MR: So, if I start and ask Tom to go right back to the beginning – your early days, your family, early influences, place and all that kind of interest.

TS: Well I was born in 1928, my father was a tailor, but particularly a cutter, and he worked in Moss Brothers in London. He was a specialist military tailor and was able to look at someone’s shape, such as Black Rod, so whenever Black Rod needed his own uniform, they would come to Moss Brothers and my father could look at a person and transform the three dimensional shape, as tailors do, into the tailor’s pattern and I often think now when I like visualising things in three dimensions that that relates back to him, anyway it makes a nice little story to start with.

So that was his ability, he had no special education, and of course he survived World War 1 and he was in the trenches for the whole period.

MR: What was his name?

TS: His name was William Henry Sears and he was extremely good with his hands, and used to make suits at home for one-off customers, so we had this very large table in a small
back room in Catford in South London where we lived in a late Victorian house, and that was a big cutting board, on which he laid out his patterns.

**MR:** So you learnt how to use very large scissors?

**TS:** Very large scissors, they seemed to be almost as long as I was at the time and he would draw these shapes with the tailor’s chalk and cut round and then do all the preliminary stitching. I remember all the multiple layers of cloth and canvas and stitching that went inside a properly made gentleman’s suit, to create the shape, and those suits literally lasted 20 or 30 years. When I go in and buy off-the-cuff suit, it always grieves me when I feel the cloth and I feel no internal layers.

**MR:** Can you judge the weight of a material?

**TS:** Yes absolutely.

**MR:** In those days they were much thicker, weren’t they?

**TS:** That’s right, thicker materials, and they almost could stand up alone and look like as if there was someone inside them, and that was a tailored suit, truly. Anyway, that was my father; but he was always very good with his hands. He had no education, but somehow he would read about things and one of my early recollections of the things he made was a crystal set, and he had obviously picked this up in some magazine or newspaper, and he bought the battery, he bought the wire and wound the coil and created the place for the crystal and then had this crystal set.

**MR:** Did he learn that in the First World War?

**TS:** Well, no, he would have learnt that afterwards I think, and what was the next thing? … he liked photography and he made his own enlarger, beautifully with joinery and somewhere probably in Soho somewhere he brought this large lens for the condenser, and he used an old tin from a sweet shop I think – one of those really big old tin canisters, and made this enlarger, and so he would print his own photographs.

**TS:** It’s like early days in physiology, when one did all one’s own printing and developing, and then after World War 2, he was utterly besotted with all the shops in Charing Cross Road, Tottenham Court Road, the Soho area, where all the wartime gadgetry – range finders, ex-government surplus oscilloscopes and so on, in Lyle Street. I told you this before, I think, Peter, but he would come home one evening and say “I saw this marvellous thing in Lyle Street, or wherever. I don’t know what it did, but it looked very important and I would love to have it, but it was too expensive” – something like that. He would just be
utterly besotted with these gadgets, which he didn’t actually have any idea what they did, but they just looked important. So that was father, but it was interesting, just jumping forward, when I first went into the lecture theatre in University College – the physiology theatre – of course there in the corner, was the ex-radar equipment that Bernard Katz used to do the demonstrations. There was a lovely course of lectures, which Fatt & Katz did on biophysics. In the corner, that turned out to be the same basic radar set-up that I actually worked on when I was a radar fitter doing National Service in the RAF, so I felt quite at home, but as soon as the theory started of course, I was less at home, but at least I knew how to operate the oscilloscope.

That was jumping forward, now jumping back

MR: Do you remember your grandparents?

TS: Yes. I only once or twice met my grandparents on my father’s side. I think they were always moving, and it was always said in the family that...I think they were connected with pubs or something, but they were always doing runs, through unpaid rent, I believe. Perhaps my wife won’t like me revealing that. Anyway I met my grandmother once - she was on that side, father’s side. She must have been 80 or so and was blind, and there were two sisters, but I never saw or heard anything more about them.

MR: But they were all South of England, Londoners were they?

TS: They were, certainly Londoners, I would say, but their exact origins I never knew. My mother was an Irish Catholic. She came to Britain in about 1921, and her father had been a Police Inspector in the Irish Constabulary, so the family was very caught up with the troubles and anything to do with the troubles, both when she first came to England and subsequently, of course, in the last decades before she died. She was very upset by anything to do, naturally, with the IRA, because the family were poised in the middle, the father being a sergeant in the police. She would have witnessed all the problems with the black and tans in Dublin.

MR: So she came as an adult or...?

TS: She came, I think, as a 21-year-old, and she was an accountant in a restaurant in ... it’s called Whitfield Street, just off the Leicester Square area?

PK: It’s off Tottenham Court Road – Whitfield Street.

TS: Oh well then, I’ve got that wrong... this one was just off Leicester Square and my father was like so many at the time, very nervous - I suppose a sort of kind of shell shock and this
Irish colleen had captivated his heart and so they married. So we were, in fact, then because she was a devout Catholic, we were brought up as Catholics, but from about the age of 15; well, I will come to that later.

So that was the early beginnings. We lived in Catford, and so the things I remember from Catford were various relations coming. My mother had eight brothers and one sister and the youngest of them became a priest in the church, having trained in America, and many, many years later, when I went to the International Spinal Research meeting in Florida – he had run a Catholic school on the west coast of Florida, and he came – and after the meeting, we met at 8 o’clock in the evening and then had a meal and drank until about four o’clock in the morning. At six o’clock in the morning, I then went off with various neurologists – Peter Rudge. This was later, of course, and the others, Peter Rudge being a great twitcher. We went off to the Everglades in a hired limousine to look at crocodiles and birds and the like. So it was very funny, so it was on that occasion …and I saw Peter recently and reminded him of that occasion … so that was Uncle Anthony.

MR: Anthony Sears?

TS: No, Anthony McGowan. The family name was McGowan.

MR: Oh McGowan, which part of Ireland is that?

TS: From Dublin. So, over the years one, of course, met most of that side of the family. In that period, before we moved to Beckenham, where I live now, in 1936 I think it was, I do remember with Uncle Anthony who happened to be visiting from America, Uncle Paddy who lived with my grandmother, who lived by Olympia, and going to the front of the house and looking up at the sky and seeing a zeppelin.

We moved to Beckenham in 1936, and I think it must have been that year – I was eight years of age at the time – I came out to the kitchen to put the kettle on for tea for my mother and father and I stood at the kitchen door and looked across from Beckenham towards the Crystal Palace area and saw this great flame and smoke going up, and that was the burning of Crystal Palace, an incredible sight. Of course, all the family came out and everybody watched, because we were on a slight promontory, so we had this extraordinary view. The whole thing, as you will know, was burnt down, but what was left was two towers. And when – I think it must have been 1940, maybe earlier – one tower, the one closest to the road in Crystal Palace, was taken down piece by piece, and the other tower – at the time Lord Haw Haw used to mention when we were going to be bombed that night and if you heard Lord Haw Haw, he also mentioned various places – and I think for that reason, Crystal Palace, which was an about 350 feet tall tower, was regarded as … the blackout was deep and impenetrable … so Crystal Palace tower would have been a good navigational aid and so they blew it up. So that was an incredible sight. Everybody knew at 11 o’clock it was going to blow up and the explosives had all been put in at the right place, so we saw the whole thing. First we saw the flash, then smoke and
An interview with Tom Sears – part I

dust beginning to emerge. It was awfully like the twin towers, the way the building collapsed. It started leaning over, and then it collapsed within itself, and a great cloud of dust went up. So those are memories from early days.

**MR:** Were you evacuated during the war?

**TS:** No, no.

**MR:** So you stayed during the war?

**TS:** Yes, and so I then went to a local school, local junior school it would have been.

**MR:** You have a brother?

**TS:** Yes, I haven’t mentioned him at all, have I? That’s strange. Well, you started with parents I suppose.

I’ll back-track then. My brother and I went to the local junior school in Beckenham.

**MR:** And he was a twin?

**TS:** Yes, he is a twin, yes, non-identical. Of course, I always suffered from it for decades afterwards, because he came out five minutes earlier and my mother always said he was a sickly boy, unlike me. So whenever things went wrong, Pat being also the first – perhaps it was the Irish side? I have just realised because the first-born male of course inherits all, doesn’t he?

**MR:** Well, a couple of minutes earlier?

**PK:** Has it shaped your entire view on life, do you think?

**TS:** Oh yes absolutely!

**MR:** Peter, we had better get the voices organised. Are you going to speak first Peter?

**PK:** Yes, well, all right. I think we ought to hear more about school days and influences: something made you into a scientist. What were the influences of school, do you think?
TS: Oh, early influences of school? None, really because this junior school that we went to was fine, but I didn’t pass the 11 plus. At the time, there were three categories of school. There was the grammar school, that you went to with the 11 plus. They were really four categories – there were technical schools, where you went to learn obviously woodwork, metalwork and the like. Then there was another category called the central school.

PK: It must have been before ’44 Education Act …

MR: This would be in the early 40’s wouldn’t it?

TS: This was 1943.

PK: Oh right.

TS: I think you are thinking of something later, because it was wartime.

PK: Yes

TS: So I went there at the age of 11, to the St Balgowan Central School in Beckenham and for the actual schooling, we had six teachers and six subjects, but no science at all. So we had History, Maths, French, English, Geography and Art. I think those were the subjects.

I got a distinction in geography, I actually had sight of - they actually took a school certificate of some kind, I think it was the Oxford Cambridge School Certificate it was called, and I meant to check that. Anyway, I had a distinction in geography and of course, during the war, so many elderly, at least 50 years of age – elderly people came out of whatever they were doing to make up for the teachers who...

MR: Often World War 1 veterans.

TS: That’s right. So we had this very old geographer, and I used to always be in trouble with him and being sent to the headmaster. Somehow, I wasn’t a cruel boy, I don’t think, but somehow we always got mixed up, and when the results came through and I got this distinction, I went to this man and there were tears in his eyes, literally he was so pleased. All was redeemed and curiously, I got a high mark in the maths, you won’t believe that will you Hugh, or Peter for that matter! But when you think, when you stopped at the age of 15, we had just done logarithms. We hadn’t done any calculus. I hated logarithms, because they were actually taught us by the French teacher; he was a francophile and of course, there were very funny mixed feeling during the war and he was always
An interview with Tom Sears – part I

downplaying English and British things. He was a francophile to a fault, and when we got to logarithms, he relished in telling us, now it was going to be really difficult, so I think that certainly set me in my ways apart from absent areas in the brain. I left school with just logarithms – that was the level.

PK: That was in Beckenham, and you have really lived in Beckenham ever since?

TS: No, no, no. But let me finish the 1940’s. 1943 was when I left school, so it was in the middle of the war and my father had fixed me up with a job near Moss Brothers in town, in a lens grinding factory, but I was taken there to be shown this place...

MR: Was it in Hatton Gardens?

TS: Yes. It so appalled me – I had to tell him over the weekend there was no way I could work there, but it was very strange.

MR: Were they scientific lens or...

TS: I think they were scientific lenses, yes. So then I had actually lost my middle finger that year. We had gone to a farm in Norfolk for two weeks holiday and while I was there playing with farm machinery, I got it caught up, so I lost my middle finger which was taken off in the hospital in Bury St Edmonds, so I always call it Bury St Thomas when we go through and the family always say, “Oh dear, not that one again”. Anyway, I pottered around for about six to eight weeks and then my mother said, “You can get a very good job at the Welcome Research Laboratory”, which was in Beckenham. I think I phoned them up and of course I was interviewed, but it turned out that this recommendation about the job was from … and then I learnt what this lady did, she was one of the bottle washers in the penicillin …they were making penicillin there during the war. So she was a bottle washer. In a way, you could say, now, that set me on my scientific career.

MR: She wasn’t a cook as well, was she?

TS: No, not as yet. So that’s how I went to Burroughs-Wellcome. That should allow me just to back track because we haven’t discussed my twin brother. We had a very happy childhood together, though he was much taller and whereas as I ended up at 5ft 10, he was 6ft 3, and that difference was there always. He was always trying to get the better of me and wrestling and things like that, but I was always, I think, a bit stronger. He also hadn’t passed the 11 plus, so, as was normal then, he left at 14 and went into estate agency and ended up with his own company and then a partner in the Nationwide Estate Agents and did extremely well and writes well, and so he found his path, as I found mine.
MR: Are there the two of you, no other brother or sisters?

TS: Yes, an elder brother, he was five years older, and I will come to that in a moment for a slightly different reason. So I ended up in the Pharmacological Research Laboratories at the Wellcome Foundation under a Dr Thorpe, directly. Dr Thorpe had done evening classes himself and became Head of this group, but what was surprising, and I think all three of you will appreciate this, through your own interest, as it is in electronics, is that he was actually building an EEG machine. Now this was 1943, when I joined in September. Do you remember the Gardiners stimulus isolation transformers? Did you use them at UC when you first ... or had they already gone out of fashion? Well, some very important isolated stimulators, developed by the man who became the Professor of Physiology in Sydney...Bishop, who worked in biophysics for many years. He developed the first square wave stimulator, and I certainly used it at UC. Why did I wander to Bishop? I lost my way... yes, I have forgotten – I have missed something out. Anyway, Roland Thorpe was developing his own...I've got it...his own EEG machine, as he realised then that the study of drugs acting on the central nervous system would play a big role after the War. He was very imaginative. For the couplings, because of the gain levels being used, and the quality, I suppose, of the coupling condensers, he was using transformer couplings, and the transformers were made by Gardiners and they were the very good isolation transformers that could transfer quite a long duration of pulse to give you the stimulus isolation and they were in use. I saw a reference to one still in use the other day, believe it not – so that’s Gardiners transformers. So I had to learn how to wire up the chassis and of course the chassis was half the size of this table with all the valves – EF36’s.

MR: Oh, yes, I remember those.

TS: Now I might even remember the number of the power supply, which was a dark dome.

MR: They were pentodes, weren’t they?

TS: Yes, pentodes. Oh well, we are on memory lane now, Martin.

MR: Well, I used those when I made my schoolboy tape recorder.

TS: Ah, right. So everything was heavy duty – the electronics in the sense of cabinets and all the wires for the filament currents and so on. Vast numbers of amps were used. Well, you would have seen both of you probably, John Gray’s set ups, did you ever overlap with John Gray?

PK: Yes
TS: You did, but not Hugh.

PK: And George Dawson as well?

TS: Yes

MR: But used EF36, for cathode followers, didn’t we?

TS: That’s right yes, so I had to learn how to solder and doing that, although I had no electronics, I just for other reasons began to learn about electricity. I built my own radiogram, as they were called in those days.

PK: So what you were doing was at Beckenham was mostly...

TS: Oh no, no. That was just that was when I wasn’t doing other experiments. No, I was mostly the lab boy, a technician.

MR: You were in your teens still then?

TS: I was 15. I will just finish the EEG. That was quite an incredible thing for him to do, and years later, when I went to Australia, we went to visit him, because he ended up as Professor of Pharmacology in Sydney University. Anyway, my very first job was, I was taken by this chief technician. He was all of 21, I think, at the time. He had been exempt from military service, because this was a key job at the time and he was very much a ladies’ man, we’d better edit that out! He was also a sort of bully, and he took me into this room...

MR: He is no longer with us, is he?

TS: He is actually. He took me to this room which turned out to be a thermostatically controlled room, and there were 12 boxes lined up and these were rabbit restraining boxes, so the rabbits were all lined up with their heads sticking out though the neck holder. He said stick those thermocouples in their arseholes – and I hardly understood the instruction.

MR: None of the words I suppose!

TS: And not having done biology. But I’d heard a dirty joke. And the key word in the joke was top hole, so I knew which orifice to put the thermocouples in, so that was the beginning
and that was my job for quite a while. What were we doing? We were getting the samples from Britain’s secret weapon of the war, which was penicillin, from somewhere called Mill Hill – no, the Medical Research Council. They had these mysterious numbers and letters, MRC and then the name of the sample, the batch sample.

**MR:** Did you have to sign the Secrets?

**TS:** No, I think they trusted a 15-year-old.

**MR:** And did you have to start early and work weekends?

**TS:** No, I just did an ordinary day from 9.00 – 5.30pm, Saturday mornings as well. That was the normal working week and so I did that, and I just had to track their temperatures over about six hours’ control period, inject and then track. He had also devised another instrument of his own, to read off the temperatures, so that was my first. The second job was to stun rabbits and kill them, and then take out the duodenum, mount them in an isolated organ bath, and make up the Ringer’s. It was all totally without any scientific knowledge, and then weigh out the substances and then test using morphine as...no, there were anti-spasmodic drugs and I had to use among other things, morphine, to compare these drugs, and then, another preparation I had to learn was the isolated rat diaphragm. So, you stunned the rat and took out the diaphragm and the nerve and put it in a bath. That was similarly to look at a variety of drugs that might or might not affect it.

**MR:** So it was like an apprenticeship?

**TS:** Oh, completely, but then I realised I had to do something with my life, so I got books together and through that, wiring, and trying to understand about electric current ... during the war you could take the matriculation exam in parts, not all at once, as you did before the war, so I taught myself electricity and magnetism. I couldn’t take on physics as a whole, so I taught myself electricity and magnetism and chemistry. The chemistry was helped by going to Norwood Polytechnic in the evenings and so I then took, I suppose it would have been in 1946, the matriculation exam and got part of it – the French, the maths and the English. Then I took it again and got the electricity and magnetism and the chemistry. So I now had a matric. Then my older brother, who was five years older – at 18 he had volunteered – and was a clerk in Beckenham Town Hall. He had volunteered and went to Canada for training as air crew and came back as an observer and I have never forgotten walking along upper Elmers End Road feeling very proud of my brother in his uniform and his wings, telling him that I had got a matric. I don’t think he had matriculated – and one month later he was dead – a flying accident of some kind. I have never forgotten, really, coming home to find the door open and wondering where my mother was. She was next door, being consoled, because an hour before she had the telegram. It is such a bare thing, the telegram you know, which said, “The Air Ministry regrets to inform you ...” and I read this and I couldn’t believe it, so we had all of that. Then I started ... I would have been sixteen.
MR: At what point did Otto Hutter appear?

TS: Well, Otto was in Dr Thorpe’s lab and he worked the in the adjacent lab, but all under Dr Thorpe’s group and he was, as you well know, a refugee from Germany...

MR: So he was older?

TS: He was older; I would say by two or three years or may be more. I don’t know.

MR: Was that right from when you entered Beckenham?

TS: Absolutely, yes, from 15, and it was his wife to be who actually first ... that was the other job I did – I did all sorts of jobs – I had to go and take blood samples from about 24 rabbits, prick the ear and put them into the test tube with some reagents. Then they came back to the chem. lab in the group, and his...forgotten her name...

MR: Yvonne.

TS: Yes, Yvonne. They actually lived opposite us in Abbott’s way in Beckenham, so we knew that family and she would do the Hagedorn & Jensen – I still remember the names – tests for blood sugar.

MR: And he was employed on the same basis as you?

TS: Yes, well I suppose so, yes. We were the lab technicians. They were more senior, of course, and at the time Otto must been doing his Honours BSc at King’s.

MR: I suppose his English wasn’t as good?

TS: No it was good. No difficulties there. So yes, I did a variety of things like that and gradually... another thing I did, I had to resurrect a Starling ... I’ve forgotten the proper name for it now – it was the perfusion pump for doing isolated hearts or perfusions ....

MR: Langendorf
An interview with Tom Sears – part I

TS: No, no, this was on dogs, we did a lot of research on dogs, so I had to go and anaesthetise the dogs in the lateral hind leg vein. I can still see it in my mind’s eye and inject, put the dog down and bring it to the lab and then Dr Thorpe or Sellers would operate on it, and then we did isolated hind leg perfusions. We did isolated perfusion of the heart, and I had to clean and prepare the defibrinator, and also it was my first introduction to condoms by the way, innocently by the way.

MR: Good Catholic boy.

TS: Yes, good Catholic boy, because the condom was used for the source of the ...

MR: But you didn’t cut the end off then?

TS: Not like Leonard Bayliss did. No. You know that story, obviously.

MR: Otto used to speak of a Miss Müller.

TS: Miss Müller was the German lady who actually was in charge of Yvonne and Otto was slightly to the side, he worked under another, a Dr Brock. He was a biochemist. Miss Müller was a very nice lady, but equally she had come from Germany as a Jewish refugee and she always spoke of ‘when I was in Hamburg’ and what she was referring to was the clinical biochemistry lab, which was in Hamburg. It was she who told you off if you hadn’t got the right blood sample.

MR: A real continental.

TS: Yes, but she was very kind and also lived in Beckenham. In fact, after the war she found her brother had survived concentration camps and, through the Red Cross, learnt that he was in Sydney and she used to go out occasionally to Sydney, and when she did, we used to look after her rubber plant. She was devoted to her indoor houseplants, so we had to do that and then unfortunately she gave it to us one year. We kept it going until she died. She lived in Sydney, while Thorpe lived in the Blue Mountains, and they kept in contact with each other and eventually Blanche and I, Blanche being my wife, we met them both in Sydney – Thorpe in the Blue Mountains and she in her flat in Sydney and so that was...

PK: So this was obviously the start of a science career. What about school friends or friends outside work?

TS: Well, the war time was very strange thing, and one’s main pre-occupation was the youth clubs, and I was also very keen on sports and joined a men’s cricket club at about the age
of 15 and was a bowler. I also played tennis and one’s friends were largely in that environment. The school friends, because of the war, so many of them dispersed and I remember the friendships, but certainly in my eventually going to University and living in London, there were no friendships of any enduring kind.

PK: You used to talk about Bob Monkhouse, at one point?

TS: Oh yes, that was a… the Youth Clubs were very important, a social, usually once or twice a week, always Saturday. My girlfriend for a couple of years was a technician at the Wellcome, and in fact, she became ultimately, a physiologist herself and is a member of the Physiological Society…she can be nameless for this purpose. I was very jealous at one stage, because Bob Monkhouse was a member of this youth club, Elm Trees Youth Club, and he was a great smoothie, very clever, and a brilliant cartoonist, and earned a lot of money even before he became known as a comedian, he earned a lot of money drawing these cartoons. They were published as well. Of course, his parents were Monk and Glass, the custard people, so a very different background from little Tom.

PK: He was a competitor for this?

TS: He was. But in fact it never came to anything, but I was just jealous whenever he danced with her, or...

MR: Was he funny?

TS: Oh yes, you know, we were all good friends at the time. Oh yes, he was funny, and when we had our club socials, he would be one of the star turns – he was very funny. My brother...

MR: But he wasn’t a friend, he was just a ...

TS: A friend in the club, yes. It began and ended there, I suppose. My brother was a very funny storyteller and he used to compere all the club socials, but was blackballed from one. He did this in several churches, because he nearly had a stage career. He was the entertainments officer. In a moment, I will tell you about the RAF.

MR: Is that where the performance side comes in?

TS: Yes, I think so, and it was fascinating. He got blackballed from one church, because he told risqué jokes as part of his compering, so, anyway, are we going on too long?
MR: No, I am guessing when to turn the tape over, that’s all.

TS: After the matriculation, and so the youth club was then the entertainment and I played cricket for this cricket club in Beckenham, and that was one’s life. Then in 1944, would it have been, yes, ’44, of course, that was the doodlebug era, because the bombing had gone on throughout the war. But in particular I was at work, and I had this telephone call from my twin brother, who had come home from Croydon, where he worked, because we had a doodlebug on the house next door but one, and so the whole house was demolished except for the ground floor. My mother, who had answered the door to, in fact, one of my school friends, Raymond Arrow, and he had come home from his school, and I was at work and there were a lot of doodlebugs going over that day and we had six in Beckenham itself. He had got frightened and so he had knocked on the door and said could he come in. We had an indoor Morrison Shelter, so they went under the shelter and another doodlebug came over, cut out and landed next door but one. They weren’t hurt at all – the Morrison had metal grids around the sides, so...

MR: This was in your garden?

TS: No, it was in our house, in the drawing room. The Morrison was a metal table, so we had that metal table instead of a wooden one. We had an Anderson type of shelter...

MR: Oh, that’s in the garden.

TS: In the garden, so I came home and so then for six weeks, we lived in the underground shelters in Elmers End Green, just living out of suitcases, and then we got billeted in a house for a short while.

MR: The underground shelters, were they the...

TS: No, these were just the underground shelters that had been built in all the local communities during the war, and this one was under Elmers End Green, so we just lived there and ate in the British Restaurant which was outside, or a café, or whatever, then eventually, we were settled in a house, in fact, in Kelsey Way, close to where I now live, in a great enormous house, which was palatial in a way, and we lived there from, when was it, 1944 to I think 1949, when our house was re-built as part of the post-war re-building.

MR: Oh, your house was damaged as well?

TS: Oh, yes, it was demolished and only the Morrison shelter saved... the whole lot was down and all there was were remnants of the upper floor, the chimney stacks, and I had to go
An interview with Tom Sears – part I

around – no-one told you it was dangerous and it was dangerous, trying to retrieve things.

MR: Did you look for shrapnel in the street?

TS: Yes, we used to collect shrapnel, but we were collecting what we could recover from the debris. Most of the furniture was useless, so then, as I say; we picked over the remains and got out bits of furniture and clothing, in particular, that was the most important thing. But most stuff was very badly damaged. Fortunately, my mother was fine, as was the boy, Roland Arrow. I actually had a feeling that we had been bombed, because in the lunchtime at the Wellcome, we used to play cricket and one knew the orientation of the mansion, and we actually had to run off the cricket field, because at the bottom of the sports field, they kept all the horses where the serum was taken for preparing diphtheria toxin, and we’d rushed down into the shelter, and that doodlebug landed in that field where the horses roamed. As I looked along the line of the mansion, and our house would have been, I don’t know, probably three quarters of a mile away, a mile away, and there was this plume of smoke and I thought, that must be in the Elmers End area, and just left it at that. It was an hour later when I got the telephone call. So that was the event for the rest of 1944. I think I must have matriculated by then and so I started on what was the equivalent to ‘A’ Levels, by going to Norwood Poly.

MR: General School Certificate?

TS: It would be the equivalent to the Higher School Certificate.

MR: Oh Higher School Certificate, that was University entrance then, wasn’t it?

TS: In principle, yes. I started doing biology for the first time, which was zoology, and chemistry, and physics, which I found very difficult.

PS: So this was at night school, while you were still working at Wellcome?

TS: Yes, every night I went to Norwood Polytechnic, and then...

MR: Did you meet anybody there that you kept up with?

TS: No, I’ve got this wrong. I’m sorry ... after the matriculation, I then, when I was 17, started going out to Chelsea Poly every night of the week after work, five nights a week.

MR: Manresa Road.
An interview with Tom Sears – part I

TS: Yes, Manresa Road.

MR: Yes, I went there

TS: There was actually a physiology ‘A’ Level there, run by the chap whose name might come back to me. He went out and became Professor of Physiology in Tasmania, where Norman Saunders eventually became Professor. You could get the name from early Journal of Physiology papers.

MR: Not the Belgian chap?

TS: No, no, a very English chap.

MR: Did you do any other subjects there?

TS: No, no, that was enough!

MR: No I’m just asking.

TS: Yes, so I did those three subjects, physiology, physics, and chemistry.

MR: Dr Dippy?

TS: No, not quite – it’s nearly there.

MR: The chemistry man was...

TS: Oh, I don’t remember the people. So that was very hard, because it was a train up to London and then a bus across to Chelsea, and I used to get home about 10 or half past ten every night, and I did that for a year, and was wondering about how to continue, but then I got called up – I was 18 – for National Service, so this was in 1946.

MR: Did you come across Elizabeth Ullman there, because she was...

TS: No, no. It was 1946 I got called up, and had to take a test, the Trades Test, it was called, and my electricity and magnetism turned out to stand me in good stead, so I got very high
marks for that – I don’t know anything about the other aspects. Because I wasn’t offered some material, because of the lack of education, and probably other things at the time at the interview …

PK: Tell us more.

TS: A few dropped ‘aitches’, I think, I went into train as an AC2 radar fitter, so I spent the first year down in Yatesbury, after the ritual eight weeks square bashing at Padgate, which is up near Liverpool, and I nearly thought my career was ended and I would be shot at dawn, because, in the middle of the square bashing, we moved to Melton Mowbray, and on our way from Melton Mowbray subsequently to Yatesbury, something had happened on the line and we were all called to get up quickly and get ready, and I when I went to get my rifle from the luggage grid, it fell out of the open window. Of course, the idea, especially for a rookie recruit, of losing your rifle, I felt I would be shot at dawn. It was retrieved, and I didn’t get any charge or anything. I was very lucky, I think.

MR: Did you have ammunition as well?

TS: No, not at that stage. It was post-war as well. So I went to Yatesbury in Wiltshire to do the radar course, and of course, we all knew about electricity. I was there in 1947, which is, in recorded history, one of the most severe winters that Britain had in many years. There was a lot of meningitis on the camp and people were nearly rioting. The conditions were intolerable, but in our billet, they weren’t, because by getting a metal bucket and a tin can, and suspending the tin can from string across the bucket, and connecting the mains to it, we had a supply of hot water in the evenings for making tea, but fortunately, it was out of sight.

MR: You invented the electric kettle, did you?

TS: Yes. One night the officer came round. He was a real prune officer – fat and goggle-eyed. I mean, it’s an awful thing to say now, but that’s how we all thought of him at the time, and nobody liked this man. He came round and begged for everybody to turn off lights, because the oil in the transformer at the sub-station was boiling, so perhaps other billets had the same. It was all radar, so everybody should have been wise to that one.

MR: Do you remember his name?

TS: No. So that was Yatesbury.

MR: Was this after your training?
An interview with Tom Sears – part I

TS: No, that was during the training – that was at the end of the first year – the winter of the second year, I suppose. Anyway, I then was posted to West Rainham in Norfolk, where I had to maintain one of the radar machines, which was [one of] the early ones that were built during the war and in fact, that was the same basic chassis as the one in the Lecture Theatre, but there were no proper manuals for that equipment and, fortunately, I had learnt enough how to set this thing up and we were ... there was also short to the much higher frequency ... forgotten the terminology ... the megahertz, they would have been. The short-wave radar was also replacing it all, but the jets were now the mainstay and these jets had to go out on training flights into the North Sea, and we tracked them, and my radar was inferior to the short stuff. That training was very good because one at least learnt the block diagram aspects of radar equipment.

[Phone call interruption – tape stopped and restarted]

TS: ... he was always going home with this haversack on his back, and he was rather unruly dressed.

MR: John Nicholls was?

TS: Yes, he was always in an old jumper, coloured jumper, and he was always being picked up on his way home and they never believed he lived there.

PK: He always wears a coloured jumper.

TS: Yes, he still does probably.

MR: They did have these very interesting ex-government pieces of equipment that you just wanted to have. You didn’t quite know what you were going to do with them, but even if you just used the screws, you know ... perhaps we had better move on.

PK: Well, the obvious use of radar equipment experience in science and physiology in particular, but you still weren’t thinking of a career in science at this time, I presume?

TS: Oh, I was, only because it was the only world I knew, having started so young in a laboratory and then I enjoyed the practical work, and so I knew I couldn’t have a career. I had no real insights early on of what a degree was, or to graduate, and there just wasn’t the ethos of the family, and it was a mysterious place you went to.

PK: What happened on de-mob then?
An interview with Tom Sears – part I

TS: On de-mob? Well, because I had been studying and I had done, I suppose, one year at Chelsea, I think I showed some native shrewdness of some kind, because the wall had gone up in Germany and my brother actually had to stay on in the RAF, but I discovered that you could have something called a Class B Release from the National Service on the grounds of your education being interfered with.

MR: It was two-and-a-half years then, wasn’t it, or was it more?

TS: Two years, but it was extended because of the war, and so I am sure this helped me in grant applications later. I wrote to Chelsea and said I had the opportunity of a Class B Release – could I have a place? And then I wrote to the RAF through the officer, of course, saying that I had a place at Chelsea. From the two letters I sent off and the outcome was that I got a Class B Release after the two years to start doing the ‘A’ Level course then at Chelsea. And that’s where I have obviously transposed time somewhat, so what I had started as an evening class fitfully, I then became a full-time student, because I also then got the government grant that helped you to do that, for ex-servicemen, and that was, I think, about £600 a year.

MR: £600. I didn’t but my brother got one, and it was a lot of money then.

TS: Yes, it was good. I lived at home for a few months, and then – no, I lived at home throughout that period, but then you paid housekeeping to your parents. Anyway, where are we?

MR: And you lived at home, people didn’t…

TS: So anyway I got that. I got my equivalent to the ‘A’ Levels, so that was in physiology, physics and chemistry, I suppose, and it was there I met Alan Howe, of course. He was doing zoology, rather than physiology, but we met…so, I must have been doing zoology and physiology…I am confused now …

MR: Was it Physiology?

TS: Yes, there was a physiology thing, but I must have done physiology and zoology and chemistry, that’s it, and Alan and I met over the dogfish, dissecting the dogfish, and we asked each other about the other’s subject. His other subject was botany, for some reason, and when I told him about physiology, he too, he was at Allen & Hanburys. I remember, he had been a technician, and so when I told him about what physiology entailed, he said, “Oh, that sounds much more interesting”, so he changed his subject and that’s how we came together. When we got our ‘A’ Levels and got a place at University College – Lindor Brown interviewed us – we actually shared a room.
MR: What was that interview like?

TS: That was very good. In my CV, of course it had something about the Wellcome Research Laboratories and what I did there, and he was so pleased, I think, to have someone with practical experience, that that, I think, probably outweighed any other issues, but he was very kindly and supportive, and so I got a place, Alan got a place, and also of course, Bernard Donovan. He was also at ... no, yes – I will have to think that one through.

MR: OK.

PK: Which year of intake was this?

TS: This was 1949. We started on the physiology course.

MR: Was Lindor Brown your...

TS: Yes, that was his first year. He had taken over after...Lovatt-Evans, so he was quite a new boy to teaching, Lindor. He came in and gave us our very first talk and he came in wearing a top hat and a jaunty air.

PK: Can I get this straight: is Lindor Brown, GL Brown?

TS: Yes. Ceremoniously.

MR: He wasn’t titled then, was he?

TS: No. He ceremoniously took off his top hat and pulled out a stethoscope, and then decided to teach us about the ECG and the three leads, and how the different waveforms were generated, and he got hopelessly confused. He got the first recording, the first pair, but when it came to how the dipoles all interacted, he got terribly lost. He said, “I think I’d better abandon that and I’ll tell you about it another time”, and of course, he never did.

MR: What was your first day like there? I mean, who were the other students? Was it a big year?

TS: Then, the medical students and the physiologists all were together, so the class would have been about 80, I suppose. The majority were medics – there were about 12 of us.
MR: Were there any women there?

TS: Yes, there was Pamela Goddard – she was the only woman in our class. She went to Bristol, and I don’t know ... we kept in touch for a while, as we all did, but I don’t know what happened there to her.

PK: Who else went on then to become stars of the Physiological Society?

TS: Well, Alan ended up in the Haliburton Chair at King’s for a while. Bernard Donovan, of course, went to work with Harris, who was the great name at the time, and still is, for he had given the primary description of the neuro-hypophysial pituitary axis.

MR: Is that EJ Harris?

TS: EJ Harris. Thank you. EJ, who was then at the Institute of Psychiatry at Denmark Hill.

MR: Who were your teachers?

TS: My principal teachers were Schweitzer and ... cardiovascular – between them, he and Michael de burgh Daly, taught us those subjects.

MR: How did they come across?

TS: Well, Michael was very serious. Schweitzer was very good, but also Teutonic. I was very impressed by him, but between the two of them, they had really pioneered at the time, the study of the carotid body and its reflexes and I was entranced by them. And we each had to do a student seminar in front of the staff and because of this maths business, in the selection of courses I took, I didn’t do biophysics. I would liked to have done, but I really didn’t think I could deal with it at all, so I did neurophysiology, the epicentre of which was spinal cord physiology, more generally CNS, but which was done through the intercollegiate courses. In fact, it was Dowman who taught us about the somatosensory pathways and the thalamus and the sensory cortex in lectures at St Thomas’s Hospital Medical School. But I gave one on peripheral chemoreceptors and, I suppose because I hadn’t had that orthodox sixth form training, I had very little in my head, apart from cricket and tennis and these subjects, so the Thane Library was marvellous. It was such a quiet place, and my digs were just behind University College; it’s a Hall of Residence now, I’ll get it in a moment. The Thane Library was a haven – it was warm and I just devoured the literature. I read every one of Sherrington’s papers – every single one. I read his Integrative Nervous System and I did the same in respiratory and cardiovascular physiology, but particularly, the carotid sinus and carotid body reflexes, and I had just
three years of French, but the technical French, so I read the whole of Heyman’s book on the carotid sinus and all the related papers, and that also introduced one to a lot of the classical physiology, and I just loved it. It just went in and filled the spaces that might otherwise have been filled with calculus and trigonometry. That’s my simple version.

**MR:** You weren’t married at this time, were you?

**TS:** No.

**PK:** Was it an advantage – do you feel you were more mature having done National Service?

**TS:** Oh, undoubtedly, I mean, it was the path with hindsight, to university, but because of the war, and we were almost at war for five or six years after the war, of course, the climate was post-war. It helped me get to university.

**MR:** I mean, not only what you learnt, but also a general growing up.

**TS:** Yes, I educated myself during that time. I read classical books and just generally educated myself. It was by the stimulation of other students. In the post-war period, there were either ex-servicemen, who were at University before the war, especially the older of the doctors. The rest were the first intake of the 18-year-olds and they were so naïve, the medical students, straight from their public schools, or whatever, and then another group of people such as myself, who had worked. We all got on very well, but we certainly had different backgrounds and one person ahead of me, Brian Newell, who was in the year ahead, in fact the same as Alan Ness, he was in that year. He lived fairly near me, but I have really forgotten...he played a very key role, because I was disenchanted with all this study in the evenings and so on, but during when I was on the RAF station in Norfolk, I met this chap, Brian Newell, and we got on well together. He had a good intellect. Mine was possibly good, but not trained, and we compared notes and he said he had a very good teacher at school, which was unusual, and they actually did physiology at school as a subject, and he was going to read physiology when he came out. He was intrigued by the fact that I had some knowledge of it, through the practical side, but he had political views and other views, and this was all new to me. It actually prompted me to decide, yes, I must do something about it, so that I then realised I was going to try and have a career in science, but I had to get this ‘A’ Level, so that prompted that series of events.

**PK:** There must have been other influences at UCL, in particular, social, political...

**TS:** Oh, well, yes, through Alan Ness at that stage, he used to try and recruit me into the Communist Party. He was a very active communist at the time, but I used to go along to the evening talks. They had talks with the Society for Cultural Relations with Russia. That was a group in London, and Sydney Hilton was another member of that at the time, and I
An interview with Tom Sears – part I

was never persuaded. I certainly was left wing at the time, not through any knowledge as such, it just appealed to my sensibility.

MR: Social justice.

TS: Social justice, yes. Also, it had really come home to one, the injustice of the Grammar School system, notwithstanding the quality of education, but it really meant that only 20% of British people were educated and the history of that still is resonant, I think.

MR: You regarded these as proper University years?

TS: For me, it was wonderful, and I played squash, so was in the squash team. I captained the UC tennis team, then I got a purple and I was University Captain at tennis. That was very enjoyable. We went all over the place playing against the Oxford. Not the first team, the second teams ... Oxford and Cambridge second teams.

PK: And this was all three years then?

TS: Yes, that was lovely, and we went to Paris, playing for the University against the University of Paris.

MR: That must have been exciting in those days!

TS: Oh, it was, yes, it was. It was while I was there, I saw this notice ‘Bureau des Voyages de la Jeunesse’, and it offered you the opportunity of staying in Cannes, Biarritz or Paris for what was the equivalent of one shilling and sixpence, and I can’t work those things out now, per night, and I had enough money on me, so I took the train to Marseilles.

MR: Seven-and-a-half p, I think

TS: And then stayed at this place. I was the only English person there. They were all French and mostly French women, young girls, 17, 18, and a little more, and that was a magical experience, which I will not give any more details!

MR: So this was your second year at university, or third year?

TS: That would have been in the third year. That was magical. Lovely. Between the tennis and the squash, and the work, and I lived just behind the college, so I spent all the time in the library. I was good at physiology, but I didn’t really ever have, and you know this
An interview with Tom Sears – part I

yourselves, I never had that mathematical underpinning of the physical side. I could use physical instruments and conceptualise, both of you know that...

MR: But you regard yourself as solving problems by experiment, rather than by theory?

TS: Yes, absolutely. Well, no, hypothesis.

MR: Yes.

TS: But that’s different from theory.

MR: But you liked to see it happen?

TS: I had to see it happen in my mind’s eye – waves, events. Yes, I see it all like that. I’ve always done, so if I am thinking about reflex function, I am an impulse travelling through, encountering obstacles. I still see it like that, but I can integrate several things at once.

MR: Do you see it as an anatomical diagram, or as an electrical diagram?

TS: Well now I see it in the same way, and in fact, I answered a question on blood flow in the Honours Year paper, by using an equivalent circuit of parallel resistive loads on the output of the generator, so I could do that quite easily, and I could probably put down Ohm’s Law, but as soon as it came to impedance, I would already get a bit buggered by it, because those frequency dependent things already created little problems...

MR: Filters?

TS: No, there were square roots and...

MR: You could do filters?

TS: Yes, I see them in terms of the effects, always.

HB: Although you always had this sort of sense of insecurity, or sense of lacking a bit in the maths, it’s never actually held you back –

TS: Not at all.
HB: There is nothing that you would have done, or you would have done better, had you been better equipped in maths. It’s just that you wouldn’t have had to employ some people?

MR: I mean…you’re prepared to delegate and accept...

TS: I can think through the problem completely, and know from the literature what maths is relevant, so I taught myself. I learnt integration truly from reading Huxley and Stämpfli over and over and over again, where they have recorded through the recording process, the differential of the currents, and then have manually to integrate.

PK: When you would have been doing that reading? So this would have been a bit later?

TS: That would have been from the time I, probably, when I went to Queen Square.

HB: When did you move to Queen Square?

TS: I moved to Queen Square about 18 months after…See, that’s another episode…I completely forgot...

MR: Let’s tie up the University College years.

TS: I gave my seminar on chemoreceptors...

MR: In your third year?

TS: Yes, and I had also given one on the spinal cord, and so Schweitzer in particular was pleased to have me, providing I got my degree. Then they had the post of Assistant Lecturer, so on the basis of my coursework and verbal performances...

MR: Were you good at that time at seminars?

TS: Oh, yes, all those things I could do, because I put in an immense amount of work and I didn’t find that too difficult. My father was an amateur singer, so perhaps I got something from him.

MR: Did you need notes then?
An interview with Tom Sears – part I

**TS:** No, I couldn’t do it from notes. I never could. He said well, if you get a degree, sorry I’ve jumped here...

**MR:** Schweitzer.

**TS:** Yes, to get the post of Assistant Lecturer, which was one of the normal paths, then. I dropped virtually one paper completely, the philosophical paper and the German translation. I just knew I couldn’t deal with it – I couldn’t write a general essay to save my life. A lot of the education came after. I just had to solely concentrate on the scholastic stuff for the subjects, so I knew I would drop it and I was expected to get – well, I did get, in the old classification, I got a Second Class Honours. If I had been under the new degree, I would have got a Lower 2, but I didn’t, I got a Second Class Honours. So, I am sure they were disappointed, because from this other performance in class work, and we did very few essays, largely it was practical work, so I got this post. Then, in the summer of that graduation year, so that was 1952, Schweitzer was killed in a climbing accident in the Alps with someone, I think he was a lecturer from one the polytechnics in Regent Street. So, when I came back after the summer holiday, GL said “Well, I don’t know what we’re going to do with you”, and of course, he must have known that the level at which I had passed wasn’t really adequate. However, he was very supportive and said, well, you’d better go and see Doug Wilkie, because he thought from his background that I could deal with that. I went to see Doug and I was quite direct with him and I said I could do the practical work, but I know I would not be able to deal with any sophisticated analytical work on muscle properties, and I don’t think it will work. He was very good and understanding, so I went back to GL and in the meanwhile, John Gray had been appointed Reader, and of course, that was... the person I forgot to mention completely because of all these sidetracks, was Jack Diamond - Jack and I met over a shared microscope in the Histology class of the Honours Physiology.

**MR:** He was a contemporary of yours?

**TS:** Yes.

**MR:** Jim Pascoe – was he around?

**TS:** No, Jim was already a lecturer in the department and Otto Hutter was already a lecturer, which was nice.

**MR:** Lippold?

**TS:** Lippold was a lecturer, and Brenda Bigland, Leonard Bayliss, the lady – Egglestone...
MR: Yes, his wife.

TS: Yes, Grace Egglestone.

MR: And the Chief Technician was still...Charlie Evans.

TS: Charlie Evans, yes.

MR: How did you find him?

TS: Well, he was fine with me, but he did ration the scalpel blades, and you were expected to use them on about five experiments.

MR: Sharpen them yourself.

TS: You went to him to get film and other things, but no he was fine. He must have had to deal with a very big range of problems, but it was he who, through Lindor really, had to deal with the introduction of all the electrophysiological equipment, as opposed to cardiovascular stuff.

PK: Some other colleagues, then? John Nicholls was there?

TS: Well, John Nicholls was, that was the year I started – I’d better finish. Because Schweitzer died, I had my own project, which I had conceived in the Honours Year, which was, I had read all these spinal cord papers, all of Lloyd’s, all of Sherrington’s, and all of Eccles, on the spinal cord. I had also read equivalently on the respiratory side, but what struck me, apart from the Pitts papers, the spinal cord didn’t enter the equation in respiratory neurophysiology. The epicentres were the respiratory centres, with their different definitions, and I thought, well, this is absurd. It should be possible to study... just as we study limb reflex physiology, and there must be a minefield here. Particularly, I was intrigued by the thorax, because of the overlapping of inspiratory and expiratory muscles in the same segment, whereas the phrenic outflow is virtually entirely, well, it is entirely an inspiratory output system for primary muscles. So, that’s what I wanted to do, and that’s what Schweitzer was pleased at, that he had had a student who had an idea of what he would like to work at. But John Gray, of course, although he was a very good neurophysiologist, his heart was with the Pacinian corpuscle, and really, apart from the work he did on close arterial injection of acetylcholine into the cervical spinal cord, he had in my view, no background in the reflexology of the kind Schweitzer would have given, so he was appointed after I abandoned Doug Wilkie. John Gray was appointed supervisor for me to do this project.
MR: AV Hill was there?

TS: AV was there at the same time. In a sense, I was unsupervised, certainly the respiratory side, but John Gray was fine when I took specific neurophysiological things to him, but there was no feed forward, if you like, from him, concerning the project. And I also didn’t organise the information very well, but I know from what I did at the time, I already had alpha-gamma linkage in those recordings that I did there, because I had developed the de novo dissections on the thoracic cord and described the cat anatomy, I think for the first time relevant to later research. That was all back in 1952 to 1954. Anyway, I developed those techniques basically, of dissecting the intercostal nerves.

MR: This was after you graduated?

TS: Yes.

HB: So you were doing a PhD, even though you didn’t really have the degree for it, and you didn’t have a supervisor for it. You were allowed to do...

TS: Absolutely.

HB: So how were you funded?

TS: Well, then you were funded from the Department.

PK: Did you have to teach?

TS: I gave tutorials, and who did I tutor? Heinz Wolf and Dennis Noble.

MR: So you didn’t consider working with Jim Pascoe or Olof Lippold?

TS: No.

MR: And Diete-Spiff was he there?

TS: Well, he came along later. No, I think Schweitzer had this background in spinal cord physiology when he worked at the Middlesex, and although he worked with Michael Daly...
An interview with Tom Sears – part I

on cardiovascular things while at UC, he did have this background and I think it appealed to him – this topic of working …

PK: But the other junior people then included Jack Diamond, and were in a similar position to you?

TS: Jack Diamond had started a PhD, but then he had always wanted to do medicine, and so after about a year of his PhD, when he had published, I think, a paper – he did the isolated carotid sinus preparation and acetylcholine action on the carotid sinus – so ultimately he then joined John Gray and they did the receptor potential papers with David Inman and the Japanese, Sato.

MR: When did you first present any findings?

TS: The first time I presented, would have been at the 1956 meeting of the Physiological Society at Queen Square. [PK note: I think it was actually 1958, at the UCL meeting.] In the meanwhile, at the end of the first year, I had to give an account of what I had done to Lindor Brown. I had made actually quite a lot of original discoveries, but it was organising it, so now my past caught up on me. Although I had got my degree, I just wasn’t able to organise the material as if it were for a paper.

MR: You had results – who did you show them to in order…?

TS: Well, one’s friends. I never took them, hardly ever, I must confess, to John Gray, because we just got at cross purposes every time I went to see him. I was on a different wavelength really, I think.

HB: So all that work after you graduated, where you developed your own preparation and your own ideas; none of that got published directly, only came out later ….

TS: No, I …

[Tape ends, then recording restarted]

TS: Lindor, I think realised at the time that it wasn’t that I wasn’t going to have a future, but I think he felt probably I might not sort out my PhD with this inability to… I don’t know, he was very kind and always, until he died, was very supportive. I must emphasise that, but he came to me one day and he said “At Queen Square, at the Centre for Neurology, they are desperately in need of someone who knows his way around spinal cord research and there isn’t anyone other than yourself”. In fact, I went into the EEG Department and they were interested in visual evoked responses, and I don’t resent it at all, because it was the making of my career, as it turned out. The hospital environment turned out to be a very
congenial one for my temperament or personality. So I went there in 1954 without a PhD – it was incomplete.

MR: Employed as what?

TS: Well, as a physiologist in the Health Service, but in the EEG Department.

HB: So that was under Bill Cobb?

TS: That was under Bill Cobb, who was one of the world’s experts at the time on the electroretinogram and of course, George Dawson was there at the time in the MRC Neurology group under Dr Carmichael.

PK: So was this an advertised post, or was it...

TS: No, it was just that GL came to me, and I do think he felt that I would do better in some other environment. It was fine – I didn’t have any expectations, you see, I mean, I was pleased with what I had done so far. I was gainfully employed as an Assistant Lecturer, and I got the same salary, virtually, when I went to Queen Square as a physiologist in the Health Service – £600 a year.

MR: Did you follow any of the research that was going on?

TS: Yes, I actually developed a method for…. with the aid of Bert Morton, who was, of course, one’s saviour also, because you learnt so much good electronics through Bert – really good stuff in the sense of how to think through, how to identify faults.

MR: Could you tell us just a little about Bert Morton?

TS: Yes I will. I think it was in that environment it turned out to be extremely good. We had all the prototypes of, first of all, George Dawson’s signal averager, and more particularly, through Bert Morton, the Digitimer, which of course, conceptually, was linked to George Dawson’s counting methods for doing the signal averaging, so I used the prototype of the transistorised version and the original version, and the isolated stimulators. They all were done to meet the needs of the animal work and the human work.

MR: So what was the first problem that you were involved in?
TS: The first problem was in the cat, setting up a simple model. At the time, children had been born effectively with no hemisphere on one side, due to vascular accidents and birth injuries. They had uncontrollable epilepsy, and the surgeons at the time found they could remove all of that damaged hemisphere. It was called ‘hemispherectomy’, and that was the only way to control this totally unmanageable epilepsy, with convulsions going on all through the day and I, of course, in the meanwhile, when I went there, had to read EEG’s and report on them. And I taught EEG, so I still have the set of lectures, trying to synthesise the EEG from synaptic field potentials and so on, because at the time, that wasn’t really known. But by analogy with the way synaptic potentials summate, one could present sort of ideas. We had a course on the EEG, an international course there. I was familiar with both the EEG and the problem was – the extraordinary thing is, when you take out the whole of the cerebral hemisphere on one side, on that side there is still a perfectly respectable EEG, and of course, it is a curiosity, of remote field recording through the scalp. I suggested that we did an experiment on the cat, and we did three or four cats in all, and we created, rather like George’s – based on George’s plastic plate, with a series of screws through it. It was tapped, so that you had a network of about 20 brass screws which were pointed, and you could put them over the cat’s skull, and, on the EEG machines then do a 14-channel, whatever it was, recording. And then through a burr hole introduce a pair of electrodes and then put a sine wave onto them, voltage sine wave, and then recorded the distribution of fields. Then through a frontal hole, I think it was, we sucked out all of one hemisphere. We kept the skull intact, but got virtually all of the brain out and then did the potentials before and after, and then just did a simple spatial map of the change in the distribution. Now, I can’t remember, truly, any theoretical things that came out of it, other than the fact that it demonstrated the extent to which … we had either air or water inside the hemisphere that we had taken out, and we showed that in either case, you could record these EEG’s on the empty side, so it emphasised the fact that there is an incredible interaction between them in the intact skull. Of course, it is dominated, or is it, that’s the point, by the local potentials, but it isn’t highly discriminating for that type of EEG signal. So that was the very first experiment I did with Bill Cobb, on the visual evoked potentials, and then I got quite interested in it and we set up the technique he used for the retinograms – there were high intensity zephen flashes and that’s how he got out the very short latency rhythms from the electroretinogram, and they were more synchronised as a result.

TS: So we had these retinograms. We had the flash thing, so I got Bert to make up these canisters with the zephen and then a long plastic tube, which was shielded, and you could put one in each eye and because visually evoked responses were just coming in through Bert and Bill Cobb...

MR: Geoffrey Arden. Was he involved?

TS: No.

HB: Martin Halliday?
TS: He was there, but that was much later. What interested me, from the knowledge of the papers, I mean, I knew all the animal papers on evoked responses at the time, and so the idea was to flash each eye separately, and then with different timing, because I had read Sherrington’s papers on binocular fusion, and I kept sort of crossing my eyes and inspecting one image or the other. Sherrington – it was a beautiful experiment he did on this sensory processing. I thought, well, one could get a handle on this by recording the visual evoked responses and seeing how they interact, so I did that and that was never published. I was under Bill at the time, completely, and I think we gave a communication somewhere, but that was interesting, anyway, using the evoked responses, and seeing what components, if any, occluded, or was there a simple summation, so I did all that just manually, by adding up curves.

PK: The interest in the neurophysiology of respiration must have been in the back of your mind all the time, lying dormant?

TS: Well, yes, I wasn’t able to do it. I did one or two experiments, and in fact, I repeated at Queen Square, the experiment I had actually done at University College, but helped, because Peter Nathan – with whom I did three or four years research on local anaesthetics and differential blocking, that’s how I got involved in the spinal roots and their usefulness for study – Peter Nathan kindly translated the original Breuer-Herring paper for me. There were two major papers, and in it, was this explanation of the fact that the Herring Breuer reflex had always been written about in terms of the inhibition of inspiration, as measured by Heads diaphragm slip preparation, or whatever. But Breuer had analysed the passive and active components. He, of course, used pressure, not electrophysiology and deduced that during the pause, there was an active contraction of the expiratory muscles, whereas all the subsequent literature, without fail, had only mentioned … So I repeated what I had done at UC, using the animal setup in the EEG Department, and recorded electromyographically from the intercostals and the abdominal muscles and then repeated Herring Breuer’s … it was really Breuer who did the experiments and Herring introduced him, rather like the Royal Society papers. So Breuer had simply closed the trachea at the end of normal inspiration, and then showed that there was a prolongation of expiratory pause. I think I was the first to do that, and when I recorded electromyographically, there was this beautiful progressive recruitment of expiratory motor neurones during the pause, and that’s how I realised how there was so much more. But, because I was also weaned on the stretch reflex and other things like that, and I was now doing it on the intercostal muscles, of course, as well as the abdominal muscles, I then decided to cut the spinal roots, because in 1918 Coombes had done a similar experiment, but he was concerned with showing what role did the spinal roots have in the generation or control of breathing. So they had these horrendous cat preparations where – the laminectomy throughout the whole length of the cord … they did the procedures, then cut all the spinal roots and looked at what the animal was doing afterwards, and of course, it continued to breathe. But since they didn’t measure CO₂ and things like that in those types of experiments, they couldn’t have known, or didn’t surmise, that if the respiration had been weakened at all by cutting the dorsal roots or modified in any sense of a way, the CO₂ feedback would have driven breathing at a higher level of CO₂ and you would have got your output back – perfect feedback mechanism. So it couldn’t have been a proof, but the loop ideas were not very paramount at the time Coombes did that original experiment. So that was very nice. That was actually my first
paper at the Phys. Soc. meeting in 1956, when I gave the paper ... you should remember this...I can’t remember the title, but it was something like ‘The Active Phase of the Herring Breuer Reflex’.

PK: Well, I don’t remember it, but there was the 1958 abstract as well. But it is an interesting lesson for today, maybe, in that obviously you would have an animal licence then, but you were doing experiments with Bill Cobb on his project, and then you were doing experiments with Peter Nathan on a separate project and another one really for your own interest. Would you have got a Project Licence today for that kind of thing?

TS: I doubt it. It wouldn’t be a sufficiently well-defined purpose, I suppose.

PK: Yes, and yet at least two of those were the basis of your later career and you developed the ideas then.

TS: Oh completely, yes. No, they were all formulated in a sort of way ... Certainly while I was at Queen Square, the modern version of, what were the dorsal roots doing? In other words, in the early 50s, from Granit and Merton and Eldred, we had the concept of the length follow-up servo, of something specific, so that strongly influenced one’s thinking. There was a loop there.

PK: I mean, Merton must have been around Queen Square.

TS: Oh yes, that’s where he did the silent period experiments, but one hardly ever interacted with him. We did in a slightly social way, but he was actually a very friendly person when you knew him well, but he could be very dismissive as well, and I must have blotted my copybook with this Cambridge don, many times. All those issues of class and background and schools were very strong then, and it certainly didn’t help, coming from my background.

PK: He was with Carmichael’s group, and did he interact with the group?

TS: Yes, well I did with Peter Nathan, because that was a very important period because Peter had, notwithstanding the fact that his family were Glaxo. Peter was a wonderful, funny, easygoing man, very bright, and was just a joy to work with. I went to see him – now we’re backtracking a bit...

PK: This is going to happen a lot now!
TS: OK. Well, while I was at Queen Square, I was having lunch with someone, and they said “Do you know Peter Nathan?” and I said, “No, I know his name, but don’t know him closely”, and they said “He is getting these interesting results where he is using intrathecal phenol, instead of alcohol and silver nitrate, dissolved in Myodil (which was the radio ... lipid radio-opaque material), to treat patients with incurable pain. The extraordinary thing is that at the time they had lost their pain, they can have knee jerks”. I thought, well, this is very odd, because phenol would be expected to kill all the fibres, but he had this differential effect. So I went and introduced myself to him at lunch one day and said “What about C fibre pain?”, or something like that, and then we got to know each other and discussed it, and I said, “Well, if you like, I could easily set up a cat spinal cord”, but at that moment there were no projects or anything, the money was there. So I set that preparation up and we started local anaesthetics. We’re straying, I think, a bit, aren’t we?

All: No, no.

TS: Well, so working with Peter...that was the other work I did, so Phys. Soc. had a joint meeting in Lund. That was my first international meeting that I went to. Not true. I have forgotten a whole episode. The other thing I did with Bill Cobb; he was concerned with the electrogenesis of signals in the frontal region in the EEG when using flicker to promote an epileptic form of activity in the EEG, as a diagnostic tool. So, he was interested in the blink reflex and so again, from my background and his, we decided to look at the latencies ... and there was another thing, they were interested in visual evoked responses. To cut a long story short, we started looking at the latency of the human blink reflex and I had read a paper by Kugleberg, who worked in Gothenburg, and he had studied human facial reflexes and found that in patients, in particular with facial myoclonus ... and then he went to demonstrate, as in normal man, that if you tap orbicularis oculi, you get a blink and there are three blinks. There’s first of all a blink that results from the spindles in the muscles being stretched; secondly, an ipsilateral reflex, which is like a flexor reflex (twice the latency) and then a third reflex which is part of the bilateral startle reflex. So, electromyographically it had that sequence, and so I said well, why don’t we use this tap reflex as a test reflex to look at the excitability of the facial motor neurones during the latent period between the bright flash and the onset of the EMG? So, we recorded our own EMG’s from orbicularis oculi – so you put a little fine needle with two wires in it (well, it was a concentric needle electrode) into orbicularis oculi. Sounds awful, but we did it. That was very nice, because one was able to see at what point before the polysynaptic type reflexes occurred – the brain stem late mediated ones – at what time was the excitability building up in the motoneurones – that was the idea of it. So it was just based on Lloyd, or...

MR: How did you record results in those days?

TS: Photographic. So I presented that at the 1956 meeting of the Congress of Physiological Sciences, so in fact, that must have been my second paper and the main thing Bill Cobb was concerned with, was whether I would pronounce Kugleberg correctly. I just anglicised it and called it Kugleberg. He had spent some time in Sweden and he would say...
Hugleberg. I said, “I’m not …”. He said, “You must, you must …”. We were sitting in this lecture theatre and he was at the back and when I got to it, I said Kugleberg – my first defiance!

MR: Did you meet Lundberg?

TS: Yes, that’s when I first met Lundberg ... Sorry ... it was later, when I went to Lund ... differential blocking of local anaesthetics – by then we’d worked out, and I presented it, and that’s when I met Lundberg, who came up to me to say he liked the paper very much, and he was glad that I saw the beneficial effects of working on spinal roots.

MR: You were still unmarried?

TS: No, I was married.

MR: We missed that.

PK: We’re going to back-track.

MR: We’ve had a break for lunch and now we’ve re-convened.

TS: So, after coming to Queen Square and learning and teaching on our International course, lecturing encephalography (reading then), I was then asked to join Roger Gilliatt, who had been appointed consultant neurologist in the hospital, to work two afternoons a week [that] he did in the EMG clinic, and so that exposed me to peripheral nerve clinical neurophysiology for the first time in any depth. Our main task was to build up a group of patients who were suffering from carpal tunnel syndrome. At the time, it was thought that this syndrome, which would now be recognised as one of the repetitive strain injuries, particularly in relation to computer keyboarding ... there was controversy as to whether it was a lesion of what they call the thoracic outlet syndrome, a very proximal lesion of the nerves innervating the median nerve innervation of the hand, or whether it was a distal lesion, and some experiments involving ischaemia has shown it was distal. A neurologist, Ian Simpson from Glasgow, had already shown very beautifully that when you stimulated the median nerve at the wrist and recorded from the thenar muscles of the hand, there was a prolongation of the motor conduction latency between the stimulus in the recording, and had therefore shown, for a motor involvement, the paralysis and atrophy of the thenar muscles. So he had an objective indication of that, but there still was remaining the problem of the sensory symptoms of carpal tunnel, which I think used to be called the housewives’ syndrome, because it was linked with the idea of people squeezing out the washing – before automatic machines, I suppose. Anyway, Roger was interested with techniques available for signal averaging, which was, of course,
photographic averaging, where you used the oscilloscope sweep turned right down, so that one sweep would not register an image, but using, I think it’s called in photography, the latent image in the emulsion. You then recorded 50 or a 100 consecutive sweeps and the image built up in the negative and at the end, when processing the film, you had an average of the position of the spots as it traversed the screen. And so it brought out, then, signals of quite low altitude in the few microvolt range, which were hitherto buried in the noise. Of course, the techniques all drew on George Dawson’s experience of signal averaging, but we used his original method, which is used to show the evoked responses associated with myoclonic jerks and they were the first human, I think, cerebral evoked responses, in about 1947. We basically used that principle to study sensory nerve action potentials in the carpal tunnel syndrome and other odds and ends that came along in terms of patient referrals to Queen Square. So we published the first paper on sensory nerve action potentials in man in a clinical situation. I think it was in 1958. That gave me an experience of human work and during that time – and this dates interestingly back to when I first graduated, I shared a lab with Alan Ness at UC, and he had a dental background – a marvellous person, which would be a whole story in itself. But he had a dental training background, and he was concerned with looking at the sensory innovation of the teeth in the rabbit (the incisors, which are continually growing), locating the periodontal mechanosensors that surround the teeth, showing the directional sensitivity, [by] pressing on the teeth in different directions and applying known forces, and recording the afferent discharges in the alveolar nerve. I grew up in a sense, since we shared the lab, and I suddenly realised that I had a way of investigating human sensory nerve action potentials evoked by natural stimulation, simply by tapping the fingernail and so synchronising the mechanoreceptors, then to be able to record in the usual way, as we had done for the carpal tunnel, by surface recording. I think at the same time, Hensel (perhaps Hugh will have the references to Hensel somewhere?), Hensel had actually done some electrode recordings in man. It may well have been the first intraneural recording.

HB: Well, he did some recordings in man, exposing the nerve. I only know about his work on the temperature receptors not on the mechanoreceptors.

TS: I think that about that time, he had published that paper. Anyway, so I had my nail-tapping paper, and that was another Phys. Soc. communication. So you tapped the fingernail and I had a piezo crystal under the nail, and Bert rigged me up with a very simple device – it was just a piezo crystal, and we simply measured the voltage across the crystal as the force was transmitted from the nail through to the force transducer. So it gave one a simple measure of the force, and in that way ... It got a picked up at the wrist and I also did (I think that was published) the cerebral evoked responses to the tap. And then, again drawing on the fairly wide knowledge of the literature, I then drew on Murdoch Ritchie’s experiments on A fibres and C fibres, using as a test volley, the antidromic volley set up by electrical stimulation. So, by using the forerunner of the Digitimer, I arranged for the stimulator, which was an old Grass EEG pen motor mounted on its side, but it was a very forceful thing – it was physically big – and so Bert, of course, got a power supply .... And we triggered that and it allowed me to develop a very brief duration tap and I mounted the force platform on ... like a microscope lathe bed, so I could raise and lower the finger for the same tap to get more and more synchrony in the tap and got the volley out, and I have still got the evoked responses that I subsequently
An interview with Tom Sears – part I

MR: With the capacitors?

TS: There were 100 capacitors to represent 100 milliseconds.

MR: They were polystyrene in those days.

TS: Special capacitors with a very low leakage resistance, so we borrowed that. So I supervised that work and generally looked after it, so I was quite accustomed to solving problems of stimulus artefact, particularly as part of the nail tapping. I also described for the first time, the digital nerve action potential in man – I realised that digital nerves were very superficial – we stimulated the nerve at the wrist and got this beautiful compound action potential and later, when I went in retirement to St Thomas’s, I introduced it as a practical class, so that was quite nice – memory lane, of course ...

MR: Alan Ness initiated this?

TS: Well, no, he didn’t initiate it, but I had the idea of tapping the nail and curiously, I did some research a few years ago which has only been published as abstracts and FENS presentations, applying the same technique to the teeth, when we studied with Duncan Banks – the [cerebral] evoked potentials from the teeth, so I followed their … Wiesendanger … I can’t remember the other name for the moment.

MR: Which were the afferents from the nails?

TS: They were lowest threshold digital nerve fibres which, if you stimulate at the wrist, say, and record at the elbow … the lowest threshold electrical fibres which produced that compound action potential. The lowest threshold ones equally are responsible for the cerebral evoked response. So first of all, I had that volley going antidromically into the finger and then using the Ritchie technique of colliding with appropriate time, an
antidromic volley, I was able to suppress the volley recorded at the wrist, set up by tapping the nail by appropriate timing and...

PK: So, Pacinian corpuscles, were they?

TS: Well, I don’t think they are Pacinian corpuscles, but yes, probably a Merkel receptor and I frankly had forgotten. I did know it at the time.

MR: Merkel is a name in the news now, isn’t it?

TS: Yes. So that was all, but part of that problem was having to recognise that the mechanical pulse wave would be transmitted through the finger, possibly at the same velocity, who knows, as the impulse (in a semi-rigid sort of structure). So that was one of the reasons I used the antidromic volley to show by collision that the actual signal I was interpreting as the diphasic nerve action potential, could be recorded. The other nice thing about it – because of the length of the finger – the very prominently diphasic signal, not unlike the ones in delayed saltation, because of the long intra-electrode distance, the two phases separate out, almost like the classical diagram of the origins of a diphasic potential in a frog in the old literature. And so I put an elastic band around the finger between the two electrodes and showed that the second phase was delayed and the first phase, the later components, then were revealed, previously buried. That was a good example of the work you were talking about earlier, Hugh, that I could see in my mind’s eye what the result should be or could be, but usually it was should be, rather than could be. I have a strong instinct for signals and it’s a visual one all the time. That was a very nice period, and it is mentioned, the technique, in Buchtal’s … have you ever read his review? It would probably be worth looking at some time – a very big review on clinical neurophysiology of the time. At that time, Roger Bannister, I think he was then probably Senior Lecturer, or may have been a Consultant, I don’t remember, and he had a patient with a Guillain-Barré syndrome – an acute infectious polyneuritis and this patient was on a respirator in what subsequently became the Harris Ward – the intensive care ward. And he was so paralysed, but he still had residual function, so we took all of the equipment from the lab up to the intensive care unit and did the recordings and we were able to show that when we tapped the fingernail …he had a complete sensory neuropathy, but also the mixed one – the motor paralysis, hence he was on the ventilator. But what we were able to show [was] that a small digital nerve action potential could be recorded between the wrist and the base of the finger, but when you tapped the fingernail, there was nothing and in the second stage, there was nothing, so he had a complete block of the sensory nerves. Now at the time, this would have been in 1960, the auto-immune diseases were simply not known very well, only anatomically and histopathologically, so what we had is a set of things which I think were well discussed in that paper, but largely ignored, it seems. Because we made recordings each day, we had a surviving motor unit, which carried on and then finally blocked, so that the muscles were completely paralysed and the sensory nerve action potentials to natural stimulation were completely gone, as were I think the electrically evoked [ones]. But about 60 days later, at a time when he was making clinical improvement, there was a response to nail tapping, which you could record distally, because there the volley was clearly … although it was very small and very
much dispersed, you could record it, because we were close to its site of origin, but further proximally it was simply absent still.

**MR:** Is this one of the first cases where electrophysiology was used for peripheral nerve examination?

**TS:** No, for action potential recording there was the paper from Roger Gilliatt, the first ...

**MR:** I mean it wasn’t used then routinely, clinically?

**TS:** Yes, it was, at Queen Square ... everybody had a sensory evoked response, then the same for the EMG clinic and so that was ...

**HB:** Another figure from early work on peripheral nerve was PK Thomas.

**TS:** Yes. At the time I was asked to join Roger Gilliatt, when he was appointed then, I think, Senior Lecturer, no, it must have been his consultancy, PK Thomas, who I had got to know at UC while I was still in that two years, he was working in JZ Young’s department. And at the time, for reasons better known to other people, there was virtually no interaction, almost hostility, in my view, between anatomy and physiology. It was a scientific rivalry, not a hostility, but there was virtually no interaction, except with the PhD students and PK was someone I knew, and he had been working on the changes in conduction velocity in single nerve fibres in the trout. And because the trout continues to keep growing, internodal length keeps growing, so that work was a nice example of Rushton’s views about saltation. So we knew each other quite well and often chatted in the library, so when he came to Queen Square, we had that relationship from the beginning. So, what the three of us did, that’s Thomas, Gilliatt, and myself, we investigated the refractory periods of individual nerve fibres in motor nerves in man, by doing occlusion experiments and using collision to block all the lowest threshold fibres and to reveal by single unit EMG recording, what the slowest motor fibres were, to get a kind of benchmark, so that when you were considering abnormal pathology in a patient – motor conduction times – were they at the bottom end of the normal range, or actually outside? Now in some neuropathies, if I remember, it was diabetic neuropathies, maybe not ... and certainly in degeneration/regeneration, the velocities were completely outside the range of the lower ferris[?] range. So that was an important paper, I think, in defining in certain nerves - the range.

**PK:** Before we get on with a more specific lines, we are going to go ... there are interesting issues here of the relationship between basic research and the clinic which you always had views on, that we shouldn’t ignore in this.
TS: With regard to that question, it is what I’ve always felt – that it’s governed entirely by personality and your interest in the topic, and if neither of those is present, it’s not got much chance, and if both are present, it works very well. For me, Queen Square – although there were immense class distinctions between the old, as perceived by the young, whether they were clinical or not, the hierarchy of physicians was very strong indeed – people like Sir Francis Walshe come to mind. They were venerated figures and still attached to the hospital and seeing patients from time to time, but that was a very rigid hierarchy, which we don’t really see in basic science – we see some of it, but not to that degree.

MR: The Scottish one you mentioned – he made the film on gait.

PK: Carmichael?

TS: Carmichael? Oh, Carmichael was wonderful. He was an independent person, Head of the MRC Neurological Unit, so they were outside. They just saw those patients they wanted to, or because of their specialities, were referred to them by the consultant physicians at the hospital, but they were in the MRC Neurological Unit, so that was Peter Nathan, Marian Smith, Martin Halliday, John Bates, who was a primary, motor control in man – stimulation studies, and so on. There was a psychiatrist, whose name is floating – I can’t quite…no, it will come perhaps later. That was the …

PK: When I came here in ’71, Gilliatt was a rather grand hierarchical figure and at the time you worked with him, he was still not that high up in hierarchy?

MR: He was socially active person!

TS: He, I think, would quite easily have followed, if it weren’t for background, because his father was physician to the former King, and with that medical background, he was destined for a medical career, but at Oxford, where he trained, he did an early paper with Whitteridge and Carmichael on vasoconstriction during inspiration in spinal man and so I really was well versed, and still reasonably am, in the literature. So, I was quite excited by this chap coming, training as a physiologist, and he needed someone to help in this clinical environment. He was perfectly good with the equipment, but he had to deal with the patient – put in needles – so I was sort of a research assistant to him, but we got on very well and we visited his home on several occasions, and he had been best man at Snowdon and Princess Margaret’s wedding. His wife was Penelope Gilliatt, who was the Editor of Vogue at the time, so Blanche and I, suddenly, when we went to his home behind Harrods, moved in exalted circles. But he and I had a very easy relationship. If he respected what you did, then it was very easy, but if a registrar came and put the two stimulating wires on the wrong side of the wrist, well ...or close to the recording electrodes, they were damned forever! That’s a parody, but he was very severe and expected his clinical registrars to be able to understand every word immediately.
PK: Before we move on to Canberra, I guess – you have mentioned Blanche already – we should ask about family.

TS: Oh yes, well. There was a chap who was on the Honours Year of Physiology called Peter Wright, and when I came to Queen Square in ’40, no ’54, low and behold, there was Peter Wright, working in the biochemistry department and that autumn, he invited me to a party in his home, which was somewhere in Islington, if I recall. I was the first of the guests to arrive, and there were three women standing, and as it turned out subsequently, they had all been at Manchester – they were Manchester graduates – and I went over and introduced myself, and they asked me what I did. Well, I was always a bit self-conscious about this kind of work, certainly the animal work, but even the human work, and I must have said something to Blanche or to the three of them, that I’m actually in EEG, which stands for electroencephalography and that means recording brain waves. I must have said it, but I think at the time, I didn’t want to be drawn into that discussion, and to talk about my athletic prowess or something, but Blanche later told me that I came across as rather arrogant. Equally, I said to her ‘I think you were attracted by the suit that my father had made’, which was a beautifully dark navy blue tailored suit, the like of which no student or academic would be wearing and I said, “I think you married the suit and took the man inside it along with it”. Anyway, we just danced and there was nothing specific, but then I was travelling from Belsize Park where I lived to Hampstead – I was going to a party, and low and behold, just a couple of weeks after, there was Blanche with some other chap in a car, and I totally ignored this chap and said “Oh hello” and “Fancy meeting you here”, and that was that (that was about two weeks later). A further two or three weeks went by, and I was living just behind Baker Street, in a single flat, and down the road there was a shop and the son of the owner said, “Would you like to come to a dance on Saturday night at the Victoria League?”, and this was an organisation for receiving refined young ladies from abroad – obviously set up in earlier times – and they had this beautiful house in Belgravia where they could meet nice people. Low and behold, there was Blanche, so anyway, it just started like that and never ceased. We were then married in 1956.

PK: So it’s coming up to your anniversary, fantastic!

TS: Yes. Next year. That’s been a wonderful marriage with three offspring, but I think we’d better get back to the science.

PK: We may come back to at least one of them later ... OK, so then you have the opportunity to go and work with Eccles in Canberra. How did that actually come about?

TS: Well, I have always kept my links with UC, not only socially, with Alan Ness, Jack Diamond, Jim, and others, but in the summer months, on many weekends, we would go up to Shenley [UCL sports fields], where there were a lot of family people and Furshpan and Potter were there about that time, so I got to know them and Ricardo Miledi much better then, and on the Sports Days ...
PK: and John Nicholls perhaps?

TS: Well, I knew him anyway. At the time I started the PhD he was doing his PhD in...or had he already got it? Can’t remember. Anyway, he was with Bernard Katz at the time, and he was just this very bright character who had got eight Firsts in his eight papers, so he was on a pedestal also, but very likeable, very engaging. So, as a family, we would go out to Shenley and Jack and his family. It was a lovely regular thing in the summer months and Rose Eccles was then at the time ... and previously, I had asked Ricardo how he went out from...he worked with...no

MR: Katz?

TS: No, before then, in Central America, he was with them.

MR: Castillo?

TS: No, the man who introduced the famous fight or flight on the sympathetic nervous system. It’s just gone for the moment. So, he said “Well, I just wrote to Eccles” and I said, “Well, that’s very unusual – here, we certainly do it through our Head of Department – you wouldn’t think of doing that”. He said, “Well, why not?”, and so I thought to myself, well, why not? It so happened, though, I then met Rose at the tennis games. The Eccles family were great tennis players, so we hit it off and I said, “What are the circumstances?” I said, “I would like to learn intracellular recording on motoneurones and how could one end up in Canberra?”, and she said, “Well, just write to Dad”. Now, this is where the three to four years’ research with Peter Nathan came in very helpful, because I wrote to Eccles and said I wanted to record from thoracic respiratory motoneurones as a way of investigating breathing, and he said he was giving a Ferrier lecture on inhibition. That was 1961 at the Royal Society – “Come to the meeting and I will see you afterwards”. So I explained everything, what I would like to do. Then I came back and saw Carmichael and he literally got on the blower to Peter Williams, who was then a Director of the Wellcome Trust.

He was so pleased that I had initiated the work of Peter Nathan and we had, I think, four or five papers altogether, that he instantly gave a good think, and he said, “Oh, Peter Williams said just write to him and say what you want to do”. Two weeks later, I had a Fellowship, with a much better salary than I got from the National Health Service, to go to Canberra, and that was how it was done.

PK: And that’s how easy it was in those days? On prepared ground!

TS: Yes. Prepared ground. I always wait for the right moment. I had to define psychological moment in a school cert. essay and I have always lived by it. You must bide your time,
and incidentally, through that process, I now have my personal licence reinstated. Anyway...

MR: You had no children at that point?

TS: No. As it turned out, when we went out, Blanche was seven months’ pregnant, so we flew out to Canberra. That was the other thing – Arthur Buller, of course, had been out in Canberra and did his work on developing muscle with Eccles and the cross-nerve transplant experiments, and I knew Arthur and got in contact with him, and he and his wife invited us to their home. They lived in London then – to wise us up on Canberra, so that was extremely helpful. We then flew out to Canberra, and we stopped on the way in the Lebanon for four or five days, a forced stop in Karachi where a relay had gone wrong in the plane, then to Delhi for five days – that was the nice thing about the stopovers without any extra cost.

MR: It was more of an adventure in those days.

TS: Yes, and you got barley sugar when you took off, to stop the...

MR: Did they weigh you before you go on?

TS: Not quite!

Then we went on to Bangkok for five days, and then finally landed early in the morning at Darwin, where before we disembarked, the plane was not de-loused, but de-mistified with this horrible stuff, which I am sure gave us Gulf War Syndrome, but then we emerged into Darwin Airport lounge, and it was absolutely wonderful. You were separated and you went into these areas, and you were given a white dressing gown and white towels and you had a shower – a wonderful shower after the long journey – it was great.

MR: What time of the year was that?

TS: This was in February, so we left in a cold February from Heathrow. We had this shower and then hot coffee and croissants and other breakfast type things and then flew on to Sydney. Then, in Sydney, we stayed with Liam Burke for five days.

MR: So the whole journey took you the best part of two weeks?

TS: Yes.
PK: Liam Burke is another name you haven’t mentioned at UCL…

TS: Yes. He was in our year, and after his PhD in Katz’s department (so that was Burke and Ginsburg on the neuromuscular junction), he went out to Peter Bishop, who was then Professor in Sydney, to work in Peter Bishop’s department. So, when we arrived, we stayed with Liam for five days and his wife and family in Sydney, and within a couple of hours, I was on the tennis court playing tennis with Liam and Peter Bishop and his wife. That was a very agreeable five days. We then went down to, I think it was called Rozelle, it is to the south of Sydney and Brian Newell was now doing physiological and other studies in a marine biology station there, so that was very nice, meeting up with him again, and then we took the train to Canberra and had two and a half wonderful years there.

MR: What were your first impressions when you arrived?

TS: Well, the Australian visitors in London – and many had come – they all used to refer to the English, particularly the British scenery, as chocolate box green, and the first impression of going to Australia was this grey green, sort of slubby green, my wife calls it, and there was that little moment – God, am I going to get used to this? – but by the end of it, I was actually crying when I left. I was so moved by it – now, all these colours and the eucalyptus and the picnic spots and all that Canberra meant. But what was beautiful was this roughness and harshness about the environment, rather than the green, and years later I went back to Kuwait to examine and then I realised that I couldn’t bear that life there and the sand everywhere, but what it revealed to one is that the environment of your upbringing, whether it’s harsh or beautiful, is beautiful for all of them, providing the upbringing is a happy one. I was quite surprised – it was part of the colour. The colour always reminds me of that.

MR: When you first walked into the lab, what was your impression there?

TS: Oh, very friendly. They were marvellous at greeting their visitors, and I was the first British person they had had.

PK: It must have been a bit of a powerhouse of all sorts of visitors.

TS: Oh, it was. Well, at the time, there was Robert Schmidt and Bill Willis – there was John Hubbard of neuromuscular transmission.

MR: Oh, the New Zealand John Hubbard?
TS: Yes. John Hubbard. There was David Curtis, who was then a reader in the department.

MR: Was Paul Fatt there?

TS: No. It was interesting. He had been there in about 1953.

PK: So ten years earlier

TS: Yes, and it was interesting because one of the things I had picked up in the Lloyds papers – and I knew them inside out at the time – was a discrepancy in the conditioning curves – they were wrong in the interpretation – and what I learnt subsequently is that Fatt had, while Eccles was away, he had done the primary work on the inhibition and extracting information about the extra delay that allowed for the inhibitory interneurone, and he had also picked it up from the discrepancies in the conditioning timings. I don’t think that’s in any paper ... the conditioning is complicated, as always, and getting the timing right in terms of when you expect the volley at the right place ...

PK: The timing of the rising phase of the EPSP, which of course was what became the critical issue.

TS: Yes, exactly. This 0.5 or so milliseconds was...of course, I didn’t understand it in the biophysical sense, but there was a discrepancy, so that was quite interesting, and I’ve never discussed that with people because it could only have been done with all the instrumentation and the thinking in the Eccles set-up, but it was very much Paul Fatt’s discovery, I think, the inhibitory interneurone, and then, of course, it was all integrated into everything else that followed.

PK: So the other people there, Masao Ito, was he there when you arrived?

TS: Yes, Ito, he was there, and also Oshima, Russell Close, who did the muscle work and came to UC subsequently, and he was a meticulous worker on intrinsic muscle properties.

PK: Apart from Eccles himself, who were the major influences that you left Canberra with?

TS: There was Rose Eccles, but perhaps above all was Per Andersen. He came in the second year, I think ...no, it was a few months after we came, and they lived in the same blocks of apartments, Forest Flats, and they had similarly three children. So we just hit it off, but what was a common factor, was that in Oslo, of course, his boss was Kaada – he did clinical EEG, but he also did the classical experiments on stimulating the orbital cortex and its influences on the cardiovascular ... Per had done EEG related things with Kaada and he had already done his pioneer studies.
PK: Kaada worked with Granit?

TS: Yes, that’s the same man, on the hippocampus in vivo and so he knew all about theta rhythms in the hippocampus and I knew quite a lot about EEG rhythms and the thalamus and...

MR: Was Per Andersen working on hippocampus then?

TS: Well, no, the first thing he worked on was the cerebellum ... He had already worked on the hippocampus, so with Eccles, they did intracellular recording in hippocampal neurones ... cerebellar neurones and the in the second year of Per’s stay, I joined in and we worked (because of my experience on the thorax) we worked on cortical control of pre-synaptic inhibition and also on thalamo-cortical relationships – thalamo-cortical relay neurones. And then Eccles went away and Per and I then spent about four months working on the electrogenesis of thalamic rhythms and showing that they were not dependent, as Adrian had not argued, on a reverberating circuit between the cortex and the thalamus. And it was very interesting, because I knew the literature inside out at the time and Adrian’s second paper was a short paper in the EEG Journal, which I suspect Eccles didn’t read, and it showed Adrian re-visiting the problem he had earlier studied, showing that the thalamic rhythm was intrinsic, but Eccles was going on the basis of some other papers (and the names escape me), and argued that it was a thalamo-cortical reverberation. I said “No, Adrian disproved that”. He said, “Certainly not, certainly not”. He wasn’t rude, but he couldn’t believe he was wrong. I said, “Well, if they have got the journal, I will go down and get it”, and it was right at the beginning of the journal – some commemorative thing – and I brought it up. He was very good; he said, “I don’t know how you do it, Tom”, because there were one or two other incidences. The other one was the Group 1a representation in the cortex, and I knew that that had been re-written by Amassian (I think it was with Berlin), because it had always been argued that the Group 1a afferents didn’t go to the cortex, but they do. And when he got the paper from Scandinavia to look at for the authors, he said “Oh, they’ve got the 1a representation to the cortex”, and I said, “Well, they’re not the first”, and I went down to the library again [Tom must have been referring to Amassian & Berlin (1958) J Physiol 143, 61P]. He was very good about that.

The general pattern was that we had the weekly seminars on Thursday afternoons, and each person would then give an account of where they had got to in their projects. He would sometimes give a seminar based on some international meeting. He was a very kindly man and made sure that all the visitors were well looked after, and if they had any problems, pointed them in the right direction...so very good. If you were working with him, you did all the basic dissections, but if it was spinal cord work, when you came to the ... depending on the experiment, but if the ventral roots had to be cut, then you went off to lunch and had your sandwich or whatever, and he would prepare the ventral roots. The experiment might absolutely require that all the ventral roots had to be cut and that was tricky.
An interview with Tom Sears – part I

MR: So he was technically very good?

TS: Oh, yes. Very good. So he would put on his glasses and look in the microscope and then would look round and be really sure that he got all the ventral roots that he wanted, and that was the way he kept his hand in. Then, as the experiment went on, always to the middle of the night, he would go home for dinner and would be near the prep and have our own dinner, or go home and leave the prep and make sure it was anaesthetised. Then, he would sit at the camera, and he had this great array of three stimulators, each of which could be controlled independently and then we would determine the nerve threshold on the basis of the lowest threshold volley at the spinal cord, and then with this arrangement, all stimulus intensities were multiples of threshold, but he would sit over one of those cameras – it was a Grass camera, a big one – and he would sit over it and move incredibly between these complex conditioning ... two shots to this, three shots to that, at different intervals, but very quickly, and all single sweeps. We did some superimposition, and then he would move the camera on, or some part of it was programmed. It was astonishing, and he watched every trace.

MR: No bad language!

TS: No, he was Catholic. He was very good like that. He got very irritated if, you know, the fluid accumulated and short circuited “What’s wrong with them?” because he expected them to be set up and they would then behave throughout the whole of the experiment.

MR: Was it all do-it-yourself, or was there a technical assistant?

TS: Oh, a good technica ... Coombes, of course, was the electronic engineer, and very good physicist, so he sorted out a lot of the electrical problems.

MR: The actual experiment – did you do everything yourself?

TS: Yes.

PK: There wasn’t a technician that came in the early mornings and set it up, not like here?

TS: No. Per and I would do, when we were on the thalamus, we would do the craniotomies, suck the brain away, expose the thalamus, expose the cervical cord, expose the lumbar cord, get all the electrodes in position, and then he would come in and the one thing he didn’t like was when I introduced measuring blood pressure. It was just an extra thing he didn’t want to do. The preparation would speak for itself, which it does largely.
MR: Was there sometimes the impression that he pruned away the aberrant data?

TS: No, I don’t think so. I mean, he worked strongly to hypotheses, of course, but I never, ever felt ...oh no, no. He had that tenacity of saying that well, if there is something not conforming to the hypothesis, it must be important, and would then pursue it, so no, very good. I never felt that. What he did privately, who would know – we don’t know that about anybody. It’s the reproducibility of results that in the end shows that up.

MR: He worked with Sherrington?

TS: Yes.

MR: What memories did he tell you?

TS: General ones, yes, but he worked with Sherrington and obviously admired him greatly. His own thesis in Oxford was actually on the cerebellum, the work he did in Sherrington’s lab, and he was very good at physics and maths, apparently, Eccles, at school, and one of the things he recognised was that a component of the force record from the major myograph that was used at the time to measure twitch and tetanus courses, had an internal resonance in it that was giving an inflection that was given a great deal of accord by contemporary workers. So he had that insight to work it out and so they had an improved myograph after that.

PK: The most critical thing in your career that I see is the respiratory drive potential, which came from Canberra, so you came away with ...

TS: I took that problem of recording intracellularly to Canberra, and that’s what I told Eccles in London I wanted to do. So Rose was very experienced at intracellular recording, so she was appointed to train me in the first few months – the first six months perhaps – and so we worked together, but someone had also come to Canberra, who was destined to work on the nerve-cross experiments – Sheeley, an American neurosurgeon. He became a well-established pain neurosurgeon, and he came to get this experience, so he was attached to me, which was fine. It worked, but it was a kind of dilution as far as I was concerned. He had no relevant background. Anyway, I worked with Rose perhaps for four months, something like that, and so I introduced them to the intercostal nerves and how to use their techniques with suspending the vertebral column. And that all had to be developed with different supports, because I had to have the access to the thorax, so we had special clamps made. And then it was a question of dealing with the movement artefact, which was particularly bad over the thorax, and just generally how best to manage the prep, and that took some while. Then I was working on my own, doing two or three all-night experiments each week and building it up.
PK: With a new family?

TS: Yes, well, we went out in February and Rachel was born in May, so that was a new experience! Then I began to realise there were some odd changes in the potentials, and at the time, I didn’t have a good way of balancing, so I thought, well, this technique of ion injection which Ito was using extensively, was a better way to understand what I thought might be going on, and so what I then realised was that the membrane potential was making these two depolarising phases for each cycle, and it didn’t make sense. So I started injecting chloride ions through the micro-electrode for about a minute, and then it had the advantage – you could turn that current off, and I had a unit built for me which allowed me quickly to back off the membrane potential and now see the DC potentials with a full-scale deflection on the ‘scope. I would read off the membrane potential on the galvanometer, which was over one’s shoulder – that always gave you the membrane potential directly, so I could switch in 40, 50, 60, 70 millivolts, but preserve a high level of DC gain, then it all started becoming simple. I could see the slow potentials, which were biphid, and then after injecting the chloride ions and getting the reversal of the potential, that was how I discovered the inhibitory phase of the CRDP.

PK: You once said to me that one of your first talks on this at Canberra elicited very little interest – what was this strange slow potential? And yet, here was a functional inhibition, when inhibition was a key interest.

TS: Yes, it was, I think because there was so much respiratory stuff, and none of them had that kind of background, that probably the impact of it in ... you see, it hadn’t been done on the limb motoneurones at that time, because there was no new reflex movement – no autonomous movement from the central nervous system. But, against all of the background of the respiratory literature (and I was well versed in that), the fact that it actually drew on this active inhibition at cord level to regulate, presumably (as one surmised at the time), afferent inputs and so on, and give a second dimension of control, for posture and so forth – all those sorts of thoughts. I think what was the main surprise, was ... Rose Eccles, because although she was doing and showing me how to do the intracellular recording, she had no concept of how I was really thinking about it, and only when I really got this slow potential sorted out, could I put it in a conceptual framework. There was more surprise, from Rose – she said “Oh, goodness me, didn’t realise all that was going on.”

PK: By the end of the time, you were getting invitations to talk at conferences...

TS: Oh yes.

PK: And it became recognised. Am I right in thinking that this was the major step in your career?
An interview with Tom Sears – part I

TS: It was, undoubtedly. We travelled back via the States, and so I gave talks in San Francisco, New York and Chicago, NIH. Eccles had spread some sort of word, but on the way back, we stayed in Salt Lake City for three weeks, where Leonard Jarko worked. Now, if you looked in the early Physiology Journal, you will see Jarko and he was one of the early people who worked on the thalamic evoked responses and published papers on that, and he had come to Queen Square to get further training as a neurologist, and had worked in the EMG clinic with Roger and myself. And I also, when I learnt his name, had got to know him, and he came to our house a few times. On hearing that we were coming back, he invited me to Salt Lake City for three weeks, and I set up sensory nerve action potential studies there. Because it was a Veterans Hospital and the lower limbs were particularly affected by these long-standing alcoholics’ problems, they wanted something on the lower leg, and so we devised a new method and four weeks later, there was a Spring meeting of the American Neurological Society and we had a paper. We stimulated above the ankle and recorded – I’ve forgotten what the nerve is called there – the lateral popliteal I suppose, and got a very respectable nerve action potential, compound, and that was a very successful three weeks. I got paid much more than I did for several years, for three weeks’ work, and when I wanted to make up the silver ring electrodes for the sensory nerve action potentials – I also did the finger while we were there – I went to the technician – I must have told you this: let’s call him Norman – I said “Norm, I need some thin silver strips wrapped in gauze, to make these electrodes.” “Sure, Tom”. He took me to the lab, his workshop, pulled open a drawer, and it was full of silver sheet – bars, rods, and I said “That’s much too thick”, so he said “Is this alright?” and I said “I think that’s OK, but I only need a thin strip, about that long, about three or four millimetres wide”, and he just put it on the grindstone and ground it to shape … and I was collecting the shavings. So that was my introduction to the largesse. Oh, I also went to Comroe’s labs in the Cardiovascular Institute, which of course...

PK: In San Francisco?

TS: Yes, it was a long trip. We stayed in San Francisco, Salt Lake City, Cleveland, where Norman Sheeley was now a well-established pain surgeon, then New York, Boston, and a 2-day trip to NIH.

MR: Which year was this?

TS: That would have been 1963, and then we came back via Montreal, so we had a couple of days in Montreal, because I was invited to go to a symposium in Amsterdam on spinal cord physiology and so ... the timing couldn’t have been worse – the reason we went to Montreal, apart from seeing one or two people we knew there, was to fly directly to Amsterdam. So we stayed with the two children in Amsterdam for the four days or so, and it was awful, flying over the UK and looking down and knowing you still had to go through this process. But that was a very important meeting, and so that was the first time I presented any of the respiratory work. That was well received. One of the things I’d done was to show the repetitive stimulation of the 1a pathway and the way it summated with the Central Respiratory Drive Potentials. That was subsequently, of
course, used in the human experiments I did on the intercostals with John Newsom Davis and Tony Taylor later. So we’ve got to Amsterdam.

PK: OK, well then, return to Queen Square, and you come back in a different guise...presumably?

TS: Yes, certainly with more self-esteem. I now had a PhD and had these extra techniques, and there was still, I think, concerns about how, in an academic sense, basic science was to be integrated formally in the Institute structure. It already was, but in a very limited way and Roger Gilliatt, who understood power and so on, I think, wanted this really to remain within Clinical Neurology, but I wasn’t prepared for that. I said “No, I think I’ve done enough already to indicate that I can work with physicians, but if I am to be responsible for my own grants and my own ideas and follow them in research, I need to be independent.” So what happened is that I had a measure of independence and I was attached to clinical neurology, but Bill Cobbs still wanted a little slice, so I remained attached and domiciled in the EEG Department.

PK: Your salary then came from the university?

TS: It had already come from the University, because in 1956, the University Grants Committee visited the Hospital and the Institute, which was then part of the 16-member Institutes of the British Post-Graduate Medical Federation. Several of the hospitals had not put the money that they had received for academic work – they had not created proper university structures – the Consultants were Honorary this or that, but there was no infrastructure. So there were just the two of us, Gerald Curzon and myself, and then subsequently George Ettlinger, who were Lectures – we were the only full-time academic employees. So, then in some two or three years or so, that would have been 1956, they had to create overnight structures, so I went onto the Institute payroll – that’s still in the EEG Department. Roger Gilliatt became the first Professor of Neurology, Professor Cummings, the biochemist, became the first Professor of Chemical Biology, in Neurology, rather, and Blackwood became the first Professor of Pathology.

PK: So you went back as a senior lecture or something?

TS: I came back as – yes, do you know, I can’t remember the Senior Lecturer bit.

MR: Was there some question of possibly going back to University College at that point?

TS: At one stage, yes. It was suggested. People like Jack and Alan and other said there was a post going and Andrew Huxley interviewed me in a very informal way and he asked me at one stage about what did I know about CO2, and the way he asked the question was really at a sort of membrane biophysical level, as I think that’s what he wanted to get at, and I
think Roger Thomas was doing some of his work on pH at the time. I did know about it, but I said “To be frank, Professor Huxley, I am not interested in the normal way, I mean, for me, it is a tool to drive breathing. I am very much interested in the output, and I don’t find the control aspects of CO₂ and pulmonary ventilation ... there are so many good people working in that field, but I wanted to continue...” and I don’t know whether ... It wasn’t pursued, so I wasn’t encouraged, and I never heard about that again, but I am glad I didn’t, because I think I wouldn’t then really have got into the demyelination work, because that environment at Queen Square was just perfect for realising that sort of interest.

PK: But you were given your own department fairly soon after coming back – was that a struggle, or ...?

TS: Well, no, it was done like this. I still had the lab in Bill Cobbs’ department on the mezzanine floor, which you probably never saw, did you?

PK: Of course I did, I did work there!

TS: Oh yes ... then Roger offered me two labs, one up on the first floor and I looked at them and I thought well, that’s a bit pigsty-ish and that’s just right for the two or three racks that were bound to come in the future. And he said, “You could have this one or that one”, and he said the strong preference was for me to take ... and I said, “Well, I think in terms of what I need to do and expect to do, I’ll take the larger one.” That was the type of thing you had to just grit your teeth and ... In good faith, the other one was fine for what they were going to do and the irregularity of it. So, I had the big lab, C11, and, of course, then David Stagg appeared on the scene.

PK: Well we have two streams there, Hugh, why don’t you go back to nerve or onwards with nerve conductions?

HB: On your return, Newsom Davis and Stagg were your next main.....

TS: Well, David Stagg came as an electronics engineer looking for a job. I had no money for a position. I pushed him in all sorts of good directions. He was clearly good and none of them took him up. He came back to me two or three months later, so I said to him, well, there’s a problem which I can conceive empirically, but it needs study. And this was that, in Eccles’ lab, we were taught that if you did not get a spike discharge from the motoneurone on the rising phase of the EPSP, the motor neurone had deteriorated, so the general experience was that the ... So, of course, the other thing I discovered in Canberra, which I described, was the alpha-gamma linkage, which was equally as strong a story at the time as the CRDP. So I had really some very original things, which were nice, and what I realised, just from general principles of anatomy and also odd papers that I had read – was it Poppele? .... So, I knew something about the probability stuff on these
very large synaptic potentials in insects, but I knew that some process like that must be ultimately determining the timing of motoneurone spike firing and so I said I don’t understand it from a mathematical point of view, but this is sort of data I could get. So there were two elements – one is that I now had the grant from the MRC, so this would have been actually in the early spring of ‘64.

PK: The grant application to the MRC must have been in very general terms, because he set up quite a big lab with it. I mean...

TS: Well, all based on intracellular recording. While I was doing that in Canberra – my notes aren’t clear enough on it, but I had various oscillatory potentials often, and they probably were not the counterpart of HFO, but some other frequencies of the kind that are embedded in some of the stuff we did together. And so one of things I said, ‘These rhythms are the signature of the systems driving the respiratory motoneurones, so if I did intracellular recordings and recorded these outputs, I would hope to correlate … I hardly knew what correlation meant then – but I knew how to go about it, and so David Stagg’s job was first to instrument the lab, which he did superbly. And, of course, I asked for this Ampex recorder, multi-channel, and there was only one other in the country at the time, in Bristol, at the aeronautical establishment, because I knew Byford who was in charge of physiological electronics there. I went to see him with Bert. So we got this tape recorder, and then we realised if it was to do what I wanted (to get the high frequency response) you had to go suddenly to fast tape speed, but there was manual switching of the amplifiers, so suddenly there was a new order of magnitude of cost to buy the most up-to-date recorder, with the electrically switched frequency responses, or at least, relating to tape speed. That only could be the FR1200, so that cost about £25,000 at the time, which was an extraordinary sum of money, and larger than the ones funding people’s salaries, but the justification and the timing were perfect of course, coming from Canberra. There were 12 papers I published altogether from there, all in the Journal of Physiology. I don’t think I had published them by the time I did the grant, but they were all in the pipeline. And so that really needed a professional instrumentation, but there was no way Bert could have done it, just because of how busy he was. And I was now attached to this other department, which created some problems, and so he looked still after the lab in the EEG department and David looked after … He and Bert got on very well – they each admired each other’s skills – but then came the necessity for David to do a PhD and so Tony Taylor, who was a close friend had attended Bruce Sayers’s course at Imperial College on Electrical Engineering applied to Medicine, and had learnt a lot about correlation. Tony was very good, because he had published some papers on motoneurone synchronisation, and he had an electronic model for that. I went to see Bruce Sayers and he agreed to act as the Second Supervisor of this PhD, so then we started doing the multi-channel recording of the intercostal spikes and using them, as you well know, to look at synchronisation, and that’s how we started with that. That was very fruitful, but then eventually, he was lured away to Yale, where he is Head of Computer Science in Pharmacology.

PK: Yes, we didn’t quite get to how the Department came to be a department.
TS: Well, that was … at this period, I had become, I think, in 1968, a Reader. At the time, there were problems with reorganisation and restructuring a little bit within the Institute and Bill Cobb was due to retire, so there were general logistical things, also relating subsequently, many years later, to the move to the new Institute building, but of course the preparations were all being made several years earlier. We were asked to submit plans for our subjects, several people. And I put in a proposal and pointed out that I had already trained half a dozen or so young clinical neurologists. And I saw this as some animal-based research, but some human, and that this was a way ahead for the future. That specifically wasn’t taken up, but I realised after a year or so that unless I fought more vigorously for an independent department, but with the same aims, there was no way I could tolerate being under the physicians, if oneself was … I mean, that was the problem generally, but how did engineers and physicists and physiologists and biochemists, in an academic sense, work in an medical environment, being second class citizens in terms of the establishment organisation. So I pursued that, and then it was recognised that yes, there was a department, only sort-of, not begrudgingly, but in a kind of limited way. And it really came to a head in a formal way, I suppose, when I went to Jere Mead – I had four months at Harvard, and I came back at Christmas time to find that my name had been … Ah, that was the Readership, that’s right. My name had been put forward for a Readership. Now, I knew there were university regulations and I knew this was a crunch point. I went away in the late summer and came back just at Christmas, and the first academic board meeting was to consider the Readership for Dr Sears in the EEG department and I thought, well, wait a minute – I haven’t heard anything about this, and I went to see the Secretary of the Institute. He was a close friend, fortunately, Colonel Gould, and he said, “Well, of course it’s in Roger Gilliatt’s department.” I said “Well, no, I’m not prepared for that. Either the Institute bites the bullet at this stage, and recognises that there is an independent department, or take it off the agenda for tonight.” I was just a member of the Academic Board then. He said he couldn’t possibly do that, so he got in touch with the chairman of the Academic Board at the time, who was, oh dear, his name will come in a second, a very nice man, but he said, “Won’t you change your mind?” I said, “No, it had all been discussed informally before I went away”, and he said “Well, can I come over and see you in your office?” and I said, “Yes, by all means” – “But where is it?” He had no idea where it was, so that just reinforced my view, so he said, “Well, I’ll come across and see you” and he tried to get me to change my mind and I said “No, if the Institute can’t make up its mind now with the information available, it will never make up its mind, and I am not prepared, I think … just like neurochemistry is now recognised as an independent basic science department.” And then I was summoned to see the Director of the British Postgraduate Medical Federation. He was a Scotsman – he was a very distinguished figure in clinical medicine. And when I arrived at his office, I had to wait a dutiful 15 minutes outside. I knew him, through meetings, and “Ah, come in Sears and sit down – now what was it you wanted to see me about?” So I said, “I didn’t want to see you about anything”. “Oh, well well, you know what I’m talking about” and I said “No, I don’t, you tell me”, and then he had to explain, so I put him in the picture and then six months later, it was all agreed that it would be a department, so I then let the readership application to the University go ahead. I think my work experience in life gave me a lot of strength to do those things at the right time. I think that’s the key.

PK: So that means yourself and David Stagg in a half time position, is that right?
TS: No, he had the first Lectureship.

PK: Oh, right, I thought he was joint at Imperial but he wasn’t?

TS: No, he was just registered as a PhD student, but he had the first Lectureship and so he did teach signals analysis at Imperial and then you were the second appointment.

HB: Before then, you got to work with Ian McDonald and that opened up a whole new area?

TS: Yes. Well, having worked with Roger Bannister on the Guillain-Barré … While I was in Canberra, Ian came through, having completed his own PhD with Archie MacIntyre in New Zealand and he knew Eccles well. And so we got to know Ian then, and he was due to come in the near future to Queen Square to get training as a clinical neurologist. But his work using diphtheria toxin and the peripheral nerve lesions (that was his PhD work), the results were very similar to the conclusions that Bannister and I reached, that you couldn’t explain the Guillain-Barré syndrome, on the basis of what we had objectively measured in the periphery, on the basis of Wallerian degeneration. And therefore, a process akin to Tasaki’s demyelination, focal, using saponin, some process like that, must have happened. Then there was a little bit about auto-immune, I think and so we were all due to go to Melbourne for the Phys. Soc. meeting where Ian was presenting a 10 minute presentation. So, the Chairman agreed, and I gave a two-minute addendum to that paper, with broadly speaking, the same conclusions, but more strongly linked to demyelination. And so we agreed that we might have the chance of working together, so when, it must have been 1966 or thereabouts, perhaps the year before, he came, studied, and then went to Denny-Brown for a year (and with the neuropathologist, Adams) and he came back and then to a post at Queen Square. It was John Cavanagh and his group that introduced … Jean Jacobs who had originally done the diphtheria toxin work and showed the demyelination, and in it, it had the explanation of the molecular action of how diphtheria worked, inhibiting protein synthesis. So I thought well, that’s quite an important tool, so when we, Ian and I, … the thing was, against the background of multiple sclerosis, was to devise preparations that allowed one to study focal demyelination in the central nervous system. I had the idea of doing micro-injections of diphtheria toxin into the posterior columns, so that’s what we set off to do. So we used a Wellcome micro-syringe (the Aglar), which I used to use at Burroughs-Wellcome. The special glass precision 1.0 ml syringe was one of those mounted in a frame containing a micrometer whose drum was rotated to deliver small volumes for doing the Hagedorn & Jensen titrations to get the colour change to give you the blood sugar level from rabbits used for insulin assays, so I knew this syringe very well. So we bought one and did these 50 microlitres, I think it was, injections into the posterior column of the cat. But the particular thing that I think was also nice is that, having worked on the thoracic spinal cord, by making these lesions in the mid-thoracic region, you could have central nerve fibres affected or not affected by the lesion. So first we studied conduction in compound action potential terms, studying the cord dorsum potential and the conduction velocity, how it was affected by this central demyelinating lesion. Ian and his group also had very good electronmicroscopy, so that we had light and electron microscopy of the lesion and got a very clear result and then described various features and then introduced a new
term relevant to our single fibre studies on intercostal muscle afferents, which was the ‘refractory period of transmission’. This was to distinguish it from the ordinary refractory period, which of course is the time between two shocks under the same stimulating electrode, whereas the new term was how long was the period of refractoriness before a second impulse could traverse a partly demyelinated lesion. And then we studied the frequency of following of these fibres, and then we had the idea of doing evoked potentials in man, but of course, that came strongly in this instance from Ian, who through his neurology, knew that optic neuritis was the most common early presenting symptom in MS. George Ettlinger and Bill Cobb and Bert Morton had recently discovered the value of the Chequer Pattern Reversal as a good retinal stimulus for evoked responses, but unknown to me, Ian had gone to see Martin Halliday – they were closer together – I frankly would have brought the technique into the EEG department, but it was a fait accompli, and Martin Halliday jumped at the chance, and they did extremely good work and Martin was very, very good. So between the two of them, they pioneered the use of cerebral evoked responses in MS (save for the Giblin Paper, I think) as a diagnostic tool for optic neuritis. I knew I had to try and pursue an understanding more at the basic Tasaki level, because, although much of Tasaki’s work was phenomenological, there were also detailed analyses. But his descriptions were virtually complete, based on the elegance of the experiments. So I was a bit ambivalent about this. However, I went to a meeting in Oslo; Steve Kuffler was there, and of course, I had met him on our return trip from Australia, and although I suspect he did not remember my science, he did remember Blanche, myself, and the two children arriving in a hearse at Woods Hole, because the person we stayed with in Boston, Bob Efron, who did the first intracellular recordings on cerebellar cells at Queen Square – he was working in the Veterans Hospital there, as a Professor of Clinical Neurology – he had invited us to stay on our way home. He bought one of Pat Merton’s old open sort of vintage cars while he was in Britain, and there he had several cars, including this Chrysler Hearse, and that was put at our disposal. So we drove to Woods Hole, with the children lying horizontal on mattresses and drove serenely into Woods Hole and Steve never forgot this...

MR: Real stretched limousine

TS: Yes, it was, but you couldn’t drive it very fast, but it drove very smoothly.

HB: So you met Kuffler....

TS: I told him what I wanted to do, and I said, I’m just a bit apprehensive about the biophysical side, I think I can manage the actual experiments...

PK: But you knew the Huxley and Stämpfl studies then?

TS: Oh yes, but I could see a way of doing it in vivo, and he said ‘you must, Tom, you must’ so that was very nice, so I resolved to do it, and then Mike Rasminsky came to work, attached to Roger Gilliatt’s department, but it was just ideal. The experiment was simply
a direct extrapolation from the Huxley and Stämpfli paper on isolated fibres. I knew that signal averaging could enormously enhance what they did, let alone what we could now do, and I knew the spinal roots very well from the work with Peter Nathan and just generally. I had envisaged that if the root was of tiny diameter and was small in relation to the internodal distances and I knew what the large diameter fibres were likely to be in a rat, then it should be possible in principle to repeat their experiment, so we electrically stimulated the tail and in some experiments, stretched the tail, and had a natural spindle discharge, and then did spike-triggered averaging to the root and, as you know, we got the sequence of longitudinal current recordings which then were analysed very simply by the ladder of latencies. I mean, the results were so much like Huxley and Stämpfli that you didn’t have to question what they meant, and then we did some manual subtractions of the currents to get the membrane current. And then of course, Rick Rogart appeared on the scene for a short while, and he ended up as a Professor in Yale in Murdoch Ritchies’ department and it was he who seduced David Stagg to go to the States. Rick had come to work with Doug Wilkie at UC, and that hadn’t worked, and Doug said I’ve got this very bright, ultra-bright American – was there any way I could take him on? So he came, bright and breezy, for just about six months, and … I have still got it at home … what I was trying to do at the time when he arrived, was to use a micropipette and high magnification and visualise the nodes of Ranvier, so that you could do focal recording, because I realised that this would be the best way to analyse directly … but it was tricky from the point of view of then the three dimensional analysis of the field potentials – were there going to be three, or what were the dimensions of this going to be? I had got to the point where I had visualised the nodes of Ranvier and gave a paper to the Microcirculation Society, in fact. I showed them a film, and it was very beautiful, and that was the thing I liked within physiology, the beauty of a new recording, or just doing the preparation was beautiful. And I have always had that strong feeling and so it was very beautiful to visualise the microcirculation. Rick happened to visit one day, late at night, and he then … Doug asked me to take him on, so he was funded from his Harvard MD programme. Using the Maxwell equations, he did a three-dimensional analysis, just as Lorente de Nó had done two-dimensionally of the volume conductor potentials around the node of Ranvier. It was interesting, because I hadn’t understood that, until he brought it out by the analysis, the way the internal potential has its own potential field, and the outside has its own electrostatic field. Anyway, he came up with some numbers and what the magnitudes of the currents would be and the likely magnitude of the external potentials, but then he went back to Harvard, so that side couldn’t be pursued.

I then met Hugh through Bert, when he used to come and visit Bert, and we had lunch a couple of times and I was interested in what Hugh had done on the olfactory system, and I knew the way you (Hugh) had approached it, that there was something original here, and then suddenly, out of the blue (one or two years later) Bert phoned me one day to say...you remember Hugh? Yes, of course I do. He was in some difficulties and hadn’t finished his PhD, and was there any chance I could take him on? I had a technician’s salary to spare at the time, so all I did was to chat-up Reg Gould, the Institute Secretary, who sorted it out – that was the nice thing about organisation in those days, and so Hugh came, and we discussed it, and nobody knew the arrangements I had made. I said to Hugh, apart from coming to me socially to hear of your progress, carry on at UC – you’ve got to finish your PhD by April 1st and I will fund you in the meanwhile – so you were funded by me, weren’t you? What was it, three/four months, something like that?
HB: I had completely forgotten...

TS: Oh well, the mechanics of it was the crucial thing. No, I was quite good at this sort of thing. I think it was the shrewdness of being a serviceman and working in the underground, so to speak, of laboratories, and all of the hospitals of course, were full of ex-service people (NCO's), who were as shrewd as anything and they worked in all the services of the hospital when they returned to work after demob.

PK: But it made the system work.

TS: It made it all work – it was interesting. So Hugh then joined me in April, was it – formally, or May was it of that year? The thing was that at UCL, Hugh was always being buttonholed to write programmes for people and not work on his PhD – that was my analysis at the time.

HB: At the time, I remember I was a bit torn between science and programming because programming gave such an immediate satisfaction.

TS: Yes, and biology didn’t

HB: Very much you persuaded me to move into science.

TS: No, I hadn’t … of course, but I realised that the topics you worked on also weren’t completely of your choice … I’m not sure about the Olfaction for which you received the PhD.

HB: That was entirely my own. The other was the chemistry and that’s what attracted me in the first place to physiology in the first place, was the smell.

TS: Yes, I had forgotten that was the prime mover. So, of course, then you had the second … well, Tony must have had the second Lectureship (Tony Pullen) …. Peter was the second.

PK: Yes, I was a research assistant for three years and then, I think...

TS: That was in the new building, was it?
PK: The department was established. Money became available for me to become a Lecturer. I’m not quite sure now Hugh’s position compared with that.

TS: Hugh had the third established position, but it was funded from the Sobell money, I think. David’s was from in-house Institute money. So, that’s how the more biophysical studies started, and then you (Hugh) came, and that was a marvellous period. But in a way, you see, I would never have been introduced to these topics in the clinically related field and the satisfaction from the overall contribution to the demyelination studies was immense, indeed, because the feedback from the physicians was now that much greater. And it made the whole environment and support for getting monies and just enabling research which, on their own, the clinicians would not have initiated. In fact, I gave a lecture some years previously at Queen Square, a public lecture, and I said as far as I can see, in relation to breathing – John Newsom Davis hadn’t appeared on the scene at the time – I said a neurologist at first determines that the patient is breathing, the only interest they have is establishing that the patient is still alive – “Oh good, I can now get on with my examination”. Of course, I said it jocularly, but I think there were frowns from the hierarchy.

That was a wonderful period, but at the same time, I was able to carry on working with Peter, though less and less hands-on, and then other people appeared on the scene. So we had Francis Crepel for example, for the year. And we initiated the cerebellar tissue slice in London, because Sukhvinder Danjal, who was one of my PhD students, had developed with me the spinal cord tissue slice preparation, so we had all the mechanics for that. And then there was Michael Pender, who came and worked on Experimental Allergic Encephalitis and that was proving another thing, that the clinical expression in that disease is the spinal root damage, not the CNS damage that all of the people who work on it had supposed – so that was important. Then we had Tom Feasby, and that was a lovely study. I think it was beautifully conceived, and we knew it could work, crushing the long sacral roots in the rat, and then studying the potentials of the growing tips of the nerve fibres in-vivo and finding the focal hot-spots, which subsequently were important later in your work (Hugh’s), weren’t they, the foci of inward current? I gave a talk on this a few months ago, and was showing the difference between the short term and long-term survival, which directly demonstrated the fascinating business of the short internodes of the recovered regenerated axon as far as the spinal cord, and all the short internodes of myelin. There is a deep interest, in the Wolfson CARD group where I now work ... they use dorsal root crushes and growth factors in studying the process of trying to promote re-innervation in the central nervous system, very actively, at King’s. So that’s been quite nice, being able to inject some old ideas. The young ones who are studying these things – they know very little relevant anatomy, and especially at the ultrastructural level, so they don’t see the problems in quite the same way as we grew up with – that’s quite an addition, so that’s my role in the department at Guy’s. I’ve got an office and a lab there, and at the moment, I’m working on knockout mice.

PK: Well if this is part of the history, we should be saying which knockout!
TS: If I can remember their wretched acronyms – it’s ephrin B3, is it? Anyway, it’s a mouse that hops instead of showing alternating progression, so as soon as I heard about that ....

MR: Crossed extensor

TS: Yes crossed extensor– I thought of Martin – not with Masao – you’ve blocked the name now for me...Shigemi Mori... so, all of these different people have allowed me ... because I had the power, being Head of the Department ... but one of things I always made of point of, whenever I got a grant, I always put some extra money in and bought us up-to-date equipment, so that whatever contingency arrived ... I think it’s true to say that most people if they didn’t have the grant ... but having the equipment around enabled people to get off the ground fairly quickly. And I would discuss with them and usually state the project, but match it to their interest, because mostly they were clinical neurologists. And in Pender’s case, he’d have a clinical interest in rabies, which gives dorsal root lesions, so the EAE, an experimental allergic encephalitis, was a very good model, and that was a very nice study. But with Konishi, what had prompted the demyelinating work and thinking about what I call physiological biophysics, was that I knew the Katz and Miledi paper on the motor nerve terminal very well, because I had learnt from it about the configuration of the triphasic action potential in this tiny non-myelinated fibre with focal recording. But at the end of the terminal, although they surmised that the impulse was actively transmitted to the end, it would, because of wave reflection at the closed end of the terminal, be only detected extracellularly as a diphasic current at the terminal. What was interesting about the mammalian nerve terminal, where this hadn’t yet been done, except by Mallart and Brigant, who had concluded that only the region at the beginning of the unmyelinated section was electrically excitable. But, in the mammal, in the mouse, this was only about 20 microns long – and I knew that from the papers on the sprouting and the terminal sprouting and things like that at the neuro muscular junction. So I thought well, that’s almost the same as the last bit of the frog unmyelinated fibre, so even though they haven’t got this triphasic current, it doesn’t mean to say that there isn’t an inward current being generated there. And the simple argument was this – that the last two nodes of Ranvier, or three, would be generating full size action potentials, and all of the core current that they are generating is overlapping by 20 microseconds or so, [and] would be generating a core current which would all be exiting through the terminal virtually instantaneously, and that means that the internal potential must be driven to the sodium ion equilibrium potential, or approaching it very quickly, as there is so much current. That would cancel out the inward current, and so even if it was potentially there, it would be cancelled out. And so by slowing – this is where it came back to my work with Peter Nathan – by slowing the presynaptic impulses down by reducing the sodium ion concentration, just as had been done in the classic experiments, we slowed the action potential down (and diminished its amplitude, more importantly). So now we could see a triphasic action current where none had previously existed, so that led us to conclude that indeed the whole of the terminal is electrically excitable, and that made sense, because when you get sprouting at the neuro-muscular junction in response to denervation, you can have continuously conducted action potentials over hundreds of microns, so it didn’t make sense, teleologically, for this patch of membrane not to be electrically excitable.
PK: How do you get those raised eyebrows onto tape?

[Laughter]

MR: Which raised eyebrows?

PK: The ones with “teleologically”.

TS: But I can still say it!

PK: There are things that ... we are not going to be able to cope with everything – there’s still a lot of time in your career in Queen Square, but there are important issues, like your chairmanship of J. Physiol and your role in the Society, there’s your role in the ENA that you may want to say something about and any other ... So many political issues.

TS: I did my time on the Editorial Board of Journal of Physiology. I can’t remember the exact circumstances, but I had met with AV Hill’s son David Hill who was on the Board at the time, and we often shared papers and references and things and possibly he had put my name forward and there was one year needed and because I was in my sixth year. So I was asked to Chair the Editorial Board, which I found very interesting and not too arduous except when one was settling really critical issues between authors and referees. But it so happened it was also the occasion of the Centenary of the Journal, so I had to organise a celebration dinner for that particular year. So I started configuring the occasion and I thought, well because the Cambridge University Press had been the first publishers, I had better invite some of them to this dinner we were going to put together and funded, probably, by help from various people. And at the time there were a lot of journal publishing interchange as journals got upset by the way the publishers were treating them. And as soon as I raised this question of who should I invite of the Cambridge Press, they took the whole financial management over. So then I had meetings with one of their senior people, a lady, and I confess her name escapes me at the moment and so the dinner was to be at the Savoy, so we had a dummy run. So we had a lovely dinner at the Savoy one evening and then chose the menu and then about 150 people came to that, and I had to make a speech and I hadn’t realised ... I’m sure I went on for too long, but this is where my memory is letting me down. I had to consult the early publication by Schafer on the history of the Society, and of course then I got involved in the background to the journal, how it came into being, and so my speech (which I have at home), which I should suppose deposit in the archives ... it was quite a long speech – the background of how it all came to being and it was intrinsically, I think, quite interesting. But I recognise in later times that it was much too long for an after-dinner speech, but I felt that the occasion had to be properly recognised, so that was that.

Out of the blue in, I suppose, in the mid-80s, I got this phone call from Elżbieta Jankowska from ... who was working in Gothenberg, asking me to ... well she said to me, she and her
An interview with Tom Sears – part I

PK: The Human Frontier Program

TS: Yes, the Frontier Programme ... he initiated that with Japanese money and when he came to Britain, all he ever met were the molecular scientists at the Royal Society. He used to stay with us, intensely irritated by this but couldn’t do anything. And, equally, this man just chaired the meetings but did nothing interim, and then we were running into some difficulties. I was asked, the year I retired, to be the president of this, but there was no money and I realised, watching what happened with Alan Davison who, once he retired, he was still secretary of MS society. And it was very difficult for him, he no longer had the power, no longer had influence ... unless you are a Nobel laureate or a Fellow perhaps ... Anyway I realised it wouldn’t be good logistically for me, so I declined, and in fact just as well, because there was no money. But that was very interesting, because an idea that I put forward is that, here we were, a group of professional bodies with different skills, and instead of the usual path of individual nations going down through their research [councils], if there was an area that needed to be studied, the logical thing was to go to the societies of those bodies and give them opportunities for (in a sense, contract based) research in a particular field or to create a panel of experts, who could meet and, through the professional bodies rather than through the councils, .... Because that’s the problem: the money in Europe comes from slices of money from the British and other research councils in the different countries, so there is never new money, just a bit of someone else’s.

PK: So, in personal terms, I think you enjoyed running the department that you created?
TS: Yes

PK: Is that more enjoyable than chairing large organisations like ENA?

TS: Well it was difficult, in the sense that, 'Would you meet expectations?' which is what I think all the time, I think psychological hang-ups from the past, from one’s schooling in a sense, but always there, because you are meeting such distinguished people and very bright people and polymaths and in this environment. So that always created a bit of stress, but usually I found I had other skills that saw me through them. My biggest fear I suppose, at the end, was my Presidential Lecture and that was very hard. But that was very original because, again, the presidential lectures are very poorly represented by the people and it was a modest audience, that’s all I will say. But what I realised, doing that job, and the EuSEB experience, was that, if Europe was expecting British science to work in a way that Americans (this is neuroscience now, but more generally) in the way that American neuroscience works – and I understood the way neuroscience emerged as a subject, or a field ... So I centred my presidential address around drawing analogies between Europe and America and the importance in America of the job market, the importance of being there, being seen there, the commercial interest and they were all represented, not only all of the States in the one body funded from the one government. The problem for Europe is that each country had its true national identity, with long term societies, and there was a major problem, particularly in relation to finances. So the job market was each country had its own career structure, so there was no way we could match them, unless a body like EuSEB had a place in the governing bodies in Brussels that was analogous to NIH in the States. Then it would have enough power to unify these structures in Europe. But, anyway, I was pleased I had actually thought through something original. But that’s never been published – I suppose it might have been – but I think, so much, if you’re satisfied with what you have done for that occasion, that’s mainly what I got out of it.

MR: I think we had better wind this up, but what kind of general postscript would you like on this?

TS: Oh, on the...overall, well I think a big regret is (and I saw this while serving on Medical Research Council Grants Committee) is the hypocrisy in the establishments concerning the future of young people. The peer groups get the big Programme Grants. They’re the big earners for the colleges and institutions and the young men who actually do all the work under those bodies have wonderful ... in the early years and then doing their PhD’s and the first or the second post-docs, but then that contribution to the pyramid seems to be forgotten in their thirties, because then, why haven’t you got a permanent position? And the fact is then all the schools are not recruiting on that basis, and because they are not seen as the originator of the ideas. So that’s a very profound effect, I think, on the ability for individuals to emerge, and unless you have that possibility of your own creativity coming to fruition, being recognised as such, that’s very difficult. I don’t know how to get round that, because it’s all embedded in the funding and assessments, so in a
paradoxical way it might have well of been easier in the post-war period to become a scientist.

**MR:** It was a more optimistic period, wasn’t it?

**TS:** It was more optimistic, absolutely.

**MR:** Things could only get better then

**TS:** Yes, it could only get better, absolutely. And now seeing the setbacks for young people in terms of grant applications and also the totally inappropriate refereeing done, because of the way the committees are structured, it’s not surprising ... The Wellcome Trust has always seem to have done marvellously, but the Councils have not helped, I think, so it’s not surprising now, with the rejection of so much whole animal work, that our subject has virtually disappeared. We now have fellowships being created to support this, but the awards appear to be made to people lacking the relevant expertise – it has come all too late, especially in the atmosphere of animal rights. Of course, things like tissue culture and tissue slice have all reduced experimentation to ordinary office hours, instead of [people] being prepared to do a full night experiment. And so there are logistical, indeed cultural matters, which relate to our science that have diminished the recruitment. And some schools seem actually to discourage this sort of work, even in biomedical sciences. Since, in retirement, I am now at the Guy’s campus of King’s College, I see many young and older people now under difficulties.

**MR:** Nothing upbeat?

**TS:** Upbeat? Yes, there are still young people entering Physiology, just as I did eventually, not knowing what future lay ahead but simply interested in the subject. There is a lot more that I could say!!!

**PK:** We could literally have another whole tape full, and we have asked only about the times we don’t know about, not ...

**MR:** Just to thank you very much – we could go on. You’ve got to read all of this when it’s transcribed. Your papers stand for anybody to read but we’ve got some wonderful background, especially on the early days.

[END OF TRANSCRIPT]