An interview with Peter BC Matthews

Conducted by Robert W Banks

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The Physiological Society

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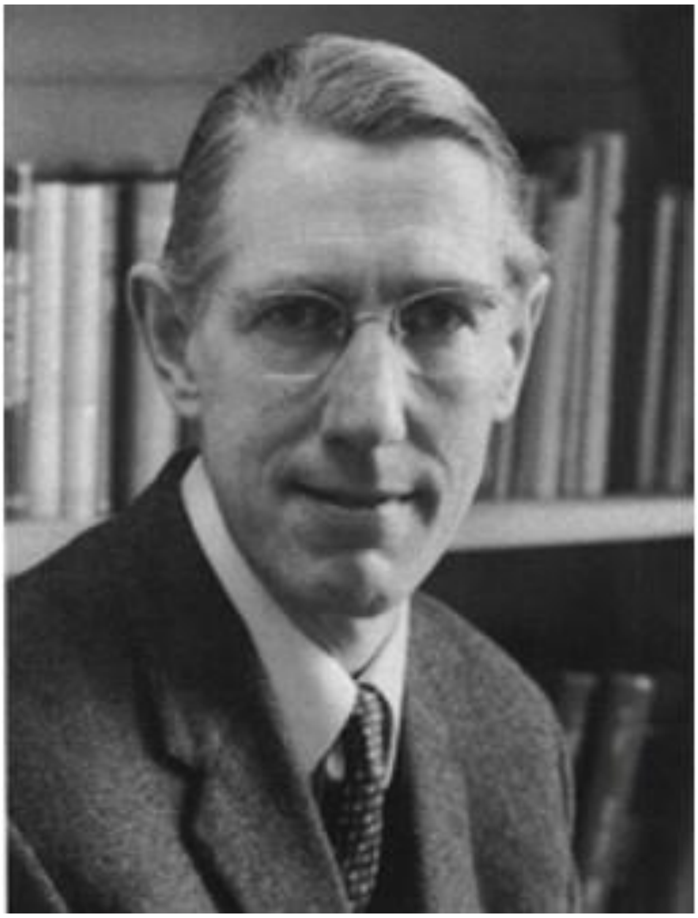

Photograph from *Experimental Physiology*

This interview with Peter BC Matthews (PBCM) was conducted via Skype by Robert W Banks (RWB) in four sessions recorded between 03 June and 22 July 2019. The transcript has been edited and annotated [explanatory details given in square brackets].

RWB: It's Monday, 3rd June [2019] and this is Bob Banks recording an Oral History with Peter Matthews using Skype. We are going to begin with the earliest memories of Peter, starting with his childhood and his schooling up to his college days. I should perhaps say that you are, as it were, the centre of a scientific family. Both your parents were scientists, your wife was a scientist, at least one of your children is a scientist. This is something quite remarkable. So perhaps you'd like to tell us about your early life. Was science inevitable, how did you follow a particular path and so on, okay?

PBCM: Hello, Bob. It's very kind of you. I was born in 1928 [the son] of Rachel Matthews and Bryan Matthews. They both worked for Edgar Adrian, who became Lord Adrian, the noted physiologist and President of the Royal Society. But my mother worked with him as an undergraduate. She went up to Newnham and then did research for a year with Adrian on the conger eel eye, which then stopped of course when she got married to Bryan. Bryan worked with him when I was about 4 years old, working on the early recordings of the electroencephalogram, which up until then had not been taken seriously. He and Adrian showed it was a real phenomenon, which then rapidly became the standard tool we know today. We were friendly with the Adrian family and I often played with Richard Adrian, who was about a year older than me. And he then became, in his turn, Lord Adrian. He became a physiologist too and did part of his physiology at Cambridge two years before me, I think, then went into research and became an FRS in his turn. He also rose up and became Vice Chancellor of Cambridge University too in his turn; he died tragically early.

So, my formative experience as a child was with scientists around me. Mother had given up; my father was still terribly actively involved. My father was a Fellow of King's College Cambridge, a lecturer at Cambridge, so I went to King's College Cambridge Choir School, not as a chorister, just an ordinary day boy. And there I had practically no science whatsoever. The curriculum tended to be Latin, Maths, Latin, Maths, Latin, Maths, Latin, Prep. I can't say I really enjoyed those years. Scholastically, it was notable also for its unconventional approach. They used to take us skating when it froze in the winter, and always had outdoor classrooms. So, it was a very odd schooling, but it was a good grounding into general education. So, is that a good start, Bob?

RWB: Yes. And, am I right in thinking the first Lord Adrian was one of the first to record from a sensory end organ in muscle?

PBCM: Yes, what Edgar Adrian did was introduce single-fibre recording on a big scale. He did cutaneous receptors and he also did muscle receptors, and he worked with Yngve Zotterman, who then went back to Stockholm and of course remained a noted physiologist all his life. Adrian did them in the frog, Bryan [my father] moved on and did them, again more thoroughly in the frog, and then very thoroughly in the mammal. Seminal papers. And I think was it '36 or '37. He did a very thorough study. He differentiated between spindles and Golgi tendon organs by their behaviour during contraction of the muscle. One of the crucial things that Bryan did was develop a better recording method. Previously they had used the capillary electrometer, which was an almost unbelievable instrument. It has an interface between mercury and sulphuric acid. You apply a potential and it moves slightly, and you then record this with a beam of light. But it had tremendous inertia being mercury, so you had to do vast corrections to it, and it gave very poor recordings. What Bryan did was develop a moving-iron galvanometer, which he had manufactured commercially, at his father's works in Bristol. His father was a pharmaceutical chemist.

RWB: I see.

PBCM: And this thing became widely taken up before the cathode-ray oscilloscope because it was fast enough to record single impulses very well. So that had a very good run from '34 to about 1940, when the cathode-ray oscilloscope came in, all quite interesting.

RWB: The technical developments are always fascinating.

PBCM: It's quite unbelievable how the world has changed.

RWB: Yes. I think that's something we might come onto a bit later on.

PBCM: Certainly, yes. Computers.

RWB: Yes. So, you're at the King's College Choir School.

PBCM: Just as a day boy.

RWB: Doing Latin and Maths and so on.

PBCM: Terribly dull. I mean I was really bored there. Then in 1942 the War was on and I went away to boarding school in Marlborough, in Wiltshire. I went there partly because my grandmother lived about 3 miles away, so we knew about the school and I went there. I did very badly in the exam on entering and was put in a fairly low form.

RWB: That's difficult to believe, but…

PBCM: Well, I really wasn't interested in the subjects I'd been doing and you went in on Common Entrance doing these. But there I really took off. I found the science fascinating. And so, after the first term, I think it was, they bumped me up two forms. We had School Certificate in those days, this was all pre-certificate. I had some proper science, but the trouble was the maths absolutely floored me because I moved into a class which had done a term, and they'd done calculus. The chap taking the class made no allowance whatsoever, so I'd sit there just listening to the [various] bits of calculus assuming I knew how to differentiate, what differentiation and integration was, but I hadn't a clue. So, I was just totally lost. It was a bad start in maths, which stayed with me some time. So, I then moved onto School Certificate, which I took, I think, must have been aged 14. Yes. I did a whole range of subjects and then moved onto the science side. I did physics, chemistry and maths, no biology at all. Physics and chemistry were tremendous.

The chemistry in particular, I had an inspiring teacher. A K [Gord 0:08:15], who had an Oxford DPhil and he made it thoroughly interesting, and I found it very inspirational. The physics man was less interesting but he did it quite competently. Maths again was totally boring. I didn't enjoy the maths then. They gave me an internal scholarship because I came top every year, and I took the Higher School Certificate after two years and got three distinctions. I was then ready to go to university. That meant there were two terms spare in which really I was still at school with nothing very much to do, so I actually did some biology with Knowles who became a noted biologist and may have been a member of the Royal Society too. Slightly quirky teaching but quite interesting, so I had some exposure to biology then.

So that was Marlborough. I'm trying to think… I hated games, I had no physio-motor coordination for ball games and didn't like them, but I had to play a bit. Fortunately, being the War, they'd ploughed up most of the cricket fields, so they didn't really want us to play cricket. But I played rugger, of course, otherwise I did a lot of running. I then took the inter-colleges. I had an event which was very typical of public schools: I belonged to a house which had house boxing matches. I didn't box at Marlborough at all, but I had boxed at prep school. Somehow this came out to the head of house, or the head of sports, and he put me into a boxing match. I was put in to box and I couldn't box. My opponent could. The fight was stopped after one round. He hit me hard on the head, and I was discharging profusely from my ear.

RWB: Oh, no.

PBCM: I then spent a month in the sanatorium, during which I'm pretty sure I discharged endolymph from my inner ear. And it could have been fatal, the accident, if I'd got infected.

RWB: Yes, this was in the days before penicillin.

PBCM: Pre penicillin but I was given sulphanilamide. Anyhow, I got through that and the discharge gradually stopped but the perforation stayed for the rest of my life and then influenced what happened. Anyhow, I had all this time in the sanatorium where I just sat there reading my books on science. Then I went up and took the scholarship exam for entrance to Cambridge, and got a scholarship to King's College, my father's college. Having had this intensive revision probably did me some good. So that was my Marlborough career.

RWB: Yes. And when you were at Cambridge, you read the Natural Sciences Tripos, is that right?

PBCM: Then I was a scientist through and through. Now, in those days, I was the age to be called up because this was 1945 and the war was not yet over. I went up to Cambridge in 1946 and there was still compulsory military service for young males, and I would have been called up. But as a scientist, in those days scientists were more valuable to science, and this was at last recognised partly through Cherwell, who became Churchill's advisor. I was given temporary exemption from military service to go up to Cambridge where, as you said, I read Natural Sciences combining physics, chemistry and my parents' subject, physiology. The tripos then was in two parts: part 1 after two years, and part 2 with one further year. So, I read those for two years. I was also expected to do maths, but I found the maths lectures hopeless and gave it up. But they put me into the exam, and I was sitting in my room and then somebody came and said, "Why haven't you turned up for the maths exam?" And I said, "I didn't know I was in for it." Anyhow, physics, chemistry and physiology got me a First, and I then moved on to doing Part 2. Part 2 was an exciting time to be in the physiology lab. There was a whole galaxy of distinguished people there: Hodgkin and Huxley had both come back from the war and were doing their seminal work, and they both lectured to us. Huxley, I seem to remember, was interested in microscopes. [interesting cross reference to Huxley's own interview] He used to demonstrate in the histology class in Part 1, and he'd move along the benches and stop next to somebody who was interested in microscopes, and talk about the microscope. And he'd come to me, who was hopeless at histology and would pass me by, not interested. In Part 2 he gave very good lectures, on muscle of course, Hodgkin on nerve. That was pre-voltage clamp, so we didn't have the voltage clamp stuff. So that was fascinating.

Now Willmer was also there. "EN" Willmer, who became an FRS for his visual physiology. William Rushton lectured in Part 1 but not in Part 2, because he had a sabbatical year in Sweden. It was the year he changed over from being a nerve physiologist to a visual physiologist. He said of his nerve physiology, "Hodgkin and Huxley have taken it all over. My rather mathematical approach isn't going anywhere." So, he changed himself over. But I remember Rushton for his Part 1 lectures, which were absolutely terrible. You'd see [the picture] on a huge scale, the audience just took against him [understanding] nothing. But he was a wonderful man. And the other thing about the Part 1 lectures, Adrian lectured at length and he really wasn't very good as a lecturer. Zotterman was there and Adrian turned to Zotterman and said, "How are you finding it?" And Zotterman said, "It's vorse in Swedish!" Adrian really was a terrible lecturer, very distant [but] who else would believe it?

So that was Part 1 and Part 2, I'm trying to think of who… now Barry Cross – there were only nine or eleven of us in Part 2. One was Barry Cross, who later became Secretary of the Agricultural Research Council. And Ian Bush, who claimed to know everything and would have had a distinguished career but died young. One of the only people who migrated to America saying, "There's much more money there."

So, it was fascinating and again I got a First. I was ready for an academic career but now my military service was catching up with me, so I was called up and I volunteered for the RAF and the Education Corps. I had the preliminary interviews for that and would have been accepted but then I had to have a medical. They looked in my eardrum - my ear which had the trouble and was still occasionally discharging - and said, "You have a large perforation.

You can't do military service, you are discharged." So quite suddenly, in the summer of 1946, I was looking for something to do. Having expected to go into the Services, I had a glorious 2-month freedom, because Part 2 ends in June and I had nothing to do then. And I also went up [to Bristol] and saw the family firm there, Simon Engineering, which made engineering parts and things and they offered me a job, and I said I didn't really want to take that. I didn't really want to get into business. So…

RWB: I think you and I, Peter, if I may say so, share an awful lot in common.

PBCM: Fascinating, fascinating.

RWB: Yes, go on.

PBCM: So I was then put in contact with Feldberg who had just gone to Mill Hill, the National Institute for Medical Research, which had just moved from its original home near Hampstead, where it had the famous F4 room, where a lot of scientists worked. Burn and Dale, they did all their seminal work there. The remnants had moved onto Mill Hill. I was given a Medical Research Council Studentship, which probably still exist, for the princely sum of £300 a year. It was absolute riches in those days. So, I went up to London and I lived with an aunt and their family and went to Mill Hill daily. Now Mill Hill is the best -

RWB: Before we leave Cambridge and King's College. I've been reading Andrew Hodges' biography of Alan Turing and he mentions that you and Alan used to attend Lord Adrian's lectures -

PBCM: Yes, and Alan Hodgkin's. I was thinking of the academic things and this was personal. Alan Turing became a very close friend at that period. Now, I was living in King's College Cambridge as a scholar, so I had rooms there. In my third year I had a rather nice room overlooking the Cam.

RWB: So which year was this?

PBCM: 1945, 1946. Alan Turing had then just come back from the War and he was a fellow of King's, and he came back to King's in the autumn of '45. And the room I was in happened to be his old room and he asked: could he have it back? So, I was booted out, but I was given a room on the same staircase, higher up. So, he was on the same staircase as me, and Alan had taken an interest in biology. I then decided to go to Part 2 physiology lectures. So here we were living on the same staircase, going to some of the same lectures, and we walked backwards and forwards quite a lot with each other, and we used to talk in the evenings as well. Of course, I had no idea what Alan was doing; he was just a rather young and immature don who had come back to Cambridge doing something I didn't quite understand and just sort of study and things. Bletchley of course was completely unknown in those days. He was interested in biology then, interested in the stripes in zebras and so on, and going into the computing lab as well. And he obviously took me there at one stage and I saw what I think was the Bush Differential Analyzer, which was a computing machine, entirely mechanical, which the differential had been produced in America during the war. They had the only one in Cambridge in '45, '46. Alan did computing that I really didn't know very much about.

I knew nothing about Alan's homosexuality. There wasn't any sign of it whatsoever. He never made any advances or anything improper. We were just male friends and that's how it was in those days. [We happened to be] at Cambridge together. There was never any suggestion, the idea was unknown. He made no approaches to me and behaved impeccably. He was just a lovely man.

RWB: You were saying that you didn't know what he'd been doing but of course at that time, he wouldn't have been allowed to say anything about what he'd been doing during the war, would he?

PBCM: No, absolutely a secret and hidden. It didn't come out for another 10 years or something. And now it's part of the tourist trail.

RWB: In terms of our own work, of course, 1945 was a crucial year because that was the year that Leksell demonstrated the function of the gamma system.

And then we move on a little bit to 1947. You are then in Mill Hill. Is that right?

PBCM: We ought to go back to Turing because there's a most awful story to tell. Now, Alan and I were close friends, but he was very diffident. He said to me one day, "I've got these metal parts I want to sell. They're bronze and they're quite valuable. Can you take them to a scrap yard for me?" So, he gave me a box with various gears in it and I put those on my bicycle and took them to a scrap yard and sold them. But only later, only 20 years, or 30 years later, I realised these must have been part of an early computing machine. I was absolutely horrified. They'd be great in any museum in the world now, but they were all melted down.

RWB: Well this incident is mentioned in Andrew Hodges biography.

PBCM: I didn't know that.

RWB: It is. And according to him, these gears were parts of a machine that Alan Turing was trying to develop for finding the zeros of the Riemann-zeta function. And this is one of the great unsolved problems of mathematics; we're still waiting for an answer to it. Proof as to whether the Riemann hypothesis is true or not. So, it's fascinating that you were there and had them... if only you'd kept them!

PBCM: They were just bits of metal to me. He must have done the work before the war; I had no idea what they were or that they were involved in computing. I didn't know anything about his great paper before the war either, so it was unknown to me. He was just a rather immature don whom I enjoyed spending time with.

RWB: Well, I mention 1947 in this connection, particularly. Coincidentally, by the way, it was the year in which I was born, 1947.

PBCM: Good heavens!

RWB: More importantly, it was the year that the transistor was invented and the marriage of Alan Turing's work and others on computing, eventually with solid state computing and so on, these are the things that have transformed the entire world and our science as well along with it in the intervening years.

PBCM: We just live in a different world from the one we were born in.

RWB: Absolutely, yes. And it all began around about that time, as it were.

PBCM: It really did.

RWB: Okay, so where have we got to. You are at the Institute in Mill Hill.

PBCM: Yes, in '47 I went to Mill Hill. So, the people in Mill Hill were - Feldberg was head of the department, and Feldberg of course was known for work on transmitters and was one of the great people who actually stood out against the electrical hypothesis of synaptic transmission. Of course he lectured us against that strongly. Eccles was the other way round. Now I was working with John Gray who became Secretary of the Medical Research

Council and was noted for his work on Pacinian corpuscles. He was working on the Pacinian corpuscle then and I joined him on that. And he was recording from single Pacinian corpuscles sitting in the cat's mesentery, where they sit by themselves. So, take out the cat mesentery and then dissect away round it and you then have a single corpuscle with its nerve and you can then prod it with a piece of bristle. Now John was interested in what caused sensory adaptation. The Pacinian corpuscle, of course, adapts very rapidly, if you give it a steady prod it does nothing at all; it's a vibration receptor. So, John wanted to know why it adapted so rapidly. What is its structure? All those lamellar layers acting as a mechanical filter, or was it the properties of the nerve fibre with the nerve fibre rapidly adapting to a current. So, what we did was really rather dull, we applied steady pressures to the Pacinian corpuscle and a steady current to the nerve and compared those. I think I told Andrew Huxley what we were doing because I met him at the Physiological Society. Andrew said, "But how do know it isn't the nerve sheath making the nerve accommodate?" And I just looked at him nonplussed, and Andrew Huxley just walked away as if it was my idea to do this not John Gray's and it really wasn't a very profitable thing to do. The Pacinian corpuscle is a lovely thing.

RWB: Yes.

PBCM: We did do one thing on the Pacinian corpuscle which was worth doing. Adrian had studied them right at the beginning of single-fibre recordings, but his conclusion had been that the Pacinian corpuscles in the cat's toe were slowly adapting, which was of course the complete opposite of our experience. So, we also spent a little time looking at Pacinian corpuscles in the cat's toe. They were precisely the same, vibration receptors. They are so sensitive these things that any vibration happens anywhere, they fire off. What Adrian had falsely recorded was something else there, and I had to give a paper on this in the Physiological Society. So, we went up to Oxford and I gave this paper saying Adrian got it wrong and who was sitting in front of me but Adrian. But of course, Adrian being Adrian couldn't care and he took it perfectly well. All quite an experience.

Now that's the other thing about Mill Hill. John Gray was incredibly kind to me. The Physiological Society then was almost akin to a dining club, there were always meetings in London about once a month. And everybody in London went. You had lunch and there would be just one theatre and the thought of having to get up and giving a paper was just unthinkable. You went to this club and you were a member of it, and you stayed, and you talked. And of course, Dale was there frequently so I saw him there. The Physiological Society was a different beast altogether in those days too. You knew everybody - people just stood up and you knew who they were. There were very few graduate students then, very few people doing science.

So back to Mill Hill. Among other people there was Harington who was notable for the thyroid, who I found a very dry, dull man. He was the discoverer of thyroxin, I think, but I'm not quite sure.

There was also Walter Perry, who was a reasonable scientist but noted for having become the first Vice-Chancellor of the Open University and played a large part in persuading Barbara Castle to set it up. I remember him saying to me, "Don't ever read a textbook. I did all my exams from my notes." So that was the founder of the Open University!

There was also B[en] Delisle Burns who moved over to Montreal and introduced the isolated brain slab, where you'd take a cat and isolate a small area of the cortex by cutting all around it, in an attempt to reduce the complication of the cortical circuitry.

RWB: Yes. Again, it's fascinating, isn't it, that some of these techniques that are so widely used now, the people who used them would be amazed to discover that they have such a long history.

PBCM: Yes, yes. No micro-electrode recording then. And there was Bill Douglas who moved to the States looking for more money. Here's the most shameful anecdote of my life ever, I think. Now Bill Douglas was interested in tetraethyl pyrophosphate, which was of course the nerve gas, and you were encouraged to study such things then. And Bill wanted to do a study on cats. We did some acute studies on anaesthetised cats, which was fine but then of all things, took some kittens and poisoned them with nerve gas. I watched the kittens die. One of the most shameful episodes of my life, taking part in that. Just horrifying. I'm afraid research does have to be restrained; you can't just leave it to people to do what they want to do.

Now the other person of whom I'm reminded by this, G L Brown, had been head of the lab before it moved to Mill Hill and G L Brown moved to University College as professor there. I used to go down there and do some demonstrating and Murdoch Ritchie was there, then moved to the States to study muscle. [Brenda Ritchie], his eventual wife, was also demonstrating in the same class. She of course became a physiologist for some 40 or 50 years and used to be a regular attender of meetings I went to. So there was another range of people I saw when I was there. Another thing about G L Brown, who'd left but he came back to Mill Hill for a party, an evening party, where he just behaved like a schoolboy. We were shooting pellets at each other out of glass tubes. G L Brown let his hair down. He was a lovely man.

Also, there was Bill Paton, who became professor of pharmacology at Oxford. And that's probably about the size of the notable people in Mill Hill. While at Mill Hill, they all had medical degrees. They said, "You can't be a physiologist without doing medicine. Nobody would give you a job in a medical school." It wasn't true of course because my father didn't have a medical degree. Adrian had a medical degree, of course. Everybody did in those days. William Rushton had a medical degree because he'd been made to do it and he hated doing medicine, but he got through it in the end. In those days if you were going to be a physiologist, you almost did have to be medically qualified. So, I rushed myself into being a physiologist when I hadn't quite expected to, because I hadn't had a period in the [education corps] deciding what to do.

I decided I'd go back and do a medical degree, so I went back to Cambridge and then had a year doing my preclinical work there. Not notable. Okay, I did demonstrating in pharmacology at the same time when Basil Verney was professor. Basil was one of the first few to do a study of the hypothalamus for anti-diuretic hormone. He had this preparation of the dog with the exteriorised carotid artery and of course the artery was in a loop of skin just below the neck, and you would inject things into this, and send them just to the brain, so you could send hypotonic fluid up to the brain when the rest of the body didn't have it, showing the receptors for ADH were in the brain. So that was during the year at Cambridge doing anatomy and pharmacology and pathology. Pathology was taught by I think Dunn, no, I can't remember his name, no, Dean [Henry Roy Dean]. But in those days, there was no retiring age at all and I think Dean was in his late 70s and almost ga ga. We had really bad lectures on pathology. But that was him, there were no notable scientific things that year. I really just had my nose to the grindstone. The whole of anatomy - in those days you had to dissect the whole of the human body. So, I had to dissect the whole human body in a year. It wouldn't quite fit into the term so I went up to Bristol and did one part in the Christmas vacation there, which incidentally I came from a scientific family and I stayed with my uncle there, Leonard Harrison Matthews, who was also an FRS and became director of London Zoo.

RWB: Oh really?

PBCM: Yet more science in the family. Going further back, my mother, my father, my grandfather had been a pharmaceutical chemist, they had both been chemists. My grandfather spent his life in pharmaceutical manufacturing, and Ruby, his wife, worked in a pharmaceutical lab. And other scientists going further back in the generations. Very much scientific breeding. I stayed with Uncle Leo at that stage. Uncle Leo was of course a great writer of books, having been to the early whaling stations, went off whaling as a young man, went down to South Georgia to study whales. Also studied basking sharks in this country. There used to be a factory up in Scotland which killed basking sharks for their oil. And that was this country.

RWB: Goodness me.

PBCM: Yes, that was my year at Cambridge doing anatomy. Did you want to go on now?

RWB: If you are happy to carry on, let's keep going, yes.

PBCM: So, I then went up to Oxford, I had to do the clinical part of my medicine. I decided to go to the Oxford Medical School. It was on the verge of extinction. It had been expanded during the war to cope with people who had come from London and it had just been kept going at a low level afterwards. And they had as their mission to train not ordinary medical people but those who needed it for a profession or something. They wanted people to have an Honours degree, so I was a natural candidate for them. I think there were nine of us that year, or ten. We had half years and of my students, they did a range of things afterwards, my contemporaries there. But some of the people who taught us were quite notable. I really remembered after I qualified.

During this period, I was very short of money because there weren't grants in those days. In Cambridge I'd had a scholarship and my father supported me. But he didn't want to support me fully here. I did a very large amount of demonstrating. I was also helped by gifts from my grandmother, but I had to work to stay alive. I demonstrated in physiology very regularly. I demonstrated once a week for the whole morning, and this gave me a bit of extra cash. I demonstrated in the cat class, which was the remnants of what Sherrington set out in the mammalian class, which every student doing Honours Physiology in Oxford had to do this practical class. Sherrington had published a book of these [experiments, still] available in libraries. *Practical Mammalian Physiology*. It was all done on cats. Cats were readily available then and cheaply. They came I think from local farms. Nobody was fussy about anything. These cats couldn't be anaesthetised, of course, they had to be dead, so they were either spinalised or decerebrated. And I used to have to come in early and do the preparation with the technicians. The ordinary demonstrators didn't want to do this. I'd get into the lab early and we then proceeded to prepare these animals. For the decerebration, they had a decerebration machine, which may be in some museums, I don't know. Basically, it was an early guillotine but not by gravity, by hammering. You put the cat in the machine, it had its head held, took the hammer and walloped the guillotine hard with the hammer. It strikes just ahead of the cerebellum, when held in the position thing. So, it went through the brainstem, removing all the cortex and just leaving enough brainstem for the animal to breathe if all went well. And so that was the decerebration preparation.

For the spinal preparation, you took a knife, absolutely terrifying like a bayonet, you inserted that between the base of the skull and the first cervical vertebra, bending the cat's head forward, driving it through and then gradually bringing it out through the ligaments, and the head came off. It was decapitation. I was doing this helped by the technicians. I was a graduate and they were the assistants. Pretty horrifying to think of it now, but it was painless for the animals. They were all anaesthetized of course. We used chloral hydrate, I think - can't remember. Something short acting which then wore off, so they weren't anaesthetized for the class. So, they experimented on these semi-living animals doing circulatory experiments and so on. And obviously very good training but it died out, good Heavens, 40 years ago, 50 years ago, in 1951 probably. Yes, that really was remarkable.

Now, during that period of course I was actively interested in physiology and I was reading what was going on and I was fascinated by - with my family history of course - I was interested in the muscle spindle, and Leksell's work as well, so I was reading that. The thing about Leksell was he was showing they {fusimotor fibres] were there, but he hadn't got very far with it. In my reading, I'd come across a paper by Francis Walshe, F. M. R. Walshe, whom nobody remembers now very much. He was a neurologist at Queen Square, and he was totally against physiologists and physiology. He'd write debunking long essays and attacking various aspects of physiology.

RWB: Really?

PBCM: A really polished man he was. I remember a lecture by him at which somebody said afterwards, "The words are so beautiful, but we really don't know what he's saying." Anyhow, this was the sort of polished side of Walshe. What Walshe had done ten years earlier was really a fascinating experiment: he'd taken local anaesthetic and injected it into muscles of patients with Parkinsonism. And the amazing thing is that you inject procaine into the muscle of the Parkinsonism patient, and the spasticity just goes, and the patient can then move much more freely than before. So, a patient who had been totally rigid could suddenly move around and do things. A very odd experiment. Walshe said, "It's quite simple: I paralyse the sensory nerves and so it's the sensory nerves that are crucial in producing spasticity. And the muscle spindles play a crucial role."

Now of course there are two interpretations of that. The one Walshe had where you block the large afferent nerve fibres, and that removes the spasticity. But the other interpretation is that you've blocked the small nerve fibres before the large nerve fibres so you would have blocked the gamma efferents. This suddenly shot out at me while I was still a clinical student, and I said, "Gosh, this could be a real possibility. You're not blocking afferents, you're blocking efferents." So I approached Liddell, who was head of the department then, and he said, "Yes, you can do some experiments." I did some experiments with smoked drums which was how you recorded contraction in – you took a cat and …

RWB: I remember smoked drums very well, yes.

PBCM: In a Leksell experiment, if you squeeze the nerve, you block the large afferents first. But in the opposite of Leksell you put local anaesthetic on, and this blocks the gammas before the large fibres, and at the same time you are stimulating the nerve above the muscle. So you take a cat muscle – cat soleus – and put procaine onto the nerve to it, stimulate above and see what happens to a) the stretch reflexes in the decerebrate cat and b) the tendon jerk. So you'd just gently tap the tendon, stretch the muscle, record the tension on the smoked drum – basically just a tension recorder; and then stimulate the nerve above the block and see the muscle twitch; then you put the procaine on and you wait – the procaine was soaked into filter paper – and after about 20 minutes the reflex – the stretch reflex was gone, the tendon jerk was gone, but the muscle contraction remains, so you know the extrafusal motor fibres are contracting, but then something's blocked – presumably the large afferent fibres are doing the same thing, and therefore there is a very strong possibility that it's the gammas.

I did that entirely on my own as a clinical student but that wasn't enough to go on. So, I came to the end of my clinical course. I could have done house jobs straight away but I was so fascinated by this and Liddell, the head of the department, agreed it was interesting, and Geoffrey Rushworth had just come back from the war and was doing experiments which weren't getting anywhere. And Charles Phillips suggested I join up with him, and we did some more sophisticated experiments, because he had cathode ray tubes and so on. I then started collaborating with Geoffrey Rushworth. We then worked for 6 months and the basic hypothesis came out true that the large afferents blocked at the same time as the large efferents, therefore the gammas blocked earlier. So, I spent 6 months doing that with him, I then did 6 months house jobs, and then came back and did 6 months more teamed up with Geoffrey. I then went back and did 6 months more house jobs. I then came back and said, "Now, I don't want to follow a medical career, I want to go back to physiology" and Liddell offered me a job.

RWB: And you were at Oxford ever since?

PBCM: I've been at Oxford ever since. I went to Oxford to do my clinical and just stayed there.

RWB: Yes. So, apart from your father's work on the muscle spindle, this was really *your* first work on the muscle spindle.

PBCM: This was my first work on the muscle spindle.

RWB: Well, I was looking at your publication history. It's absolutely remarkable, Peter. There must be some sort of record. I used the admittedly imperfect Web of Science and they came up with 137 papers

PBCM: That's not many for people these days.

RWB: I know but you were the sole author on 55 of them.

PBCM: That's unusual, yes. Either that or just one collaborator.

RWB: And given that we're recording this for the Physiological Society, we should probably note that you published 54 full papers and 53 abstracts in the *Journal of Physiology* alone!

PBCM: Well, that's my preferred place.

RWB: That must be some kind of record, I should think.

PBCM: I doubt it.

RWB: Well, 54 full papers, and actually your publication record is quite astonishing. When you look at your publications probably from about 1956 onwards, up until the 1990s, you were publishing at an almost linear rate of just over 3 papers a year. That's quite astonishing.

PBCM: Yes, yes. It was remarkable that I did a teaching job. I wasn't a pure researcher, I had a university teaching job, teaching both in the lab and in college. But I should get back to the clinical at some stage to comment on the people I saw there. Now the Radcliffe Infirmary had various people who became notable in research. I did two periods of 6 months there. The first period I worked under Leslie Witts who was a haematologist, who was very keen on blood and did some of the early work there. And Sheila Callender was also on the firm, and she became famous for that too. So, Leslie Witts and Sheila Callender, both good clinical scientists who also did serious work. There was Gwyn MacFarlane, who became - did the work on haematology clotting factors - very famous. Also wrote the excellent, two excellent books, one on Florey one on Fleming, which are still worth reading to this day. And for my second period of 6 months, I worked in neurosurgery under Joe Pennybacker. Now Joe Pennybacker published nothing, but he was a wonderful clinician, terribly wise. And I was on the wards, not in the operating theatre, seeing all the neurosurgical patients with their horrid lesions because they mostly had tumours. So, I was well versed in clinical neurology, a useful experience to have had.

Just going back to Walshe. It seemed to me the [possibility] of doing something local on Parkinsonism really hadn't been explored because they had too many drugs. For some forms of spasticity in children, where they still cut dorsal roots, it might be much better to try some selective blockers for fibres in the spinal cord – the spinal theca, rather than just cutting all the dorsal roots for childhood spasticity. So that's my clinical period over. Now research starting. That's probably about enough from the moment.

RWB: Shall we take up the story another day?

PBCM: Do you want to name a day now or will you contact me later?

[START OF PART 2]

RWB: This is now the 10th June [2019]. Picking up Peter Matthews' oral history.

You were with Geoffrey Rushworth in Oxford. Can you tell us something about the department that you joined?

PBCM: First of all, Geoffrey had been in the army doing neurology there and wasn't quite sure where he was going to go, so he was keeping his options open. He was doing one day a week of general practice out in a little market town near Oxford. He took me there once and it was just out of the Ark, with the old days, no appointment system, nobody else, just Geoffrey there. He'd sit in a room and outside there was a room full of people and he'd go and call them in one by one in turn. They just sat there until they were called. And he did all sorts of the usual GP things, and in the end, he finished off as a clinical neurophysiologist, and didn't do very much more research. His son moved on and did research in a big way. That's Geoffrey Rushworth.

Now, I came back from doing my clinical work in neurosurgery in 1956, it must have been, and like you, came into physiology with a personal departmental demonstratorship and that was full time paid and people in the lab then were quite interesting. The professor was Liddell. Liddell was one of Sherrington's pupils, and was sort of rather a grey man, one of the people from Mill Hill called him "that Bishop". Somebody told him he had a heart problem very early on and after that he never moved fast. He'd had the lab built to his specification, which was the new physiology building, which is very old now of course, and in his rooms he had a private bathroom put in so he didn't have to go to the lavatory with anybody else. Everything was sex-labelled in the lavatories. We had *n* lavatories, either labelled Men Staff, Men Students, Female Students, and so on. Liddell said that he had so many put in because then he knew they could be converted later, and some of them were, to useful rooms. And Liddell was a very kind man and gave me a leg up. He was very kind to me, because he'd done no science for years and years, didn't do any then. His lectures were the height of boredom. People went but didn't enjoy them. So, he was head of the department, and it was run on a shoestring. It was him and one secretary and that was it. He ran the whole department on that.

Of the people in the department Charles Phillips of course rose to distinction. Charles worked on the cortex and very good at making micro-electrodes and things like that. And a very nice man indeed.

RWB: Which were then fairly new, weren't they?

PBCM: Yes. And he did the first, early cortical recordings, in the anaesthetised cat, and he then carried on, but he was ambitious for the higher things in the world and became a member of the MRC. I got on well with him. He did a lot of editing, and eventually decided to become professor of anatomy, when he wasn't feeling his research was going far enough. So, he became professor of anatomy for his last years. Then he took early retirement because of that horrid dementia, Lewy Body Disease. He saw that coming and kept a diary of it. It eventually caught up with him and killed him. One of my saddest things was seeing him at the end, when he would talk nonsense; one never knew whether he thought he was talking sense or knew he was talking nonsense. One would say something to him just to get a reply. So that was Charles, a lovely man, very active in the Physiological Society which he was secretary of for many years.

Then there was George Gordon, whose father had been head of Magdalen College Oxford, who stayed on in Oxford. George was a very quiet man, never achieved distinction, but quiet and pleasant and effective and became head of department temporarily. Then Bob [RW] Torrance, who worked on the cardiovascular system, and who had a fast racing car. He was a sort of typical Oxford don, very keen member of his College, taught well and worked in the lab but never got very far. Then Jean Banister, who always made a great deal of noise, was a Fellow of Somerville and I think David Whitteridge, who'd be impressed with her later, always regretted she'd been appointed because apparently Rushton could have been appointed instead or somebody. But they obviously wanted a woman. Jean Banister lived into her late 90s, was a great character, never achieved much of distinction but she also was the supervisor of my wife, Margaret, and I'm grateful to her for that.

I think that's about the size of it, except this new building which was very grand in a way that the old building hadn't been. The old building had been Sherrington's, and this was just a very modern building [so there was a complete change around] when I arrived in the building.

: I think I've finished the people I remember who were notable. What I would say about the department was those days were entirely self-sufficient and there were no centralised services, we had our own lovely library, we had our own mechanical workshop, which made various things, our own photographic department, so everything was done in-house. One knew all the people doing it, knew the technicians as friends.

RWB: Yes, indeed. I think this was the norm wasn't it, then? When I went to Toulouse, we had our own mechanical and electrical workshop. In Durham in zoology we had our own mechanical workshop and so on.

PBCM: Yes, electronics were built on site to one's own design.

RWB: Yes, and we built a lot of our own equipment of course.

PBCM: I built amplifiers with my own hands and so on. Later I had a technician who built things in my room for me. That really was a different world. I think it was because everything was done on a university grant then. The university was funded to do scientific research. I didn't have a grant for years and years. One didn't need to. One was supported by the university, supported by the UGC, the government grant committee. Again, it was a different world.

RWB: We lost some important things certainly.

PBCM: One always spends one's time writing grants.

RWB: Yes. Now at the end of the previous session of recording we were talking about the work you were doing on procaine, and the effects of procaine. And this was your first real introduction to experimental work on the spindle and it seemed to me that this acted a springboard for everything you did after that, and you've ranged through reflex actions and the effects of vibration on the primary ending, and the central effects and all sorts of things. But I think it all sort of began with that. Would you think that was reasonable?

PBCM: And of course, I was interested in that partly because of my father, because he had been credited with doing the early work on the spindle.

RWB: Yes. Of course, yes.

PBCM: So anyhow, having started there I stayed there, and it went on and on and on. It was a very good thing to have worked there. The nice thing was one's colleagues became one's friends. One has the impression that molecular biology is intensely competitive, people might be your enemies, people are guarded against and not told things as one's friends. When I think of the story of Rosalind Franklin and Watson. People we worked with were our friends as well as our colleagues. As, for example, yourself. So it was a wonderful world, then.

I came back to the lab, I tried out on the gamma work, doing better recording and so on. I did that in about two years doing intermittently with house jobs. I then started off on my own. Now, I'm trying hard to remember, was it Michael that I worked with first? Michael Brown or Robin Harvey?

RWB: After Geoffrey Rushworth I think you started with Michael Brown.

PBCM: Michael Brown was my first research student and he of course had a Medical Research Council grant. He was interrupting his medical studies for that. Michael Brown was a lovely person to work with. We had a very happy time working together. (I have to throw my memory back in time.) So, we didn't work on the spindle, I think, if I remember, we worked on muscle contraction.

RWB: Yes.

PBCM: We worked on the back response, a very odd thing, with Pat Merton, who was a great character in his time and will be known of by many people listening to this, based out in Queen Square, neurologist. Had a great character. Now he did experiments on humans and found the effect of two successive stimuli could be less than one stimulus and investigated that on the cat. He thought it was a very odd property of muscle, and we showed it was a re-excitation of the motor fibres by the muscle contraction. I did a nice paper with Michael. Michael then carried on with his clinical studies and then came back to the lab after that and worked with me on spindles. But he decided he liked the lab and stayed for the rest of his life. We worked together for many years and then he worked on his own on spindles and then he took off in a completely new area. He went out to Norway and worked with Jan Jansen, who we will be talking about later, and got into regeneration, and then his distinguished work on regeneration of nerves.

RWB: Indeed, indeed.

PBCM: Very interesting mutant mouse which didn't have Wallerian degeneration in the normal way and he published a book on Wallerian degeneration.

RWB: He did, yes.

PBCM: Very well known in his field, in that field in his later life. He's now happily retired in the Highlands of Scotland, with several acres of forestry, which he looks after. Right out in Gairloch, rather cut off from the world. But he comes south regularly and I've kept in contact with him. His wife, Hillary, worked with Jean Banister and then came back to the lab after she had her two children, and worked regularly there for many years. So, the friendship is keeping going strong. They are both lifelong friends.

RWB: Right. So, was it the influence of Michael going to Norway that led you into -?

PBCM: That was long afterwards. Jan Jansen came to work with me. And that connection carried on when Michael was back in Norway. By then Jan had moved over to regeneration, Michael wanted to have a change of field, so he went out to get more experience with him. I should also mention an interesting thing at this stage: I'd long had an interest in Ragnar Granit and his work on spindles, the work in the Karolinska with the gammas. I applied for a grant to go out and visit Granit. So, one winter I took a boat - no flying in those days - from Harwich up to Gothenburg. A small boat, dreadfully seasick going there, and then spent a fortnight there. Also, at the same time there was Saburo Homma from Chiba in Japan, who had come over to work with Granit too and get trained in Western ways. Homma had practically no English and I had absolutely no Japanese so we'd wonder around Stockholm together, sort of keeping each other company but not actually talking very much. But Granit put our names on the paper. It was memorable going up there to Stockholm in winter.

RWB: That must have been a challenge, I would have thought.

PBCM: It was indeed.

RWB: I've spent winters in Calgary, and I know how challenging that can be.

PBCM: I was in Umeå in winter too, which is even colder. I've visited Stockholm in the winter when it's been minus 22, which you have plenty of experience of.

RWB: Well in Calgary, minus 20 yes, certainly. Probably drier though than in Stockholm. It's a bit more bearable.

PBCM: Yes. More light in Calgary, very little light, especially in Umeå. That's another great thing about our life, it gives us a lot of travel.

RWB: Yes, that's right.

PBCM: By going and meeting these people to work, but none the less being able to spin off and see these wonderful cities.

RWB: Yes, absolutely right. We wouldn't have had the chance otherwise probably. That's quite right.

PBCM: I had a lot of trips to Gothenburg, visiting people there. So that's been a wonderful aspect of the life, too. Also examining DPhils. I've examined DPhils in several countries. I've been up to Norway to examine a DPhil, and Sweden to examine a DPhil. And while we're talking about it, I examined the DPhil of Lars Velo, who then left physiology and became a minister in the Norwegian government. I heard him speaking in public once about defending the practice of whaling. And Sten Grillner in Stockholm who of course became a very well-known physiologist. These are people that I've examined. So that's another great thing about this life: travelling to examine people in Europe.

RWB: Yes, certainly I feel I've been extremely fortunate in meeting many of the people that I have met over my career, most of whom have been thoroughly, thoroughly nice and pleasant people and lovely to work with. Just lovely characters. People like Paul Bessou, for example.

PBCM: We really are. We're not competitive like the other fields. We try to help each other. I should mention also that later on, with my wife, I took trips over to Finland to visit Granit in his home. Daisy, his wife, was still alive then so we took the boat from Stockholm. There was actually a meeting of the Royal Society in Lund, jointly with the Scandinavians, so I drove up to Lund with my wife. We then drove up to Stockholm, left the car —which was a little A35 van in the lab of Sven Landgren, who worked for Charles Phillips— and then took the boat across from Stockholm to Finland, stayed with a friend of Margaret's and went on and had a two-day visit to Granit in his home, by a lake. Absolutely idyllic there. Again, just a funny story: one of the things we found there was a cast adder skin on one of the islands. So, my wife said, "I must keep that," put it in a box and we took it home with us. Coming back home we put it in the car under the seat and coming through customs at Newcastle, the proprietor of this [car] said, "Now, what are you describing that you haven't told us about?" and found this adder skin. So that was a trip to Granit. Lovely.

Again, that developed out of knowing Granit as a colleague.

RWB: Yes, so just going back to the work for a little bit, you collaborated briefly with David Westbury?

PBCM: No, Robin Harvey came next, I think.

One of the things I'd been fascinated by was the difference between the primary and the secondary endings of the muscle spindle. And my father had published a paper in '36 differentiating two kinds of spindle behaviour, A1 and A2. And he thought that might be the difference between primary and secondary. And Robin and I looked at that, but it came to nothing. But again, Robin qualified in medicine and came back to physiology and lectured in Bristol for many years. He went out to New Zealand to finish his life there. Also caught up with him again when I went to Australia because he was spending a year with Eccles. I saw Robin there too. So that was a happy year with Robin, but we didn't do major things.

RWB: Now, by the time you were working with Harvey in the early '60s, Cuy Hunt and his colleagues had already then developed the single-fibre technique, hadn't they?

PBCM: That was very exciting. Hunt and Kuffler, yes. Steve Kuffler of course was terribly well known. I visited New York, for the first time in '62 and saw Steve Kuffler then and visited him in his lab. They were then working on the inhibitory transmitter from inhibitory nerves in the lobster. And to do this you had to have huge numbers of lobsters to take a few nerve cells out, and what I remember was they had huge mountains of pieces of lobster in the lab. That was quite something.

And John Nicholls was then working with Kuffler and John Nicholls of course again became distinguished in the field [of glial cells]. Cuy Hunt was less distinguished than Steve Kuffler. I remember, I stayed with him once. Two things struck me. First of all, they had very rare furniture and they had a cat and they had it de-clawed, which I found terrible. The other thing about Cuy which shocked me is that he had a wife and children and he just walked out on them to go with his second wife. I met the second wife, never the first one. I wasn't used to that kind of thing then. I was just shocked to have someone who deserted his family. Nonetheless, I liked Cuy and had a nice visit to him. I think, no it was another time I visited Vernon Mountcastle. That again was very informative, and I enjoyed seeing him. Vernon, he lived well out into the country and he drove into Baltimore in an MG sports car. Of course, Mountcastle [was] terribly distinguished and did so much work and published so many useful textbooks. Again, those were people one met through one's work.

So, coming back to Oxford, I then had a research student, Paul Noon, though he wasn't cut out for it. We were sitting in the lab one morning and he was measuring records - we were looking at Golgi tendon organs - and he said to me, "Can I talk to you about something?" I said, "Yes." He said, "You know, I don't like what I'm doing." I said, "Yes." He said, "I want to stop." I said, "When?" He said, "Now." So, I said, "Well, go." So he went back to clinical work and never did anything after, became a psychiatrist. There I was working on my own and Charles Phillips was in the lab of course, and he had with him Jan Jansen, who had come from Oslo to work with him and get more experience. They were working on a project on fish, and it really wasn't working out. I think they had to catch the fish, for one thing. But anyhow, the experiments weren't going anywhere. And Charles suggested Jan might like to work with me because I hadn't got a research student. And Charles had actually been too busy to spend too much time in the lab, he was doing more and more other things. But I should emphasize, always in the lab, we weren't pure researchers, we were university teachers with a full time contract. We shouldn't do more than 18 hours of teaching a week in order to leave leisure for research. That is pretty killing. I could do 10 hours of supervision in a week, and 8 hours of demonstrating. One had to fit the research in in between.

RWB: Yes.

PBCM: It was pretty hard on one's wife because one was not there most of the time.

RWB: Yes, my wife complains about that as well.

PBCM: Just out of interest, of course, the story about Hodgkin and Huxley who'd go off to Plymouth and their wives were called not widows but "squidows". They were working on squids.

RWB: [laughs]

PBCM: Anyhow, Jan Jansen came to work with me, and we had a really lovely time together. Jan is a super personality. So, first of all the science: Now I am really fascinated by the difference between primary and secondaries and I wanted to investigate it. It occurred to me if we looked at them in the decerebrate [animal] where there was known to be gamma tone, we might get some clue there. I thought the clue was they lay on different parts of the intrafusal fibre. The primary lies on the equatorial bag, which has no myofibrils. Secondary lies on a region where there are myofibrils, and they might be differentially affected by intrafusal contraction. So, we said, we'll go and look. We'll take decerebrates, we'll record from primaries and secondaries and we'll see how they behave when we cut the ventral roots and remove the natural pre-existing fusimotor tone, which we knew was there - nice work.

When we started on this, we had terribly primitive machinery. As a means of stretching the muscle, we had a large screw thread driven by an electric motor, which was connected to the screw thread by an electromagnetic clutch. So, you'd start the thing going, the electromagnetic clutch would clutch-in the motor, the screw thread would wind up, and pull the lever going onto the soleus muscle, and so stretch it, and then at the end it would come up against a micro-switch and the screw thread would stop turning. So, we could put in a stretch of variable distance but not a very wide range of velocities. I think our maximum was about 3mm per second stretch. Now the first thing we did, so we started doing that, and we were recording all the stuff on photographic film. So we had a film running at high speed and ran through 100ft of film just like that. The physiology lab had great stairwells so we would develop the film ourselves in large tanks and then hang it out to dry after it had been fixed and washed, to dry in these stairwells. And then we'd wind them up into rolls and we'd go through them. Now I was busy teaching so we'd do the experiments together, but Jan would do all the sort of counting just done with a ruler. You'd see how many spikes were in a given length of film and that gave you your frequency of firing.

So that gave us our initial results and that showed quite clearly that, what really interested us was what happened at the end of the stretch: the primaries and secondaries behaved quite differently. At the end of the stretch, the primary would go up to a high frequency, and the moment you stopped stretching it the frequency would drop down rapidly, whereas the secondary just didn't bother. It went on firing at the same rate when the stretch came to an end.

RWB: Yes. And you measured that using the dynamic index, which is still being used now by people, yeah?

PBCM: Yes. The frequency changed in the first half second after you reached the end of the lengthening stretch.

So, that was the primary and the secondary, we wanted to find a difference there. And the other thing we noted dramatically was the difference on cutting the ventral roots. When the roots are intact, some primaries showed a big drop, some showed a much smaller drop. Now, I'd seen this before with Robin of course, the difference of the efferents on spindles, the difference between primary and secondary. But what used to shock us, Jan Jansen and myself, was that some of the primaries where it didn't show proper dynamic behaviour when they got fusimotor tone on them, and they looked much more like secondaries, whereas the other ones just had typical dynamic behaviour, or even larger. So, we found two kinds of behaviour. But when you cut the roots they went back to behaving like typical primaries, so gammas actually had two kinds of action on primary endings, either to make them look like secondary endings, or kept making them look more like primary endings. So, we said this has to be two kinds of fusimotor fibre.

Now, at that stage, physiology was going on apace, with Ian Boyd and David Barker. Huge controversy about what was going on there. But Ian had plumped for there being two kinds of gamma, so we thought this would fit in with that. But we didn't know what they were. We wanted to call them by their effective names, so we called them gamma dynamic, or sorry, we put the names on later. But we had two effects, and we argued that there had to be two kinds of fusimotor fibre. So that was the major thing we did, Jan and I did together. We put that on the map and speculated very hard at the internal workings of the spindle. Now, the other amusing thing at that stage was the measuring of the spindles by the ruler was actually excessively tedious and I had heard about a thing called a frequency meter, which was being used for other things but not for this. And I worked out and designed my own instantaneous frequency display. Electronically, you take each interval from each spindle and measure an instantaneous frequency. Now of course you can do it with a computer just like that.

It all had to be done with analogue circuits. You had a whole series of condensers which I had to discharge and in order to get - a condenser of course discharges in an exponential - what you wanted was a function of 1/f so one had to have a whole bank of condensers, which switched from one to another as the frequency fell down. There was this great bank of condensers all sort of carefully tuned to give the right shaped curve. So that was on the instrumental side. But the thing which really shook us then was the moment you started [to stretch] the spindles, we found it had a very curious instantaneous pattern, they were firing as doublets and triplets. We got terribly excited thinking there was something important going on. I think there is an infrared receptor in the python which behaves in a funny way. We said, "This is a way of signalling." And then suddenly we realised that our motor was vibrating, we were just picking up the vibrations of the motor. It was terribly sensitive to that.

RWB: And that of course led on to a very productive area of research in vibration sensitivity. But what I was going to say is it's another example of the necessity really just to be aware of what's going on.

PBCM: It really is.

RWB: And the importance of doing work which is exploratory rather than, you know, you've designed a programme of work, you go through it, and it's set there to either prove or disprove some hypothesis or something. If you only ever do that sort of work, you're going to miss all sorts of important things.

PBCM: The other thing is making your own instruments. Be critical of your instruments. When you buy them off the shelf, you think they're perfect. When you make them yourself, you know you're looking for artefacts the whole time. So, we were really aware there could be something funny in there for ages. We traced it down. We could have made fools of ourselves by saying, "Here is something very important."

RWB: Yes, absolutely.

PBCM: Going on with Jan Jansen, that was a wonderful two years. At the end of it, I got glandular fever, so it was actually very hard getting papers written fully at the end, and Jan had to be very patient with me. Glandular fever is very debilitating. So, Jan then went back to Oslo and spent the rest of this life there as a physiologist. He carried on with spindle work for the time being and then decided to branch out and went into regeneration and made a name for himself there. Jan Jansen was of course of distinguished lineage. His father would have been professor of anatomy there and worked on the cerebellum. These things tend to run in families everywhere.

RWB: Yes, interesting, yes, yes. There was one thing I did want to ask you, Peter, while we're still talking about the differentiation of the two kinds of gamma. And that is that you published a number of papers on that, but in some ways the key one was a paper you published in 1962.

PBCM: That one by myself.

RWB: By yourself, yes, in the *Quarterly Journey of Experimental Physiology*. Now, I think that's the only paper you ever published in that journal.

PBCM: There's a very simple reason. The *Journal of Physiology* was being terribly slow at publishing. Pat Merton was one of the society officers here. Pat, as I say, was a maverick character. He spent a lot of his time sailing and so the papers he was refereeing would come back stained with sea water, and they could take months and months and months. And you had something red hot here and I wanted to get it done quickly. *Quarterly Journal* I knew would be more sympathetic and pass [it] so I did it for the sake of speed of publication.

RWB: I see.

PBCM: I felt it was something that really needed to be published quickly. It was red hot then.

RWB: And that's one of your classic papers. According to the Web of Science, it's accumulated a total of 264 citations.

PBCM: Gosh, that's good. Now, that was hard work because I did it on my own entirely. And there was the dissecting and the recording, and I remember being up until 2 o'clock in the lab and worked through solidly by myself. That was of course, at the one stage we were anaesthetizing with ether and one tended to inhale a certain amount of ether oneself. With the sheer physical effort of doing a long dissection and then doing all the electronics, it was really rather considerable.

RWB: Yes, indeed.

PBCM: Really quite proud to pull that off. The other thing I should tell you that I was rather proud of: in the period with John Gray, I had this very happy time working with him and then John Gray went out to Sweden for a year, I had gone to Cambridge (?) to do my medical studies, and the Physiological Society was going to meet at Mill Hill. Now the Physiological Society has a great tradition of demonstrations and John Henderson's wonderful Pacinian corpuscle presentation of an isolated Pacinian corpuscle. So, I went there to Mill Hill for the weekend, got the apparatus going, and it was a quirky apparatus, set it all up, and demonstrated on my own to the Physiological Society. And I'm really quite proud to have brought that off, I was only about 23 or something, 22. Those were technical skills I inherited from my father to be able to go and get all that apparatus working again, because this was all sort of ex-service equipment and so on. Very hard to use. Anyhow, I was proud of that, the '62 paper, having managed to do that on my own. The other thing I'd managed to do, which was crucial for the '62 paper, was make a better stretcher. Now the motor had only limited use for this. It had a very limited range of stretch speeds and I wanted to be able to stretch much faster. And so obviously the point about the muscle spindle is it's an instrument for measuring movement, not a static position sense, but moving, for controlling movement, so you want it to be able to move fast. The way forward was using an electromagnetic stretcher. So, I got a huge electromagnet and that then had to be controlled electronically, it had a range of movement of about 1cm, and must have weighed about 20 kilos or something. It had to be mounted on a huge metal table tightened up to avoid vibration, which must have weight 50 kilos or something. Absolutely massive. There was some debate as to whether the floors would take it.

So I had to engineer this one. I had to design the circuitry for that. Now, I was helped in a lot of that by Roy Kay [Kaye?], designing the circuitry for this. You had to get well into servos and I knew how to control the servo so it was stable by having feedback - it had to have a length transducer on the stretcher, which was a magnet moving inside a coil, which is a very crude way of doing it, which would pick up the rate of movement, and also position. Well, position needed to be differentiated to get rate of movement. And the final problem with the stretcher was to get enough power. Valves were just coming to their end, and we got some very large valves, power valves for radio transmission, which fortunately had just enough power. Fortunately, power transistors had just come in then, so it all had to be done with transistors in the end but the laboratory tended to be just blowing up occasionally, so it was a bit dicey.

Talking of this work, it had an instantaneous frequency display, which then came into absolutely standard use, and an electronic stretcher, which again came into standard use. So, without those I couldn't have done those experiments. I'm proud, one shouldn't be proud, but life is an odd business, one is proud to have done the technical side of it and then proud to have brought it all to physiology.

RWB: Well, why not, yes.

PBCM: So that brings us up to static and dynamic. The problem is now it's about time to stop, isn't it? We've had an hour.

RWB: We've had an hour now, yes. Do you want to call it a day for now?

[START OF PART 3]

RWB: It is now 1st July [2019]. It was somewhere around about 1960 [being discussed] when we left off from the previous session. Peter, you were just telling me before we started recording about your contacts and meetings with Roger Bannister, which I'm sure, since he was a member of the Physiological Society [from 1956] would interest the listener.

PBCM: Roger Bannister, I had contact with in two ways. He of course did his running when he was a research student in the physiology lab. I met him a few years later when I was doing house jobs in the Oxford Infirmary and he was doing house jobs there as well, internships. I saw quite a lot of him in the common room and of course he's a wonderful person. My other link there was my wife, who worked with Jean Banister as a research student, overlapped with him directly in the lab and she got to know him quite well and she was actually there at the race track when he did the 4 minute mile. He gave her an autographed copy of the racetrack meeting. We've still got that. Rather valuable. The other contact with Roger Bannister was my sister, who was at St Hugh's at about that time. She saw quite a lot of him, and he obviously got interested in her at one stage. Roger Bannister was a fascinating person, who moved through the lab at about the same time as I did.

RWB: What was the research that Margaret was doing with him?

PBCM: Now, Roger Bannister of course worked with Dan Cunningham, a respiratory man, and did a BSc on that, working on respiration. My wife, Margaret was working on what was called critical closure in blood vessels. There was a theory that if a blood vessel got too small it suddenly clamped down under Laplace's Law. I mean, as you blow up a balloon, the tension in the wall gets less and less and the vessel is able to hold itself, but when it gets smaller and smaller, the tension of the wall goes up and the vessel can suddenly shut itself down. The theory was: in the blood vessel you've got two tensions which would suddenly go bang, shut, and have an occluding effect. She investigated this in the frog. It didn't really get anywhere. That's what brought her into the lab and that's how I met her, which is of course a wonderful thing for me, and I hope for her too. We're still married after 60+ years.

RWB: Shall we get back to your work now?

PBCM: Now the next stage of course is going to Australia, I think. I got a Royal Society fellowship taking me to work with Jack Eccles in Australia in 1965. Sorry, I have to think of the years and it's very hard. This is now '65 to '66. I had by then done the 7 years in the lab and it was the first time I could have sabbatical leave. I could get a year off with pay. Of course, it was very expensive going to Australia, I had to have a Fellowship from the Royal Society to do it. Now in those far-off days it was cheaper to go by ship to Australia than it was to fly. So the whole family went aboard a P&O ship, wife and two children and took 6 weeks to get out to Australia. We were travelling first class, which was cheaper than flying tourist class, in those remarkable days. We had to take all our luggage with us. We were going to stay for a year, so it was nice to have plenty of luggage. I went out to Jack Eccles for a year to sort of expand my experience, and when I arrived there, I found two people I knew from Oxford there already. David Armstrong -

RWB: This was in Canberra, wasn't it?

PBCM: This was now in Canberra. Then we had the ship journey out taking 6 weeks, which I went through most of Jack's papers again, because it's a long time to fill in. And now of course people cruise for pleasure, for me it was just killing time. But they looked after us very well in first class. You were all strictly segregated, no contact with the tourist classes at all. They had just stopped going out on the £10 free visit to Australia. So, it was David Armstrong there, who worked with Charles Phillips, and Robin Harvey, who had worked with me. And what Jack's policy was, was to expand the Australian lab very much into the National Institute at Canberra. And his way of doing that was to bring bright young people from all over the world, build up huge teams, and as postdoctoral fellowships both David and Robin had come there. So, I was put to work with them.

Now, Jack Eccles was a wonderful man of course and he was characterised by terrific drive rather than great inventiveness. He was a man who got things done and brought people together, but not I think of terrible originality himself. This is in a way rather like Lord Florey, who again did all the crucial things for penicillin but did it not so much by his own originality, which came from other members of the team, but by his great drive and organising ability. So, Jack was a great sort of pusher and he got things done but basically he didn't think very far. He set us to work on the inferior olive [inferior olivary nucleus], saying, "Can you find out what that does because it's connected up to the cerebellum?" and he'd just been working on the detail of the cerebellum and the olive had a strong connection to the cerebellum. It was his idea to simply carry it over from his spinal cord days. We set up every nerve he could think of, all working on cats of course, we would set up every nerve in the hind limb to stimulate and see what potentials you find out about the brain. And our job was to go into the olive with microelectrodes, find whether we could make any sense of the signals. Now of course the brain is far too clever for that. No sign of a sensible code coming up. There were just odd shocks coming from the muscle nerves, and they didn't get anywhere very much. It was a useful experience doing that and it was a great experience being in Australia. It was really rather unusual to go to Australia then.

RWB: And during this whole period, round about that time when I look at your publications, a lot of the work was centred around reflex action and this sort of thing generally, and particularly how the spinal cord is organised, of course.

PBCM: Yes. I was getting very interested in vibration, and I'd been interested in the stretch reflex, and so I was interested in both the stretch reflex from the spindle, and while I was there, because we were getting absolutely nowhere with the olive, I persuaded Jack to allow me to do some more vibration experiments. Now, I'd been very taken by the fact that vibration was such a powerful stimulus for the spindle primary and I was interested to study that in the decerebrate. In Canberra I set up decerebrates and vibrated the muscles and got a lovely tonic vibration effect in the soleus. It was very interesting to compare the power of that, with the power of the stretch reflex in the soleus, the TVR [tonic vibration reflex] being rather stronger than the stretch reflex in many cases. So that started off a new line there and came out of the spindle recordings.

But again, doubling back I shall do Dick [RB] Stein because before that, just before going to Canberra, I had worked with Dick. This shows the value of having a communal tearoom and a range of physiologists just chatting about nothing. Now Dick Stein was a very bright physicist by training who had come to work with Denis Noble. The work with Denis Noble wasn't getting far, but Dick was interested then in nerve firing. And the interesting thing about the spindle is that the regularity of the primary ending is much less than the secondary ending. The primary ending has a highly variable discharge whether or not it has gamma discharge activating it, even when it's just passive, whereas the second ending has absolutely metronome-like regular discharge. So, with Dick Stein we settled down and measured the coefficient of variability of discharge and showed that they really didn't overlap very much. Do you want me to say something rather fundamental, which shows how we worked out what we were finding in terms of information theory, how much information came from the spindle? But in a sense, you can carry more information in a regular secondary than in an irregular primary, which seems very strange, more actually. So that was another sort of string to the bow. That was my meeting with Dick Stein, and that got Dick Stein interested in spindles and reflexes, because he'd kind of been doing different things. Then, when he went back to Canada - he was American - he got a job in Edmonton, and settled down to do a vast amount of work on reflexes and spindles, and of course is still very well known today.

RWB: It might be worth pausing at this moment and mentioning that the secondary endings even now tend to be overlooked rather, don't they? You had an interest in them from the point of view of the responses and their roles in reflexes and so on, I've had an interest in them from the point of view of the numbers of these things in different muscles, and how different muscles are provided with them and so on. And they don't get the press that they deserve, I think, if I can put it that way. They're clearly an extremely important part of the entire system.

PBCM: I agree with you entirely. I think basically this is one of the clues to what the spindle is really doing. You have one sense organ sending two different kinds of signal out of the same place, and that's a huge amount of information relating what's in the primary and what's in the secondary, telling you what's actually going on there. All kinds of clever mathematical tricks you can do, which I'm sure the nervous system is well up to. But as you said, the secondaries are largely neglected because their reflex connections are all multi-synaptic and nothing so strong as the monosynaptic reflex, which has had far too much attention. So, I agree with you, the secondaries remain the great enigma of the spindle. The crucial thing about the muscle spindle is that it's not a position receptor, it's a movement receptor, it tells you when you're moving and two different coded signals of one and the same thing, it probably gives crucial information. So, I couldn't agree with you more. Maybe somebody else will do it in the future, now it's sadly unpopular. Rather few people left who actually set out to record these things. You need a vast amount of personal skill to be able to do the preparation and of course cats used to be terribly cheap to work on, and you had to use cats because they're suitably tough. But cats firstly became terribly expensive, and secondly of course became terribly unpopular to use.

RWB: Yes.

PBCM: I should double back on cats and experiments, and tell you what I used to do for a short time for demonstrations while I was teaching, it's really quite inconceivable now.

RWB: Yes, decerebrations.

PBCM: Yes, a crucial thing in understanding reflexes is the difference between the decerebrate cat and the spinal cat. Again, between a man with spasticity and a man just with flaccid muscles. So, I rubbed this in when I was talking about reflexes by doing a demonstration on a cat. This is quite unbelievable now. In the lecture theatre in front of 50 or 100 people, I would have a decerebrate cat, I would show it off, show how it behaved. I would then take a large dagger, go behind the screens, and cut off the head and make it spinal and show the difference again. So, the students were sitting in the room where I was turning a decerebrate cat into a spinal cat. Now I would probably be murdered for doing that. It had an educational point, but none the less I wouldn't want to do it now, even on my own moral scruples, because of course I like cats.

RWB: Well, I think all of us who've ever worked with them as physiological preps like them very much.

PBCM: Yeah.

RWB: So, you know we were always careful to ensure that they didn't suffer.

PBCM: I only did anaesthetised cats. They were anaesthetised and then, for most of my work, or made decerebrate for reflex work.

RWB: Exactly.

PBCM: I only worked on unconscious cats. Even so, when there were conscious cats, as Arthur Prochazka and people did, for implant recording, they didn't suffer.

RWB: Okay, now you've published a number of major reviews of one sort or another and the first one was influential on me when I started as a research student, and that was the *Physiological Reviews* one from '64.

PBCM: I've always enjoyed writing, which is really strange because I didn't enjoy writing at school. But when I had something I was working on, I'd find it interesting. '64 was really great fun to write. I'd been reading a lot. When I was working on the spindle, I read everything I could, so I decided I'd put it together. There had been nothing very much since David Barker's huge review some 10 years earlier.

RWB: Yes, he wrote a paper in the *Quarterly Journal of Microscopical Sciences*. Is that the one you're thinking of?

PBCM: Yes.

RWB: That was based on his own observations as well as being a review, certainly.

PBCM: It was a seminal paper.

RWB: Yes, indeed. But then in '64 of course we had this dual model, you having found the distinction between dynamic and static gammas and Ian Boyd saying that there were two systems of intrafusal muscle fibre, each with its own separate motor supply, everything seemed to fit into place.

PBCM: It was all so simple. Ian Boyd was a great simplifier.

RWB: And David Barker, of course, said, "No, it's not like that."

PBCM: Of course, they were at loggerheads for about 10 years. Great feature for the Physiological Society, the two of them turning up and contradicting each other.

RWB: They certainly livened up the meetings that I attended when they were both there.

PBCM: It was great fun. It must have been rather like before the war. Great controversies between Feldberg and Eccles on how synaptic transmission takes place, was it chemical or was it electrical? Their game reverberated for many years.

RWB: Yes, I'm not sure we have these sorts of debate any longer, really, are you? I'm not aware of any major ones.

Now what happened after you returned from Australia?

PBCM: I then came back and said I'd consolidate the spindle work. I had a research student, Alan Crowe. Crowe was a physicist and I was interested in getting as much information about what the spindle was doing mathematically going. Alan came and joined me. But actually, he was a man who was leaving physics rather than carrying it with him. He just became a physiologist. But we settled down and did much more spindle studies, studying the [gammas] in greater depth and with a greater range of velocities and stretching and so on. We published two papers which rather consolidated my earlier paper just saying there were two systems, and really established the two systems were functionally distinct. The really crucial thing being if you took one gamma, static or dynamic, and tried it on a range of spindles, it usually kept its type, absolutely pure

RWB: Yes, that was very important, wasn't it?

PBCM: It was.

RWB: And Alan Crowe in fact, after he'd worked with you, went to Durham and worked with David Barker for a while, or at least he was a lecturer in zoology for some years.

PBCM: Right. I don't think he liked it there very much.

RWB: He wasn't there long and then he moved to Holland, to Utrecht, I think.

PBCM: Yes, it shows how international and pan-European our physiology was then. You moved around without a thought. Now of course it's all meant to be [different] as things proceed toward Brexit.

RWB: Yes, well, perhaps we had better not go too far down that… [laughs] Certainly not while we're recording! Okay, we've got that and your interest in reflexes and so on. I started work as a research student in 1969, so your *Physiological Reviews* article was extremely influential on me. And while I was writing up, I think it must have been, you published your great monograph in the Physiological Society's monograph series.

PBCM: When Alan Crowe was working with me, I started writing the book then and carried on for another year afterwards. So that was a major undertaking, and again I enjoyed it very much.

RWB: And it continues even now to be very influential, I think. It's still considerably consulted, I'm sure.

PBCM: There's been no successor to it. There's been a lot of very good reviews, of course, [but no] large book.

RWB: Yes, you've got multi-volume things where people like Cuy Hunt and Archie McIntyre and so on, have covered different aspects, but there's no single monograph equivalent to yours certainly.

PBCM: That was a very nice thing to have done. Now, about this stage, I came into contact with Yves Laporte, who worked in Toulouse. Yves, of course, had moved into the spindle and done this beautiful work and he was really a very great Frenchman. And working initially in Toulouse and then later on in Paris. I visited Toulouse, which is a lovely city, and there of course saw a lot of him and his wife, who was a tremendous gardener and tremendously kind. And got to know Françoise Emonet-Dénand who was working with him then and stayed in the field thereafter, who was a very effective person but very quiet and actually shunned publicity of any kind. But he carried on working there and then went to the Collège de France. Again, this shows international collaboration.

RWB: I can certainly endorse all of that because I followed on some years later and went to Toulouse myself, just after Yves had moved to Paris, to work with Paul Bessou and Bernard Pagès. And then subsequently in the '90s, long after that, I often used to go to Paris mostly [to work with Françoise and with Julien Petit] She was a superb experimentalist.

PBCM: Yes, I went to Paris, I worked there. It was very interesting. It was lovely working with her, Yves himself tended to come in for the exciting bits, a bit like Jack Eccles, but he had much more inspiration than Jack. Françoise and I, you probably had the same experience, we did the work and he was there sort of taking a general supervisory part.

RWB: Yes, exactly.

PBCM: He was far too busy to spend time doing experiments then. But he had of course, when in Toulouse, had the time for that. He became a very prominent figure in French physiology.

RWB: Yes, and of course from my point of view certainly the interesting thing that emerged from the work you did then in Toulouse, was the subclassification of different kinds of fusimotor action.

PBCM: Yes, we decided to look at that more thoroughly. Again, a memory of those days. Margaret Gladden, who worked with Ian Boyd in Glasgow, went out to work with Paul Bessou. We were a sort of band of brothers in physiology rather than a band of enemies fighting each other. When she worked in Toulouse, Paul Bessou was completely taken aback to find not only Margaret but a rather young baby with her. She brought him with her. It was inconceivable to a Frenchman, but Margaret did it. She was a very effective person who combined family life with being a very good physiologist with Ian Boyd. Do you know that story?

RWB: No.

PBCM: Yes, she took an infant with her to Paul Bessou's complete consternation.

And there was Paris. The Paris lab was very nice. Actually, one of the things there - again how the world has changed - because I was interested in spindle variability amongst other things, I wanted to get some more records of these different kind of things. I took a tape recorder with me then. Of course, tape recorders you would actually play back and analyse them for detail. It had huge mass. So, I was able to take a night train, which went across the channel on a boat, all the way from London to Paris, carrying this huge, heavy tape recorder. Now of course there's no problem recording things in vast detail all over the place. In those days you had a good tape recorder to play back for signal quality. For long periods it had to be very large, reel to reel. Again, a remarkable change in physiological technique.

RWB: All very familiar to me as well, Peter, yes. And about this time you took on as a postgraduate student, I think, a great friend of mine, Manuel Hulliger. That would have been in the early '70s, would it?

PBCM: It was, yes. I got interested in sinusoidal stretching and wanted to do a detailed analysis. I started this with Dick Stein. If you apply small sinusoidal stretches to the spindle, you find very interesting properties of the muscle spindles, that's the difference again between primaries and secondaries. And if you're talking about a servo, the amount of phase advance, given response time signals, sinusoidal signals is one of the important properties. So, we wanted to work out whether gamma static and gamma dynamic had any difference on the phase advance of the spindle. It was a project with Manuel. Manuel had a very good mathematical background and was also very good at programming. Now in those days, I mean computing has just changed, as everybody knows, beyond this world. The work with Dick Stein on variability had been done on the university computer. This sat in a separate place, its input was paper tape, so all recordings with Dick had to be digitised by punching holes into the paper tape. Dick would scrutinise the paper tape and check the holes were okay. All the programming had to be done on paper tape. This had to be carried over to the university lab, played at odd times of night, and you got the result the next day. By the time Manuel had come, we had our own computer in the lab. This was a PDP-12, which was a large computer, which had I think, heavens, I haven't checked about this, 12K of memory. One just can't believe it now.

RWB: I know!

PBCM: It really was I think 12K of memory. And it was all done in cores then, you see. You had little ferromagnetic cores with the memory all in. And programming a thing like that really was the hard work. Manuel was totally adept with it. PDP-12, of course, this was going to analyse the variability of the data. PDP-12 had vast reel-to-reel tapes, which you had to store stuff in, and it also had a drum you could record on, which believe it or not, the drum was about 18 inches diameter and about an inch thick. And the equivalent to the drive in a computer now. I mean, it's quite unbelievable. And this cost me seven hundred pounds, I think, this huge magnetic disk that you would write onto the surface, weighing the odd kilogram. Again, how the world has changed is just inconceivable. Actually, to experiment then was like the early moon shots that were done with very crude computing, but they got to the moon. And we did physiology with these things.

RWB: Yes, you worked around the problems, didn't you?

PBCM: But it took much longer and was much harder work; now it's simple. And, as I said, I'd made a frequency meter with reciprocal interval display, it takes a huge amount of electronics. Now of course you just feed it in, digitise it and it's done just like that. It's the technique itself, everything is technique. As the techniques improve you can do more and more, and it gets easier and easier. But you have harder and harder problems.

RWB: Yes, I'm sure that's true. On the other hand, when you had to work around problems of that sort, or create your own equipment and so on, you got a much deeper understanding of the whole problem, I think.

PBCM: That's really true. You knew the instruments had limitations. Now you just buy your expensive bit of equipment and you never question it; you just accept what it says without a thought. If you made the equipment yourself, you wonder whether you could be giving the wrong result or something. And if you had written the program, you'd wonder whether [it's true] or if you had made a mistake. You were on the lookout for it.

RWB: Yes, you knew that there might be these problems and you were on the lookout for them, exactly. And so Manuel - I think he of course had done a medical degree at that stage, and he came to you after doing that? Then I think he went to Sweden?

PBCM: He then went to Umeå. What was very exciting at that stage was Åke Vallbo's recording from single fibres in Man, and he'd done that in Umeå. Obviously, if you wanted to know what the spindle was doing in Man you wanted to be able to [record] the way they were doing things. So, Åke produced these wonderful records and he lived and worked in Umeå up beyond the Arctic Circle, so Manuel went there to gain experience with him. He spent two years there or so. That of course was quite a life because to get exercise you went skiing and there were illuminated ski paths they always used to go on in the winter. There is no daylight at all. Manuel had a period there.

RWB: Right, and of course eventually he followed Dick Stein on into Alberta and ended up where he is now in Calgary.

PBCM: He went back to Switzerland for a bit.

RWB: He did, yes.

PBCM: [Ecke ? 0:30:46] was the head of the institute and he was very favourable to this kind of work. I think when he left, everything changed and the new people really weren't very much in favour of Manuel and he moved onto Canada. He moved to Calgary where he spent the rest of his working life.

RWB: Around about the time that you published your monograph, you were also I think elected to the Royal Society.

PBCM: The Royal Society was probably '74 but I'm not quite sure now. I was very honoured to get that. Basically, some subjects are in and some subjects are out. When your subject is in, you get elected relatively easily. When your subject is out, you don't. People don't get elected for that kind of thing now. They get elected for molecular biology. It's partly who the electors are now, of course. There were then quite a lot of physiologists, now they are all different, a new breed of people.

RWB: Of course, yes. I've never got anywhere close to that, but I did wonder whether it was necessary to have done one particular thing, let's say the work you did on distinguishing static and dynamic gammas. Would that have been the key thing? Or was it just…

PBCM: Basically, they want work of major distinction. It's not like a Nobel Prize, which has to be just one thing. It's basically the corpus of work, I think. A great sadness of mine [was] that neither David Barker nor Ian Boyd got in. I think microscopy was basically unfashionable and of course it hadn't come out clear cut, both muddied the water for each other. Together they did this really crucial work. I supported Ian, I put Ian up myself. Again, it didn't come through.

RWB: And the story that came back was that neither of them was elected because it couldn't be decided who had made the crucial contributions about the numbers of intrafusal fibres, I think. Anyway…

PBCM: Okay, now one of the other major things I thought was important was the effects of vibration affecting human position sense.

RWB: Yes.

PBCM: That was with Guy Goodwin and Ian McCloskey. Now Guy Goodwin was working with me on spindles and he got to know Ian McCloskey quite well and thought he'd introduce the idea to him. I think it was Guy who put out the idea we should look at vibration on reflexes. I'd already looked in cats, now let's do it in man. So we decided to have a look in humans using vibration - using a vibrator - which had already been done by the Swedes. We decided to have another look at that. So, we did a whole series. It was also controversial whether spindles produce any conscious sense or is just doing reflexes. Pat Merton was very strongly of the view that the spindle had actually nothing to do with sensation or consciousness, it was purely for automatic motor control. Of course, it's a huge job it does do. It also contributes to conscious position sense in the most general sense.

So, we settled down with a vibrator, deciding the position of the arms and in about 9 months it was very clear that vibrating the muscle produced all kinds of weird illusions of where your body is. We published a very large paper in *Brain* on that.

RWB: I remember that experiment very well, a beautiful experiment. Simple in its conception and absolutely clear in the outcome, yes.

PBCM: Again of course we didn't ask for ethical permission at all. I mean, we'd happily inject each other with local anaesthetics without any sort of medical supervision or ethic control over it.

Now, the really fascinating thing at that stage, one of the things we were interested in doing was local anaesthesia and we were doing it with cocaine. I think it was cocaine, not procaine. Anyhow, at one stage we wanted to buy a lot of cocaine so we got several hundred grams with no trouble just on a medical prescription and for a long time I had this in the lab left over, which was worth, God knows what it's street value would be. There were practically no controls in those early stages on it. If you were medically qualified, you could just write yourself a prescription and get it.

That was a very profitable period, done very quickly, all done in about a year.

RWB: Yes. But very, very influential and some of your most highly cited work, of course.

PBCM: It really is. It was great fun, and great fun demonstrating in public too. Now, one of the things we'd done was at a Royal Society soirée where Mrs Thatcher came and my wife elicited tonic vibration in Margaret Thatcher.

RWB: Really? [laughs]

PBCM: Really.

RWB: And what was Margaret Thatcher's reaction?

PBCM: She just took it, I think, I've forgotten. But it was really quite interesting that.

So that really was very influential to other people, and as I say, it made a wonderful lecture piece: pulling people out of the audience, doing it on them. And still raises today major questions about the body image and so on.

Now, Guy Goodwin used to say, he started life as a medical student and thought this is no way forward, everything is maths now, and when paired with me, he believed strongly in maths that he'd never do medicine. He then went back and did applied surgery at Magdalen and then through that went into medicine and qualified as a doctor, and then went on to become a psychiatrist of some distinction, and a professor of psychiatry. He had a distinguished medical career and [did] no more physiology. Whereas Ian McCloskey went back to Australia, had a very distinguished career, and became the head of the Research Institute and did a huge amount of work on vibration and other things, and also was very influential in getting Simon Gandevia into the field.

Simon Gandevia, of course, a very wide-ranging physiologist, still very active, studying both vibration position sense and sense of effort, and also a large amount of studies on fatigue. Whereas Ian McCloskey's career came to a very sad end. It sort of fizzled out and never got anywhere after [that]. He took on a wide range of administrative duties, and his marriage failed and after that he sort of packed up as an effective scientist, which was very sad. He gave up as Director at the Institute, just retired into being nothing. Very sad indeed.

RWB: Peter, the connexion seems to be breaking up a little bit more at the moment, so I think it might be a good idea if we stop recording for now.

[START OF PART 4: 22 July 2019]

RWB: This is picking up Peter Matthews' Oral History. So, Peter, I'll hand over to you straight away.

PBCM: It's perhaps interesting to talk about some of the personalities I knew. The two French colleagues I knew and loved very well are Yves Laporte, who I have talked about already, and Françoise Emonet-Dénand. Years later, about 1998 or something, I visited them both in the south of France, where they had properties, both long family homes. Yves Laporte had come from there and had this lovely property, which Beatrice spent her life gardening. Now they'd been dissenters, and dissenters couldn't be buried in burial grounds because they weren't Catholics - so they had a family morgue, and this was a house with a tree at the bottom of the garden where people had been buried. So, Yves Laporte had this sort of fantastic history of the past with people buried on his own property.

He also had a large hay field with a well in it, but the hay field was so overgrown that he couldn't find the well when he looked for it. Yves Laporte just loved the south, so did Françoise. Françoise had a family vineyard there which in the summer we helped with the pruning of the vine, getting the vine strung up onto the wires. Also, this of course was a Roman area and she gave me a bit of Roman pottery they'd dug up in the vineyard. She had this lovely house, all carved, with the wine vats there. So, there was this fascinating insight into the other side of her. When she retired, she went down and just lived there, running the vineyard, with assistance of course. So those are two lovely French people. That shows how important European collaboration was in those days. No formalities, no trouble. Just went across and of course we were all Common Market then, we belonged to the EU too, so there was no fuss or any trouble of any kind. Travelling was immensely easy.

RWB: Yes, my wife Gillian and I visited Françoise's property, which was just outside Béziers.

PBCM: Yes, that was it.

RWB: It was quite amusing, in a way, because we stayed in the main house and Françoise, whenever she went there, at least before she retired, just stayed in a little sort of - well it was an outbuilding I think. I'm not sure what it was originally, but she always slept in that building and never in the main house for some reason.

PBCM: We slept in the main house. I don't remember where Françoise slept. I do remember nightingales in the garden. No wonder the French are so attached to the South.

RWB: And the wine from the region is very good, too. When I was in Toulouse, I don't know whether you had this experience, but when I was in Toulouse in the late '70s, we used to have wine, local wine delivered, a bit like milk, to the lab.

PBCM: I'm relatively teetotal so I don't know about that.

RWB: Oh well, when we were doing experiments, we'd stay in the lab for lunch and prepare it in one of the prep rooms, and we always had wine with the lunch.

PBCM: Ha ha!

I'll add a bit to the physiology. Probably one of the most memorable things is the work on the Klippel-Feil syndrome. Klippel-Feil syndrome, you make movements on one side of the body and they reproduce on the other side. So, sort of mirror movements. We were very interested in the long-loop reflexes and how much they went via the cortex. And you have short latency responses, but when you stretch the thumb, you have short latency responses and long latency responses. And the point about the Klippel-Feil, I really haven't looked it up again for years, was that you elicited a movement on one side that also came out on the other. It was indeed a cortical connection going across. These people seem to have their cortical wiring tied up so one side of the cortex talked to both sides of the body and these long-latency reflexes did the same thing. So, the short-latency reflexes were strictly unilateral, but the long-latency reflex was bilateral, which showed it had a cortical component. So that was rather fun. It was done with colleagues from London who had a patient with Klippel-Feil. She came up to Oxford for one day and did all the recordings in half a day, that was enough to make a paper. It's not often one gets one experiment that makes a paper by itself.

RWB: Yes. I certainly recall the work that you did on long-latency, long-loop, reflexes, an area which was outside my field, but I'm not aware of many other people following up on it.

PBCM: No, things seem to have gone quiet since that experiment.

RWB: Yes. In so many areas, you've been influential in breaking the ground as it were. In some cases, these things have been followed up by others, but in others shall we say they're sleeping until somebody else picks it up again.

PBCM: Basically, with new techniques and advances in genetics and molecular biology are taking over [physiology] fascinating stuff we never dreamt of is getting down to the molecular level. It's what's happening in the spindle. We've been so productive in finding drugs which interfere with them. And this has basically swept all the older approaches away.

RWB: Yes. It's interesting from one point of view, which some people listening to this might like to know about, and that is you and I both did a lot of our work before the Human Genome Project got underway. It was relatively easy in those days, if we needed it, to get grant money from the Medical Research Council for our kind of physiology. And then along came the Human Genome Project and more or less overnight, everything shut down for our sort of research.

PBCM: It was. And the sad thing is whereas we appreciate their work, they don't appreciate our work.

RWB: Yes, there was a large element of that, I think, at the time.

PBCM: Things aren't going anywhere. Basically, if you want to deal with human beings, you have to deal with the whole of the human being and know how things are actually working. The chemistry alone isn't enough.

RWB: Guy Bewick and I started working together around 1990, I think, or '95, something like that. And for a long time, we were really just depending on money we got for project students and this kind of thing.

PBCM: Good heavens.

RWB: And then there was a change of regime, a bit later than that, in the Medical Research Council, when Colin Blakemore took over as chief executive. He reinstated response-mode funding, which had been abolished for some years, at least for individuals anyway. Guy and I put in a proposal to the MRC to do some of our work on what we've called synaptic-like-vesicles and that was immediately funded. So, the circle turned round a bit, I'm happy to say. And now, of course, we are making use of genetic information ourselves in some of our own work.

PBCM: We were actually relatively cheap and these new chemicals, seem to me, eye wateringly expensive.

RWB: Yes, that is true. And of course, one of the great things then was, although we did collaborate across labs across countries even, when we did experiments, they tended to be fairly small affairs, you know? Sometimes ourselves alone, sometimes one or two people.

PBCM: Personal, one had to do it oneself. One had personal skills. Whereas now it's all about hiring a team. It always used to be: you met a DPhil student, give him a project, and leave him alone to be by himself. Don't pay any attention at all.

RWB: And one of the great things then, of course, was that you had to understand everything about the experiment that you were doing. These days, if you are collaborating with let's say geneticists, it isn't always easy to have that sort of first-hand feeling, is it? You know, on all aspects of the work concerned.

PBCM: The amount of authorship has gone up and up and up. The number of papers published by people by themselves has gone down to nothing. So, you have this huge team of people, all contributing different things. And as you say, not even understanding each other necessarily. It's a very different world.

RWB: Yes, it is indeed. Now, we've been talking about the work over these recordings, we've been talking about people. Are there any things you want to end on? Do you have any final stories you want to add to our recording session?

PBCM: Just that technique is everything. There's been techniques to prove the thing you're doing [is] the key. It's like warfare: if you advance technique you win; if you don't you lose. It's all entirely technique.

RWB: One of the things that has occurred to me in the years I've been working - well of course it's been obvious in a way, trite almost - you have to keep an open mind to things that surprise even you. You weren't anticipating them when you started out on something. So it's one thing to design experiments where you anticipate what the outcomes might be, but it's also extremely important to do the sorts of experiments where you really just wonder, "And now what, what might happen?" And you are willing to be surprised by the outcome.

PBCM: Yes, precisely. And now you're making a grant application in which you virtually have to predict the result. I think the other thing I've been impressed by is the work of the early pioneers, how much they did, the beautiful things they achieved with really very simple means.

RWB: Yes, I certainly agree with you there, Peter. And actually, maybe that's a good point to add a final sort of coda, which is to recommend to anyone to read some of the early work in their field and they might be surprised to find that the early workers were just as sophisticated as they are, they asked all the questions that we ask, it's just that in some cases they didn't have the technology necessary to answer those questions.

PBCM: And where they struggled with instruments, they made their own.

RWB: Yes. But they certainly asked all the questions that we're still asking, I think.

PBCM: And the early microscopists, the way they set the scene by describing all these beautiful things there to be looked at.

RWB: Yes, if I can be allowed a recollection there: when I look back at the work that David Barker, my mentor in Durham, did in the 1940s, using silver on the slide staining and reconstructions from serial sections of silver on the slide, it's actually astonishing that he was able to get anything near to the final answer.

PBCM: Yes, it is.

RWB: Well, Peter I think I might stop recording now, if you're happy with that? And then we can chat a bit more.

[END OF TRANSCRIPT]