "Jack" Mongar, d.2004*] came in saying, "Did Lynn get her First?" And the answer was no, and it was quite right that I didn't actually because I wasn't really that much of a high flyer but I got my upper second alright. And life with Olof was absolutely wonderful.
- LS: Sorry, where did Olof come from?
- LB: Olof Lippold is English, with that extraordinary name. His mother was Swedish and his father's father had come from Germany. And Olof was married to someone he met nursing at UCH during the war and they had six children. At the time I knew Olof I think the youngest was three, and I can't remember exactly how old Jennifer was, the oldest, about 14 I think. And Olof worked with a psychiatrist called Joe Redfearn [*Joseph W.T. Redfearn, 1921-2011*] and one day a week we went to Graylingwell Mental Hospital. So I would go by train to where Olof lived, I would go from Waterloo to Claygate and then Olof would drive me down to Chichester once a week and then occasionally I would drive and pick up Olof and we'd go down there. And Joe Redfearn worked at Graylingwell Mental Hospital. And so this was my first exposure to mental patients and it kind of gave me a very good rationale into doing research into neurophysiology and the neurophysiology of the cerebral cortex, which I was doing with Olof, because apparently quite a high percentage of women at some stage or another would get depressed and I could see that

it was really important to get to know more about the brain. So this was where my interest in medicine fed into my interest in doing research.

And Olof and Joe had done an extraordinary experiment in which they were passing low level DC currents through the brain of patients and they, as a research project, said to me, "Would I like to do the same thing on anaesthetised rats in the lab?" And they had been working on muscle spindles...

LS: Sorry, why did they do that experiment in the first place?

LB: That's what I was just about to explain.

LS: Oh, I see, okay, sorry.

LB: They were working on muscle spindles and for some reason they wanted to polarise the nerve endings and I'm not sure why it was, but what they observed was they increased the firing rate with current in one direction, decreased it in the other, but there was a funny sort of hysteresis in that once they'd passed the current that made the firing go up, it stayed up at a higher rate than in the control and they thought, "Hmm, a long lasting effect. I wonder if we can do this in brain and in cerebral cortex?" So in the anaesthetised rat we were recording from the cerebral cortex and we did two sorts of recording. We recorded an evoked response to stimulation of the contralateral forepaw, in a localised region of the cerebral cortex. And we also used slightly smaller micropipettes to record a group of neurons firing spontaneously and the rate at which they fired was determined by the level of the anaesthetic so we could control their firing rate. And if you use an anaesthetic called urethane it didn't wear off for hours and so you could get a very stable level of recording of either the evoked response or the firing rate of the cells. And if you then passed a current that was positive at the surface of the cortex, with an indifferent electrode on the body, it increased the firing of the cells lower down in the cortex and enhanced the size of the negative wave of the evoked response.

You got more spread of the depolarisation up the dendrites so that the early positive wave was followed by a negative wave. But with surface negative current, you decreased the firing because of the polarity of the apical dendrites of the cortical neurones. And the astonishing thing was there was a long-term after effect of doing this when you had passed current for about 10 minutes, and we followed the after effect for hours. And that was my first *Nature* paper but of course it was all Olof's and Joe's ideas for doing it. We had a *Nature* paper in 1962, and *Phys Soc* communications and that was all written up in 1964 as a *JPhysiol* paper. And amazingly it's still being quoted today. And I'll now go back to the humans because it's been quoted as pioneering work for the human polarisation work. So Joe and Olof, Joe was a psychiatrist and Olof was a medical doctor, they had some patients who were very depressed and didn't talk much and there was one chap who was actually catatonic which meant that if you put his limbs in one position, he would hold them there. And they started passing these very low currents through their brains, having passed them through their own brains first. So the tradition was, if you did experiments with humans, you always worked on yourself first. And as a PhD student, going to Graylingwell with them, the current was also passed through my brain, once with bad consequences. We'll come back to that.

So with the patients, I wasn't there when they had their amazing results, but the catatonic man began to speak and he began to echo what they were saying, so echolalia. And he began to move spontaneously and it outlasted the current and there were depressed people who talked much more freely. I've seen it all on a film that Olof and Joe made. Now we were still experimenting with us and measuring response times and trying to get some handle on this, and what happened with me was that the technician in the

laboratory had, unknown to us, got the coarse control ten times the value that it ought to be, and we were passing, I think, well it was certainly tenths of a micro amp in through the microelectrodes in the brains of the rats, but of course it was larger in the humans. We had big saline soaked pads of gauze on our forehead and we were passing the current between our foreheads and an indifferent electrode, again with a saline pad to stop the current burning, on our legs. So the current was nominally going into the cortex and possibly not because if you think of the fact that the skull is two shelves of bone with a lot of blood in between, who knows where the current got shunted to? And some may have gone in through the eyes. Okay, so when I got ten times more current than they had planned for me to have, Olof and Joe were in the lab with me, the technician was in the instrument part, and I began to - not fit but I could feel that my mouth and lips were getting very, very stiff and my hands and wrists twisted up into a spasm. And they were chatting so they didn't notice and I knew, I didn't lose consciousness, and I knew there was something wrong. And talking was inhibited. I think this was surface negative current.

And finally they did turn round and look at me and they said my lips were blue and they turned the current off and that had a long-lasting effect on me, and I don't mean that in a sarcastic way or as a joke, but I felt very strange. When I was driven back home to Olof's and I then got the train, and I remember crossing Tottenham Court Road from Warren Street, I think I had my car parked at college, and I remember saying to myself, "This is a dangerous road, you must take care crossing it." So there was kind of dissociation there. And the next day I killed four rats trying to do an experiment. I was just too clumsy and my handwriting was funny in the lab notebook. But then after that it was alright and I don't know if there was any damage or anything like that. Olof and Joe of course reported it and there was a meeting of psychiatrists at Graylingwell and they asked me to be present, and they wrote it up in the *Journal of Psychiatry* in which the papers were published. And years and years later, when they both retired, I was invited to meet a German physiologist, a colleague of Paulus and I'm afraid I've embarrassingly forgotten the name, it might have been Nitsche [*Prof. Michael Nitsche of Göttingen*]. And he asked to meet me to learn about these early experiments and I told him about the anecdote, which is there as a footnote in the paper, and I said I thought it was quite dangerous. And he said, "We've never had any problems with our patients and we did experiments on ourselves beforehand." And it turned out what they were doing was polarising from one side of cortex to the other.

And years later, also, in my random reading of books from the library, I came across a description of polarisation of current across midbrain and people having this same type of spasm of the hands. So I think the current wasn't actually cortical, it was going somewhere else. Anyway, he wanted to know what was going on and what had happened with these experiments, and I put him in touch with Olof and they always cite our work as being important. So it was tremendously interesting work and Olof and Joe were funded to do this. And I don't think it was that incident that made me stop going to Graylingwell, because I did it for six months, and I thought, "Well, I'm not a psychiatrist, I'm not a doctor, I've seen what they're doing here, it's given me a good rationale but I would really rather like to be doing experiments on my own in the lab." And that gave me a day a week in which I was entirely in control of the apparatus and could do my experiments and, organise them the way that I wanted to, and that was really rather fun. I enjoyed that.

LS: What sort of experiments were those?

LB: Well, these were carrying on with the polarising and getting the after effects and we were trying to get the after effects with different methods, and we were experimenting with warming and cooling the surface of the brain and warming and cooling the whole rat.

When you cool the surface of the brain you actually increase the firing rate rather than decreasing it, which was the opposite of what happens if you cool the whole body, and vice versa. If you warmed the cortex, you got a slightly smaller evoked response and less firing. If you warmed the whole animal everything got jazzed up as you might expect, like when you've got a raised body temperature. And that's written up in the *Journal of EEG and Clinical Neurophysiology* in 1962. And other experiments we were doing, evoking short latency responses in somatosensory cortex and looking at the relationship between the spontaneous changes in the evoked response and relating it to the background firing and the DC level in the cortex. The evoked response spontaneously varied from surface positive to positive then negative responses in the way that we could drive it to do by applying the different polarity currents, and so we were trying to see what might make this happen in normal life.

And we had this idea that if you could increase the firing rate by other means such as cooling, there would be long term after effects, and there were. And then when I was a research assistant with my own lab I started trying to find more natural stimuli and using somatic stimuli to jazz up the background activity and alter the evoked response, and see if that had after effects, but that goes on to being a postdoc. I should finish talking about my PhD. I loved working with Olof and Joe, partly because they were such lovely people, I mean they were very gentle. And one of the things that struck me and had really an important effect on the rest of my life, was that even when I did stupid things and perhaps killed the rat or broke something or the experiment went wrong, no one ever shouted at me or ever said "what a stupid thing to do". I've come from a family where my mother and father were very kind to me and loved one another very much but they were very outspoken. And my grandfather had a terrific temper and some of that had passed down to my mother. And my mother would really shout at my father and would be quite rude to him.

And so we grew up in this atmosphere where it was alright to be quite rude to people if they'd done something wrong and you know, my father would get very upset with my mother and so finally his patience would snap and he would shout back and so on. And I really learned an important life lesson, which was very valuable for my relationship with Geoffrey [laughter], to be a bit more controlled. So that was Olof. And Olof was also Tutor to the medical students and he didn't have a separate office but he had the first bit of the lab partitioned off and the medical students would come in and they didn't always know that I was experimenting behind this partition wall. And sometimes the medical students had got some very serious things they wanted to discuss with Olof, like one of the women was having a baby and it was illegitimate which was difficult in those days. And some of the students were failing their exams, and I learned such a lot about Olof's treatment of the students under his care, and his kindness, and sometimes his strictness, but in a very kind and adult way. And I thought, "These are two people I really admire and I would like to be like them." And Olof and I became lifelong friends and I still see him and phone him.

What else is there to say about PhDs?

LS: Well, I mean, presenting your work?

LB: Okay, yeah, we did some experiments with gamma amino butyric acid and Olof had this idea that it was a non-specific inhibitor because when you poured it onto the cortex in what they called a 'McDowell experiment' - at the end of the day you would take a drug and pour it on the brain and see what happened [laughs], and we did this with GABA. And as it percolated down through the cortex it shut off all the activity and so we, I gave my first Phys Soc presentation and we called it 'The Non-selective Blocking Action of Gamma Amino Butyric Acid'. And once that was published as a *J. Physiol* paper, Jack Diamond

came to see me and said, "What's all this about the non-selective action?" and I explained. And he said, "Yes," he said, "But supposing it's the normal inhibitory transmitter, couldn't there be inhibitory receptors on every single neuron?" So it was having a very selective effect but on every cell so it shut the whole brain up in a reversible way. And I said, "Hm, yeah, yeah, you're absolutely right but I'm afraid it didn't occur to me earlier." And I gave that as a Phys Soc presentation and Olof said, " "You've got 10 minutes to talk and you'll have to learn it by heart and you've got six slides. Right, sit down and write your talk." I didn't know how to write a talk to be given in 10 minutes and so I needed quite a bit of help about how you did that sort of writing.

And we rehearsed it and I didn't mind standing up and talking in front of people because I'd done quite a bit of acting at school, providing I knew what I was going to say. And the rehearsal in the department was very helpful and as a PhD student I gave a lunch hour talk, as all PhD students did, and the Phys Soc passed off quite happily. I don't remember any particular comments. There were questions so that was a useful thing. So we were prepared to give talks and research talks but I didn't learn the technique of writing research papers and that was my fault, not Olof's. And Olof's technique was that we would go with Joe and we would drive to a lovely hill in the Surrey countryside and we'd take a picnic lunch with us and we would write the paper together jointly and we would discuss the work as we were going. And I had my eureka moment about [laughs] the effects of polarising current and the natural, spontaneous variation responses of evoked responses in anaesthetised rats there because it suddenly dawned on me that the variable forms of the evoked response you got in the normal situation of the anaesthetised rat was what we had been producing in a more consistent way by the surface positive or negative currents. So I suddenly understood what we'd been doing and I'm sure Olof and Joe realised it all the time but they didn't ruin my eureka moment [laughs].

LS: You mentioned rewriting your PhD thesis after Jonathan... [was born]

LB: Yes. I knew how to write my PhD thesis, I mean I was very happy doing the historical research and writing up the results, the methods and the results, but I didn't really know much about writing a discussion. I wrote a few pages and I discussed a few of the points, probably things that we'd written up in the papers. And Olof handed this back to me and he said, "This discussion is your one opportunity to write whatever you want about how the brain works. This is really exciting. You can think and write about what the cerebral cortex is there for and how it works so I'd like you to effectively tear this up and go away and write something totally original." And I thought, "Hmm, what fun." [laughs] And so I rewrote the thesis, after the pregnancy. . And I was able to speculate about what the role of the apical dendrites was and I had this idea of maybe there would be afferents coming into the surface layers that were testing or tapping into whether or not the activity had spread up the apical dendrites or not, because you'd have depolarisation coming in from this direction and maybe there was some way of feeding that information out again?

Now, a lovely anecdote about Andrew Huxley, and I'll go back to talk about Andrew coming as Head of Department. Andrew came to me one day and said, "Lynn, have you got any other copies of your PhD thesis?" And I said, "Well, yes, there are carbon copies" and he said, "Good." He said, "Would you mind if I took your thesis to Stockholm with me and read it on the journey?" because he'd just been awarded the Nobel Prize in my third year. And I said, "But you don't want to read that when you're going to get your Nobel Prize!" And he said, "Oh, yes," he said, "Because it's a long plane journey and I usually read papers to referee on journeys." And the amazing thing was, not just that he took my thesis away to read on his journey to Stockholm, but he read every PhD thesis from cover

to cover that was produced in the department, and he wanted to do that before it was submitted. So he really took tremendous interest in exactly what was going on in his department. And he brought it back after he'd got his Nobel Prize and said, "Yes, yes," he said, "That's very nice. I enjoyed the discussion." So that was very pleasing. I submitted it and it took a long time for it to be read by the External Examiner and I had my *viva* after I'd had Jonathan, that's for sure.

LS: Who was the External Examiner?

LB: Gordon - George Gordon [1920-2002] ... a very nice chap.

George Lindor Brown was offered the chair at Oxford and he went to Oxford. And he had been a really nice Head of Department, concerned about the teachers. I told you about him advising me not to go to the States. But also when I was a young first year student I painted posters for department talks because I loved painting, and George Lindor Brown gave a talk on desert animals and their physiology and he talked about giraffes and also camels and their physiology. It was comparative physiology, a lecture. And I did a superb camel in poster paints as a poster and I think I put down, MRCP.

LB: But he came and found me and said, "I really like that poster. Could I have it when we're finished with it?" And I said, "Well, actually I've entered it for an art competition in the college but you can have it after that with pleasure." And he said, "What you should really have put down was that I was an FRCP because it's quite an important thing to be elected a Fellow of the Royal College of Physicians," so he was educating me a little bit about scientific etiquette as well. And the sad thing was, I put this painting into the college competition and it was exhibited, but I was late picking it up and it had been thrown away. Wasn't that a shame? [laughter] So poor Sir George never got his camel.

LS: I had a nice experience with him as an undergraduate student. He was going up the stairs on the outside of the department from the back quad, whatever it was, I was rushing up the other side late for a lecture. And he opened the door for me and said, "Thy need is greater than mine. Please go ahead." [laughter]

LB: Yes, yes, they were a very, very friendly lot. Yeah. You see when Andrew came, I'd just started my PhD and he would pop around almost every evening, he did a tour of the labs and would pop in.

I remember one evening he said, "What are you doing?" "I'm having awful trouble here," I said, "I'm trying to calibrate the micrometer, pushing the microelectrode with this fine control. I know I use the Vernier scale for the coarse control but I want to calibrate this fine control and it's not shifting the Vernier at all." And he said, "Ah." "Well, now, the Vernier scale is for this coarse control, moving this bit on this, but the fine control at the bottom moves this bit on this." And I said, "Oh, how stupid." And I blushed an absolutely brilliant red with humiliation at being so utterly stupid that I hadn't realised that. And he was a very kind chap and he said, "You didn't spend your youth playing with Meccano sets like I did."

And he was really very kind. I became pregnant while I was still doing my PhD and Olof was not pleased about that because Brenda Bigland [now Bigland-Ritchie], a previous student of his, had not written up her PhD for about 20 years afterwards. After she had her children, she suddenly found she needed it and she came back from America and wrote it up, and got her PhD. And so he was quite fierce with me, Olof, and I said, "I am not Brenda, I will finish my PhD!" [laughter] And I did. And I went to see Andrew and we got permission from the MRC for me to have six weeks off for the first child, I think it was. And we got permission again from, I think the MRC were funding me, for the second child.

But when I went to see him when I was pregnant with Miriam, so that was, in 1968, and I said, "This is definitely going to be my last." And Andrew said, "Well, we had three, then we had a pause, and then we had our next three." And I said, "Well, this is going to be my last. And I'm having it at the end of July". And he said, "I don't think we'll bother to let anyone know about it and get permission for you to be away because you don't seem to take very much time away and it's the summer holidays anyway." And it's true, I actually came back even before the six weeks were up with Jonathan because I couldn't bear being at home on my own alone after being at work, so I brought the baby in in the carry cot to see all my mates and work and feed him when necessary, which was a bit sad really but...

AC: How, I mean how easy did you find it sort of running a career and a family at the same time because you know when they're tiny you can tag them around, you know, and feed them in a corner if you need to.

LB: Well, Geoff had been very clear before we got married that if we had children he wouldn't be prepared to give up any of his work to look after them. He would do his bit when he was home but he wouldn't give up work and that bit would be my responsibility. And I said, "But is it okay if we have trained people to look after them?" And he said, "Yes." And his dad was a GP and he was very keen for me to carry on working. And he said, "Anyone can look after a child, I can't bear the idea of all your energy being just focussed on your children." So as he was a GP I felt that was incredibly encouraging because he knew about problem families and problem children. And my parents were wonderful and you know, particularly as I hadn't finished my PhD, I said, "I must finish my PhD and then maybe I'll take a pause". And they said, "Well, whatever help you need, we'll give you" and they were on call for when our nanny was ill. We had a daily nanny five days a week, and so that was easy, and she would come at 9:30 and I would get into work. And one of the things my parents did to help was to give me a car so that I could drive in quickly and drive back quickly if needed and in those days you could park, so it was quite a quick journey.

And in fact we had such a lovely nanny, who had worked for a couple of doctors and they gave her a wonderful reference, and they'd had several children and carried on working. So I thought that was alright. So with all this parental approval and Geoff helping when the nanny wasn't there, it seemed perfectly okay and I managed to finish my PhD and then it seemed perfectly alright to carry on working. And then when I had our second child two years later, Andrew, with great tact, went to Olof and said, "Wouldn't it be a good idea if Lynn worked part time now she's got two kids?" And so Olof rather than Andrew broached that to me and I said, "Hmm, well, that would be good" so I decided I'd work four days a week. And it was very flexible and so that's how I started working part time. And I worked four days a week until Miriam was at school full time. And by that time Doug Wilkie [*Douglas Robert Wilkie, 1922-1998*] had given me a lectureship.

LS: So what stage are we exactly at, sorry?

LB: I continued working, although I'd had a child, to finish my PhD and because I was able to manage it with a nanny, it seemed daft not to continue working as a research assistant and I did that until Jonathan was two years old, and I had Daniel. And that's when Andrew said, "What about working part time?" and then I dropped down from five days a week to four. And I stayed working four days a week for 8 years until 1973.

DM: So, initially you were research assistant with Olof, or...?

LS: PhD student.

DM: PhD with Olof, but then...?

LB: I was an MRC Research Assistant.

DM: In your own right?

LB: I think in my own right. I mean obviously I was still very friendly with Olof. But I had my own little lab by that time and then I got an assistant lectureship for four years and then there wasn't a lectureship and I became a research associate. And I didn't get the next lectureship that came up, it went to Tony Gardner-Medwin because Andrew was very keen on having people with physics and maths backgrounds. And he was also very sensible I think about bringing people in from outside UCL.

But there is a story behind that which is to do with attitudes to women. I was approached by Jim Pascoe, who came to see me and said, "Lynn, there is a lectureship in this department and Andrew has appointed a man called Tony Gardner-Medwin who is at Cambridge. And I want to tell you that a number of us felt it was unfair that you didn't get it. But when we said, 'What about Lynn having it?' Andrew's response was, 'Well, Lynn has got Geoffrey to look after her and Tony needs a job.'" And I was very taken aback by that because I hadn't been aware of different attitudes to women at UCL - except for the staff Common Rooms [laughs]. And never among the staff.

LS: You should explain those, Lynn.

LB: And, interestingly enough, Olof, when I was interviewing him said, when I went to work in his lab, "I got her to make cathode followers for the equipment because we all made our equipment in those days and I thought I wonder how a girl is going to cope with that." So he had it in mind that there might be things that I wasn't any good at doing but he never let on. And of course that attitude of helpfulness in the lab., I forgot about that, that was in the very first, at the beginning of my PhD studies. And so I said, "Oh, a cathode follower, what's that?" And he explained you needed them to limit the current that went from the preparation into the amplifier otherwise you'd have milliamps passing through the amplifier and that would be polarising the brain, you couldn't have that. So he said, "Go to Geoff Read who is the electronics technician." So I went to Geoff Read and said, "Olof wants me to build some cathode followers." So he gave me the circuit diagram for it and I said, "Have we got all the equipment in the store room?" And he said, "No, we've got the wires and stuff but you'll need to go down Tottenham Court Road to buy the valves."

Olof said, "You may as well build a bank of four while you're about it." So I bought probably a dozen valves for making cathode followers, Geoff Read had told me what kind to get, from the second hand shops on the Tottenham Court Road. I must have had some help from the mechanical workshop in bending the metal to house it all. Later on I did a workshop course and I learned how to use a lathe and drills, and I made the ear bars for the rat holder and my own rat holder. Anyway, I made these four cathode followers and, as I said, it was really like following a knitting pattern. But they didn't work. And I had no idea why they didn't work and I checked the circuit. Alex, I have to tell you, has an electronic engineering degree, so she's smiling rather sweetly, and it was because I didn't know about soldering and they were all dry joints. And so when I went back to Geoff Read and said, "Why doesn't this work?" And he said, "Well, they look to me as though they might be dry joints." And I said, "What are they?" So he showed me that you've got to have lots of flux and the joints should look really shiny.

And so I re-soldered all the joints and lo and behold they worked. [laughs] Like not having Meccano, I didn't use a soldering iron as a kid. And in the science of those days it was a great help to make your own things and know how to do that. But it didn't matter at UCL. No one was patronising to me and they didn't assume that I couldn't solder or couldn't

use a lathe or a drill: and that was the workshop technicians, Joe Osborne and Geoff Read as well. And so it was just great learning all these new skills. And histology, I can't remember who taught me how to do histology but I learned that up when I had to.

DM: You suggested Lynn might explain more about the staff room and sexist attitudes there?

LS: Yes, that's right. Well, the joint staff, in college itself.

DM: Ah, the Common Room?

LB: Yes, the College Common Rooms, which weren't really important to me at that stage because we had our department coffee room, Common Room, where the staff and PhD students and third year science students met. But the College Common Rooms, there were the Haldane Room where men and women could meet and a Housman Room where only men were allowed, and a Lonsdale Room where only women went. And in later years, when I was no longer having tea and coffee with Olof, we used to go to the Haldane Room. There's a lovely anecdote in the obituaries to Gertrude Falk, that we told about Gertrude using the men's staff Common Room and being asked to leave by one of the Beadles. And she looked at him and said, "But I'm going to finish my coffee first" and she sat there and sipped her coffee until she finished before she was escorted out. And it took years before it was used by men and women because they were clubs and so they weren't covered by the Sex Equality Act.

LS: I always thought it bizarre because UCL was the first place, well...

LB: It was founded to be equal for men and women and people of all religions and none.

LS: Well, it was founded because of equal religion.

LB: Sorry, you're quite right, regardless of religion and nationality. And that was in 1828 and it wasn't until 1876 that women were admitted. They came in 1876 under the same terms as men and they were allowed to take degrees in 1878, so it was the first mixed college where they studied alongside men and took their degrees alongside men. And Oxford and Cambridge women students came to take the degree exams at UCL because they weren't allowed to at Oxford and Cambridge.

DM: That's right. They didn't take the exams although they took the courses. That's right.

LS: I remember the battle for the men's staff Common Room. There were people in our department who were very much in favour of the separation. I wasn't.

LB: I wasn't aware of them.

LS: Yes. I can tell you, if you like. [laughter] But anyway it got sorted out in the end.

LB: I was never aware of the sexist attitude in the department at all. Grace Eggleton was a Reader, and Margaret Harkness was a lecturer, and Lucy Brown was a research associate there before Andrew.

LS: Barbara Banks...

LB: Well, Barbara came later.

LS: Yes, she did.

LB: And so later Barbara Banks and ...

DM: Clara Franzini-Armstrong.

LB: Clara Franzini-Armstrong and Barbara and I were pregnant, [me] with my third child and their earlier children at the same time. And I think at the Phys Soc dinner, Andrew made

some humorous comment and said, “I wonder what we should call it. Maybe we should call it a Bank of pregnancies in the department” because he was Jodrell professor.
[laughter]

LB: Is it lunchtime?

LS: I think it’s probably lunchtime.

...RECORDING BREAK

DM: restarting and it’s early afternoon, half past two.

LS: Yes. You talked about your PhD, mentioning towards the end that, about being an assistant lecturer, so if we can take up from there on?

LB: Yes, I was going to bring in an anecdote about John Nicholls and going right back to the beginning of the PhD he came to see me and said, “I hear you’re going to work with Olof? That’s absolutely wonderful. He’s a wonderful person,” he said, “but only pay attention to what he’s written down.” And I thought that was such an interesting thing to say because Olof doesn’t explicitly say, “I’m thinking around the subject and speculating” and he’s very careful what he writes. But he can put forward all sorts of interesting ideas when he talks.

AC: Yes, any conversation with Olof is like a sort of brainstorming session, isn’t it?

LB: Yeah. It’s a very good way of putting it. [laughs] So assistant lecturer, so I think for the next 10 years I was very involved with teaching and developing some teaching models and I don’t think I really matured in research until the 1980s. And so I was trying to get a natural stimulus for getting long term after effects and Nicole Boisacq- Schepens came to work with me. She is from Belgium, from, she was originally from Louvain when I first knew her, and her husband is a doctor and came to work at the Royal London Homeopathic hospital in Queen’s Square. And she had two young daughters the same age as our two sons at the time. And she was a very clever person and a medical doctor before she went into research. And together we worked a little bit on looking at the pathways that went into the brain, into the cortex, to produce these long-term increases in spontaneous firing rate. And I think what we were really looking at was what later Clifford Wolf developed as the windup theory of pain because we could only get long-term effects when we used a very strong stimulus, a pinch, and a prolonged sort of pinch.

So, of course, we were worried it was local peripheral tissue damage. So we exposed a peripheral nerve and produced the long-term change by electrical stimulation of the nerve. And then we used local anaesthetic on the nerve to make sure it wasn’t anything coming from the periphery but was central. But we didn’t know that it was cortical, it could have been anywhere. And we knew that it was crossing in the thoracic cord, that it was the ventral contralateral quadrant to the limb that was stimulated and that was as far as we pushed that.

And then I’m not sure exactly what happened. My first PhD student I thought was Brian McCabe and he invited me to his retirement dinner a little while ago and the Head of Department pointed out that I wasn’t his PhD supervisor, it was actually Olof. But he was working in my lab. [laughter] And although we started off working together, Olof then gave him a project to find out if there were long term after -effects of squirting glutamate by iontophoresis onto cortical neurones, which was a good idea. And the increased firing did persist long after you think the increased level of glutamate would. But it was my apparatus and he, Brian, was using it. And then there was a Chinese young man was sent in to my lab to do an MSc and so I had my lab and my equipment and no opportunity really to work on it. And Olof invited me and Alex, you *were* my PhD student, *weren’t* you?

- AC: I was your PhD student definitely. [laughter]
- LB: Did you come in at the very beginning of the pyramidal tract stuff?
- AC: Yes, I think I did.
- LB: Right. So we worked in Olof's lab instead. [laughter]
- DM: Pushed out by your own students.
- AC: I was doing that separate stuff looking at the magnesium on the cortex, wasn't I, in your lab?
- LB: Oh right, yes.
- AC: So I got a little rat set up in your space but the big experiments, the long ones, were done in Olof's.
- LB: Yes, and the magnesium was involved in that because what we were doing was stimulating pyramidal tract cells in the cortex and recording the volley down the pyramidal tract. And we were also antidromically activating them so we knew we were getting post-synaptic activity although there would have been collaterals and other cells active and we could get long term changes of excitability in pyramidal tract neurones. So this was an advance in that we knew what kind of neuron we were activating and testing. And we stimulated the cortex and made sure there was occlusion between the antidromic and the orthodromic responses to know that we were activating the same fibres. Then Olof had this idea of soaking the cortex in magnesium chloride to block synaptic transmission so we would get the magnesium to wash in and block the synaptic transmission, which we tested by stimulating a paw and measuring the disappearance of the evoked response, and then do our antidromic conditioning and wash the magnesium off again. Of course, we had to do controls, and we found that the long term changes in the pyramidal tract response persisted so we got long term increases in the absence of synaptic transmission.
- And then we did a few contralateral activations which were not antidromic and only synaptic and they gave a long term decrease in pyramidal tract cell excitability. And I remember I was supposed to give a paper on that to a Phys Soc meeting and I hurt my back, an I was in bed and couldn't walk, with a slipped disk. And so Olof had to give it instead of me. So that was 1974, 5, 6, 7.
- AC: Yes, 3, 4, 5, 6.
- LB: We did 110 cats altogether so it was very demanding, it was a lot of operative work exposing...
- AC: They were long experiments because we'd start in the morning and we'd finish about...
- LB: We'd start at eight.
- AC: One the next morning.
- LB: And sometimes three.
- AC: Yes.
- LB: I remember having Olof back to the house to sleep here once.
- AC: Well, I used to come back because you used to give me a lift back and I'd stay here overnight.
- LB: Yes. [laughs]

AC: Well, there weren't any night buses then, were there?

LB: No, no, that's right. And we were really quite exhausted for the rest of the week. I remember Bob Simmons was doing very long experiments with Andrew at the same time and he said how it really ground one down after a while. And I think probably we were doing those experiments for two or three years.

AC: Yes.

LB: To get through the 110 cats. And we wrote it up and it was sent off to a *JPhysiol* editor and nothing happened for months and months! And I remember one day saying to the Distributing Editor "Do you know if anything's happened about our paper?" And he went white and he said he would look into it. And it turned out it was just sitting on JJ's [Julian Jack's] desk and had been for a year! And so when the Distributing Editor got onto him, he edited it very quickly and I think he suggested one or two other control experiments, which were good, and it got published. And that was, in a way, the beginning of my independent research career because I went to a conference in Paris, did you come too?

AC: No.

LB: And Chuck Woody who was an American from Los Angeles was giving a paper on learning and changes in the pyramidal tract cells in awake cats. And he was recording in cortex and I thought, "This is fantastic, this is so complementary to what we're doing." And so I went to chat to him afterwards and he said, "Well, when your paper comes out will you send me a reprint?" And I sent him a pre-print as soon as we got the proofs and he just wrote back and said, "Come over and work with me." So I said to Geoff, "Can we go and work in California?" And he said, "Yes, why not?" so we went to California. To start with in 1980 we went at Easter and I stayed for six weeks and Geoff brought the children home after the Easter holidays, after three weeks, and looked after them for the next three. And then I went again in December and the whole family came. I was three weeks there, and Geoff brought them back after two weeks. And then we went for four months in 1982. Geoffrey went as a visiting professor of law, and Jonathan had a place at Cambridge already, so he worked in a UCLA lab. and Dan was just in the first year of Sixth Form studying A levels in doing French, English and politics, including American politics, and he went to evening classes at UCLA in politics. Miriam was 14y and went to Junior High School in LA. That was when I learned to do intracellular recording in awake cats.

Chuck was medically qualified but he had an interest in psychology and conditioning and he wrote a book. He was very despondent that his book had been turned down by the publishers. And I said, "Ah, I know all about that" because what I've left out completely is writing *Neurophysiology of the Cerebral Cortex* with Olof. "I know all about that, you just find another publisher, it happens all the time." I was quite blasé by then. [laughter] So sure enough he did, and I read his book. It broadened my horizons a lot with the technique and putting it in a context of learning and memory. And he got me involved with a whole American group which then became an international group on learning and conditioning and the neural changes that were involved. Dan Alkon was part of that, he worked on invertebrates and occasionally Eric Kandel came along, and I got invited to conferences all over the place and started giving papers on what I was doing. When I came back to England, I started doing intracellular recording in anaesthetized rats. I was really difficult because of movements and I had to get a good table, anti-vibration rubber bungs [laughs] and heavy metal plates and all that kind of thing.

And Tim Biscoe had by then come to be Head of our department and he said, "Why don't you try recording in isolated brain slices?" And I said, "Well, no, I want to study things happening in the whole animal, slices are just so artificial". But I did think he was probably

right that I ought to have a go. So I went to spend a day with Alex Thomson in her lab at Oxford and she was incredibly hospitable and welcoming and showed me all the tricks of the trade. She had little meshes for putting the slices on and all those little techniques you see when going to another person's lab. And then I came back and started working on slices with a PhD student called Clive Prince. We realised that the input resistance of the cells on the slice was much higher than in the awake rat, and one possibility would be that you cut a lot of the dendrites of the neurones off so it was effectively a smaller neurone; and the other possibility was that you were losing all the on-going transmitter activity of noradrenaline, acetylcholine and so on and that [had] increased the input resistance. And so we wrote a paper which was a comparison of neuronal properties in the anaesthetized rat and in the slice and it occurred to me that if we measured the capacitance of the neurones we could tell whether they were smaller on average. And they weren't, probably because they were in the middle of the slice, and less likely to be damaged.

And so measuring the input resistance in both directions was something I picked up from Chuck Woody and that gave me the idea of measuring the capacitance because of the charging up with the pulse to reach a steady voltage.

And I have a paper with Chuck Woody. One of the things I criticised about his setup was that he had AC amplifiers and I said, "You've got to use DC amplifiers because you're not getting a true wave form." I had a very good friend in the graduate technician, who had a Master's degree there. I started making chlorided silver wires for the intracellular electrodes and we got DC amplification going. And then we found, after a pyramidal tract stimulus in these awake cats, there would be a spike discharge and then there was a hyperpolarising wave and of course there always is after- hyperpolarisation following firing. But when we hyperpolarised the cell with additional current, the negative wave sometimes got bigger. And the input resistance was increased during the hyperpolarization. So the wave was probably due to cutting off a sodium current, transiently. And I found in the literature that there was an example of that in the cerebellum.

When I'd gone, Chuck did some experiments depolarising the cells to a positive membrane potential and he saw a reversal of that wave. Now, whether they were damaged by that time I don't know, but anyway we got that paper published and it was an interesting bit of biophysics. My UCL biophysics grounding was very helpful in understanding what was happening. But I learned a hell of a lot from Chuck so it was really more that way round.

LS: That was part of the point...

LB: ...Of going, absolutely.

LS: Could I take you back a bit? Sorry, if you've finished that bit.

LB: Sure.

LS: We've jumped over, you said earlier on that you were interested in teaching in particular, and you and I collaborated in all various things there so perhaps you could take us through some of those?

LB: Right, well I always did enjoy teaching. I realised in my first year as a PhD student, when I was giving tutorials, that a lot of what I thought I had understood in physiology - when I actually came to teach it - I didn't understand it thoroughly. So I spent quite a lot of time reading *ABC of Acid Base Balance* and other books to really get on top of the subject to teach it, and I enjoyed that. At one stage, when I was a research associate, after the Assistant Lectureship and, there was no lectureship around, Andrew said, "I pay research

assistants and associates less than the equivalent teachers because they have more time to do research and further their career that way.” And I wasn’t at all happy with that because he was telling me how he ran the department and it wasn’t what I wanted to do. I knew I always wanted to teach as well as do research and I wasn’t consulted, I was told. And also I just wasn’t that good at designing research projects at that time. When Olof effectively took over my equipment, with Brian McCabe and the other student, I spent a lot of time writing teaching materials. For example I realised that when we were designing experiments to do for the medical students, there was nothing on endocrinology because the practicals would be too long.

And so I wrote a booklet with questions and answers based on calcium metabolism bringing in para[thyroid]thormone and calcitonin. And at some stage or other we had begun to realise we had to teach the students more about interpretation of experimental data. I think it was when the Second MB course went from being five terms with a revision term for the people who failed, and it suddenly became a six term, two year course. There was no teaching to help the ones who had failed to retake in the September. And Laurence, you and Pamela Holton, I think, set up the self-instruction audio tapes and booklets.

LS: She came a bit later actually but...

LB: Oh, she came later. So it was Laurence’s idea that if we weren’t teaching the students directly we should give them effective distance learning or self-instructional material. Olof had done a, what’s it they call that...

LS: Multiple-choice questionnaire?

LB: Well, it was more the branching type of questions and answers on respiration. He came back from working in America and wrote something on respiration, and then Laurence, which were your first audio tapes?

LS: Well, I went to America on sabbatical and I did, and I used to give the lectures on haemostasis and blood grouping and I thought it would be quite... I’d been to a conference, no I’d been to, doesn’t matter anyway, I decided it would be quite interesting to make these tapes and see how effective it was compared to ordinary lectures, which is what I did.

LB: Very good to actually test the outcome.

LS: I mean, this is your interview rather than mine, but the interesting thing was that the average score went up a wee bit, but not much, but the standard deviation increased enormously. And we investigated that a bit more carefully. Because the booklets and tapes had been released by the library, I’d asked them to keep careful records of who had taken what and so on, we were able to go back and look at the scores and the reason the standard deviation had gone up was a group of very high scoring students and a group of very, very low scoring ones. To our relief the very low scoring ones were the ones who hadn’t bothered to take the tapes, and the ones who scored highly, apparently some of them kept, there was a little film and other things with it too, and highly scoring students did apparently: they were so fascinated by the technique they listened and listened again until they really understood it. Anyway that was my first experience of research into teaching. But anyway after that Lynn and I ... you should take up the story now.

LB: Well, Laurence and I collaborated in producing a booklet and a pack on contraception. And the reason we did this was because at the time, in my spare time outside work and the family, I was doing some evenings for the Marriage Guidance Counsel on youth counselling and education, and I was ‘the sex lady’ because I knew physiology. And I

realised how ignorant we were in this country. We didn't teach kids about contraception, and our medical students didn't know anything about it either, on the whole. And this was a disgraceful state of affairs. We thought we'd better produce a booklet on that. And then once, at the end I did autonomic nervous system with Mary Davies. And then I produced this calcium booklet and we also put it on tape. And when we ... to go back to this business of the students not having any revision, in, I don't remember who introduced it into the exams, whether it was you or Brian, a quarter of the First Year medical exam was interpretation of experimental data. That was the UCL thing, the way we read those papers and discussed them. So we started writing multiple choice questions and I did the whole calcium booklet as a basis for interpretation of experimental data. So there were graphs and the students had to interpret the graphs, meanwhile they were learning about hormones and calcium homeostasis. Have I got a bit out of order on all of that?

- LS: As I recall, two interesting things: we also introduced roughly at that stage, multiple choice questions, and [Doug] Wilkie said, "Why don't you publish it?" And so we produced a book with...
- LB: And that was another quarter of the exam. And there was a calculation - a quarter of the exam.
- LS: Yes, that's right. That's about five quarters already. [laughter]
- LB: Well, three, and only one quarter was an essay. So it was important in the MCQ book that we had practice calculations and practice interpretation of data as MCQs.
- LS: That's true, we did, didn't we? And we also... The multiple choice questions, there were quite a few books around at the time, but none of them gave the answers. And so we gave the answers as well so people could actually learn as well as...
- LB: And we put explanatory comments with the answers when we knew it was difficult.
- LS: Yes, that's right: 'this is true because...' or 'this isn't true because...'.
- AC: Can I take you back to the contraception thing for a minute? Were you aware of the unofficial subtitle of that unit, the contraception one?
- LB: Well, they called me Aunty Lynn then.
- AC: Aunty Lynn and Uncle Laurence show you how to make a condom out of a used squeeze bottle. [laughter]
- LB: How wonderful! Well, part of the reason that it was Aunty Lynn was, I discovered afterwards, that the man who was the technician for recording the tapes at the British Medical Association was actually deaf. And he recorded it all too slowly so it was, "And now children, take out of the pack the condom." [SPOKEN VERY SLOWLY] [laughter] So you know we were absolutely ripe for being lampooned in the student review...
- AC: But actually there was tremendous admiration behind the ribaldry really because we all recognised this sort of great hole in education, [WHISPERS] ...nobody talked about it.
- LS: We even told some jokes and I was taken to task about that because you know there were jokes and also one or two cartoons in it.
- LB: Yes, oh yes.
- LS: And this was a serious subject, you shouldn't joke about it. But I agree with you, the students didn't know about it. One student came up to me and said, "Thank you very much for doing that. I wish I'd known more about it." Because he was a graduate student already and...

- DM: Can you put a date on this so we can anchor it for ourselves?
- AC: Well, it was while I was there so it was between '73 and '77.
- AC: Let's say '74, '75 possibly.
- LB: Yes, right. I mean the MCQ books, I've got them here.
- LS: It went into several editions, didn't it?
- LB: It did. So the very first one was this one [taking it from the bookshelf] and I did one on the nervous system with Pete Ellaway. Then we amalgamated it into the second or the third edition. We amalgamated the two books, so where do they have...
- LS: It's in the back, I think.
- LB: Well, looking at the sales it came out at the end of September '78, so quite a few of the things...
- LS: Is that the first one?
- LB: That's the very first one.
- AC: But you were using material while I was still there.
- LB: Right, yes.
- AC: I think the publication... came later. As I remember, in those days the first impression of multiple choice was, "Well, that's a doddle, you only have to tick one answer." But when we actually got round to looking at the way you constructed the questions, they were testing. You know, it was not a walkover, it was a jolly good exam.
- LB: And it wasn't just fact either.
- LS: That's what we tried to do because the early ones were just facts and we wanted to get people thinking.
- LB: And I think having got them in an exam, we built up quite a big bank before we published anything because we realised it was good to have answers And that was the motive for publishing really. So I can quite see they were in use for several years before we brought it out in '78.
- LS: The other thing you did, or we did rather, was to make a film.
- LB: Oh, yes I do remember that. Well, we were measuring lung surfactant, wasn't it. What happened was we were doing this really wonderful experiment on a dead rat where you inflate the lungs with air and measure the pressure, collapsing pressure, for a given volume of inflation and plot the volume against pressure. And it was a hysteresis, different on deflation. And then of course when you did with water it was up and down the same pathway because you got rid of the surface tension effects. And then we talked about surfactant and how, in fact I remember lecturing to the students, I blew a soap bubble down a tube and out the side and then another smaller bubble down the other side, and then opened a tap between the two and the little bubble collapsed into the big one. And that was very effective in showing how little bubbles would collapse into big ones, and why didn't that happen in the lungs because the alveoli were all different sizes? Well, okay, the lung surfactant would stabilise this. And so we got the students to wash out surfactant from the lungs and floated it on a trough and got them to measure surface tension of saline and surface tension of it with surfactant, but it was so difficult to do in the classroom because there were people walking around and wooden floors and wooden

benches and there was too much wobble. And either Laurence or Brian said, “Why not make a film of it?”

So I thought, “Yeah, that’s a good idea, we’ll put it on film.” And Laurence had the even better idea of going into the hospital and filming babies in incubators...

LS: I’d forgotten about that.

LB: ... who had been born prematurely so they didn’t have the mature form of lung surfactant, so they were really struggling to breathe. And you saw on this film the way in which, when they were inspiring, the sternum was sucked in to be concave. A really effective bit of teaching that.

LS: The other thing...

LB: And we put it together.

LS: You did it time lapse, it was time lapse, it was a Langmuir trough, so it was time lapse so the whole thing happened quite quickly. Because the other problem in the student lab, it took forever. And by the time you’d finished somebody had walked past and...

LB: Yes, because the altered the surface area and it compressed and expanded. I’d forgotten that bit, to actually see the genuine hysteresis from the surfactant.

[INSERT *Later on, I made another teaching film about balance and testing the function of the labyrinths with the late Barbara Banks [Barbara E C Banks 1934-2014], because her sense of balance was wrecked by gentamycin antibiotic treatment. We compared her nystagmus and balance with mine, which was normal then!*]

DM: Just as an aside, did you interact or have any contact with Alec Bangham at all?

LB: Never heard of him.

DM: Oh, really? The inventor of liposomes. And one of the things that [technology] was used for, of course, was putting surfactant into the lungs of premature kids.

LB: Oh yes. I knew about the New Zealand doctor who gave steroids before term if they thought it was going to be premature and that helped the surfactant develop. ... And we had Leonard Strang [*Leonard Birnie Strang 1925-1997*] at UCH, didn’t we? So, there was a very good premature baby unit there. And Daffyd Walters went to work with Leonard Strang, didn’t he?

LS: Yes. He was one of the PhD, sorry, intercalated BSc students. And I think you went to India at this time.

LB: Ah, okay, well before India we really ought to mention *The Neurophysiology of the Cerebral Cortex* because you know I said I wasn’t much good at research and writing up research papers and I didn’t mature until the 80s, well so the whole of the 70s and early 80s I was doing the teaching and the teaching programmes and the MCQs and working with Olof and Alex on the pyramidal tract stuff. But also Olof and I wrote this big, big textbook *The Neurophysiology of the Cerebral Cortex* which was published in 1981. We wrote some chapters together and some chapters separately so the chapter on the cerebrospinal fluid I did myself, as I was fascinated by Davson’s book and realised that topic should go into a book on cerebral cortex. And Olof wrote his chapter on the relation between slow potentials and firing including his work on the origins of the alpha rhythm. That was such an interesting and controversial bit of work. I was involved in that too, indirectly, and briefly, that is because when Olof tried to measure my alpha rhythm - I hadn’t got any. And I also had perfect eyesight. And he then got me to wear his specs, probably +2 or +3 dioptries all day and when I really had a headache and eyestrain I had

good alpha rhythm. And so he looked at people's visual acuity and alpha rhythm and you only had alpha rhythm if your eyesight wasn't perfect.

In brief, he hypothesised that modulation of the corneo-retinal potential by eyeball tremor was the likely cause of alpha rhythm, and measured correlations between the two when experimentally altered., and also many other experiments. He wrote a book called *The Origin of the Alpha Rhythm* [1973] in which everything was explained really well and I think it was a brilliant hypothesis and really well backed up. And Andrew Huxley was terrifically supportive of him.

So life was just very busy with writing books and teaching materials and doing the actual teaching and raising the three kids and working part time.

DM: Can I just ask a little bit about the book publishing because that's something that's collapsed almost entirely. There are very few people now who can, one way or another, devote the time, energy and enthusiasm to write. Monographs, and so on—in the forms we're used to them—don't exist. And large comprehensive textbooks are now largely written in a very different way, I think.

LB: Yes.

DM: But your books, were they commissioned as a result of the publishing you'd already done or did you write them on spec?

LB: Well, Olof had done the respiration book with Freemans [publishers] in the States and we wrote an outline and a chapter of the book on the cerebral cortex and Freemans agreed to publish it. But we took so long doing it, because actually it's a very big book. There's a lot of reading and research in it, and we were doing other things at the time. Freemans got fed up and said, "Sorry, we're no longer prepared to publish it." And so we went to Edward Arnold and they took it over and brought it out. And you're quite right, if we had been under pressure to publish research papers for an RAE, there would be no way I could have spent these years developing the teaching and developing the book. And that book was half and half, Olof and me. That was a really good collaboration.

DM: And I think it's fair to note that there was a huge blossoming in this area of interaction, teaching, addressing this kind of material for students, the MCQ approach and the underpinning you had with tapes, and so on. This became much more widespread, yes.

LB: And the Open University was using this, do you remember I mentioned the Feiser and Feiser book, which was American on organic chemistry and the test questions were part of each chapter. So all the Open University texts had got test yourself questions and they were quite interactive in many ways, and that was all part of the same movement.

DM: Certainly in Glasgow we picked it up partly through proselytising by, well Sheila Jennett, I would think and Oliver Holmes [1933-2004]. Between the two of them, they were very instrumental in bringing it strongly forward in Glasgow and we had interesting internal battles. I was certainly one of the people who was completely converted as a result of just the sort of intellectual discussions about the nature of the MCQ approach. It was very important to have examples like this. It was quite a transformation over that period of time. I think you should take good credit for that.

LB: I think the credit really has to be for Laurence. And to what extent was Brian Jewell involved in the interpretation of data exam business and the calculations?

LS: I can't remember now.

LB: Because he was very involved in undergraduate teaching. He was a good, clear teacher.

- LS: He was, yeah. Was there something...
- LB: So what I brought to this was enthusiasm for doing my bit [laughter].
- LS: And you did your bit very well actually, I thought. Something you said, I can't remember. It's funny how these discussions, suddenly you have a fleeting thought that disappears again.
- LB: What was the India bit to do with? I mean I did go to India. Was it about the lectures?
- LS: No idea. It was just said what about the...
- LB: Ah yes. That's, it's very funny because Geoffrey was invited to give a talk at the Fourth Commonwealth Law Conference, which was in Delhi and he said, "Would you like to come with me?" And you know we had to think about how we were going to have the children looked after, but parents stepped in plus a nanny. And I said, "I can't possibly go because I'm giving the respiration lectures." And he said, "Well, can't you ask to have them changed?" And I said, "No, I couldn't." And he said, "Well, just ask." So I went to Brian Jewell and said, "I know this is really impossible but Geoffrey asked me to ask. He's going to India and he said would I go with him, but I'm giving the respiration lectures at that time." And he said, "I think we could reorganise that." And he reorganised the timetable. And as he pointed out to me some time later, he was very aware of the fact that I was a research associate and I wasn't being paid that extra bit for teaching and yet I was very happy to do the respiration lectures when asked. And I'd actually taught respiration in lectures in Regent Street Polytechnic in the evenings. So he knew I had some experience of doing it.
- And so Brian reorganised it and I was able to go to India. And I went to Paintal's lab [*Autar Singh Paintal, 1925-2004*] while I was there and met his daughter, and they entertained me. So I learned all about the J receptors in the lung and about how, when you went up to high altitude and you got lung oedema, the J receptors were activated. And that actually inhibited motor neurons so that you didn't do more exercise and end up by asphyxiating yourself, which I thought was a lovely story. Do you remember that? That bit of respiration and Paintal?
- LS: Oh yes, well actually, by then I was at Charing Cross, I think. And I think Abe [*Abraham 'Abe' Guz, 1930-2014*] was working on those as well.
- LB: Right, right.
- LS: Yes. At this point I left UCL ... [laughs]
- AC: I'd like to go slightly sideways at this point and say sort of through all this time, how much did your children know about what you were doing? Were they interested, aware?
- LB: Well, when they were old enough they certainly did. Jonathan remembers coming into the lab with me, and a bird's nest of coloured wires, and he came down to the animal house with me. In those days it wasn't all screened off and they weren't so worried about infection, although they should have been. And we would go down, and in fact Charlie Evans let us take home two baby rats [laughter]. We borrowed a rat cage and so the three kids had a pair of white albino rats for a while. One day I came home and "Mummy! Mummy! The rats have had babies!" [laughter] So this was wonderful. So we went in and it was fascinating because the mother kept sticking up her nose to the top of the cage and I said, "I think she needs some nesting material" because it was only in sawdust, so we took the father out because we knew he might have eaten the babies, put him somewhere else. And we fed tissues through the bars of the cage, and the mother rat would come and grab the tissues and then rush back to all the litter, pile and stamp them

round and round and turn the tissues into a nest for all her babies, so that was all fascinating.

Yes, so the kids knew a bit and they certainly all came to the lab when they were much older. I remember Dan, who is two years younger than Jonathan, coming with me to a BBC programme on vivisection and anti-vivisection and he was quite gung-ho about us doing experiments because we needed to learn more. Jonathan started out reading Natural Sciences, and in his second or third year studied Physiology and Pathology, a course recommended by David Eisner. He liked it, and did some research, and decided to transfer to preclinical Medicine at Cambridge, then on to Oxford for the Clinical years.

Miriam, it's interesting that she wasn't that interested in science but she studied English, German and Biology when she started her A level courses. Then she came home one day and said, "I'm finding all this botany really boring and I miss the maths." So she did maths instead and I said to her, "That's good. I think what you really need is to have some sort of biology as a background. How about doing human anatomy and physiology?" And she thought that sounded quite interesting, and she did anatomy and physiology O level at the end of the First Year Sixth.

And that was important for her because at the end of the Second Year Sixth, she'd already been offered a conditional place to read English at Manchester when her school, Parliament Hill Comprehensive, did an open fortnight on psychology in the UCL department. She came back and said, "I like psychology, I think I'd like to read that instead of English." So she rang up Manchester and they said, "No, sorry, it's very competitive getting into psychology and our Arts places are full." So she came to see me at college, distressed at the rejection, and we had a coffee together. I said, "I'll tell you what: Xerox a copy of your UCCA form with all your grade As at O level, and write a covering letter and say why you want to do the psychology and the fact that you're studying Maths A level. And by return she got a phone call offering her a place on the psychology degree.

AC: At Manchester?

LB: At Manchester, yes. So she read psychology and then did a PhD in psychology on language and meta-language at the Institute of Education. Her older brother was by that time a doctor and maybe already training as a psychiatrist. And she said, "I don't think I want to do research for the rest of my life, I'd like to be a clinical psychologist." And he said to her, "You're not going to be taken seriously by medical doctors unless you've got a doctorate." So she finished her PhD and then, when she did the clinical psychology, in fact they lengthened the course with a research project leading to a doctorate, so she's got two doctorates.

And Dan turned towards law and became a legal journalist. He read politics for a BA, then an MPhil, and finally another Masters in Journalism. He very much shared Geoffrey's interests. So each of us has handed some of our interests on, I think.

AC: Yes. One of the things that sort of interested me, because you've had a long career teaching students, have students changed?

LB: At one stage I felt there was a rather worrying loss of honesty. They were copying practicals and plagiarising and people said to me, "They always used to do it, you just weren't aware of it." So I was a little bit worried about people being prepared to copy other people's work and that sort of thing, but otherwise I wasn't aware of it. I thought the students we had at UCL were fantastic. The medical students were all interviewed and they were selected for having done some sort of public service, something to get them

aware of the kinds of things they might face in medicine. The science students could be pretty indifferent sometimes and other times they were really exciting.

AC: I remember you discussing anonymously a gay student...

LB: I remember a BSc student coming to see me and he was very worried because he was being blackmailed by his lover. And he said could he come and see me at home? And he came to us on the weekend and I said to the kids, "Don't interrupt us." And after he had told me his problem, I said, "Look, I think it's essential you tell you parents and remove that threat of blackmail." And he didn't think he could at first and I said, "Well, do you think your parents would reject you completely?" And he said, "Well, no, they've got gay friends." And I said, "just try them. I think you'll be pleasantly surprised." And he did. And brought his parents to meet me when he graduated, which was rather lovely. It was very interesting doing the pastoral counselling as well as teaching. And the fact that I'd worked with the Marriage Guidance Council helped because I'd been trained to do the education counselling and I knew quite a bit about listening, and letting the students talk, and reach their own decisions. So that was quite handy.

DM: Doing that pastoral work, was that part of your official duties by then? Had you got to the point where you had...?

LB: There was a year when I was Assistant Tutor to Norman Saunders for medical students but then I stopped that because I became Tutor in Physiology to the first year medics instead. But then with the science students we had pastoral tutees as well. That was quite a late development, I think. I did lots of examining as well and went to many Universities as External Examiner and I also became part of the Physiology team for the Quality Assurance Agency, as the representative of the Physiological Society.

The really awful thing that happened before that was discovering fraud in my lab, or more precisely in legal terms, scientific misconduct. And that was by a student who had done a PhD with me and then stayed on to study medicine. He transferred over to medicine. His name is known and it's published but we can just call him GX here. But while studying Medicine he carried on doing experiments in the lab. with my then PhD student, but not with my postdoc. And when Mike Barry, my PhD student, was to write up the work I said, "Look, it's quite difficult when you've done joint work, you must only include the experiments you personally did, or that you did together, but not any that, GX, did on his own. And you must make sure you've analysed them yourself" because obviously they could have been analysed wrongly. And he came to me one day and said, "You know when we increase the stimulus width during the conditioning to activate the cells much more, I found one result where the stimulus hasn't been turned down again. You can see that the artefact is wider afterwards."

I said, "Okay, well you obviously have to dump that experiment, it's not valid, and make sure you're very careful to see what's going on in the others." And he began to follow through and find out that in the periods when we were measuring the input resistance from the intracellular recordings, sometimes the spreadsheet with the results seemed to have results which couldn't have been obtained at that time because the resistance measurements were being done. And they purported to be measuring EPSPs instead. And he found four experiments like that. He talked to Richard Vickery as I was out lecturing on neurophysiology at the Middlesex to doctors studying neurology, and then, I tried to ring them both at home and they were both out that evening. Apparently they were so miserable they'd gone out. And in the morning we all got together and they said, "Look, we think there's fraud here." And it quite clearly was because when you went to the very detailed lab notes, and when we noted the tape number and wrote down when we were

putting in the current pulses to measure the input resistance, then at exactly that time on the spreadsheets of the analysed results, there were measurements that were supposed to be of EPSPs. not the input resistance!

And what GX had done was to prolong some after effects. And so this led to an absolutely appalling situation. I tried to get hold of Mike Spyer, my Head of Department, that day. He was out for the day, so I arranged with his secretary to see him in the morning. I tried to ring Bob Lieberman, who was Dean, and he wasn't there either. And I thought of going to Don Jenkinson in Pharmacology as a very decent, straight guy. And Richard Vickery, my postdoc, said, "Look, Lynn, I think it might be more sensible to keep it within the department." And so, we went to Gertrude Falk [1925-2008] instead. We phoned GX and he came up to the lab right away, and Gertrude had a chat with him. She said afterwards she [had] said to him, "Whatever you do, don't go blaming other people." And in the morning I was going to see Mike Spyer and Richard said, "Lynn, can Mike and I come with you?"

And I said, "Well, you don't have to, it's my responsibility, but if you want to of course I'd be happy to have you with me." And we went to see Mike and he was very good about it. He knew how to handle it because he had known something similar through the Phys Soc or through his editing work. And he said, "Right, everything in the lab must be sequestered right away." And he said, "Would you please write a report of exactly what has happened so far." So it was the most terrible thing to have all your lab notebooks and all your lab results and tapes and everything taken away to the Registrar. So, you were left with no experiments and not knowing what was real anymore. I mean, what you thought you were finding out, this truth, what was real and what wasn't? The college were really terrific. There was a professor of IT and he Xeroxed every single lab notebook page and gave that back to us, and made copies of every computer disk, actually it wasn't tapes anymore. It had been tapes and then it was disks, so that we could carry on doing our own analysis. We had found, I think, 12 falsified results. And Mike Spyer went to the Provost and he said, "We should set up an external panel of enquiry."

And what was so fascinating was they in UCL were acting properly and very openly but there were people in the Phys Soc and one or two people in the department who said, "It takes far too long to do this kind of thing. It sews up people's time and it's really not worth it. Just sweep it under the carpet." And I said, "Well, you can't do that because it leaves the three of us in the lab, as well as GX, under suspicion. We know it's him, but other people won't. And there's a postdoc and a PhD student and me, and they've got their whole careers ahead of them."

"And you've got to look into it properly." And Mike Spyer and the Provost were absolutely keen to do that. Mike Spyer delegated Peter Mobbs to do it and he was so shocked at the idea that GX would go off and become a doctor that he was really assiduous about digging into it. And the three external panel people were Tom Sears and David Armstrong from Bristol, and the late Alan Brown [1939-2006] who was at Edinburgh.

Geoffrey said, "You all need to have legal advice." GX was at Oxford so he went to the Oxford authorities. Geoffrey asked one of his partners to look after us but Richard Vickery said no, he was quite happy to use the Student Union and it was Helen Donaghue who looked after him. And Jackie Dyson, Dean of Students, happens to be a lawyer, she looked after Mike Barry. And I said, Robin Lewis will look after you all, but they thought it was better for them to be independently represented. And so we kept copies of everything we submitted to the Registrar and all the investigations. Along the way, the panel said, "We agree that there has been falsification of results but we're not prepared to say who has done it."

This was the worst of all possible worlds and my lawyer, Robin Lewis, was ill at that time, so I talked to Geoff about it and Geoff said, “This absolutely cannot be allowed to happen.”

We got up at 5.30 in the morning and I took him through the whole story again and which the really crucial experiments were and where things had been falsified and so on, and how each of us had written our lab notes with experiments we were responsible for, so you could identify who had done each experiment from the handwriting. And he wrote a letter to the college and the panel and said that it was completely unacceptable to say they couldn't say who was responsible for the falsification. They had been appointed to carry out this investigation and they had to keep working at it until they could say who was responsible otherwise there would be consequences. Geoffrey said you always had to point out the consequences, which is very much a lawyer's approach, which greatly upset my colleagues on the panel, I have to say, when they got that letter. That was a pity because they were doing a time-consuming and pretty thankless task as a service to the scientific community.

So they carried on working and sure enough we were able to show that fortunately, GX had only falsified data in experiments where he was clearly the one doing them, with his own handwriting in the lab. notebooks, and he hadn't interfered with ours. And there was one experiment that he had been involved in that wasn't falsified. When we went into the details of the analysis of it with Peter Mobbs, we found out I had measured it up on the calibrated oscilloscope face and all the paper sheets of measurements were in my handwriting – and it would have been too dangerous for GX to fiddle the data in the computer spreadsheet.

Part of the problem was that I wasn't able to run the computer analysis programme, I hadn't actually come to grips with it. And I think that allowed him more freedom to fiddle with it, so in that sense I was culpable. And also I was responsible in that I had looked at the graphs of the experiments but I hadn't gone back to check the original EPSPs except where we wanted pictures for publication, and I hadn't come across a falsified one.

And there were two abstracts that UCL (as they held the copyright) retracted in *JPhysiol*. Proceedings and the Society for Neuroscience Abstracts. So it took at least a year of the analysis once the panel started. And there was a year after that, before they were able to come to their conclusion. And meanwhile GX was doing clinical studies. The Oxford authorities were told. But once the results of the enquiry were known and the panel had drawn their conclusions, the legal appeals by GX went to the High Court and the Court of Appeal who upheld UCL's procedures, and hence the panels findings. UCL could then publish our retractions. And we had problems at first getting it published in *JPhysiol* because the Physiological Society had members of the committee who were nervous that they would be sued by GX. And Geoffrey said, “He will not sue. He will not pay the money that you have to pay to bring a lawsuit. And anyway it's not in his best interest.” And Mike Spyer was not happy with the way the Committee behaved. Also Mike thought that they should have taken out insurance, to cover them for that sort of thing so that they wouldn't need to be worried about being sued.

And I have to say there were one or two members of the Phys Soc Committee who wrote to me individually and did not hold that view, and were totally in favour of making it public and publishing it. It was really important so that when GX was involved in any future career we would be able to say, “The results of the enquiry are this and they have been published.” And indeed the late Murdoch Ritchie [*Joseph Murdoch Ritchie, 1925-2008*] rang me from the States, because I knew him and Brenda and had visited them in Yale, and he said, “Lynn, I have a friend who wants a chap called GX who worked with you

to come and work in his lab. Would you recommend him?" And I said, "Well, he'd certainly get results, Murdoch, with him but they won't necessarily be true." And I said, "You know I've been moaning incessantly about this scientific misconduct in my lab? Well, this is the chap." And Mike Spyer went to see Bridget Ogilvie, who was Chief executive of the Wellcome Trust at the time, and she rang me up and said, "Lynn, how absolutely dreadful for you. I can't give you more money for animals but will you write out what you think your expenses might be for all other aspects of your research so that you can regain your position?"

And the Provost, Derek Roberts said to Mike Barry, "Your PhD has been disrupted, I will fund you for a further year so you can complete your PhD."

And we [Mike Spyer and I] were able to write letters to Richard Vickery's Australian bosses when he went back to say he wasn't actually involved, he was doing independent experiments. We published papers together on AMPA receptors and something on LTP and metabotropic glutamate receptors and on LTD following transient perfusion with low calcium solutions. Richard got three papers out of his almost three years with me.

So that was okay, and he got his lectureship and thrived. Mike Barry got a postdoc position in the USA but since then he's worked at commercial pharmaceutical firms and so on.

DM: But our man GX... qualified?

LB: Yes.

LS: Really?

LB: Oxford said he had to take a year's break and so they delayed it. Now my lawyer, Robin Lewis, had said to me "they will not prevent him earning his living as a doctor as a result of this. You will find he will qualify" which was really surprising and Peter Mobbs was very upset because he'd wanted him not to qualify. And they knew about it at Oxford and we also went into his PhD thesis because I had got some graphs that were different from something he published in his thesis. And I knew that I'd measured that bit and it was correct. And so his thesis was looked into. A tutor came down from Oxford to be with GX: in the end they decided not to take his PhD away, but Oxford knew about it. It was a London PhD. He qualified in Medicine and he had a post qualification job at the Hammersmith. Mike Spyer met me in the bus one day and said, "I must tell you, Lynn, what's happened about GX. I was at a meeting and an eminent clinical scientist and Head of a Department at the Hammersmith [the Royal Postgraduate Medical School] came to see me and said, 'I've got one of your students and I must say I'm really surprised he did so well, judging by his clinical performance here.'" And Mike Spyer said, "Is his name GX?" And the Hammersmith Head of Department said, "Yes." And Mike said, "Right, at the end of this meeting you and I must talk." And the clinician was absolutely furious that the GMC, who had been notified by UCL and had it on record, had not passed this information on. And the Head of Department at Hammersmith said, "Whatever his future jobs, I am passing on this information with them." From time to time, I look him up on Google and I can't find anything for him.

DM: Okay, we'll take that as a good sign.

LB: Yeah. So I can't tell you...

DM: So this wasn't in any sense an isolated incident under the pressure of having to achieve research and so on? It was a character trait.

- LB: They did say to me at the panel interview, “Why do you think he did it?” And I said, “I didn’t know but I think it’s just ambition.” Because at one stage he wanted to leave my lab and go and work with David Atwell and Peter Mobbs but he hadn’t got a very good degree, and there was no way they’d take him. He’d only come to us because we had a PhD scholarship left over and he had done a bit of research and had some references from there. And you know he was bright enough, he didn’t need to do it.
- DM: No. There was there a flurry at that sort of time of similar incidents, but you don’t hear so much about it now, scientific misconduct.
- LB: Well, Alex, wasn’t it you who discovered scientific misconduct in your first postdoc job?
- AC: Well, yes, it was Mike Purves, wasn’t it?
- LB: Yes. And Alex was the person who blew the whistle on Mike Purves.
- AC: I was the whistle-blower there.
- LB: And she wrote to me and I took this, with Alex’s permission, to Andrew Huxley.
- AC: I mean that was on a...
- DM: An industrial scale.
- AC: An industrial scale. A shame because he was a very inventive thinker, but a shade too inventive [laughter].
- LS: Slightly unfortunate term.
- AC: Yes. But...
- LB: Remember this is being recorded.
- AC: The difficulty is, as you found out, it’s very difficult for the people around an incident like this to avoid the fall out.
- LB: Yes. And then you went to France and you found that some of the results were being altered in France.
- AC: The whole lab was at it. It was awful. I couldn’t believe it. It was there like a kaleidoscope of déjà vu. I was completely horrified.
- LB: Were you quite pleased to get out of scientific research? Or was it really more a question of not being able to cope with the boys and work?
- AC: Well, I went to Cambridge from there and that was okay except that I became aware that, you know, two bad labs in a row, well, you know, perhaps there’s no smoke without fire? I just wasn’t being taken totally seriously and I got offered a job in medical computing and I took it.
- LB: And with your engineering background...
- AC: Yes. I was fed up of being trapped on the soft money and always having to move every three years and you know you lose your friends, your location, I’d sort of had enough. So Lynn, there’s one bit we haven’t gone into because you went to Auckland as well, didn’t you?
- LB: Oh yes, that was all part of the really enjoyable intracellular investigation of long-term changes in the cortex. Olof had worked out in Auckland and he’d met a woman called Sue Pockett and he said, “Why don’t you invite Sue to come over?” And so Sue came over and joined my lab for eight weeks, I think, something like that, to work with Kerry Murphy and myself. And it was a paper that we published in *Journal of Neurophysiology*. We made

intracellular recordings in cortical neurons in the anaesthetised rat and obtained associative LTP, induced postsynaptically. It was a paper on long-term increases, and long-term decreases, in synaptic transmission. And then Sue said, “You’d better come to Auckland and work with me.” And so I asked the Royal Society for some money, I think, for the travelling. I went out to Auckland for eight weeks and we did a study in hippocampus. So I learnt to do hippocampal slices with Sue, and we did a study on long-term depression after antidromic firing, applied the magnesium *etcetera*, and did we do anything else together?

That was two papers. I think that was it. So that was really enjoyable. She’s a very bright woman, on soft money permanently but she didn’t do any teaching.

DM: Tough.

LB: Yeah.

[[INSERT. Something else I haven’t talked about here was the work with the late Alison Brading [1939-2011] and Tilli Tansey on the book Women Physiologists, which was published in 1993. What I would like to add is what a wonderful person Alison was. I am sad she is no longer with us and able to share in the centenary celebrations of women first being elected to The Society. We got to know one another on the Committee of The Physiological Society, where we were the only two women on the Committee of 1987 – 1991. Alison had had polio as a teenager, and was somewhat handicapped by weak motoneurons, but drove a car, and got around with a stick to help her walk at that time. We became good friends and stayed at one another’s homes, and shared a love of reading so we always swapped books.]]

Alison was crucial for the success of the *Women Physiologists* book in many ways. The first was to recognise that we should ask Tilli Tansey to become a co-editor and author with us because we were pretty ignorant of history and its contextual approach – I had to ask Tilli what that meant! That invitation was essential for the book’s final form. I don’t remember which of the three of us thought about adding an extract of the scientific writing in a key paper at the end of each chapter, but we thought that important. Alison was an excellent scientist and wrote with great facility, so her editing and biographical contributions were very valuable. It is sad that she died at the start of 2011, in her seventies.

AC: How long is it since you retired? It’s an awful long time, isn’t it?

LB: Thirteen years, I think: in 2002. My mother had a multi infarct dementia, becoming obvious in her mid -eighties, and she kept going very bravely and died at 91. But I was getting exhausted, coping with carers and the money and the anxiety and stress, and getting old [laughs] and so I actually took early retirement at almost 64. I retired on March 31st, and my mother died on May 1st. But I was really pleased to be able to be with her while she was dying and [sighs] then I did all sorts of interesting things in retirement.

I became a magistrate sitting in adult and then also youth courts. After five years of that, I became a trustee on the Prisoners Education Trust combining the experience of crime with my education interests. And John Foreman, who knew all about GX, asked me after I retired to come onto the College Committee for Ethics for Human Research. I did that for eight years with John Birch as Chair, who was a UCL Council member and an ex ambassador from the Foreign Office. A very fine man, very humane, very tactful. And I resigned from that. We were supposed to only do six years and he said would I do another three with him so that we didn’t have too much of a change of personnel. But after eight

years I was finding it quite hard work going through all the papers and there were some very stressful ones to do with medicine and also with psychology and autism.

And at that stage we'd discovered our eldest grandson was slightly autistic and his mother, my daughter, is a clinical psychologist, and also a specialist in autism. And I knew from what she had been teaching me and my understanding of my grandson, was that what some of these educational psychologists were planning to do in schools was completely unethical because they weren't following up. They were making the child, in trying to teach the other children about autism, vulnerable to bullying. And so we got some expert advice in and the applicant was advised on how to do a different sort of project. And there were two or three projects like that, which were quite stressful and I thought, "I think I've done this long enough and it's time to move on." So I did.

DM: But you maintained good contact at the College for a long time?

LB: Oh yes. And I'm still on the Animal Welfare and Ethics Committee at College, and I still am an Honorary Reader at UCL. I did carry on doing research with new PhD students and other students after the GX affair had blown over, which was good.

So, as I said, College treated me very well. But it was a horrible period and for the first time I discovered what the word 'honour' really meant because I felt my honour was being impugned, and I minded very much.

AC: It is a horrible feeling, isn't it?

LB: Yes. And I had much more support than you did. I mean though no one ever thought you were involved in it, did they?

AC: No, I was never directly accused but the whole thing just fell apart.

LB: Yes.

AC: Nobody was... and the sad thing was, I mean, we got some... we used to get on and work without him, actually. We got some terrifically valid results but nobody would publish them.

LB: No.

LS: Really? Tarred with the same brush.

AC: Yeah, because he was the head of the lab at the time and I mean... I haven't heard in so much detail before, quite how long and how much effort it took to go through all your records because it would have taken similarly that amount of time to go through it and show that, you know, what we got we really had.

LB: Months, yes.

AC: And the rest of us were all on soft money, we were all dispersing. You can't do that if you can't stay in the place.

LB: No.

LS: The good old lucky thing I suppose is perhaps that you had all been working on this. If he'd been working entirely by himself it would have been very difficult to sort anything out, wouldn't it? Because he could easily falsify the whole lot.

AC: Well, thank goodness for handwriting because you can't fake it, can you?

DM: That's just what I was thinking. Now there's almost no handwriting at all.

- LB: Yes, yes. Well, that's what Stephen Bolsover said. Stephen and I did some calcium imaging experiments together and he said, "I never doubted you because I knew what meticulous notes you took and if you had wanted to falsify results you wouldn't have written everything down like that," which was very nice. And you learn who your friends are. Tim Bliss was lovely. I mean I told him about it because he had to know, you know, that those experiments were not to be relied on. And he emailed me and said, "Lynn, no one who knows you will doubt your integrity," which wasn't the case with everyone. You know some people say, quite reasonably, we have to wait and see what the result of the enquiry is. So...
- LS: Well, yes and no. I mean I think anybody that knew you would share that view.
- AC: But you're dealing with a bunch of scientists and we're nothing if not striving to be objective [laughter]
- LB: Indeed, and what is the evidence? Quite right.
- AC: And the objective position is: we don't know yet.
- LB: Yes, yes.
- LS: I had a horrible experience with one of my PhD students at one point. It was after yours. You shared some of your problems with, well not your problems, some of the problems with me and I looked at my student's results because they conflicted with some of the others that I had within the lab anyway. I thought, "Oh God, is X cheating?" On further examination it became clear that this wasn't the case, but I became much more aware of this potential issue after that.
- LB: The other thing was, it is so wrong if you do sweep it under the carpet.
- LS: Oh yes.
- LB: And you don't know and it's not publicised because he could have gone to other labs and wrecked other people's lives and you know, another three years and lots of other colleagues. And...
- LS: He got away with it once, he'll get away with it again.
- LB: Yes, absolutely.
- AC: Michael Purves already had been swept under the carpet once.
- LS: Oh really?
- LB: Really?
- AC: It had happened before and they decided to turn a blind eye.
- DM: Terrible.
- LB: It just goes to show.
- AC: And I think it's probably, you know, if a person is minded to do that kind of thing they will do it at any point.
- DM: It's quite fascinating, isn't it, we've got 'examples' in great science now. There's the one about Gregor Mendel's statistics that have been gone over ... but you probably know that story? That seems to have been the gardener who was aware of what his boss was looking for. So it has got a benign explanation. But things came out a little bit—nowadays you can analyse it statistically—that are too good to be true. And then there are others

like Millikan, the measurement of the charge on the electron, and the oil-drop experiment, that's another one.

LS: Oh really?

DM: And that has, I think, an error measurement in it which shows that it cannot possibly [have been as reported]. There are various ways it could [have come out]. It should always have been a slight underestimate, or overestimate, for various internal complex reasons. I think that's the story [DM adds: *The value for the viscosity of air he used in calculation had been incorrect by Millikan*]. I'm garbling. But anyway it turns out that just simply, clearly, a 'selected' set have come through here, and so it's terrifying.

AC: It's very, very hard. The degree of objectivity and standing back and thinking, you know, 'Am I really measuring what I think I'm measuring?' It takes effort, doesn't it, to do it right.

LS: Yes, there's that getting rid of outliers.

DM: That's the point. I mean on the one hand you have your very professional hat on and say, 'Look, even though that's quite interesting I'm going to have to bin that result because it's clearly rubbish'. And then what you've ended up doing is inadvertently skewing everything else because you have got the 'one-in-a-hundred', and it has popped up, and you've only done the thing five times and you think...

AC: I was horrified to disc...

LS: There are statistical ways of doing that, though, aren't there?

DM: Yes.

LS: If there's a valid, objective way of doing it without, "I don't like that one" then you could do it. You need to report it anyway actually.

AC: I was horrified to discover that my sons were being taught at O level to scrap the outliers. I thought it was totally unacceptable.

DM: Well, like we were saying, it presumes one knows what the outliers are. You have to know what the distribution is to realise you've even got outliers.

AC: If somebody was stamping down the corridor at the time and things went off, if you know why you can scrap it, that's fine but you don't scrap it because it happens to be a bit different from the rest. [laughter]

LS: Actually, it's quite interesting. We mentioned Babkin and Schachter earlier on. I remember Schachter once telling me, Babkin told him and he told me, "Treasure your exceptions" because they're often the...

AC: Wasn't there quite a famous example of that to do with the early work in sheep? This is so woolly in my memory but...

LS: Sheep, woolly, good! [laughter]

AC: But everybody has scrapped this one outlier and it turned out to be a very interesting result. And you can see it happening.

LB: Yes.

AC: It's a bit of a sort of sad way to wind this up. I think it would be nice to just go back to the social side of University College London because it was such a fabulous department.

LB: What we haven't talked about are the Christmas parties.

AC: The Christmas parties and Shenley and the strawberries and cream and the...

LB: Oh yes. There was all the time in the world as the pressures to publish just weren't there. And so you could go out for a picnic day to write up your results, and in fact Olof often took me to write results down to an island [*Wheatley's Ait, or Eyot, in the middle of the River Thames above Sunbury Lock*] and we were chaperoned by his parents and his mum would make up cucumber sandwiches and his dad would go swimming in the Thames at the age of 90, which was very impressive. And then Olof said, "The people next to our hut are leaving, they're retiring. Would you and Geoffrey and the kids like to come down?" And so we borrowed some money from my parents and bought this collapsing wooden hut and my dad said, "This needs pulling down and rebuilding." [laughs] But we kept it going for another 10 years before we got a new, wooden chalet and so Olof taught my kids how to repair motor mowers and things like that, the boys, which was really good. And we became two families...

LS: Not the girl?

AC: Yes, why wasn't Miriam taught about the mower? [laughter]

LB: She wasn't interested in the mower but Dan taught her how to mend her car. So she wasn't left out. And then we had the staff student tennis fun at Shenley every summer, and there was cricket. Well, there were technical staff and academic staff, with the postgrads.. Not undergraduate students, you're right.

AC: Wasn't there a tug of war as well?

LB: A tug of war, yes, yes.

LS: Actually if you're going to have photographs of this I've got a great photograph of Lynn playing tennis with Andrew Huxley.

DM: Brilliant! I think I might have it. Is there some reason why I might have seen that somewhere? He's in shorts, isn't he?

LB: He would have been, yes.

DM: I'm sure I've seen that. Probably in relationship to Andrew Huxley's obituary and so on. I'm sure I have a recollection.

[Photo below courtesy of LS]



Lynn Bindman and Andrew Huxley at the UCL sports ground, Shenley

- LB: Right, right. And then there were two sorts of Christmas parties. There was the department, technical staff, academic staff party and we played games like musical chairs and Kim's game and all sorts of things, and there was this wonderful one, Alex, you talk about the yard of ale, the race for the men and how to...
- AC: Yes, well Andrew Huxley, he was so competitive, he would hyperventilate before starting to drink his yard of ale and the contestant was sitting on a stool up on top of the bench in the main teaching lab so, you know, everybody could watch. And you could see Andrew Huxley hyperventilate and then he'd have one eye closed, and he'd be getting the angle of that yard just right. [laughter] What I missed, apparently Hugh Davson [1909-1996] could always beat him at that one.
- LB: Right, and Jim Pascoe was good at it too.
- AC: But then we had this lovely Ugandan, he was an electronics technician whose name was Michael, I don't remember his surname. Do you remember it?
- LB: Ntow. I don't think he was from Uganda, he was from central Africa.
- AC: Okay, and being Muslim he wouldn't drink beer but he had a crack at it on Coke and he came very closely to Andrew Huxley's time. [laughter]
- DM: You mean Coca Cola?
- LB: I think you mean Coca Cola! [laughter]
- AC: He must have been exploding afterwards. If you swallow Coke that fast you get...
- LB: All the bubbles.
- AC: But the other thing I remember... do you remember when Andrew Huxley got his knighthood and we had that impromptu party for him?
- LB: Oh, in the big teaching lab? Yes.
- AC: Do you remember his speech?
- LB: No.
- AC: It was wonderful. I still remember it verbatim. It was short and to the point and he said, "When I was seven years old my aunt asked me whether I would like to be known as Andrew or Fielding." And he said, "I remember saying 'I think I'd like to be known as Fielding because I think Sir Fielding sounds better than Sir Andrew.'" [laughter] And that was his entire speech.
- LB: How wonderful.
- AC: It was a masterclass in speeches.
- LB: Lovely, yes, to take the mickey. I remember at that party saying to him, "Well, congratulations and it's good to have someone we can be proud of instead of Mr Gannex" Do you remember, they were the Wilson...
- DM: Yeah, Wilson's raincoat thing.
- LB: Yes, raincoat manufacturer [Joseph Kagan, Baron Kagan, 1915-1995] who was then corrupt. And ended up in prison. And Andrew said, 'Now, now, then.' [laughter] Much too political.
- LS: I remember, you talked about him being competitive, again, one of his games...
- LB: Musical chairs?

- LS: Musical chairs. It was only he and I in at the last, so who was going to win, you see. And I suppose I was probably more competitive, I thought, "Should I let him win?" [laughter] You know, those things go through your head in a split second but my competitive streak must have taken over so I jumped in there first. [laughs]
- LB: I remember another time that there was a fight between Dave Allen and Jim Pascoe for the last chair and I don't know if Dave tripped Jim up but he ended up on the floor and Dave Allen gently but firmly put his forearm across Jim's chest, and Jim just couldn't get up. David was very strong. And then Barbara Banks said, "David! David! Unhand Jim immediately!" [laughter] Quite rightly.
- LS: Yes, those were the days, yes.
- LB: It was very enjoyable socially and you came down to the island?
- AC: Yes, I did, I swam in the Thames too.
- LB: Yes. And Brian McCabe remembers that Olof and I imprisoned him on the island. We took him down there to write his PhD and provided him with food, he stayed in my hut, and we gave him food and Olof rowed over every day and he said, "You are staying here until you finish writing your thesis!" [laughter] And I think he kept him there for about a week so that he got over his writer's block.
- LS: Did he finish it?
- LB: He did, indeed. Yes, of course. He's a very able chap, Brian McCabe and he did very well at Cambridge. But they were very poor about giving him a lectureship. It was very difficult at Cambridge. I remember probably it was Gabriel Horn [1927-2012] who wrote to say would I give Brian a reference because he was up for a lectureship. And when I went over his work, he'd been lecturing for years, doing the teaching as well as the research and yet they hadn't given him a lectureship. And I mentioned this to Geoffrey and he thought there would be a case for suing them for unfair employment practices.
- DM: Yes, serial short-term contracts weren't permissible.
- AC: I ended up in that lab with Brian when Gabriel was Head of Department [*of Zoology, Cambridge 1978-1994*] and there was definitely, you see it was the Department of Zoology and there was a lot of sort of feeling against these nasty people who did vivisection, which of course was us lot.
- LB: Right. Really?
- AC: Well, they were zoologists, you know, they put rings on birds and watched them and you know...
- LB: The Robert Hinde out at the other end of Cambridge?
- AC: Yes. I mean I'm not decrying the research they did at all but there was definitely a feeling, they had a big insect house there and a lot of work went on, and there was just this one lab where we were working on chicks and the imprinting, Gabriel's stuff, and we were not viewed as the nicest people in the department. And that may have been something to do with it.
- LB: Yeah. I do remember being discriminated against once in a very light-hearted and unimportant way and that's when I wanted to go to some conference that was on the cortex and George Dawson told me afterwards that I was the only female who applied and they felt it wasn't fair that I should be there all on my own, so they didn't accept me.
- DM: That's a sort of complicated double whammy.

- LS: Well, they might have asked you, mightn't they?
- LB: They might have asked me, yes. Yes. So I mean...
- LS: Make a decision about me without me.
- LB: Yes. As it happened, I grew up with a brother and lots of male cousins and never minded being with men, didn't have any difficulty relating to them but... People. [laughs]
- AC: Yes. Well, I'm the same as you, I had nothing but brothers, nothing but sons, my life has been a fairly female-free zone, particularly since mum died, and I've never really viewed an awful lot of discrimination in most areas of life. There are the obvious ones of course.
- LB: Yes, and the fact that Geoffrey is an equal opportunities lawyer you know and has worked on race discrimination, and he was legal advisor to the Equal Opportunities Commission at one stage. And so he's always been extremely encouraging to let me do what I want to do in terms of going off to New Zealand to work and that kind of thing.
- LS: We talked about some discrimination against women, if you like. Did you ever get a feeling in the department that if you were interested in research, sorry, if you were interested in teaching, you couldn't possibly be interested in research?
- LB: Certainly there was in the Physiological Society because I remember mentioning something about Pamela Holton and the programme to write learning objectives for all the teaching topics. "Learning objectives! Learning objectives! Don't talk to me about that!" You know, just someone I happened to be sitting next to at dinner and didn't know very well. [laughs]
- LS: Useful things actually.
- LB: It's interesting that I think some of our best researchers are also some of the best teachers.
- DM: That's true. Don't you think it goes into two very distinct camps in most people's experience. Annoyingly, some of the very, very best researchers are simultaneously the best teachers. They also happen to be sort of concert-, sub-concert-level pianists or whatever. Horribly multitalented, and that tends to be the way of it, doesn't it? There are some very, very narrow people who are also absolutely excellent but have absolutely only one string—one very big, thick, string—to their bow. So yeah, that sort of dichotomy that you get is very interesting. There doesn't seem much grey in the middle. It doesn't shade over quite so much.
- LS: Bernard Katz was a brilliant lecturer.
- LB: Yes. Andrew wasn't, unless you were pretty clever. Andrew was hopeless to Second Year medics but fine for Third Years.
- LS: He was hopeless for first years Better for the second year.
- LB: And fine for graduate students.
- LS: Oh yes, true.
- LB: And he was one of those people who gave you plenty of time. I remember once, asking Jim, why is it that the—I can't even remember what it is now—but something to do with the slope of, if you plot $1/e$ against what, I can't remember, the slope is always constant. And Jim stopped Andrew in the corridor and said, "Lynn is worried about why this is so." And Andrew looked at me and he said, "It just is." [laughter] Something to do with e but I can't remember what the equation was.

- DM: Constant rate, yeah, yeah, I wonder what he was thinking of.
- LB: Which actually was a very helpful reply.
- LS: Yes, that's right, it just is. Why does two plus two equal four? It just is.
- DM: This is always a shock to me, I read Feynman's *Quantum Electrodynamics* book, actually when Derek Steele was the PhD student I had at the time. He's a professor in Leeds now. I remember him bringing it to my attention. I was already, in theory at least, fairly senior as it were, and thought I was interested in the philosophy of science and so on. The thing that struck me in Feynman's [book]—it's quite a small book that particular one—but he points out that you don't get any *explanations* in physics, you know, of why things are [as they are]. It's just what it is and that's very much Andrew Huxley's approach. I can really remember it as a sort of intellectual penny drop, to see it written down by somebody of the eminence of Feynman...
- LB: Who was a brilliant teacher.
- DM: He was a brilliant teacher. But also that you could therefore take it confidently as being very likely to be so, you know. And you realise, "Yes, of course". And this is one of the things about the Hodgkin-Huxley equations. Famously, they protested about the way in which a lot of the scientific community took it. They were very explicit that this is a quantitative description of what was happening: they had a complete quantitative, formalised description of what was happening. But they didn't have a mechanism. They didn't know the 'why'. Even at that sort of sub-level of 'why', you know, there were the speculations of...
- LS: It was a description.
- DM: Yeah, so it was a completely coherent mathematical description. And then I think it's fair to say that Andrew did get worried when people started talking about the elements—you know, m, n, and h that they would use in the equations—having to have physical reality. There would be some process that would have the same characteristic as the way these [elements] behave, but they didn't have to be entities in themselves. And it was a surprise really that a lot of inhibition turns out—the inactivation of the sodium channel—turns out to be probably a very simple, one-off event. And yet that's why it fits a very simple mathematical description. But it's interesting how so many people were swayed by that thinking. [Even] having that description, a completely reliable description, if you are seeking an *explanation*, you won't necessarily get one from here.
- LS: Hypothetical models never do unless they, by themselves, it when you get experimental data, if it fits, then in a sense the whole thing makes sense, doesn't it? If it doesn't fit then well...
- LB: I've always felt in physiology and neurophysiology, the modelling was always trailing behind because it needed us to find the parameters to put into the model.
- DM: Exactly. Yeah. Normally we haven't had that degree of mathematical sophistication and rigour that was exemplified in the Hodgkin-Huxley equations. Now we're seeing more of it. So the kind of Human Physiome project that Denis Noble has been driving, coming from his heart electrophysiology: the cell and then whole heart and now building it up further than that. That's a whole different approach.
- AC: I remember sort of vaguely feeling that I was verging on the miraculous that you could actually, you could find a bit of biology that an equation could be fitted to. It seemed, you know, as if it was getting to the heart of things.
- LB: Was it difficult going from electronic engineering to physiology?

- AC: What was difficult was my very poor background in physiology. [laughs] You know, there's an awful lot to mug up on very fast.
- LS: It's a different language, isn't it?
- AC: Yes, it is a different language.
- LB: First Year lectures, yes.
- LS: I mean, I reversed the sort of feeling when I went to America to sit in California and then I was in a Bioengineering Department. It was actually the Department of Aerospace and Mechanical Engineering, so I was a visiting Assistant Professor of Aerospace Engineering which I was very proud of [laughter] But anyway, the point about, they had a group of bioengineers working there and the fluid flow, which is what I was working on, and it's the same thing. And what struck me as a physiologist, if I wanted to be quantitative, I'd draw a graph. It didn't necessarily mean anything, it just described what was happening. They would write an equation, which is one step further on. And I found that very interesting and became marginally more quantitative, but not much, as a consequence, because I didn't have the maths.
- LB: But that question you asked me about snobbery, about teaching versus research, did you feel it?
- LS: Yes, I did.
- LB: And so, people were probably politer to me, being a female. It's interesting that...
- LS: Well, it's probably more of a feeling. I'm not sure if it was always ever made explicit but it could have been.
- AC: I was sort of vaguely aware of it. It was like a sort of background aura, wasn't it? Real men do research... [laughter] ...only the wimps that teach.
- LB: Well, there was always that catch phrase about those that can, do, those that can't, teach.
- LS: Yeah, those that can't teach, teach the teachers. [laughter]
- AC: Well, it's amusing and trite but it's not very true, is it?
- LS: No, not true. But actually Andrew was a very good lecturer, if he got his audience right. ... I mean I remember going to, in fact I think I ended up organising the faculty seminars and he could be really good at that level because everybody was up a bit.
- But he didn't really understand the normal human mind, did he? I mean, you know, if there's a sort of scale, he's up at one end, you know, and the rest of us are somewhere a bit further to the left.
- DM: A bit to the left. [laughter]
- LB: I remember sitting behind him in the first of the Starling lectures I went to and he was, his head was sort of drooping on one side.
- LS: Oh yes, he's famous for that.
- LB: And he woke up at the end and there were three razor sharp questions, you know. He'd been listening.
- DM: There's a school of folk of that era who seemed to cultivate that. I know a number of people who seem capable of doing that, snoozing through a lecture and still coming up with questions and you know they can't have gleaned them beforehand. They have to be paying attention to what went up. So, quite a skill. Parallel processing.

- AC: The one bit of Lynn that we haven't touched on is the painting, and actuallyWe're surrounded by them.
- LB: Well, as I said, I was quite keen on doing A Level Art. [laughs]
- DM: Did you keep the painting going throughout your career or did you really put it to one side?
- LB: I absolutely had to put it to one side. I painted before I had children. So when I was in Olof's lab, you know, I told you that day I killed four rats because I felt grotty from the day before, I did a painting of me and a rat and the apparatus and everything like that which Olof kept for a long time in the lab. But once I had children there was no time for that sort of thing, absolutely not.
- AC: No, once you've got kids, they do the painting, don't they?
- LB: Yes. [laughter]
- AC: I mean they are tremendous and it's, the majority of them are portraits and they're very accomplished portraits. They're full of expression and...
- LB: Well, I think portrait painting is really the ultimate challenge because if you can bring something of the personality over, that's wonderful. And I don't know how one does it really. And just sometimes it's successful and it's usually when you know the person and you're painting them partly from what's in your head, rather than just copying what's in front of you. So if any of you want to have portraits, I'm very happy to paint you. Actually is it you David, who is doing something to do with *Physiology News* now? Or is it Roger Thomas?
- DM: Roger Thomas is the new Editor.
- LB: A long time ago I had a little article in *Physiology News* and I published the portraits of Howard and Laurence and perhaps someone else, and I said if anyone would like their portrait painted, I'm very happy to do it for free. And Bob Lieberman came and David Colquhoun, no, David was afterwards, And I'd love to do that again.
- DM: Well, we can work up an article or I mean *you* can work up a little article. That would be wonderful.
- LB: It doesn't have to be an article.
- DM: As you know, one of the Oral History transcripts that has gone online now is David Coquhoun's and we've used Lynn's portrait of him.
- LB: But you might not go to that last page to see the portrait because the script ends on the previous page. I think maybe it needs one more sentence on that bottom thing saying that there's a portrait of him on the next page.
- DM: Yes, exactly because it looks as though you have reached the end and you don't. No, that's true, we'll have to do a bit of editing of that. But around that is, around that, when we've started corresponding about your lovely portraits and I was asking if we might use some more of them. So I had hopes that on the web page where one goes to find these things, which I hope will be more readily locatable in future, we would perhaps have some of those portraits that are relevant to the people there, as it were part of the page.
- LB: Are you going to do Tim Bliss? Because Tim is quite an eminent person.
- DM: Tim Bliss was done some time back. I think that's right.
- LB: I don't know if he likes the portrait enough but he's got it.

- DM: Obviously we'd have to get the agreement of people, so David Colquhoun was happy with his portrait being used. But no, I think it's a wonderful extension. In fact I was talking about it a little bit with Roger Thomas at our first *Physiology News* board meeting. About the business of portraits and so on. And he pointed out that he'd got this fantastic painting of a snail done by Tony Hillier, is it, at Cambridge? So Roger, when he went home afterwards, emailed me a copy of it. And this painting was about this big and it is entirely a snail. It is absolutely gorgeous and it's a perfect zoological high spec painting but it has that charm about it. It's painted especially for Roger, I think, perhaps when he retired or some time around about then. It's dedicated to him across the top. So, not a portrait as such but very appropriate, personal. And a painting brings those extra qualities, even to a snail [laughs].
- LB: Well, we're thinking of moving now. We're still in our family house and have been here for 46 years. And Geoff's 82y and I'll soon be 77y and we think we need to move into a flat, and what is Geoff going to do with his books and what am I going to do with my paintings and books?
- DM: It's very hard. We dumped a whole lot. We didn't [actually] *dump* any books, but we got rid of a lot of books when we moved from Scotland down south and still have a lot of books, I'm pleased to say. But I think that is for me, that's the hardest.
- LS: Yes, I moved a lot of books. The librarian at the Wellcome Trust, when he retired he moved out, and he found himself a house in order to have a house big enough to put all his books, it was more money than he could afford. But eventually he found a place which had a barn and he kind of turned it into a little library and he said he's only ever sold three books in his whole life and he regretted it ever after, ever since. [laughter] As a librarian maybe you would. But books are very special, aren't they? I mean they are decorative in a way but it's more than that, it's me, or elements of me anyway.
- LB: Well, the only row Geoff and I had in our married life was when he moved books off my bookshelf in our joint study in the roof. [laughter] That was absolutely outrageous, to shift my books. [laughter]
- DM: There's such a difference, I think, to browsing as well. I mean the word is used in respect of what happens on the internet, but the nature of that is fundamentally different from having either your own personal library or visiting a real library.
- LS: It's one of the things I regret about the way science has gone. When you went to the library you wouldn't pick up the right volume or a book before or a book after...
- LB: Yeah, absolutely. You would browse and you'd learn something and there would be a Starling room talk and you'd say, "Well, I remember there was this book and there were U shaped fibres going from one bit of cortex to another so you can't just look at spread as though it's going along the surface layers, you know. You have to make a cut and put some mica in it or something to test that it's really going across there.
- LS: And in your talk, let's call it talk, about Andrew reading the PhD thesis, I mean he read mine too which is a field nothing to do with anything he'd ever done or was interested in particularly but he still, he was very sharp because he was my, my supervisor left before I'd finished my PhD...
- LB: So he took you over?
- LS: Yes, and Andrew became my formal supervisor.
- LB: But he felt he had to be, I'm not saying he had to be in control in the sense of being a control freak, but I think there was a great sense of responsibility for everything that went

on. And someone was ill and I took over a tutorial for them and he was quite concerned would I be able to cope with the tutorial. And when he heard I was doing evening lectures at Chelsea College, he said, "Have you ever done any lecturing before?" And I said, "No." Then he said, "Ah," he said, "Well, I found it was very difficult to write sums up on a blackboard," he said. "They need practising before you do it. It's different from writing in front of you." So handing on his little bits of experience.

LS: Well, one thing I learnt from him when I became a Head of Department myself, I read every paper of everybody in the department produced. They didn't produce very many. [laughs] But I saw the PhD students, I did, I tried to do an Alan Ness, I was only a second-hand Alan Ness because he really was very good at doing that with the PhD students every week. I had a Chinese student who was really quite shocked by the whole process because he was a postdoc actually and he came along, because you had to revere your ancestors, revere the great men.

LB: And your teachers.

LS: And your teachers in particular. And he found it very difficult to do that. And I thought that was quite an interesting insight into, he was quite a good scientist but wasn't really critical in that sense, and Alan was brilliant at it.

LB: He didn't publish any papers though.

LS: No, I know. He did. Didn't he publish one?

LB: Did he?

LS: No, he didn't. He was very good at that. Jim didn't finish, publish, many either.

LB: Well, once Tim Biscoe came to the department, Jim started publishing. Jim was very bright and he was able to, he just had a hang-up about it, I think.

DM: So, officially, thank you very much, Lynn. We'll carry on talking now but we're going to stop recording.

LB: Thank you.

[END OF TRANSCRIPT]

(see next pages for interview photos)



Lynn Bindman (Self Portrait, 2015 - from a photo by Patricia Pank)



Alex Cooper



Laurence Smaje



Lynn Bindman



David Miller

Photos by DJM

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