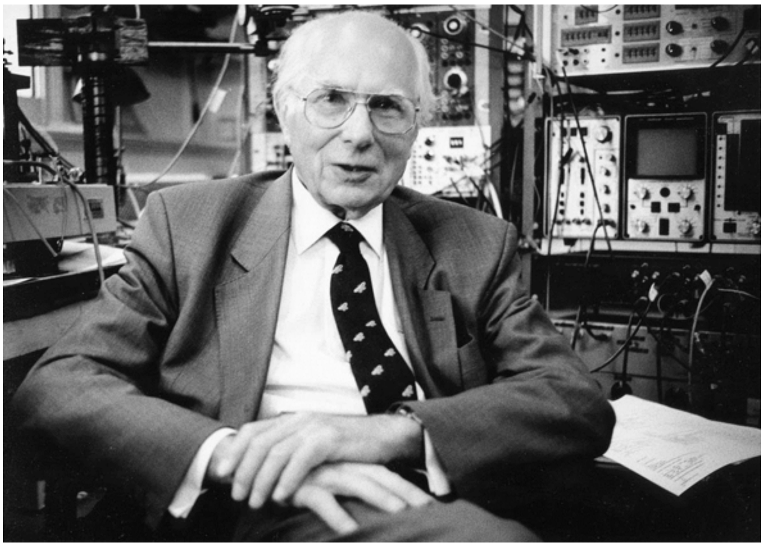
An interview with Sir Andrew Huxley

Conducted by Tony Angel at Trinity College, Cambridge in 1996

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

The Physiological Society



Sir Andrew Huxley photographed by Martin Rosenberg in 1997

AA: Now as I said, I am as naive at this as anybody else. You were seventy-nine this year, so you were born just before the end of the First World War.

AFH: Yes, that's right yes, in November '17.

AA: What are your first memories?

AFH: Well, I think I have a faint memory of meeting one of my great grandparents, my mother's grandfather. He gave me a small wheelbarrow, but I don't know how old I was. Otherwise, just doing things at home, none of which I can date. I don't have any datable memories.

AA: You came from an extremely well-to-do background?

AFH: Well, certainly well-to-do, I don't know about extremely, but yes, certainly a comfortable background. In those days there were usually three domestics in the house, there was a housemaid-cum-nurse, and her sister who was cook and usually a third. Well, I had one full brother two years older than myself, so we were brought up together and occupied much of the time of the nurse/nanny.

AA: At what age did you first go to school?

AFH: I suppose it was about seven. We did have a governess for a time, because my brother had had measles and was unwell for a time so we had a governess for him and I went to the same sessions with her before going to school. I think it was probably at age seven.

AA: And where did you progress from there?

AFH: Well, I started at University College School in Hampstead. It had originally been in Gower Street, but it moved out its main part to Hampstead, I think, in 1910, and it had a junior branch down near the heath and I went there at probably age seven and I had one year in the senior school and then my parents moved me on to Westminster, where I was a day boy. I was a day boy throughout.

AA: Education was obviously different in those days. Your generation have a more global grasp of the world than mine and I think I have a more global grasp than the young twerps we are getting now.

AFH: Well, I don't know about that. The present generation seems to get around the world in a way that we certainly did not. We always took our holidays on the west of Scotland except for one. My grandmother took us on a Hellenic cruise when I was about twelve or thirteen I suppose and we visited the famous sights in Greece and also Venice and a little bit in the south of France, where the cruise ended, but otherwise I don't think I went out of this country at any time before the Second World War.

AA: At school, I don't know whether your school was the same as mine, I went to a Grammar School at eleven and given an extensive education in everything and then when I was fifteen I had to choose between the sciences and the arts. Did you have to choose in the same sort of way?

AFH: Yes. I started at Westminster as a classic and I remained a classic until one year after the School Certificate and my mother had some difficulty persuading the headmaster to let me switch into science, which he regarded as a low occupation, but I did and had altogether three years in the upper forms.

AA: So you were swopping from classics to science?

AFH: Well, I had always been keen on and quite good at hand work of various sorts, played a lot with Meccano and zero-gauge clockwork railways when small. My parents gave my brother and me a quite good lathe when I was about twelve or so, which I still have. It's a screw-cutting, metal-turning lathe, which I still use for building my own equipment and I was self-taught on that and I think my interests had essentially been of a rather mechanical kind. I think I was quite good at the classics, and I found it fairly easy to learn foreign languages up to a not very professional level, but I think it was clearly right that I should switch into science and eventually I did. I was very well taught in physics. I had an excellent physics master, named Rudwick. Chemistry, less interesting, and I think I enjoyed physics more, partly because of being better taught and partly because in those days chemistry had a large element of memorising and I preferred physics, where within reason one could work things out, providing one knows the background and the theory and is reasonably proficient in maths. A small amount of biology in my last year and in those days entrance to Cambridge there was a scholarship examination at about Christmas, so I had I think just over two years of science, before I took the scholarship exam and got an open scholarship to Trinity, Cambridge, where I am now.

AA: What did you come to read specifically?

AFH: Well, in Cambridge the degree examination is known as the Tripos and is divided into two parts and there is a natural sciences Tripos in which in part one, well effectively, you had to do three sciences in part, that's over the first two years, and then you specialise in the third year and naturally I was aiming for a part-two physics when I came up so I naturally did physics and a small amount of mathematics and chemistry was an obvious second, but for my third, now I had known from fairly early boyhood, I had known Ben Delisle Burns, a long-standing member of the Physiological Society, a Fellow of the Royal Society, he's a few years older than me and lived near us in Hampstead and I think that at the time that I was making up my mind what to do, he was at Cambridge and I think he was probably doing a part-two physiology at the time and he told me that physiology was a nice, lively, subject in which even in the first year you would be learning about things that were still controversial, unlike physics and chemistry, where the part-one work was all stuff that had been cut and dried for either decades or centuries. So I took physiology originally simply as a make-weight, but got interested. I was very well taught by William Rushton [William Albert Hugh Rushton FRS, 1901–1980] and Jack Roughton [Francis John Worsley ('Jack') Roughton FRS, 1899–1972], met Hodgkin, David Hill, John Humphrey, Dick Synge, many others in the College, and got interested and in the Cambridge system there was no difficulty in choosing physiology for my final year, although I had originally expected to go for physics. So that was how it came about. Well, a long time later, I re-read, I must have read it before, but I hadn't been conscious of it, my grandfather, that's Thomas Henry Huxley's, short autobiography that he wrote and he says his original ambition was to be a mechanical engineer, which I probably would have become if I had done physics, and he went into medicine really through family reasons, he had two brothers-in-law who were medics. He says that he recalls that the only part of his medical course that interested him was physiology, because it is the mechanical engineering of living things, which is exactly my own interest in it. He'd tried to get posts in physiology, but there were none in England at that time. He tried, I think, in Scotland, Australia, Canada, without success, and made his career in comparative anatomy, palaeontology, evolution and so forth. But he continued to take a deep interest in physiology and was influential in getting physiology started in Cambridge, which was really done through my college, Trinity, which was wanting to get going in the sciences in 1870, asked my grandfather for his advice and his advice was make an appointment in physiology. The College agreed. They then asked him whom he should appoint and he recommended Michael Foster and the College did appoint him as praelector in physiology, that's a flexible teaching post and he started up and then a few years later the university instituted a department of physiology and Michael Foster was appointed as the first professor and first head. Well, that was really how the strong tradition in physiology in this college got going and I stepped into it, as I said, as an undergraduate. My grandfather was also influential in getting Mr Jodrell to endow a Chair of Physiology alone at University College. Previously Sharpey had been Professor of Anatomy and Physiology, but the College after, I think, some doubts, did accept the benefaction and I think that was the first full-time chair of physiology in England, England as opposed to Scotland, and I held that post from 1960 to 1969. So in various respects, I have benefitted from my grandfather's interest in physiology which was as I say, and mine own interest in it is very similar to his, but through the opportunities that he created I have been able to make a career in physiology.

AA: When you came to Cambridge, did you have a sort of cohort of students that you went around with or was it mainly a College cohort?

AFH: Well, there was a bit of both. From the social point of view, it's a College cohort, but to some extent there was a cohort in each department and yes I found myself doing practical classes with people of my year. Not all in the same college, but some of them.

AA: Did you get on well with them?

AFH: Yes. I was very shy as a boy, but I made friends very quickly and there was a very good atmosphere there in the College. People often say there's an advantage in going to a small college, where you are brought into contact with all sorts of activities. Well, Trinity, it was then by far the largest. It still is the largest, by not by such a big margin, but no difficulty in getting into social life at the College. And a great advantage of a big college, you can keep away from the people you don't have much sympathy with. Yes, there were groups in the College. In those days of course there were wealthy undergraduates, who had very little interest in academic affairs, and lived a perhaps slightly riotous, perhaps hunting, shooting and fishing existence, and I didn't have much to do with them and had no need to.

AA: Do you still see any of your fellow students or has time taken its toll?

AFH: Well, one close friend I made at that time is David Hill, son of A. V. Hill, who followed closely in his father's footsteps. He is now living in Yorkshire and doesn't come south very often, but when we travel north we try to visit him and we did so earlier this year. Richard Keynes came up towards the end of my time as an undergraduate. Another, Jeffrey Trevelyan, a nephew of the historian, who became Master of the College during the war, but I had also a family connection with him. Another, Paul Harwill [?] who died a year or so ago, who I had known at Westminster. His subject was anthropology and through him I did go to a number of lectures and discussions in the anthropology department, with much interest. But, yes, I made a fair number of fairly close friends, as close as any friends that I have outside the family.

AA: When you were being taught physiology, who did you like teaching you and who did you hate teaching you?

AFH: Well, I don't think there was anyone I hated.

AA: Not personally hated. You just thought they weren't good at teaching.

AFH: No, I was taking it in the sense of the teaching. In Cambridge the teaching is divided into university teaching in the department, which in the science department, consists of lectures and practical classes and when I came up the head of the department was Sir Joseph Barcroft, I came up in 1935. He retired in 1937 and was succeeded by E. D. Adrian, so my part one lectures were some by Barcroft, Rushton, Roughton, Winton, Tunnicliffe, an extremely good teacher, but mostly involved in College and University teaching, not much research, until he retired. I think those were the principle lecturers in physiology. Neville Wilmer ran the histology class. In Cambridge histology is taught with physiology and not with anatomy, because I, like a substantial number of the physiology undergraduates, was a science student, not a medical student at that time, and they are taught together, and from the point of view of the science students it's extremely important to have classes in histology. I enjoyed histology. I had had an interest in microscopes as a boy, which has continued to the present time. And the other half of the teaching in Cambridge is supervisions, things that would be called tutorials at Oxford, that is to say once a week spending an hour with a member of the teaching staff of the College and that for me was principally with William Rushton and Jack Roughton. Rushton, of course, principally on the nervous system, muscle, Roughton on blood gases, proteins, biochemistry. I never had a proper course in biochemistry, but there was a substantial amount of biochemistry of physiological chemistry in the physiology course, mostly taught by Tunnicliffe in the department.

AA: When you were an undergraduate, the emphasis was more on physiological chemistry than physical mechanisms. Did you not find that?

AFH: No I don't think I would say so. There was, as I say, a substantial amount of physiological chemistry in the course and many of the people taking physiology would have taken biochemistry as well. Biochemistry was the one year option in part one and was also a part two subject for the final year and as I say, I did not take a biochemistry class.

AA: Presumably by this time your mother had come to terms with you becoming a scientist and not a classical scholar.

AFH: As I said, my mother encouraged it. It was my headmaster who had been resistant. My mother, she died only two and a half years ago, at the age of 104, she was very skilful with her hands and had done a fair amount of woodcarving when young and had done a lot of very delicate sewing and knitting and she encouraged me in the use of my hands for which I am very much indebted to her for, and I am sure it was her influence that we were given that lathe which I mentioned.

AA: OK, now we come to the next choice time, because obviously if you had read physics with physiology and some maths, you could have gone anywhere. What really made you choose applying a lathe to a piece of animal tissue?

AFH: Well, I think an element of it was that I had been very well taught physics at school and part one physics at that time, a lot of it was going over things that I was already familiar with, so I was not much stimulated by the physics course. Unfortunately A. J. Ratcliffe was on sabbatical leave when I was taking the course. He was a first-class lecturer in electronics and if I had been lectured to by him, I might have been more inspired. But I found physiology extremely interesting. It was very well taught, both the university courses, lectures and practical classes were very good, and Rushton was an inspiring teacher. And I got to know many budding physiologists slightly older than myself who were living in College as I was, including Alan Hodgkin, David Hill, and other well-known names, including many biochemists, like Dick Synge, John Humphrey. But I was not attracted to chemistry. As I say, I have never liked chemistry and these cycles that we had to learn didn't attract me, but physiology, this thing of how things work, is what I have been interested in from a boy. So I decided to move to physiology for my final year and I was advised to become medically qualified, I think really for career reasons, I became a medical student and my third year I spent doing anatomy for the second MB examination, not a degree examination. In Cambridge in science the minimum requirement for a BA was to have been classed in part one of the Tripos and to have resided in Cambridge for at least three years and in those days many of the less bright students took part one straight over three years, instead of two, and they would then get a Cambridge BA on the strength of that. So my Cambridge BA, indeed the only degree that I hold that's properly earned by examination, or thesis, I have never written a thesis, my only honest degree is a Cambridge BA based on part one of the natural sciences Tripos and a certificate of diligent study in my third year when I was dissecting the corpse. I enjoyed dissection now and again. Part of this is interest in handwork and in microscopy. Some people don't enjoy anatomy, but as I say, I did. I got my second MB. Indeed, in the mollification after finishing part one, I had to take biology. Well, the first MB I went to a crammer for a month or two in that summer. So then I did part two physiology, already having a degree, so I had a BA, and that was in 1938/39. A fascinating course. Twelve of us doing the course. Now I think five of us got firsts, John Waterlow, now a Fellow of the Royal Society, who became Professor of Nutrition at the London School of Hygiene and Tropical Medicine, John Gray, who became a professor, not head of department, but a professor at University College of London, and then became Secretary of the Medical Research Council. Dick Hammond also got a first, son of John Hammond, famous agricultural physiologist, Nachman Ambache got a first and became well-known in pharmacology. Andrew Barlow, elder brother of Horace, Fellow of the Royal Society, also had a first as I did. And then there were three or four 2.1s, there was one 3rd and there were two who were not candidates for honours, people who were taking the physiology part two who already had a degree from another university and were spending one year in Cambridge. So it was an extremely interesting and lively group of people – one of the most enjoyable years of my life.

Another person who stimulated me to move into physiology and was one of our teachers in the part two was Glen Millikan, son of the famous physicist of the oil-drop experiment, an extremely lively, outgoing, person. He had been a research Fellow in Trinity. He was a lecturer in physiology and still living as a bachelor in Trinity, but extremely good to many undergraduates of my generation – took us out in his car at weekends, expeditions of various sorts, interested me in his research, he was working originally with Jack Roughton, but became independent, but he was recording changes of oxygenation, particularly of myoglobin in the cat's soleus muscle by a photoelectric device. Well, after I was settled into physiology, he began developing what would have been the first spectrophotometer for biological use. He was in contact with Keilin [David Keilin, 1887–1963] who, of course, had done masses of spectrometry or spectroscopy on cytochrome and other pigments, mostly with a direct vision, with a visual spectroscope attached to a microscope. Millikan started developing this spectrophotometer in collaboration with Unichem, a newly formed company at that time, and he involved me in this, both mechanical and optical things that I was interested in, and when he went back to the States at the beginning of the war, I sort of took over the contact with the Unichem company, but well the project collapsed because Unichem became involved with war work and I moved into clinical medicine for a year and when I went to London early in 1940, contact with Unichem came to an end. But as I say, he was one of the people who stimulated me in many ways. He married the elder of the two daughters of Sir Ernest Mallory who was killed on Everest, married her just before the war, they moved to the States on the outbreak of war, and Glen was killed in a climbing accident very shortly after the war, so he's not as well-known as he deserves to be. The younger of the two sisters was another of those taking part two physiology in '38/'39. She married another American David Robertson, who was then doing a year in Britain as a postgraduate. A delightful person and yes they went back to the States. She died of breast cancer in the 1950s – quite young.

AA: You graduated in 1939?

AFH: Technically, I graduated in 1938, that is to say I got my degree on the strength of part one and three years residence, but I finished undergraduate work in the summer of 1939.

AA: Was that a difficult time for students or were students having such a nice time, they didn't see the cloud gathering?

AFH: Well, everybody was conscious of the gathering clouds. Of course, there had been the Munich crisis in the autumn of 1938 and Hitler had marched into the rest of Czechoslovakia in 1939 and most of us regarded war as at least probable and perhaps inevitable, but certainly probable. Very many of my contemporaries were far to the left in politics, several of them card-carrying members of the Communist Party. I had never been interested in party politics, so I wasn't one of that group, but as I say, many of my close friends were. It didn't interfere with friendships and I think it was a combination of a feeling against Nazi Germany, uncritical regard for the Soviet Union and a feeling against the system in Britain that had led to the great unemployment in the early 1930s and a general dissatisfaction with the system that it then was. But as I say, I didn't take part in any of these activities.

AA: When you finished your undergraduate studies, you then moved to London to continue in medicine is that right?

AFH: No, I had been expecting to do a couple of years research in Cambridge in physiology, before starting as a clinical student, so I had not got a place at a clinical teaching school and there were several people in my position and the then Regius Professor of Physic in Cambridge was John Ryle, he had loved teaching at Guys, where he had been before coming to Cambridge, so he was needing to teach and here were us needing to be taught, so he started an introductory clinical course at Addenbrooke's, the Cambridge Hospital. It ran for the first six months of the war. We were taught mostly by him and by Professor R. A. McCance [1898–1993], the world's leading authority in nutrition, who died, I think, only about two years ago. And that ran for the first six months of the war and then I had six months at University College Hospital in London, though many of the students had been evacuated first to Watford and then to Cardiff, but the teaching there stopped when the bombing of London began in 1940 and I was diverted into quite different war work. So I never completed in medicine, but I am very glad that I did have that year. My career has been largely teaching physiology, largely to medical students and well it enables one to see the clinician's attitude to things which has to be very different from the research worker's attitude. I enjoyed the clinical work, if I had completed my course, I don't know whether I would have come back to physiology, or whether I would have stayed in clinical work of some kind, or made a career in work, it's impossible to judge. If I had, I think I would have enjoyed it. As far as that goes, if I had stayed in physics, and probably become an engineer, and I think I would have enjoyed that, but I have at least equally enjoyed being a physiologist. So I have no regrets.

AA: What sort of war work were you suddenly finding yourself doing?

AFH: Well, this was one of many influences from A. V. Hill. In the First World War he had been a leader of a team developing anti-aircraft gunnery at HMS Excellent, the naval gunnery school at Portsmouth. He had been the principle author of the textbook of anti-aircraft gunnery, the work that I had to dip into in the second war, multi-author work. I think that something like seven of the authors already were, or later became, Fellows of the Royal Society, but A. V. Hill, through his interest in anti-aircraft gunnery, had been in contact with General Pile [General Sir Frederick Pile, 1915–2010] Commander in Chief of Air Defence G.B in World War II], the general officer commanding anti-aircraft command, and felt that Pile needed a scientific adviser. Hill put Patrick Blackett [Patrick M. S. Blackett FRS, 1897–1974, Nobel Prize winning physicist] in touch with Pile. They hit it off. Pile appointed him as scientific adviser. This was not quite a year into the war. All the physicists and mathematicians were already busy with war work, so A. V. Hill proceeded to provide Blackett with a team of physiologists. There was Leonard Bayliss, son of Sir William. Leonard was then a reader at University College London and then A.V.'s own son, David, who I have already mentioned, a year or so senior to me, and through him, myself. So we started off, three physiologists, doing operational research on anti-aircraft gunnery, most of the work was trying to apply radar data, very crude at that time, early in the war, to night firing by anti-aircraft guns, heavy anti-aircraft mostly, the 4.5 inch gun, later the 5.25 came in. So we spent quite a lot of time going round the gun sights on the outskirts of London, trying to improve the accuracy of firing, trying to discover what errors were principle trouble, adapting the available predictors to work with this very poor radar data, but it was interesting, I think of some use. All this blind firing remained very inaccurate, until radar on a much shorter wavelength became operational much later in the war, but this three-metre radar, accuracy of only a matter of degrees instead of minutes of arc, only gave a very poor indication of the position of the aircraft targets.

AA: Was that then a full-time occupation, you spent all your time doing it?

AFH: Yes. That went up for a bit over a year. Blackett moved on from anti-aircraft command first to coastal command and then to the Admiralty where he became chief adviser on operational research and he then got me transferred to the Admiralty and I continued doing operational research in the gunnery division of the naval staff in Whitehall. This was sort of the user side of the gunnery in the navy, quite distinct from the naval ordinance department which had largely been evacuated to Bath, but that was the department that was concerned with the development and production of gunnery equipment. So we were in close contact with them. But my work was again on the user side, again adaptation of radar data, experimental firing trials, looking for sources of error and so on. Again, very interesting.

AA: Was it highly mathematical as well?

AFH: Yes. There was a fair amount of mathematics. I got familiar with sort of medium level statistics, both while with anti-aircraft command and while with the navy. A certain amount of theory of servo-mechanisms, automatic control of guns of power, gun laying was coming in and that was servo-control at a fairly high level. Yes, all of those things were useful later on when I was doing with Hodgkin servo-controls, membrane potential of nerve fibres, and then later servo-control of the length, of sarcomere length in muscle fibres. Well, statistics are inevitably useful in all contexts in physiology.

AA: And presumably the second order of differential equations which eventually ...

AFH: Yes. I didn’t actually do anything closely analogous to that during the war, but I had met enough of those numerical methods in the mathematical course that I took as a natural science student at Cambridge. Well, I wasn’t horrified by the idea of solving differential equations by straightforward numerical methods. I spent the best part of a year grinding away with a Brunsviger calculator that I still have. I think it’s on the shelf up here.

AA: We have the embryonic half of the Hodgkin–Huxley theme with a little bit of fortuitous help from Adolf Hitler.

AFH: Yes.

AA: When did you start as a real physiologist? Other than helping the war effort.

AFH: Well, Hodgkin took me with him to Plymouth in the long vacation of 1939. More or less immediately after I had finished part two physiology. I forget just whether we had taken a holiday in Scotland, as we customarily did, before going to Plymouth. But we had a month or so at Plymouth and I left a week or two before war was declared. We would have stayed longer, if it had not been for the threat of war. We went there with the idea of doing some experiment on the giant nerve fibre of the squid. Hodgkin had already done experiments on it in the United States in collaboration with K. C. Cole, so he was already familiar with the preparation, but we didn't have any very clear idea of what experiment to do in the physiology course. I found myself cannulating small arteries. So I cannulated one of these squid giant fibres with the intention, at Alan's suggestion, of measuring the viscosity of the ectoplasm by dropping liquid, but there was nothing further to do about it, but having got the nerve fibre set up vertically with a cannula in it, surrounded by sea water, which is the effective Ringer's solution for animals like the squid, Hodgkin thought of pushing an electrode down the inside, which we did and we were surprised and pleased to record the full size of the resting potential, which had previously been recorded only with external electrodes when you record only a slightly indeterminate fraction of the resting potential, but we got it directly, and we got this value of 50 or more millivolts and when we stimulated the fibre and it gave an action potential, instead of doing what it ought to have done on Bernstein's theory, which was that the internal potential ought to have approached but not got beyond the zero potential, the external potential, in fact, it went far beyond the inside, having started in the resting state of about –50 millivolts relative to the external solution at the peak of the action potential, it went … +50. Well, I remember being delighted at getting something like 100 millivolts of action potential. I think, I am quite sure, I did not appreciate the significance in the way that Hodgkin immediately did, that this was entirely contrary to the Bernstein theory in its complete form. Hodgkin had already had hints that the action potential might be bigger than the resting potential from external recordings on the fairly large nerve fibres that you can dissect from crabs and lobsters. Well, the recording from the outside of the nerve against an injured point, where the potential difference between inside and out must be zero, he did record action potentials that were bigger than the interim potential. But, of course, there are these big uncertainties about the absolute value of the potentials that this represented. There had been an earlier hint of an action potential bigger than resting potential from work of Faiver [?] in Germany on, I think, frog muscle, again using external electrodes, but it's mentioned only in a small type section of the paper, I think, not mentioned in the summary and had not got generally known.

Well, we just had time to get some records and publish some records. Hodgkin wrote it up swiftly and we sent it into Nature and it was published in the autumn of 1939, just as an observation, well of obvious interest, but without any theoretical discussion and, of course, that's what we came back to after the end of the war.

AA: We got to just past where you got your first intracellular action potential and had published it in Nature and then, of course, you had to go back to your war duties.

AFH: Well, this was just before the war and after that I was a clinical medical student for a year, first year in Cambridge, then at University College Hospital and joined up again with Alan Hodgkin just after the war.

AA: Was that back here in Cambridge or at Plymouth or at both?

AFH: No that was here.

AA: What was your relationship? Was he your PhD supervisor?

AFH: I never did a PhD.

AA: Would he have been?

AFH: Yes, yes, he would have been.

AA: How did he choose you? I mean PhD supervisors usually choose their pupils.

AFH: Well, you'd have to ask Alan that question. He invited me to join him in research, and invited me down to Plymouth. I also had an invitation from Neville Willmer [Edward Neville Willmer FRS, 1902–2001], which I was quite interested in, because I have this long interest in microscopes and he was still partly on his tissue culture cell physiology interest, but partly already switched to colour vision and it was a subject I was interested in, colour vision experiments, at one point. I was quite attracted by both of those invitations and chose the one with Alan, but, as I say, I might have gone either way.

AA: Then both of you started to take it to pieces, as it were, having seen what you had got, you then wanted to know how it was produced. What led you to think of a feed-back amplifier?

AFH: Well, that was definitely Hodgkin not me. Of course, it was also an approach that K. C. Cole used. Well Hodgkin became very expert in electronics. I mean his undergraduate work was in general biology. I think he took physiology, botany, and zoology, but he made himself very skilful in mathematics, physical chemistry and electronics before the war and he spent most of the war in short-wave radar development at Malvern at QRE the RAF place. I visited him there a few times during the war, because there was also an army establishment that I visited there. I think that was before the war, I am sure, he already had the general idea that the essential thing in the action potential that made it all or none was an unstable current-voltage relation and he wanted to get at the current-voltage relation and that it might not be unstable if you controlled voltage, just as in gunpowder if you manage to control its temperature, it would not explode, it would burn at an increasing rate as you raised the temperature. And, of course, in a sense the obvious thing to do is just to push down a wire and alter its voltage and hope that the axoplasm will follow the potential of the wire and essentially what Cole and Curtis did, but Alan Hodgkin realised that electropolarisation would mess up anything that you tried to do of that kind, because the current densities on a wire are quite high, and any electrode will polarise so that the voltage of the inside of the fibre will not exactly follow the voltage of the wire. So he realised that you needed two electrodes, one to record the potential and the other one to put the current through, so that polarisation of the current wire would not upset your measurement of the potential. Now Cole did use feedback to control the voltage of his wire, but he did not use, initially and they worked the same sort of time as our first measurements after the war, he did use feedback to provide effectively a low impedance voltage source to his wire, but he did not use two wires, so as to avoid the effects of electropolarisation and I think for this reason his system was alright for the initial currents, the sodium currents, but it certainly did not work properly for the long-lasting potassium currents when much larger quantities of electricity have to flow. I think Hodgkin had become familiar with these things from his war work. It certainly originated with him and not with me. I remember him say he wanted to do this to control the voltage of the inside and I said “well, why not just stick a wire down and put on the potential that you want” and he had to explain to me that this would be defeated by electropolarisation.

AA: How long did this superlative partnership last for? I mean it has lasted for ever, but for these particular experiments?

AFH: Well, the reason it came to an end, after we had written the papers that we published in 1952, was that we could not see what to do next in relation to nerve excitation. Of course, immense progress since, but all unforeseeable. They realised that if there were gates controlled by membrane potential, they would have to carry charge so that they would move charge across the membrane when the gate was opened, and we looked for gating currents of this kind, but they were not detectable with our methods of recording, averaging had not been invented at that time, so that seemed to be a dead end and we realised the currents were small and therefore each gate must pass a large number of ions, but how large, totally uncertain. I don’t think we even thought about recording single channel currents. We were certainly thinking in terms of channels, but the idea of doing an experiment like Neher and Sackmann didn’t enter our head, and if it did, certainly didn’t enter my head and if it had it would have been dismissed immediately as being totally impracticable. Watson and Crick had not yet published their paper. It was 1953. So the genetic approach simply wasn’t something that you could even think about. We did think about looking for carrier molecules, but the quantities involved would have been so small that that seemed to be ruled out. We realised that something like tetrodotoxin would be extremely useful to get rid of the ionic currents and see if there is something left, which, of course, there is. But neither of us had a pharmacological training, so that didn’t seem a promising approach. So we moved into different fields. I mean Hodgkin, of course, continued on nerve and indeed on these giant fibres for a good long time before he switched into vision, but it was largely in relation to sodium pump and things of that kind. That was how the active partnership came to an end.

AA: During that time you must have got employment in the university?

AFH: Well, I came back. I was elected to one of these Research Fellowships at Trinity during the war under special wartime regulations that allowed the College to elect a research fellow without a full-size dissertation, and I put in a token dissertation of a few pages, based on what Hodgkin and I had done at Plymouth, recording the overshoot of the action potential and I suppose with strong recommendation from Hodgkin and possibly from Rushton and Roughton, who were both Fellows of this college, and who had taught me, but anyhow by reputation and recommendation I was elected to a Research Fellowship and, as I said earlier, I never wrote a full-length dissertation. I didn’t take a PhD because of the war, and I was let into a Trinity Fellowship under these wartime regulations. These are four-year things, but, again, under wartime regulations, they were automatically extended by the amount of war service. So I had four years available, and the natural thing was to come back to the College. The College provided me with a room to live in College and I also had a university Demonstratorship. So this was the ordinary start of academic life but delayed by a few years and I then continued. I was, in fact, elected to one of Trinity’s senior Research Fellowships, I think actually before my junior Research Fellowship had come to its final end and I held up for a year or two and then I had a teaching Fellowship in the College for a few years, before I moved to University College, which was in 1960.

AA: Was that a pleasant move?

AFH: Yes. Another point where A. V. Hill influenced me. As I say, we'd got to know each other through his son David and then working at Plymouth, we'd visited him at his holiday place in Ivybridge and, in fact, during the war when I was working with David for some months, I was living at their home between Hampstead and Highgate, so I got to know him well as closely as one can know anyone so much older in age. Of course, in 1960, he was long-retired, but still very active at University College London, and when he heard I'd been offered the Chair, he wrote saying that he hoped I would accept, saying that he'd been in a similar situation in, I suppose, it was 1920, returned after his anti-aircraft gunnery during the war, had returned to Cambridge, comfortably established, I think he had a university lectureship I think in physical chemistry and teaching Fellowship at King's College, he'd started at Trinity, but he was comfortably established in Cambridge, when he was offered the Chair at Manchester and Lord Rutherford, the physicist, had just come back to Cambridge, to Trinity College, from Manchester, so A. V. Hill asked Rutherford's advice, and A.V. quoted this advice to me and said that he had followed it, that he had never regretted doing so and the advice was "Cambridge is a splendid place when you are young, and Cambridge is a splendid place when you are old, but for the middle of your life, for God's sake get out!". No, I don't regret at all having gone there. It gave me opportunities that I wouldn't have had had I stayed here. I mean I had thoroughly enjoyed being here and was very comfortable, but no University College at that time certainly was, and I think still is, an extremely pleasant place to be, very good general atmosphere, I think perhaps more cooperativeness between departments than exists in some universities, but I think being there was enjoyed by all levels, undergraduates, postgraduates, post-docs, staff and I certainly enjoyed being there.

AA: Did you take to being head of department, or did you make yourself be head of department?

AFH: I think I rather ticked over. I didn't do anything very active in changing the character of the department. I mean, I am not the right person to do that sort of thing. But it's interesting and it's a natural, I suppose, stage in one's existence. Things were easier in those days than they are now for a head of department. A large slab of government research funds came direct to universities. Of course, in the case of London University, apart from Imperial College, it came direct to the university and then the university shared it out among the colleges. But still it came with a large slab supporting research and it was only at the end of my time as head of department that it began to be important to get an outside research grant in order to do research. Mostly, people existed on university funds. I remember, I think, one of the first substantial grants in the department was for Norman Saunders who was working on sheep in foetal physiology and he needed a substantial number of large animals, which, of course, are expensive. But the present pattern was something that already existed in the United States and we were very glad to be spared the effort of writing grant applications and refereeing other people's grant applications, which swallows an enormous amount of time and effort now.

AA: What did you think of your contemporary professors in physiology while you were around. What did you think of people like G. L. Brown? [Sir George Lindor Brown FRS, 1903–1971]

AFH: Well, of course, he was my predecessor not my contemporary. So I didn't overlap with him, but he had done admirable work. It was all in fields where I hadn't worked myself, but major things like his share in establishing cholinergic transmission when he was at

Hampstead at the National Institute, mostly central nervous system; his myotonic goats, that's another important entry into muscle. And, of course, he was probably a lot better than I was as a head of department, because he was so outgoing and talked things over with people more easily than I would. A story that circulated at UCL about him when I came – there was a departmental grants committee in the College which provided a lot of money for equipment in the various departments, sharing it out between departments. It was government money again. And on one occasion he applied for a digital audio-frequency generator and this was duly granted and he then explained to the committee that what this was was a piano for the departmental common room. He then withdrew his application, to avoid embarrassment. But I think he was allowed a piano. An old upright stayed in the Starling room for a long time. He established a lot of social things in the department with great success. An annual trip to the College playing fields, staff both academic and technical staff and secretaries, took part in various games and frivolities. Parties in the staff common room. He moved on to the Chair at Oxford and organised an annual cricket match between his department and UCL. So I am sure, well in all the social respects, he was an admirable head of department. I am sure he was an admirable head of department in all ways. But as I say, I didn't overlap with him, so I never had direct contact with him in academic matters.

AA: The two you did overlap were George Dawson and Bernard Katz wasn't it?

AFH: Yes, Katz succeeded A. V. Hill. Under A. V. Hill the biophysics thing at UC was a part of the physiology department. I think it was not even technically a sub-department, but it was a unit funded, I think, at one time largely by the Rockefeller Foundation, but without teaching duties, and then when A.V. retired it was made into a separate department and Bernard Katz became head of it and continued to run it in very much the same style as A. V. Hill had done with many excellent visitors from overseas, really a research department. Bernard Katz continued to assert that he was a physiologist, and not a biophysicist. In fact, a lot of the work done in physiology was more biophysical than things being done in biophysics. Many people would call my work and Doug Wilkie's work biophysics, although I certainly regard myself as a physiologist as I say Katz did. And together with Miledi he moved into ultimately molecular genetics of excitable processes, well almost biochemistry. Well, whether you call that biophysics or not, I don't know. That's when I am asked what biophysics is my usual answer is "biophysics does not exist, one half of it is physiology, and the other half is biochemistry". Well, in so far as my work is biophysics, it is in the physiological half.

AA: What made you choose muscle to go for is the next question?

AFH: Well, one thing was that David Hill, A.V.'s son, who, as I have said, I became a friend of his as an undergraduate, he was a year or two senior to me, he had got a Research Fellowship at Trinity under the proper competition just before the war, and he also came back to Trinity to the physiology department immediately after the war and he was working on muscle and so he was the natural person to give the lectures on muscle to the part two class and then after a few years, I forget exactly which year, he was appointed as physiologist at the Marine Laboratory at Plymouth and I was asked to take over his lectures and he gave me his lecture notes and that was the immediate way in which I became interested in muscle. And the other factor was my long-standing interest in microscopes, which dates back to boyhood again, with a couple of Victorian microscopes in our home, not from my Huxley ancestor, but on my mother's side from the days when many amateurs had their own microscope and I had become interested in microscopes and microscopy and I had had an idea of how to build an interference microscope and reading from David Hill's lecture notes, I became conscious of the phenomenon which was well known in the late 19th century of the reversal of striations. When you look at striated muscle with an ordinary light microscope, the contrast between the bands is due to differences of refractive index. The A-bands which we now know contain a lot of myosin have a higher refractive index than the I-bands and when muscles shortened heavily that contrast reverses – the place where the I-band had been requires a higher refractive index than the A-bands. And this had been studied in the 19th century on insect muscle fibres, choosing muscles with much broader striations than vertebrate ones and with fibres teased out and watched under the microscope they undergo spontaneous contractions, but this depended mostly on visual observation and you can't control the activation of these fibres. There was no indication whether this change in the striation pattern was simply a function of the length of the fibre, or whether it was due to tension development or whether it was due to activation, something that had to happen before contraction. Now, the other key observation known at that time was the work of Ramsey and Street around 1940 getting undamaged single fibres from frog muscle, well both the fact that you can get undamaged single fibres that will work for a matter of days and give proper propagated responses. Another aspect of that work was also very important, but it seemed to me that one could get an idea of the significance of this reversal of the striation pattern by seeing whether it happened when you activated a fibre without allowing it to shorten or whether it was only as it developed tension, or only as it shortened. As I say, you can control activation well in these frog muscle fibres, but they have very narrow striations and large diameter so that with an ordinary light microscope, well you see the striations, but you can't reliably measure their width, or you just know that the thing has got a striated pattern and you can't get a reliable pattern from it. What you need, is a microscope that will work with high numerical aperture to get high resolution and a small depth of focus. Now you can do that on a thick specimen like frog muscle fibre with polarised light, but this reversal phenomenon does not happen in polarised light. The position of the A-band remains birefringent, even when the contrasts due to differences of refractive index has reversed. So we needed a microscope that would give a good optical section in a thick specimen where the contrast is due to difference of refractive index, that is to say an interference microscope and I had an idea of how to make one. Of course, in a sense, this is what a phase-contrast microscope does, but phase-contrast microscopy does not work satisfactorily on a thick specimen. It needs a thin specimen. If you go out of focus either way, the contrast reverses, so if you have a thick specimen, you've got a mixture of positive and negative contrasts, and its uninterpretable. So when Hodgkin and I had run out of ideas on excitation, practical things to do on excitation, I had this idea of a research project and of an approach based on my boyhood hobby, of how to bring it about. So the first step was to develop an interference microscope, which I did, first a low-power one – quite easy to make, then a high-power one – much more complicated – and required a very special prism which I had to have made specially by the firm of Deck who were extremely cooperative and helpful. And then when I got that working, I then had Rolf Niedergerke as a collaborator and we looked at these isolaters. I got the dissection going just following what Ramsey and Street had done and looked at it under this interference microscope and immediately saw when we stretched one of these fibres that the A-bands stayed at constant width. This was the reverse of what was in all the textbooks. Niedergerke knew a little about the 19th century history of physiology and had a recollection of work of Krause [Johann Friedrich Wilhelm Krause, 1833–1910], so I followed this up. This was all common knowledge at the end of the last century. Everybody knew the A-bands stayed at constant width and they knew birefringence was due to little droplets in the A-band and that myosin was in the A-band and all this was either lost or back to front by 1950. That was how I got into it. As I say, this combination of getting interested in muscle through having to give lectures on it, an experiment based on things I learnt from David Hill's lecture notes, combined with exercising my hobby in microscopy. It's a very good recipe for interesting work.

AA: How did you get on with George Dawson?

AFH: Oh, very well. At first, when I was at UCL, I think his appointment was still at Queen Square and then I think he came to us. I think he had a non-established chair at University College in the department. We got on very well. Good friends. It was very sad when he died rather young.

AA: Because he was very much like you. He took his childhood hobbies into his research life with him.

AFH: Yes, of course he was the first person to do averaging of responses and recorded nerve responses through the skin and brain responses through the skull, repeating the stimulus many times and getting an average, which he did by a mechanical switching arrangement which worked very nicely but, of course, was very clumsy compared with electronic methods which were developed later largely by him and Dirk Norton [?]. I forget what was the right way to describe his appointment.

AA: Dirk was Principal Physicist, I think, at Queen Square.

AFH: Oh yes, at Queen Square, and he was indeed an extremely ingenuous electronic person and he and George did a lot of things together and I think the first electronic averager, which was referred to as a dry CAT as opposed to a physiologist's wet CAT (the computer of average transience). I think theirs was the first in the world wasn't it? I am not quite sure about that. There might have been an American one.

AA: I think it was. There was a nemotron CAT produced by the Americans, just before George produced the first digital averager as opposed to his first analogue averager, which would never be allowed today because the Health and Safety Executive would never allow us to spin mercury round in a bowl.

Why did you leave UC, because in theory you could have gone on for ever?

AFH: Because I was appointed Master here. I was very honoured you see. I did nine years as head of department, from '60 to '69, and then the Royal Society gave me one of their research professorships, a very nice appointment, and you see allowed me to stay on and carry on my research there, which I did, and I did so until called to the Mastership here, which was not a very attractive appointment, but one would need very strong reasons to decline.

AA: If we slip back a bit to your Nobel Prize with Alan, what did that do for you personally, not academically or ambitiously, but what feeling did it give you?

AFH: Some people ask did you feel inclined to sit back on your laurels? And my answer to that is very definitely no. I mean, I have been at least as active scientifically ever since as I was before, and from that point of view, I would say it was an encouragement. It means that one has been very highly appreciated by one's peers and I think in the case of all the Nobel Prize winners I know it has acted in that sort of way. Hodgkin, Katz, and Eccles, Perutz, and Kendrew and all the rest of them have carried on at least as actively as before. I don't think it made any difference to appointments that I have held. Because the Royal Society, on the whole, has managed to get Nobel Prize winners for its presidents, except in the case of Michael Itayah who is now Master of Trinity, but he's a mathematician and there is no Nobel Prize in mathematics, but he'd had the Fields Medal which is the equivalent. But whether it influenced the Royal Society in that case, I have no idea, and no means of telling. You'd have to ask other people. But I am sure that in a sort of technical way, it hasn't in any direct way influenced my career. It has no doubt brought me various invitations to take part in meetings and speak and so on. The financial aspect of it was, in fact, quite important, as we have a large family and educating them, of course, is expensive. They were all at fee-paying schools, though they all started in the village primary school. Although there was a system of grants for university education, they depend on parental income and in those days there was a system that if you had a child at university, that gave you an additional grant, but if you had two at university at the same time, you only got one extra grant, so putting them through university was a substantial expenditure. And I had started academic life late, as a result of the war. I wasn't, in fact, given any seniority on that account. My wife has never had a paid job, so we were dependent on my salary. I had inherited some money from my grandmother which went towards buying a house, but as I say the financial aspect was very valuable to us.

AA: This is a difficult question to ask. How much sour grapes are there attached to a Nobel Prize? That should have come to me rather than your sort of attitude.

AFH: It must exist, but of course it must depend inevitably on the individual. Did either Cole or Curtis leap up and down and say "unfair I should have had it"? Hodgkin and I both felt that Cole deserved a share and I think we would have been very happy had Eccles shared his with Bernard Katz and von Euler perhaps who did share it with Bernard. The rules of the Nobel Prizes strictly forbid dividing it among more than three, but I think he wouldn't have been human, Cole, if he hadn't felt that he deserved a share, as I think many people did. Of course, Cole did make one or two important mistakes. He was initially against the sodium idea for two reasons, one he recorded an action potential with an absolutely enormous overshoot that was an electronic artifact. He'd put in compensation for the electrical capacity of the microelectrode, but he overdid the compensation and got an action potential so large, that it could not have been due to sodium entry and in one of their papers Cole and Curtis stated very clearly that the action potential was unaffected by replacing the external solution with a sodium-free solution. How they made that mistake I have no idea. Although as I said earlier, he did use electronic feedback to control the voltage of his internal wire, but it was only a well-known electronic trick for getting a low impedance source, but I am sure that he could have done equally well, by just applying a voltage from a low-impedance voltage divider to his wire, by using feedback, while in Hodgkin's arrangement the feedback was an essential element in getting rid of this artefact due to electropolarisation. But I think probably the more important factor was that Cole did not take the analysis further in the way that Hodgkin and I did, showing which ions were carrying which component of the current, separating these, and showing that the empirical equations fitting our observations would account for the propagated action potential and well Cole did not do anything of that nature. But of course his earlier work, well the experiment of Cole and Curtis when he demonstrated the very large fall in electrical resistance of the membrane …

AFH: Yes, well I was just speaking about the famous experiment of Cole and Curtis before the war when they demonstrated by means of a high-frequency alternating current bridge that the electrical resistance of the surface membrane falls drastically during the rising phase of the action potential and this was a strong confirmation of one aspect of Bernstein's theory that the action potential is due to a drop in the resistance to ions crossing the membrane. But, of course, then the observation that during the action potential the inside of the fibre goes strong positive, that contradicts the other aspect of Bernstein's theory which was that the membrane became permeable to all ions irrespectively, so that it created a short circuit across the membrane, which would have brought membrane potential near to zero, but would not have allowed for reversal. So one major aspect of Bernstein's theory was confirmed by Cole and Curtis. Something of the kind had been done earlier on a plant cell, on I think it was *Nitella*, one of these fresh-water algae by I think it was Fessard [Alfred Fessard, 1900–1982] and I forget who else, but that was a much easier experiment because that was a long-lasting event and he did not analyse it in the way that Cole and Curtis analysed the drop in impedance of the membrane and showed that it was a drop in the resistance, there was very little change in the capacity. This was a technical triumph at the time of very great importance to what one thinks about the action potential.

AA: You became Master here. I know you enjoyed that very much. What did you achieve during your Mastership?

AFH: I would say I didn't achieve anything, except that the College still kept on in more or less the way it had been before during my Mastership. I wasn't responsible for any notable development in the College. Again, as when I was head of physiology at University College, I think I am not the person to innovate in that sort of way.

AA: But you did change the entire lighting of all the paintings in the lodge didn't you?

AFH: Yes.

AA: So that must have been an achievement – to have the nerve to do it.

AFH: Well, no I think the Master, well within limits, had a free hand in the Master's Lodge in matters of that kind, yes, he has quite a free hand. We did improve the appearance of the pictures a lot by shining light on them in the right direction. And again, it was my interest in optics that assisted in that, but that cannot be described as a major achievement. In Trinity the Master has less duties and less direct influence on the affairs of the College than in most other colleges. He presides over the College Council and when all the Fellows meet to debate major things, again he presides, but he only has one vote and in most other colleges the Master would probably be chairman and certainly a member of all the important committees in the college. But in Trinity, the Master is not even a member of important committees. Committees report to the Council and that's where the Master can contribute, but as I say, the Master has a less active part to play in either the development of the College or in the appointment of the teaching staff. The Master is chairman of the body of

Fellows who elect the research Fellows each year, but well it's a very democratic system in which the Vice-Master, who is one of the Fellows elected by the other Fellows, has many of the duties that in other colleges would be carried out by the Master. But there is plenty of opportunity for a Master to initiate things, but on the whole I didn't initiate anything very notably. Of course, one important potential duty of the Master is to keep the peace if there are serious disputes among the Fellows. There were none while I was Master, and I was very grateful for that fact and I suppose it may be that the personality of the Master may have an influence in avoiding serious dissensions, but I am in no position to judge whether I had a hand in that, but certainly the Fellows were very friendly to me and it was a very agreeable time and I think, as I say, I didn't achieve any notable positive change in the College, but I think no notable negative change happened either.

AA: Probably more important. You eventually became President of the Royal Society as well, did you enjoy that?

AFH: Yes, yes. In fact I became President of the Royal Society before, in 1980, and then I became Master of Trinity in 1984, so I had one year of overlap, President of the Royal Society is normally a maximum of five years and it has recently been held for the full five years. That's a very interesting job. Again, I didn't achieve anything very notable. One important thing that the Society did while I was President, and I think I had a hand in this, was the institution of the university Research Fellowships. These are Research Fellowships for junior persons for whom, at the time, there was little prospect of university appointments due partly to the beginnings of the cutback in university fund, combined with the age structure in universities due to the large number of universities that had been started in the 1960s. So there were relatively few retirements happening at a time when many young people were coming on, at a stage of their career where they might expect to get university posts. So the Royal Society instituted these university Research Fellowships, tenable for five years and could be extended for another five, with the intention of tiding over young people until opportunities in universities came up. And this has been very successful; it has been much expanded since my time. The Society also started the investigation the effects of global warming and greenhouse effect, that was run by John Mason, who was treasurer all the time that I was President. But again, mostly, the Royal Society in my time carried on much as it had before.

AA: When you finished becoming President, George Porter became Lord George Porter, you remained Sir Andrew, why was that? Was it not automatic?

AFH: Oh no. Well, going back, we'll take A. Thompson, he was never made a Lord nor was Gowland Hopkins, nor was Dale. Adrian was. Now I can't remember whether Todd was made a Lord before – he might have been made a Lord before he became President of the Royal Society, I can't remember. Well Hodgkin was not, I was not, George Porter was. And since his time, Michael Atiyah certainly not during his Presidency and well Aaron Klug is his successor now. I didn't in any way expect it. I don't know whether I would have wanted to accept if offered. It would probably have been a temptation I would not have resisted, but as I said earlier, I have no ambititions in the political direction and the Lords is now very strong in science, well it's much stronger than the Commons and I think the debates on scientific matters in the Lords have been both very important and very influential. With Lord Porter, Lord Walton and, of course, the younger Adrian [Richard H. Adrian FRS, 1927–1995] was extremely good in his time, but of course he is no longer with us, but in the debates about experiments on living animals his contribution was very important and Lord Sherfield [Roger Mellor Makins, 1st Baron Sherfield FRS, 1904–1996], who else, the names don't come to mind as readily as they used to. I think I was probably older than George Porter at the time that he was President, and for that reason Porter would have been more useful as a member of the House of Lords, more likely more outspoken than I am. But as I say, I don't think there is any general expectation.

AA: If you look back over all of your PhD students.

AFH: I had very few when you say all.

AA: You had three or four while you were at UC didn't you.

AFH: Yes, but for a total career really very few and very different in style from many physiologists at the present time who expect to take on one or two every year.

AA: I thought you had more than that. I thought you'd continued after.

AFH: No, I didn't have any after coming back to Cambridge. I was really in the lab very much all the time that I was Master. I mean, I have been back in the labs since, but I have not had actual research students. I now have a very helpful research assistant, but he is not a PhD candidate.

AA: The question I was going to ask you was of all your PhD students which was the one you learnt most from?

AFH: Well, interpreting PhD students to mean people I had working in my laboratory, well one person on whom I relied a great deal was Stuart Taylor from the States who was omniscient about the muscle literature. I have never been good at keeping up with the literature and I used to rely on him to a large extent, if I couldn't remember who had published what. Well, Bob Simmons came in with more of a background in physics than I had. I mean, I had part one, as I mentioned, but Bob had done a degree of physics and in fact had been doing research in x-ray crystallography with David Phillips for some years before he came to University College initially to do a master's degree in physiology and then he joined me. So his physics is more professional than mine. I've never got involved directly in x-ray crystallography. If I had done, Bob would certainly have helped me. Well, it's largely through him that I got to know about these techniques like the optical trap for manipulating actin filaments and measuring single molecule forces, but that was long after we had separated and he'd moved to King's College London, to be Head of Biophysics and I moved back to Cambridge.

AA: When did you meet Richenda?

AFH: Well, we met at a dance in more-or-less New Year 1946. Now, before the war I got to know two members of the Barlow family – great grandsons of Charles Darwin. Two of them were undergraduates at Trinity, one a few years ahead of me, Erasmus. He read medicine and on the whole psychological medicine and he became chairman of the Cambridge Instrument Company, following his grandfather Horace Darwin who had started the company and I forget whether he was doing a part two physiology or biochemistry when I was probably a first-year undergraduate, but we met in College. And then his next younger brother Andrew was my exact contemporary. We did part two physiology together in 1938/39 and through this undergraduate contact – their family used to have a dance at their parents' home near

Wendover, a large, very attractive, country house and I was invited to that certainly once, it may even have been twice before the war, and then I was invited again at the beginning of 1946, when they started having this dance again. And I was there, as I say, through this university contact with the Barlows and my wife was invited – she was a rather remote cousin, her mother was a Wedgwood and the connection is that both Darwin's mother and Darwin's wife were Wedgwood, so there is a strong connection between the Wedgwood and Darwin families. My wife's mother had known one of the Darwin families in Cambridge when younger and so she was there through those connections and that was how we met. Her parents lived in Girton village, just outside Cambridge. She was an undergraduate at Newnham at that time, reading among other things, physiology. So I was, in fact, teaching her and demonstrating to her in physiology classes and her parents invited me out to lunch. They had a regular thing of inviting out undergraduates with whom they had a connection, a tradition that my wife has followed up in an active way. We regularly have Sunday lunches for undergraduates, in which the initiative is really taken by my wife. Well, we got engaged a few months later, but put off being married until she had taken her degree. So she was at the end of her second year as an undergraduate when we became engaged. And a very happy marriage.

AA: Yes, one can see that. That's obvious – it doesn't need to be said. I can't think of anything else that I want to ask you.

AFH: Now is there anything else that I feel I ought to say. I think you have covered the ground fairly well. Yes, I had one other minor interest in physiology which is the mechanism of hearing. I originally became interested because my father was, I think, briefly at St Andrews University, before going to Oxford. That was back again teaching Greek after he had finished his degree and this was back in around 1880. But he had as a prize from St Andrews, Helmholtz's book on the mechanism of hearing, and I had sort of inherited that book and read it while I was an undergraduate, or parts of it, and got interested in the mechanism of hearing for that reason. And at one point, I can't remember exactly when [...] but probably after I had done one or two things on muscle, I was wondering whether to continue on muscle or to move into [the] mechanism of well tuning of the cochlea as my main line of work. I asked the advice of Bryan Matthews, who was then head of the department, who also had an interest in hearing, and he said no, its mechanism of pitch discrimination has been solved by the work of [Georg von] Békésey [1899–1972] working on the isolated cochlea and his conclusion [was] that tuning in the cochlea was very flat and that it was sharpened up by a brain mechanism, and this appeared to have been confirmed by De Sarkey and Hallowell Davies [1896–1992]. So I took his advice and did not go into hearing. Of course, it turned out in retrospect that his opinion, which was the general opinion at the time, was totally wrong. Extremely sharp tuning is achieved in the cochlea itself. I think this was first seen by the Japanese Katsky, worked out in great detail by Knells and Chang [?]. Well, the mechanism of this sharp tuning is still uncertain, probably involving positive feedback which was suggested very shortly after the war by Tommy Gold [Thomas Gold, 1920–2004, astronomer and geoscientist], who later became well known in cosmological theory.

AA: And also there's little bits of muscle in the ear cells, which relax and let them get longer and detune themselves under certain circumstances.

AFH: Yes, but those actual muscles in the middle ear

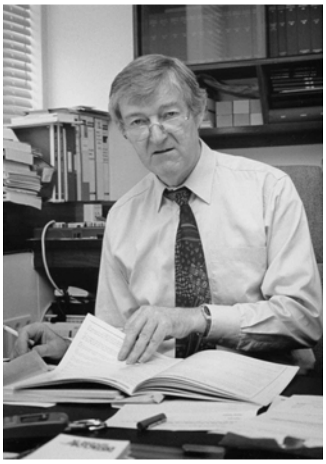
AA: No these are muscle molecules in the wall of the hair cell.

AFH: Well, there are contractile systems there which probably do, but yes the question is how much they actually give some positive feedback to sharpen up the tuning, but I don't think it would be right to call them muscles. There are certainly actin there, I don't remember that anybody has found myosin as well. I think even if they were both present, I think you couldn't really call it a muscle. Of course, the muscles in the middle ear are genuine muscles and they are also interesting, but in a quite different way. But as I say, that has been another thing I have continued to take an interest in, but had I had a short curious rhetorical note in Nature about this at one time not been generally taken up. It wouldn't surprise me if there was something in it, but I am sure it is not the whole story.

AA: There is one last question I have for you. What do you think of physiology now, compared with physiology when you were just starting.

AFH: Well, I think it would be fair to say that physiology is not as attractive to the young as it was at that time, though even at that time there was this tendency to think that biochemistry has the solutions to everything which had got going very early this century, with the rise of biochemistry and to a large extent I think the loss of the 19th century information about muscle, was largely due to the rise of biochemistry early this century. In influential papers you find statements that contractility is a molecular process. You can't see molecules with a light microscope, so there's no point in paying attention to what you can see with a light microscope and, as I say, these things that turned out in the long run to be the clue to the sliding filament mechanism were actually lost by 1950. Well, there was still this feeling among – and many of my contemporaries were in biochemistry and I think biochemists of that time felt that the secret to life was in their hands and in a sense, of course, it was, but in a kind of biochemistry that really didn't exist in those days. The customary statement in biochemistry books about the genetic material was that it was, in fact, proteins in the chromosomes that carried the genetic material. The function of the DNA was unknown and it was generally dismissed as of no importance, just as the material of the A-bands of muscle was dismissed. Its function was unknown, people had forgotten that it consisted of myosin, one of the essential proteins for contraction. But, molecular genetics now has very much the position that classical biochemistry had when I was a student and starting up in physiology and has great attractions for the young and, of course, it is providing solutions to problems that seem totally insoluble before molecular genetics developed. So it is very right it should be attractive, but it has the consequence of drawing interest away from the higher levels of organisation dealt with by physiologists that are in the long run at least as interesting, as important for medicine, and I think that this is a situation that is generally recognised by physiologists and the International Union of Physiology is doing what it can to excite interest in integral physiology and generally physiology at the level of organs and organisms as opposed to molecules. My own work has all been at the intermediate level of individual cells, how they work, but I have always maintained an interest in the function of the nerve or muscle in the whole animal, as well as an interest in the way in which the individual cell does its particular job.

AA: Fine, I have really dried – that was almost two and a quarter hours. Thank you very much.



Tony Angel photographed
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