An interview with Roger Woledge

Conducted by Nancy Curtin and Martin Rosenberg on 20 February 2006

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The Physiological Society

Roger Woledge and Nancy Curtin photographed by Martin Rosenberg.
RW says: 'The apparatus in front of us in the photo is a Levin-Wyman ergometer. It is used to make controlled, constant velocity length changes of a muscle, while measuring the force. From force and length change work (ergs) can be found … Nancy and I both cut our teeth on this apparatus. It is a nicely engineered bit of kit and those who used them often got rather fond on them. So they get kept. We still have that one.'

This interview with Roger C. Woledge (RW) was conduced by Martin Rosenberg (MR) and Nancy Curtin (NC) at Imperial College London on 20 February 2006.

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NC: OK Roger now the idea is to talk about your career as a scientist, maybe you could start by telling us what brought you into science?

RW: I went into science because I was encouraged to do so by my parents. My father was Professor of French at University College London and both he and my mother were very keen that their children should receive a scientific education because they never had a scientific education and science was very fashionable when I was at school in the 1940s. I was born in 1938, so the 40s and 50s was when I was at school. One could choose in a grammar school of those days. I went to a Grammar School at Amersham which is still there; it's called Dr Challoner's Grammar School. It's an old foundation, been there since 16 something and it's still there. I went to that school and in that school you could choose to do science or not do science when you were I suppose 14 or something. My parents were very keen that I should choose to do science and so I don't think I had any particular idea of what I wanted to do, but that was their idea, and they had a similar influence on my sister who also became a scientist. So I did that and I studied A levels in chemistry, biology, physics. Another choice you had to make was whether to study *either* biology or

mathematics in those days. I chose to do biology, I think because I liked horses and I think by then I developed ambition in life which was to become a vet and I probably was 16 or something at that stage.

NC: […] Going back to your choice... did you have any outside school activities or interest that, you know, looking back away from academics that sort of pointed towards biology, or natural history or…?

RW: Yes. My parents were very keen amateur naturalists and so they knew the name of every bird they ever saw and if not they would look it up. They knew the name of every flower they saw and they would go for country walks and they sort of prided themselves on knowing every flower and every bird and that kind of thing.

NC: And so you and your sister went along on these…?

RW: We went along on these hikes. My sister became very keen on specialising in flowering plants and in fact she became a botanist. I became interested in non-flowering plants, mosses and liverworts and things like that, which in the end I knew quite a bit about. There certainly was an amateur naturalist thing and I read books like that. […] There's a series of book published by Collins you probably know them. Martin you know them better?

MR: I only know the *Oxford Book of Wild Flowers* and all the other Oxford books.

RW: Clapham Tuting and Warberg was the book and it was very systematic, everything had to be looked up in a key so you would study under a lens. You would dissect the flower and open it up and how many of this has it got and how many of the other thing and you look it up in this book which has a key.

NC: And you actually did that?

RW: Oh yes we did this. We would take plants on our walks and go home and then in the evening we would do this.

MR: Did you have a microscope or anything?

RW: No we had a lens we just peered at it with a lens.

NC: But it was all done very systematically, so in a way it was done in a scientific way?

RW: Yes done in a scientific way. I mean there was a proper key and I think this book was the standard flora of its day so that was the authoritative way of establishing whether it was this plant or that plant.

MR: Did you learn then how to write things up?

RW: No.

NC: Did you keep notes on these things?

RW: No, no.

NC: You then had a list? Did they have a list?

RW: My father kept a list of all the plants that had ever been decapitated in the neighbourhood by us, you know a personal list, and there was another list of the birds and it was a great day if a bird could be added to the local list. These were certainly family interests which I picked up on.

I didn't pick up on all the family interests. My family were very interested in architecture and old churches and things like that which bored me stiff. I couldn't have the slightest interest in that at that stage of my life.

MR: Your grandparents, were they an influence?

RW: They weren't an influence directly on me because the only one of my grandparents that I knew at all was my mother's father. He was quite elderly when I was a little boy so I just don't remember him very well, but they had an influence indirectly because that man was a schoolmaster and took a great interest in all kinds of natural phenomena, so had a very inquiring mind and no doubt had that influence on my mother.

MR: So do you think that all comes from your mother's side?

RW: Yes I think the natural history interest comes from my mother's side, thinking about it, because my father's interests from his family were more literary and he was very interested in music. My father was a very good musician and in fact he nearly became a professional musician.

NC: What did he play?

RW: He was a pianist. He was a very talented pianist when he was a teenager and there was discussion about whether he should become a professional pianist or go to University.

MR: Did he encourage that in you at all?

RW: Yes he did and we performed chamber music at home all the time – I mean this was a regular thing we did in the evenings. And so I played recorders and the flute – and I became moderately good at playing the flute. I used to teach other people to play the flute when I was a teenager.

NC: And your sister played too?

RW: No my sister had no musical interest at all and I don't remember her even picking it up.

MR: Could you play the flute now?

RW: Oh God, I don't know, it's been a long time since I tried. I imagine that the fingers...

MR: Or the recorder?

RW: The recorder would be much more ingrained you know, but my fingers still know where all the notes are on the recorder, but I don't think they do on the flute.

MR: Do you enjoy music? I mean when you can hear it?

RW: Yes sure I like music and I have an interest in classical music, which I don't follow very avidly, but you know I listen to Classic FM when I'm in the car and enjoy this and that.

NC: And we dragged you off to – was it a prom – last year.

RW: Yes we went to a prom.

[…] So there was music in the family and that was my father.

MR: And that is a kind of analytical thing isn't it?

RW: I guess it was. My father was very interested in analysing music but I couldn't follow his thought processes.

MR: So the interest in language was also musical?

RW: Well interestingly enough my father tried to get me to take an interest in music. He didn't try but he encouraged me and he would sit at the piano and play whilst I would play the recorder, so you know we would perform pieces together, duets together and I think we must have done that several evenings a week for many years.

MR: But there was no pressure?

RW: There wasn't pressure on me to do that, no. My parents never encouraged me to learn French, which is surprising because although I learnt French at school – everyone did – my parents were both very good linguists and my father could speak many languages. My mother could speak French very fluently; both had lived in France and worked in France and it would have been very natural to encourage their children to become linguists and so on, which they did not do. I think because they wanted us to become scientists.

MR: Yes. But nevertheless you have a talent for languages?

RW: No I don't have any talent for languages, I've absolutely zero talent for languages!

MR: You said you can order a menu in Italian.

RW: Well I've got a talent for food! That's about food; I can get a meal in a large number of different languages. But I have no talent at all for speaking a foreign language.

NC: Well it sounds like from your family there were lots of options presented and some of them you took up like the music and the natural history, and then other ones like the architecture and the languages you left behind.

RW: Yes that's right.

NC: You just didn't take them up, and similarly your sister picked up some things but not others things.

RW: I think that is right yes; you know we were very free.

NC: But there were lots of choices?

RW: Yes there were lots of choices and we were very free, and one of things we were free to do was roam, and we lived in a country place, we lived in Wendover which is not far from London.

NC: And it's a village.

RW: It's a village, it's on the edge of the Chilterns and at a very early age when I was perhaps eight or nine I had a bicycle. […] After the war I did get this bicycle. It was a child's bicycle. It gave me freedom because I was able to take off on the local roads. And I would go out all day and disappear on my bicycle and come back in the evening. […] I went all over the place, usually went out on the Aylesbury Vale because it was less hilly. […] We went to live in Wendover in late 1943 I think or early '44, something like that, when the war was drawing to a close. In the time when there was a lot of bombing and so on, we were evacuated with the College to Wales, so we lived in Llanfairfechan near Bangor – the University College departments were spread out in various places, but the French department went to Bangor.

MR: Oh you get good exercise there.

RW: Yes well we did, we walked up the mountains there, so that was good.

MR: Were you influenced by Wales?

RW: Oh I had a nice time when I was in Wales; they tried to teach me Welsh, which didn't work.

NC: Maybe that's what put you off!

RW: Yes because when I was at primary school I had to recite nursery rhymes in Welsh.

NC: So you started primary school in Wales did you?

RW: Yes I started my education in the village school in Wales, yes that's right. Every day you had to do some Welsh and some English. I was very resistant to learning Welsh because I think I didn't know what was going on and probably…

NC: […] Anyway back in Wendover…

RW: Yes back in Wendover, so we were discussing why I studied various things. So there I was at school, I'm now seventeen or eighteen.

NC: And you have your A Levels?

RW: I'm going take A Levels in chemistry, physics and biology.

MR: Maths?

RW: No couldn't do maths, I had to opt out of maths – interesting.

MR: Which of those three did you favour?

RW: Oh I liked physics best of those, but that's to do with the characters of the teachers nothing to do really with the subject.

MR: Chemistry in those days was much less analytical wasn't it?

RW: I mean I liked chemistry because you learned what the ores of all the different metals were. […] We did these beautiful analytical exercises, practical work in the laboratory where you were given a mixture of two salts – it always had to be a mixture of two inorganic salts – and you had to identify by means of a series of tests which cations were present. What happened to the anions? – nobody cared about them. But it was identifying metals which is what it was all about really.

NC: So you liked the practicals?

RW: Yes I loved the practicals.

MR: Did you do any of the experiments at home?

RW: Oh yes […] We did things at home which were amazing: in my bedroom I used to smelt lead and over a bunsen burner.

NC: Where did the gas come from?

RW: There was a gas fire in my bedroom so it was easy enough to divert that onto a bunsen burner.

MR: With full parent approval?

RW: No my parents didn't seem to mind my smelting lead in the bedroom. As I said, I had a lot of freedom! So I would melt lead and I would pour into little moulds and make I don't know what, but things.

MR: Soldiers?

RW: No they were more practical things. And then I made explosives. I used to make some stuff which is made out of iodine. You make it by soaking iodine crystals in ammonia and then drying them off, and you get iodine trinitrite, which is a highly explosive substance which whilst it's wet you can scatter about the house and when it dries people walk over it and it crackles in an amazing way and gives out little puffs of purple vapour – so it's very satisfying.

MR: And your sister was quite happy you doing this?

RW: My sister didn't join in these. My sister was a horsy little girl and she was down at the stables.

NC: So is she older than you or younger than you?

RW: My sister is four years older than me.

NC: Right, so she ignored her little brother's pranks.

RW: Little pranks that's right.

MR: […] But did you have hobbies or was that horses or did you collect things?

RW: Yes horses, I did not collect things. A bit later on, I suppose when I was 12 or so, I started riding horses, and my sister and I shared the ownership of a pony, and we started riding, so that occupied a lot of my time. I think the bicycle got neglected in favour of the horse and I used to wander off on the horse into the hills, and at that time you could go up hills on a horse so much better than bicycles, so instead of going north into the vale I used to go south into the Chiltern Hills on the horse.

NC: So then when Jane went off to college then you had the horse?

RW: Yes but there was enough horse for both of us; there were other horses, horses seemed to be everywhere because people lent us their horses. So there were always horses, at least one sometimes two or three horses, which people would lend us and we would ride it. So that was very nice, that there was always something to ride.

NC: OK so you had you're A Levels.

RW: Yes I had my A Levels and so I needed to apply to University and so it was assumed that I would go to University College London.

NC: OK so there was no big decision?

RW: No decision about where to study.

MR: Was your father the head of the Department?

RW: My father was Head of the French Department, a job he took up in 1938, so he had been there for already quite a long time; 1938, that's right, the year I was born, he took it up that year. He had been there a long time as Head of Department and in fact he stayed there for more than 30 years or something as Head of Department. So it was assumed that I would go to University College and I was happy with that. The question was what subject to study; I think I had no opinion on the matter really. My father asked various people that he knew, other Heads of Department, what is a science subject that my son might like to do, 'he likes biology,' probably he said. One of the people he met was G. L. Brown, who was Head of Physiology, who was a very outgoing and friendly person. And he said to my father, well bring him up to see me and I will show him around the physiology department, which duly took place.

MR: Did you have any idea what you would do after? Did you have in mind you might be an academic or be in industry or a teacher?

RW: I had no idea whatsoever and I've forgotten, and why wasn't I being ambitious to be a vet? I don't know. I think I was drifting along in a pleasant sort of way and not caring too much. I wanted to do something that would be interesting but I don't think I looked beyond that in the slightest.

MR: So there was no sort of pressure to earn a living?

RW: Absolutely none. I have never…

NC: That's was all in the future sort of thing if it was any sort of concern presumably?

RW: It never has concerned me that I ought to earn my living and you know it never has crossed my mind that I ought to earn a living until in recent years people have pointed that out to me!

NC: So you went along to UCL, can you remember this interview?

RW: Yes very vividly because G.L. was a very nice person and I was a very shy teenager and found it very intimidating to talk to academics. But he put me at my ease and encouraged me in a very nice way. He took me into the lab, showed me experiments that were going on, no doubt involving dogs and sympathetic nervous system, and explained everything to me. I don't actually remember the experiment or whatever, but I do remember that I got some inkling of his enthusiasm for it and I got some inkling of what was being investigated. I suppose he managed to relate that to the bit of biology that I knew in some way, and so that seemed like that was a nice subject. He said if I applied I would be accepted into their course; and so I think you did have to fill a form in, and so I filled a form in and sure enough I was accepted into the course. And so that was my big moment of decision making.

MR: What year was that?

RW: 1956 it was. So I went to University College in 1956 at the age of 18.

NC: How many were in physiology that year?

RW: Six. Six people in physiology, I wish I could remember how many there were in the medical year. We studied physiology alongside the medical school. Well I mean in the first year we followed the medical physiology course and I think there were 38 people in that class but I'm not sure that I remember that correctly.

MR: Who were the key teachers that you remember?

RW: The key teachers: Otto Hutter, who gave lectures that were interesting, absolutely. Jim Pascoe was not there the year that I entered because he was on some sort of sabbatical I think – he went to Scandinavia for a year, he was with Granit. So Otto Hutter is the one I remember. G. L. Brown gave quite a few of the lectures, and there was another guy who I can remember the face of…

MR: Michael Daly was he there?

RW: He was there but we didn't encounter him, was he there? Well Hugh Davson was there; I occasionally saw him but not much as a teacher. Grace Eggleton was there; I remember her teaching vividly for not having heard a single word of it.

MR: Why because of the volume?

RW: Yes, so I was deaf at that time, I still am deaf, but at that time I had no hearing aid and so I didn't hear the lectures very well anyway.

MR: Did the deafness come at that period then?

RW: No I became mildly deaf at the age of 10 or so. I was not all that deaf at that time but I did have some hearing impediment and this contributed to the fact that certain lectures were totally inaudible to me, and in particular Grace Eggleton's lectures. I think they were inaudible to most people but certainly...

MR: You certainly pushed the benches forward when she gave a lecture.

RW: Yes that's right; but I do remember, I think, I went to her lectures, which is remarkable because I didn't go to everyone's lectures, but I went to her lectures, so I must have got something out of them. I remember her presence; she had a remarkable presence; you knew she was in the room even though you may not have been able to hear a single word she said, so that was quite interesting.

MR: Olof Lippold, was he there?

RW: Yes he was and he certainly taught us, but I don't remember much about that. Who else? Leonard Bayliss was there; he taught us in the third year – I don't think he taught us in the first year…

MR: George Dawson?

RW George Dawson was there.

MR: Huxley hadn't arrived at that point?

RW: Huxley was not there for a long time after that.

NC: Was A.V. [Hill] there at that time?

RW: A.V. was there and he was a figure in the background, but he didn't teach. He was retired already, so he didn't teach undergraduates.

MR: Which of the technicians were there? Charlie Evans?

RW: Charlie Evans was there, Rene [Smith] and Beryl [Moon], who used to run the classroom.

NC: They were there from when you started?

RW: Well certainly Rene was, I wouldn't swear about Beryl; she might have been, but Rene I remember, and Charlie Evans I remember. Terrorised the classroom. Of course we had a lot of practical work; this sticks in your mind actually more than the lectures because we followed the first year medical course and the first year medical course had a weekly practical in physiology and so that went on for two and half term semesters and probably 25 practicals through the year.

MR: You must have done anatomy with J. Z. Young?

RW: No because we didn't do anatomy.

MR: Oh you didn't?

RW: No we didn't do anatomy, so why not? Because we were doing physics, we did physics and chemistry and German and French, which we had to study.

NC: Physiology.

RW: And physiology.

NC: Did you do any biochemistry or chemistry at all?

RW: Chemistry we did; biochemistry? I can't remember doing biochemistry.

NC: I don't know; there was a great sort of schism between these subjects.

RW: That's true; maybe we didn't do any biochemistry, I don't know.

MR: These were called ancillary subjects were they?

RW: Yes the chemistry and physics were ancillary subjects and you had to pass them; two years of chemistry one year of physics. […] So physiology was the main subject, then chemistry was the next one down, you had to do for two years, and then physics you only did for one year. We did practicals every week in all three subjects...

NC: So you have lectures in the morning and practicals in the afternoon?

MR: Charles Vernon would have been there then wouldn't he?

RW: Yes he was, but he wasn't the person who taught us because ancillary teaching was sort of in the hands of lesser beings. […] We did an afternoon and a full day of chemistry practical and a physics practical and physiology practical every week so there was loads of work in the laboratory, and a lot of that work was very quantitative – in fact most of it was.

NC: So the physiology practicals you did, were they the same as the medical students did?

RW: Yes they were.

NC: And were they in that big classroom?

RW: Yes in the big classroom.

MR: How did you get on with the medical students?

RW: Oh just fine, and you know made friends with some of the medical students and there wasn't any sort of schism between them; they were just classmates and I suppose our group of six was not large enough to have a sort of that separate identity really.

So of those six people I think all become professional scientists actually.

NC: Who were they?

RW: Peter Witherington.

MR: Brian Davies?

RW: No Brian Davies was a year before us; it will come back to me in a minute. Direct question didn't work but if I approach it peripherally I shall remember some of their names.

NC: Was Denis [Noble] one?

RW: Denis was a post-graduate student at that time. So Denis was either one or two years older than me; he was a post-grad with Otto Hutter in the lab when I first went there.

Let's say some more about the practical classes because I think it's very interesting what went on in the practical classes. Firstly that it was very quantitative and in the chemistry practical you had to measure amounts of stuff. You had to do titrations and the whole point of this was to get the right answer; so you were given by the demonstrator a solution and you had to determine the quantitative amount of something or other that it contained. You had to look up a method to do that – there must have been some established book or manual that you had to look it up in – and you weren't all doing the same thing at once. You went to the demonstrator and he said, 'well you do this one,' and he would give you this bottle of stuff and then you had to determine something or other about it and you had to go and look up the method to do it and then set up the method to do it and then you went back and if you got the wrong answer he would make you go and do it again, and so if you got the right answer then you would be given something else to do.

NC: So all the bits and pieces the glassware, the burettes and everything, you went and got those out of some cupboard?

RW: Oh you had your own cupboard.

NC: With a kit in it?

RW: Yes with a kit in it and you had to pay some money for your kit at the beginning of the year and then if you broke it that was your kit.

NC: That came out of your deposit or whatever?

RW: Yes that's right and there were burettes and pipettes.

NC: What about the chemicals and stuff?

RW: The chemicals, they must have been free I think.

NC: […] So did you go over to the chemistry department to do that?

RW: Well it was then housed in a temporary building which isn't there any more; it was a sort of hut, a chemistry hut it was known as.

NC: And you had to draw graphs?

RW: Yes, that's right, you had to keep a notebook. So you had to write down in your notebook the results of your titrations. The training was every time you weighed something or measured something you didn't write it on a piece of paper, you had to write it in your notebook. Weighing was wonderful: we had balances where you had to count the swings and they were completely un-damped. The way you determined the position in which you would of settled after five days of oscillation is by counting the number of divisions that it went swinging past that way and swinging past that way and so you know it took a long time to weigh something.

MR: It was slide rules in those days wasn't it?

RW: Yes. So any calculation was done on a slide rule or you could do it with logarithms if you preferred, and you know possibly you felt that the slide rule was a bit of a shortcut. I did certainly use the slide rule as a standard bit of kit and so that was fine. The chemistry was very educative; in physics we did things like determining acceleration due to gravity, and that taught us how to fudge the answers because unlike in the chemistry where you didn't know the strength of the unknown solution, in physics you knew very well what the acceleration due to gravity was and you made sure that the equipment gave the right answer by fudging the numbers.

MR: Did you enjoy the experimental approach to achieving a formula?

RW: I liked having something to do which was fairly well defined but not too rigidly defined and so there is some room for initiative like fudging the results in the physics lab or looking up what method to use in the chemistry lab, and you could use your intelligence a little bit to use this method rather than that method. There was enjoyment in that, certainly, and the actual process of doing things was very satisfying, that is, you know, you make up this solution … and titrations are lovely because of the colours involved.

MR: So experimentation you loved?

RW: Yes absolutely I loved it. […] How nice it was to do a titration and how nice it was to do three titrations and get the right answer because they all came the same. You know, it's very satisfying to put that one drop of acid into the thing and then the colour changes, just changes, and you know you got it just right and you haven't put in too much. It's a very satisfying skill to be able to do that.

NC: I can remember having these flasks and you put a piece of white paper behind it so…

RW: We had special white tiles for that purpose.

NC: So you could see the first sign of pink.

RW: Yes the first sign of pink and you put the pink in and swivel it around and is the pink going to go? Is it going to stay?

MR: And it's how you hold the neck of the flask.

RW: Yes that's right holding the neck of the flask.

MR: Do they do that sort of thing now?

RW: No of course not, can you imagine!

MR: Oh right its all done with computer modelling?

RW: Well look it would be a health and safety issue for one thing.

MR: You might break the glass.

RW: You might break the glass you know you are using acids!

NC: Did you ever remember there being any accidents?

RW: Oh there were lots of accidents, yes. I remember ... we also did organic chemistry practicals where you had to prepare materials which used organic solvents which had to be evaporated off. These frequently caught fire while being evaporated off and so we had lots of little fires in the lab and we became adept at extinguishing by using metal foil, which is good way of extinguishing them.

MR: Did you have bench centrifuges with no protection?

RW: Yes, we did that right that in the physiology lab. So let's say a bit about the physiology practicals. Unfortunately I can't remember terribly much about the physiology practicals.

MR: Well can you remember the first physiology practical you did?

RW: No unfortunately I can't that's a shame isn't it.

MR: Did it involve a frog or a mammal?

RW: Well we certainly in the first year we would not have worked on the mammalian preparations except the demonstrations, but we did work on almost certainly the frog gastrocnemius preparation, sciatic gastrocnemius, or possibly a whole gastrocnemius; unfortunately I can't remember that.

MR: Was Bernard Katz there?

RW: Bernard Katz was in University College at that time, but again I didn't encounter him until the third year.

NC: Because he was off in Biophysics.

RW: Biophysics Department which…

MR: No other biophysics people were there like…?

RW: No, like Paul Fatt, and Gertrude Falk were there in a separate department and Miledi was not there at that time I think.

MR: Niedergerke?

RW: Yes Rolf Niedergerke was there and he taught us in the third year, so all of these people we encountered there in the first year of practicals. What I remembered about the first year of practicals was converting the… let me see this is strength–duration curve… and the point of this experiment is to determine the stimulus strengths required to elicit an action potential in the nerve as a function of the duration of the current. In order to vary the duration you had a piece of apparatus which consisted of two switches which could be mounted a variable distance apart and a strong spring, which was a metal leaf spring which could be released on a catch, and when you released it, it rapidly crossed these two switches knocking the first one and then the other and generating a duration, a known duration of stimulation, and you determine whether that stimulating or not by having…

NC: And this was a nerve–muscle?

RW: It would be a nerve–muscle preparation, and so the muscle would either twitch or not twitch and you determine that by looking at a smoked drum on which the…

NC: Kymograph?

RW: Kymograph

MR: So the smoking the drum is also a skill you know?

RW: Yes I learned how to smoke a drum, that's right, and I remember setting fire to my hands with benzene. It was very painful. Anyway, getting back to the strength–duration, having done the experiment, I then decided that this spring could be modified to launch projectiles across the laboratory. So we have some hypodermic needles which we put some plasticine at the back end of them, and put them on top of the switches so that they were caused to move rapidly across the lab when the trigger was pulled.

MR: So you are not a passive member of the group then?

NC: Where was your demonstrator at the time? Or were they cheering you on?

RW: I think they probably turned their back. Anyway, it wasn't terribly successful at making these projectiles go, I have to say, as they didn't go point forwards as they didn't have any flight and, you know, didn't have any feathers on them. But after a few trials in which we tried to modify this behaviour, unfortunately the spring broke; so we managed to get the weight of the projectiles such as to cause some nasty thing to happen to the spring.

MR: I think Len Shaw was another technician then?

RW: Len Shaw was a technician, yes, at that time, that's right; they had a separate laboratory that Len Shaw was in charge of.

NC: But it wasn't the end of your scientific career breaking the spring; somehow that resolved itself.

RW: Yes, can't remember what happened then; I probably ran away!

MR: Well the proper punishment would be to send you away until you got it to work properly.

RW: Yes that would have been good. But anyway I survived all of these things and I had a lot of enjoyment in the sort of the day to day activities but didn't learn any science that I can particularly recall in the first two years.

NC: But you must have learnt how to do a lot of experiments?

RW: I learnt how to do things and I had a very good memory, so I went to the lectures and if I didn't understand the lectures or hear the lecturer I would look it up in the text book; I was reasonably assiduous at. For example we had a course in thermodynamics and it was all done by the lecturer writing certain equations on the board and I copied them down from the board. I didn't understand what they were about, I still don't understand what they are about, but I learned them off and I was able to reproduce them faithfully in the exam; and I got 90% in the exam and that was entirely memory work with absolutely zero understanding of the subject.

NC: At that time?

WR: At that time, yes. I mean the subject I found that quite interesting because I knew the equations must mean something, but nobody discussed what it meant. It was a formal bit of teaching and the chemistry was a bit like that too: you know, this is the structure of this compound, it can be prepared in this way, and not much effort was made to systemise that.

MR: Although you didn't understand, like the symbolism of mathematical equations, did you understand?

WR: No I understood that. I understood the symbolism of the mathematical equations. I could see how by algebra you could devise how to do this one from the earlier ones, but what was this…

NC: What difference does it make why there was free energy or entropy or…

WR: Or enthalpy or what were those concepts? And I mean this is a traditional way of teaching thermodynamics actually, but it is also fairly typical of the style of teaching, which was not uncommon then, and physics was taught in a similar way. I was not critical of that; I accepted that because a lot of my education had been in terms of you learn this. The physiology had much more logic to it. You know when they are talking about the way circulation is controlled, it meant something to me; I could understand the concepts of pressure and flow, and the equations that were used were explained in a way which meant something.

NC: But physiology was say building on the fact that you understood pressure and flow, which presumably you learned about in physics in this rather abstract way, do you think?

RW: Well I think the two of them got together because I think if I hadn't been doing the physiology I would have had much less idea about what pressure and flow

were than if I had done the physics on its own; then it would have been the same as thermodynamics – there would have been nothing to relate it to.

So let's talk about third year physiology.

NC: So third year. Year one, year two you, passed all of your exams; year three…

RW: Yes year three. I actually got interested in science when I was in the year three physiology; it was the first [time] I actually personally got interested in it other than passing the time.

NC: So you remember this as a turning point?

RW: Yes it was a complete revelation to me.

NC: What happened, what triggered this?

RW: I don't know, either some neurons joined in my brain or someone said something to me that interested me or some combination of those things, but over a period of a few months my attitude to study completely changed. Of course[…] we were studying in a different way, we did not read text books, and we read the literature of the science, scientific literature and…

NC: And this was not just the six of you doing physiology working together?

RW: Yes, but there were some other people because there were […] medical students doing intercalated BScs, so there must have been a dozen in a class altogether.

NC: But fewer than before?

RW: Yes fewer than before. So we were in a smaller class. We were being taught in a different way because we were certainly being taught from the original literature. We were expected to go to the library, read the original papers and be able to talk about those in seminars and write essays about them. We had the impression that we were being taught about things which were sort of hot off the press and exciting. That excitement certainly communicated itself to me and for the first time in my life I thought, wow, you know, quite interested in this, I'd want to sit up late and read the latest whatever about it to understand it.

Now we had choices in the third year; can't remember what they were, but one of them was to do Biophysics as it was called or not to do Biophysics. I think you had to do the Circulation and Respiration, that was the compulsory course, and then you could do Biophysics or not; I don't know what the alterative would have been. Then you did one more unit which in my case I did something that was I think it was Endocrinology, but it really consisted of Leonard Bayliss and Grace Eggleton teaching us, and they taught us about antidiuretic hormone?

MR: Did they teach you as a couple?

RW: No they didn't. They taught us as a double act more, you know; either one or the other was there. They were both highly entertaining and if you got close enough to Grace you could hear what she said, a bit different from her lecture, and we did practicals on urine flow and things like that which they supervised.

NC: And was the teaching such that you were doing just biophysics for six weeks or something like that?

RW: Yes that's right; you did six weeks, and in those six weeks we did biophysics for three weeks, three weeks were muscle and three weeks were nerve. And so the three weeks on muscle we did was taught by Wilkie and who else? Brian Jewell, probably, who was Wilkie's PhD student at that time and was contributing to the teaching, and the person who was doing a lot of teaching on the nervy part of the course was Rolf Niedergerke. So those were the people who taught us.

MR: Who were the most effective teachers do you think?

NC: Or inspiring teachers or whatever.

RW: Inspiring teachers, well Doug [Wilkie] was certainly an inspiring teacher. Rolf was very inspiring too and he was very interactive; he got us involved in dialogue a great deal, and certainly Doug did that. The practicals we did were very good and we studied endplate potentials with microelectrodes, and we studied the active state curve as it was known in muscle, which is something to do with stimulating intervals, but it's a subtle experiment that you do with stimulating muscles and releasing them in various ways, and so that was very satisfying. I mean these things all satisfied my itch for quantitative information; that's what I remember about them, that's the things that was stimulating about them, was that you plotted a nice graph and drew some conclusion from the graph.

NC: Did you get from those practicals an appreciation of how you had to do it skilfully in order to get the right answers? Or maybe you already felt that, already appreciated that because these don't sound like they are particularly easy.

RW: No they weren't; these were experiments that I think even today we would think of as quite advanced experiments to set up in a practical class.

NC: And get them to work?

RW: And get them to work, and they did work by and large, so yes I probably underestimated the amount of pre-preparation that had gone into these by the people teaching so that they did work. I certainly felt that we could feel proud of ourselves for having got this going. We used to work at it a lot; I mean it wasn't a 2–5 [pm] practical class – you worked on it in the evenings.

NC: So if it didn't go well the first time you did it again?

RW: Yes you worked long hours; the demonstrators would stay there and you'd get it done and work at it until it worked. Certainly there were factors so that dark

forces of evil that would wreck it on numerous occasions, but still you know there was a commitment to getting it done and it was a sort of a team effort. Practicals were mostly done in pairs in physiology. We had a partner and the two of you and the demonstrator would work together and you would be in competition with the other little groups, so there was a lot of group ethos about doing the practical work which probably contributed to it. I still have the practical notebook from this, 1959, now; I still have the practical notebook which I look at.

MR: Did you get good marks?

RW: Yes I got a good marks and I occasionally think about throwing it away and I look at it and I clutch it to me and think, hey, that's rather nice, look at these nice graphs I drew.

NC: Do you look at it and think yes how beautiful my handwriting was?

RW: Well I do think that! I do think, you know, what a nice practical notebook I think.

MR: You could create things then with your hands?

RW: Yes that's right, it was accepted. I mean it was very… you would draw a graph and you would use a ruler and a pen and it was a piece of craft work and a very nicely sharpened pencil, that's right.

NC: So did you get interested in muscle in the third year?

RW: Yes, that was one of the things, one of things I do remember is discussing with Wilkie the thermodynamics of muscle contraction and the fact that the usual definition of efficiency, that is to say the ratio of heat, heat plus work, is not constrained between 0 and 1 and therefore should not probably be called efficiency; and that was an application of thermodynamics I learnt in year two. […] I can remember that I as a student was pointing that out in the practical or in the discussion that that value was not constrained and it ought to be, and I remember Wilkie agreeing with that and saying that, yes, that needed to be recognised.

MR: Was that a revelation to him or..?

RW: No, no, no I'm sure Wilkie was well aware that that was true.

NC: But the fact that you could dredge it up showed …

RW: Yes it showed that I was putting the pieces together, and that gave me some satisfaction because I could see that I was putting the pieces together. I remember in my third year that I got very exercised about the difference between diffusion and bulk flow, and this is to do with the permeability of capillaries. Now why is it so very important to me? … I can't remember now exactly what the argument is about, but it's certainly about experimental tests of the Starling hypothesis and the Starling hypothesis is that the balance between inflow and outflow across capillaries depends on the balance between colloid osmotic pressure and the hydrostatic pressure. And the issue

here is whether you can get bulk flow caused by osmotic pressure; and hydrostatic pressure of course will cause bulk flow through a hole that is large compared to a size of a water molecules. What kind of flow is caused by colloid osmotic pressure? It's not obvious that it will cause bulk flow with a shearing of the water molecules as they go through the hole. Actually it is now known, it wasn't known then I think, but it is now known that it does cause that. But if you think it doesn't and think about the mechanism, you can well persuade yourself it doesn't. You have a dilemma because you can't get an equilibrium across the membrane which is independent of the size of the hole because if you change the size of the hole the position of equilibrium between the colloid osmotic pressure and hydrostatic pressure would change because the type of flow two forces could cause is different. I remember writing a long essay about that and having been very satisfied that being able to debate that. Well I came to the wrong conclusion, actually, but it doesn't matter. I was very interested in thinking it through and trying to apply what physical chemistry I knew to that problem, and I could see that was an interesting problem in biology … how big holes in capillaries were. […] Makes a difference to how you look at it.

MR: So it's not sufficient that they prevent the particles passing through?

RW: Well actually, now, looking at it with hindsight, I now know that the position of equilibrium is independent of the size of the hole. You could prove that by thermodynamic principles and therefore it follows that colloide osmotic pressure does cause bulk flow from holes just as much as hydrostatic, and it does that by creating local hydrostatic pressure gradients, and that has been worked out by scientists a few years after that. But it was a dilemma that was probably debated in the literature at the time, which, you know, I got drawn into. Another thing where I was personally interested in the science that I was studying.

John Gray taught us in the third year and I remember him teaching us about receptors and receptor potentials, and he seemed very remote to us. What I remember about his teaching is he gave extremely lucid lectures in which you could understand exactly the intellectual argument from experiment to conclusion. He made that abundantly clear; he was not someone with whom I or, I don't think, my classmates could discuss things; you know we couldn't argue but he presented this beautiful understandable argument which had a lot of beauty to it. When I'm writing reviews or that kind of thing, I always think of a review written by John Gray which I read when I was an undergraduate which seemed to me a sort of perfect example of how you should write a review.

NC: And you recognised it as perfect when you read it?

RW: Yes when I read it I recognised it was perfect, and I just sort of thought about that and it has been my model – I've probably never looked at it since, but it doesn't matter, it's in my mind as a model or how you can do this. You can make it perfectly comprehensible to somebody who doesn't know it, and it's a logical deduction from this because of that, there is this, and this and this, and must work like this.

MR: Just side-tracking, where did you stay while you were at college?

RW: Oh yes, well that's interesting. I went to stay in Bentham Hall.

NC: Where is Bentham Hall?

RW: Bentham Hall is in Cartwright Gardens, and it was a hall of residence at University College London, men's halls of residence. I stayed there from 1956 until 1962; actually I stayed there for […] five years.

NC: And they were all students studying all different kind of things there?

RW: Yes students studying all different kind of things there.

MR: Did you enjoy the hostel life?

RW: No, I didn't, I was very miserable when I came to London.

MR: Did you go home at weekends?

RW: A bit, but I was…

MR: Oh your father was there anyway wasn't he?

RW: Yes I saw my father in London fairly often, used to have lunch with him once a week, I think, but I didn't like being in London at all. I felt very isolated and sort of didn't know what to do with myself when I wasn't working.

NC: But you did have friends that you made in hall?

RW: Yes I made friends in hall and I particularly remember a Nigerian medical student who is now the Leader of the Senate in Nigeria and I have seen him quite a bit since then. I remember an Indian student who was very keen… I was very interested in poetry at that time and I was introduced to the work of Rabindranath Tagore by this Indian friend of mine. A lot of my friends were foreign students, I think because they were abit less gregarious than the others and I was a bit put off by gregariousness.

MR: Were they in a similar position to you in a way?

RW: Yes I guess so, that's right, we were the odd ones out.

NC: And you were friendly to one another?

RW: Yes we were friendly to one another.

NC: So the kindred spirits recognised one another?

RW: That's right yes, and this Nigerian guy, who was the man in the room next door actually, and he and I spent a lot of time together.

NC: Did you have to cook for yourself there?

RW: No. No you were given [two] meals a day.

NC: What back at the college or...

RW: No you had your breakfast and your evening meal in the hall and then…

NC: Somebody else cooked them?

RW: Oh yes somebody else cooked them, yes, that's right.

NC: No wonder you were miserable, because it was probably awful.

MR: But you enjoy cooking don't you?

RW: I do enjoy cooking, I was taught to cook by my mother who thought if she could teach me how to cook, she wouldn't have to worry about my getting fed when she wanted to go away! Which is quite right, she didn't, so I like cooking. The cooking you could do there was limited to making toast in front of the gas fire, which of course you did.

NC: But was the food in the hall terrible?

RW: No, no the food was alright. We had grilled herrings with mustard sauce that was very nice.

MR: There are plenty of restaurants around or maybe you couldn't afford to use them?

RW: I only patronised the restaurants at breakfast time; I used to sometimes get out of bed too late for breakfast and then I would go and have ham rolls and coffee at the local place, there was a 'greasy spoon'.

MR: There was an Indian restaurant up near Drummond Street; it's changed its name again now.

RW: Yes I know that place. The Shah is what it was called.

MR: Yes it was The Shah.

NC: Oh The Shah, The Shah!

RW: Yes we used to go there quite a lot a bit later in my life but not as an undergraduate. I mean what there was when I was an undergraduate was Lyons teashops. Now this is to do with appetite, but at the end of practical classes the physiologists usually used to go to the Lyons teashop, where I used to have double Welsh rarebit, which was very nice.

NC: Where was the Lyons teashop?

RW: The Lyon's teashop was in Tottenham Court Road.

NC In Tottenham Court Road. Gee!

MR: […] There were several weren't there?

RW: There were several and this one was quite a bit further north on the west side of Tottenham Court Road, fairly far north. Anyway this was it, so we would go there, I suppose this was five o'clock, and then have our high tea and then would stagger back to halls for this seven o'clock dinner.

NC: Oh I see!

RW: Which was always a three course meal: we used to have soup, bread soup they used to make. Bread soup sounds horrid but bread soup is alright really, followed by you know a roast and two veg and then an apple pie or something like that.

NC: Yes with custard on top.

RW: With custard of course. It was good wholesome food.

NC: […] OK, now you finished your first degree, now what happened next?

RW: What happened next? I was called into his office by G. L. Brown who said, 'A. V. Hill needs a new student. You would like to do that wouldn't you?' So I said yes because I didn't know any other better thing, and A. V. Hill seemed to me a very intimidating sort of character.

NC: He made an appearance.

RW: I had seen him stalking the corridor; he was very tall, distinguished looking, grey haired, remote.

MR: Smiling?

RW: No, no looking over the top of his glasses in rather a befuddled kind of way, so not smiling; so he seemed terrible to me, but I took things as they came.

MR: Was this the end of year three?

RW: Yes it must have been towards the end of year three.

NC: So you didn't go straight into doing a PhD because A.V. wasn't into PhDs was he?

RW: No, I had a grant from the Medical Research Council, £400 a year, which was for training and research methods, a three year grant.

NC: To you?

RW: To me and I was paid this sum of money and they must have paid a fee to University College but I was not required to register for a degree, and A. V. Hill said to me...

MR: What was your first encounter with him? How was that?

RW: My first encounter with him was when I went to see him to ask him when I was to start, and he had completely forgotten that I was coming.

MR: Well he hadn't agreed to have you yet had he?

RW: Oh no he had, he had been told that I was coming, but had no idea – it had completely gone out of his head. So I had this sort of embarrassing conversation with him and I tried to explain to him who I was and why I was coming to see him. Eventually the penny dropped and he said, 'oh yes, I remember, yes.'

MR: How did he strike you? Was he with it then?

RW: Like an alien being who lived on a completely different plane was how he struck me at that time and then he said to me well… he obviously was a bit taken back and wasn't ready to kind of…

MR: Was he like a fatherly…?

RW: Well later on he got to be very fatherly towards me but at that time he was very remote. He sent me off to Plymouth for the month of September. He said go to Plymouth and sort of look at some animals. Vic Howarth had been his PhD student and recently got a job at Plymouth so he asked Vic to look after me in Plymouth. So I went to Plymouth where Vic had no idea what I should do either and put me in a cellar with a piece of equipment for measuring osmotic pressure differences between things, and so I spent a fairly useless month there I think, but I got to see Plymouth and work in the marine biology lab.

NC: And did you feel like you made friends with Vic then?

RW: Yes I certainly made friends with Vic. Vic was extremely amiable, open, easy-going character and he obviously wasn't worried about what I was doing and probably I wasn't much worried what I was doing either but that was alright.

I don't know who else was there? Eric Denton was there, another person I have never been able to hear [Sir Eric Denton, 1923–2007].

NC: Was Quentin [Bone], do you know Quentin from those times?

RW: No, he must of been there, but I don't remember him. I don't think I encountered him. But I did encounter Eric, who was very kind to me, but I couldn't hear him so that was difficult. […] Well Vic was the one who really made an impression on me. Then I came back, I suppose at the end of September, I came back to University College by which time A.V. had sort of realised that I existed and was ready to show me some experiments.

MR: So what year was this about?

RW: It's the autumn of '59, and the first month I was there A.V. did experiments in the laboratory and used me as an assistant. He sort of said well this is how we do it, I will show you all the steps of the procedure and then as you can you take over doing certain bits of it.

NC: And the experiments were?

RW: Well the experiments that were being done were heat production, in frog sartorius muscles and they involved using Levin–Wyman ergometer, so we were studying shortening. I think A.V. had decided that he wanted me to work on the thermoelastic effect, which is the heat that comes out in a muscle when you suddenly change the force in it. The Levin–Wyman ergometer was there in order to allow the force to be changed quickly without dissipating work in the muscle, so if you suddenly let the muscle go all the work stored in the elasticity becomes heat which stops you seeing any more reversible effect which also exists.

MR: Was A.V. skilful with his hands? But by that time he was old then...

RW: A.V. was quite old then […]. He was retired and he must have been in his 70s I think. Was he skilful with his hands? Yes he was reasonably skilful with his hands. I mean for a 70-year-old I think he was probably remarkably so. He did the dissections. He didn't use a microscope, and he never used a microscope – although he used a microscope to look at the thermopile sometimes – but not for dissection. He used a pair of magnifying spectacles and he would only dissect when there was a bright light coming in through the window because the dissecting light that we had, a sort of arc light, more or less cooked anything within half a mile, so that wasn't a very good tool for dissecting.

MR: Well that side of the building that was only in the morning wasn't it?

RW: Well the lab faced south so we had sunshine for quite a large part of the day actually and the building had been built to take advantage of the natural light and so the windows were extremely tall and there were great many of them so they were very well lit laboratories actually. I became adept at running the experiments and I also became adept at troubleshooting them.

NC: Because things went wrong?

RW: Things went wrong; the experimental set-up had a lot of pieces of wire in it which were twisted together in various places to make contact between this and that.

NC: And they came apart regularly I suppose?

RW: Yes they came apart and you had to figure out where they had come apart, and so the voltmeter was in daily use the AVO meter was in daily use, and you had to troubleshoot everything with AVO.

MR: AVO model 8?

RW: Yes an AVO model 8, that's right. And you had electronics; the electronics were in a open racks with 400 volt lines on them; you could put your fingers in – you would receive painful shock, but you had to put your fingers in to poke around with the AVO meter to find out why it wasn't working. So you had valves of course and valves were inherently a bit unreliable so you had to change the valves from time to time.

MR: So you learnt to tolerate electric shocks?

RW: Yes you learnt to tolerate 400 volt electric shocks.

NC: You probably also learnt to think about where you put in...

RW: Yes you learnt to put one hand in your pocket and so you learnt a lot of stuff like that.

NC: So did you like the experiments, did you sort of take to it?

RW: Yes well I took to. I mean the experiments, yes; I liked it because I could understand this sealing wax and string approach; you could see why the electricity went where it was and it went down this curly bit of wire around...

NC: And if there was a gap.

RW: If there was a gap it wouldn't go across there so it was inherently understandable and the timing was done by means of gramophone turntable and knocking down switches and so you can understand that too and...

MR: Were you more or less doing what you were told then?

RW: Yes I was doing what I was told and also I was shown the way to work out the experiments. Experimental results were accumulated by taking a photograph of an oscilloscope screen, and the oscilloscope screen had a raster display on it with a time base going many times across the screen. The oscilloscope brightness was modulated at 50 hertz so that you had 50 dots on the screen for every one second of that.

NC: So you had your timing from that?

RW: Yes you had your timing from that, and photographs were taken on pieces of photographic paper, what size is that?

NC: About a 4 inch square or something.

RW: Yes something like that. These were developed in the laboratory, which was blacked out; you take an observation, you develop it then and there and then you have this piece of white photographic paper with black dots indicating this and while it was wet you would hold it up to the light and decided what had happened and think what to do next. So there was a lot of feedback there. And then overnight the paper would be dried and the next morning the first job to do was to put that into a special rig by which you could prick holes through certain of the black dots, in the centre of the black dots. Then with a ruler you would measure the elevation of the black dots against the base line which you did to a tenth of a millimetre and then you wrote that down on a piece of paper, squared paper. And A.V. was very interested in the process of measuring and he examined the distribution of the last digit that you put down so he would point out if you always put down 3s or 7s or 4s in the last digit.

NC: This is how he checked up on you?

RW: This is how he checked up that you were actually reading to a tenth of millimetre.

MR: Was he a good statistician?

RW: Well he had an instinct for statistics.

MR: I mean a good mathematician I mean.

RW: He was a very good mathematician.

NC: Yes he was superb yes.

RW: He didn't have a lot of statistical jargon in him but he had statistical concepts absolutely.

MR: Was he creative mathematically?

RW: Well yes he was and of course is most famous for his mathematical creations and the so-called Hill coefficient, which is used in lots of fields of science to described binding curves, is one of his mathematical inventions, and so is the force–velocity curve, which is another mathematically inspired discovery which he made.

MR: Did he have many – because he was known for all his good works he did outside of physiology – was he involved in anything then?

RW: Well he was, but I saw nothing of that. His habits of work were that he would come in and do laboratory work with me and then he would go away and he would work in his office and do his own things. He kept sort of chunks of time for these different purposes and he would have a chunk of time to come in the lab; he wouldn't be in every day, but he would be there three to four days a week.

MR: Was he sought after then, did people want to visit him?

RW: I think they did but then that was kept very separate from me and so I was not part of that. After I had been working with him after about a month he went off on a visit to South America for two months I think, leaving me alone in the lab and said well I want you to sort of solve this thermoelastic thing. So I was left alone to my own devices then, which was very good for me because I then had to devise ways of making definitive experiments on thermoelasticity, which I did, and within a few months in fact I had found what he asked me to find and that was published in *The Journal of Physiology* the next year I think.

NC: So like when he came back did you then sort of show him your results? Do you remember that you sort of prepared this summary of…

RW: Yes I would prepare this… I mean we had ways of communicating and they were through numbers and graphs. He was very interested in looking at the sheets, these pencil sheets, on which numbers were written. A lot of calculation was done on those sheets because the essence of the research that we were doing was to get the most time resolution out of this we could which involved a process which is now called de-convolution. He had a method – he called it 'analysis' – he had a method of doing that which involved writing numbers on two pieces of squared paper and moving them with respect to one another and then doing the calculation on the numbers which were brought adjacent to one another and then moving it along and repeating the calculation on the next series of numbers. It was quite a laborious procedure – you had to multiply as well as add up. Well let's talk about adding up first of all; we used averaging, so typically a record, an observation, will be repeated on the same muscle fibres six times so you would have six records of the temperature change in the muscle. You wanted good detail so you would average those six, so these measurements made from the dots on the paper

they were written down on the sheet of squared paper and then averaged. In those days I could average six three-digit numbers in my head so I could very easily go along the line of numbers and average them, and I was very good at that. I can remember when I lost that ability, which was when I was some years later doing a lot of exam marking and I was adding up the numbers in my head and I found that the gears were slipping and that I was writing down the wrong thing, and I have never been able to quite get that gear back.

MR: There was an incentive there then, you were doing research.

RW: Yes but when I was at that age I could do this very easily and so I was very good at that, and then came this procedure which involved the Trinks-Triplex. The Trinks-Triplex is a calculating engine and it could multiply and divide. In order to multiply two three-digit numbers together you have to turn the handle a number of times equal to one of the digits of one of the numbers. Well first you set up one of the number on the keys on the front, and then you turn the handle a number of times equal to the first digit you're multiplying by, then you shift the carriage along one, and then you turn the handle a number of times for the second digit, and then you shift along one, and then you turn the handle a number of times for the third digit.

MR: Quicker to do it in your head isn't it?

RW: Quicker to do it in your head? Well I couldn't do three by three multiplications in my head quicker than that actually. The Trinks-Triplex would also divide and you did it by turning the handle the other way and when it wouldn't go another time it rang a bell, so you went back one and shifted the carriage along, and so it did kind of a long division by means of having a bell ring every time you had to shift position.

MR: It was a big day when that machine arrived was it?

RW: Well that machine had been there a long time and it was in fact probably war salvage. A.V. was involved in gunnery research in the first war and he did a lot of calculation at that time, numerical calculation; quite likely this machine had come from that time. He worked with Hartree. Hartree was somebody that he picked up at that post-war, post the first world war, time and who was an ace calculator and was famous for his calculations done of molecular orbitals at a time when there was no electronic calculators and it was done with this type of calculating engine. So these things were about at the time and this was certainly not new when I got to use it. But they last for several centuries these machine. I still have it; I'm sure the bell still rings the same as it always has done.

NC: So you did that project… or you did these three years on the MRC scholarship and then what happened after that?

RW: Well during this time I worked at various things that A.V. nominated to me. And I asked him if I should do a PhD and he said 'oh I shouldn't bother with that new fangled rubbish, you know if you are any good you don't need a PhD and if you are not any good then you will go do something else anyway.' By

this time A.V. was extremely fatherly towards me; during the first year or so he became progressively less remote.

MR: Did you visit his home?

RW: Yes in the second and third years I did sometimes visit his home. He had a sort of open house on Sundays at home and he used to invite me there. We would have tea in the garden and pick apples from his tree and that sort of thing.

MR: Who did you meet when you went round?

RW: His family mostly, yes his family, his son David I remember meeting particularly and his wife.

MR: Where was his home?

RW: In Highgate, it was a big Victorian house in Highgate.

NC: And were there scientific visitors in the lab?

RW: No, while I was there I was the only person working in the lab. There were no post-docs or scientific workers other than me.

NC: So all that era of visitors to A.V.'s lab that are in *Trails and Trials* were between sort of between the wars sort of thing [A. V. Hill, *Trails and Trials in Physiology*, Edward Arnold, London, 1965].

RW: Yes but it had finished by then in 1959 anyway, that's right.

NC: So after that?

RW: Well Wilkie persuaded me that I ought to do a PhD degree. And he said to me it's important to be called Dr so and so… Otherwise people won't take you seriously, so you better do a PhD. So when I had finished my three years of training the department gave me some scholarship that they had in their gift, which was a post-graduate scholarship known as the Sharpey Scholar, was it? […] Well it was in their gift and they gave me this thing which supported me for three years in essentially a post-doc position. I was given some place to work in Doug Wilkie's laboratory and Doug encouraged me to do a PhD. I chose to work on tortoise muscle, something that had been suggested to me by A. V. Hill, and so I did a PhD on tortoise during those times.

I also started teaching a lot; when I was a post-graduate student with Hill I taught medical tutorials and I used to demonstrate on the practical classes. When I had this fellowship I did quite a lot more of that, in fact. I think I was running one whole-day practical and one half-day practical a week, which I was responsible for organising and supervising, which I spent a lot of my time on because I particularly enjoyed devising experiments for that group to do. We had MSc students about that time I think and some of these MSc students were very rewarding to teach, so I got a lot out of the teaching. I think we should say something about talking about science.

NC: So after you finished this stint as Sharpey Scholar, then did you become a lecturer?

RW: Yes I then became a lecturer probably an assistant lecturer in physiology and...

MR: This would have been in the early 60s?

RW: Well it would have been from 1959 to 1962 I had this MRC grant. From '62 to '65 I had this whatever it's called fellowship and from '65 onwards I must have been lecturer in physiology. Then in due course I became a senior lecturer and in due course I became a reader and then in due course I became a professor in physiology. After that I became head of the physiology department and then after that I became Director of the Institute of Human Performance. All of these changes were completed seamless as far as I was concerned so I just kind of moved sideways and just flowed and I went with the flow and there were no sort of problems.

NC: Or crises?

RW: Or crises involved in that; as far as I was concerned I went into University College in 1956 and stayed there until I retired without the slightest problem of considering where the money was going to come from because it just happened.

MR: I think we better fill in some of that.

RW: Well we will fill in a bit of that, but that's just about the kind of formal career progression and what's interesting is where the science came from really.

MR: Did you enjoy the teaching?

RW: Yes very much. It's hard for me to remember that now because in my last years I became a little jaded.

NC: Overwhelmed?

RW: Yes overwhelmed and jaded by teaching but when I was in my early 20s and late 20s I suppose, I really enjoyed teaching. I put a great deal of effort into teaching and devising what methods for teaching and so on, and was extremely enthusiastic about it and I found it extremely rewarding. I think I learnt a great deal from the way in which I taught things. I learnt about modelling things and explaining things and by learning how to explain things you learn how to explain things to yourself so you actually end up with a much better mental apparatus for understanding things which are hard to understand, and so I think that's a huge part of the education process.

I remember thinking about the methods of teaching of counter-current flow in the kidney, for example, something which students find difficult, but you know if you sit and think about it you can figure out how it works and you can devise ways in which you can present it to them so they can get some instinctive feel for it.

MR: So did you teach the whole curriculum or a good bit of it at some time or other?

RW: Yes, I never taught much neurophysiology but I certainly taught endocrinology. When I was a PhD student I earned a bit of extra income by giving evening classes at the Regent Street Polytechnic. I used to go there one evening a week and give […] an hour's lecture and two hours' practical work which was about physiology, and in that I was required to cover within a year the whole curriculum. I had to have a certain amount of knowledge of the whole curriculum in physiology at that stage.

NC: So getting back to the muscle research, so after you did your PhD…

RW: Yes the next topic was the efficiency of tortoise muscle. I mean with great hindsight we can make some kind of logical connection between the bits. I was interested in tortoise muscle because A.V. got me interested in it.

MR: What was the particular rationale for it? Was it very robust?

RW: It's very slow and because of that it might get around the problems with the limited time resolution of your equipment.

MR: But it's not slow at its preferred temperature?

RW: Well that doesn't matter because you can make tortoise muscle contract at zero degrees when it is extremely slow. By doing that you have a preparation which is so slow the time delays in the apparatus don't matter; that's why Hill was interested in it. When I started to study it, I realised that it's efficiency was much greater than that of frog muscle. This got me interested in the concept of efficiency, and because I was interested in the concept of efficiency I got interested in where the energy was coming from. Now that was a currently hot topic.

NC: Hot topic then, yes, wasn't it?

RW: So that was my particular way of getting into it but it was hot topic because shall we say something about the controversy of the time between…

NC: Well it was biochemists and physiologist's wasn't it?

RW: Yes it was really, so to put it in prospective we need to know that back in 1938 Hill had established some things about the way in which shortening influences how much energy is produced by muscle. He summed these up in a certain equation. This equation is really related to a period of fixed time within which the velocity of shortening is … a parameter which can be varied. Now it was discovered in the 1950s really that phosphocreatine splitting was providing the energy, the process driving muscle forward, driving muscle contraction, and so it was expected that the amount of phosphocreatine splitting would follow the rules established for amount of heat produced by Hill because the underlying idea was that the heat was coming from phosphocreatine splitting. But when the biochemists did these experiments – the biochemists consist of R. E. Davies (Bob Davies), who was Nancy's PhD supervisor, and there was Spike Carlson,

John Hopkins and there was Mommaerts in Los Angeles. So these people are, if you like, the biochemists, but particularly R. E. Davies. They found that when muscles shortened rapidly they did not produce a bigger splitting of ATP or phosphocreatine compared to when they were not shortening rapidly. They studied, on the whole, whole contractions rather than periods of shortening, but it was thought because of some rather poor experiments which Hill had done in the 1940s that the same rules that he had established in 1930s for fixed periods would apply to whole contractions and in fact that's not true. Because of that, but possibly not wholly because of that, there was a controversy; the results obtained one way didn't appear to match the results obtained the other way – that's why it was a hot topic.

NC: That these were strong personalities is the other thing, so there were lots of fireworks.

RW: Yes that's right. Bob Davies's work was published in *The Proceedings of the Royal Society* […]. [*Proc R Soc Lond B Biol Sci* (1969), 174, 293–313 and 174, 315–353.]

NC: […] Yes that was Marty's PhD [Martin Kushmerick].

RW: Yes right so this went on for a long time.

NC: Oh yes because there was all this business about how the biochemistry was wrong because it wasn't frozen rapidly enough.

RW: Yes that's right and all that. Anyway it was a hot topic at the time and I got interested in it partly because of that and partly because of seeing this efficiency thing. That made me interested in finding out how much energy was released by phosphocreatine splitting. That's a chemical experiment, how much energy is released by phosphocreatine, not a biological experiment. It's chemical calorimetry so in order to undertake that kind of experiment… I'm not sure this is true, what I am saying but let's say it anyway. I set up a laboratory to do chemical calorimetry which meant buying commercial equipment. Now we didn't have commercial equipment in A.V.'s lab I should say and nor in Wilkie's lab. The majority of the equipment was home made or salvaged ex-service equipment, and if you wanted something you made it, you didn't buy it. It was a very revolutionary idea that you would get some money and go to supplier of scientific instruments, buy something to do a job and put in your lab and use it so this was a big departure.

MR: And it would be white?

RW: Yes would be white or shiny or would have...

NC: You didn't twist wires together?

RW: No it was all inside and there was a cover over the 400 volts DC … so it was completely different world. Anyway in some sequence I had forgotten I had got interested in calorimetry there, I got interesting in protein calorimetry too, so I need to explain a little about that because one of the things I do remember fairly clearly is that I wrote a review about the discoveries that had been made

in heat production and in chemical change in muscle and this was published in 1971 [*Prog Biophys Mol Biol* 22, 37–74]. […] The conclusion was that not all of the energy that is produced by muscle in a contraction comes from phosphocreatine splitting – that seemed to be the conclusion. In order for that to be clearly apparent you needed to know the heat of splitting of phosphocreatine better than it was known. Also the possibility was raised that some of the energy came from changes in the protein components in the muscle; in particular myosin might wag its tail about, which would possibly make heat or absorb heat or have some thermal changes associated with it; and in order to know something like that you ought to study myosin in isolation. So in order to do that you know this is completely beyond my experience to be able do anything of that sort so I then recruited…

NC: Well you sought advice from [Setsuro] Ebashi.

RW: Yes that's right.

NC: Who was the great expert on myosin and…

RW: Ebashi nominated someone […] he sort of told someone that you are going to London to do this calorimetry.

NC: And before you go I will teach you how to … make myosin.

MR: So he trained somebody?

RW: He trained somebody in doing myosin.

NC: Not just somebody, somebody who is a special person!

RW: Anyway the person he selected is called Takao Kodirma and he came to my laboratory in […] 1973. And the purpose of that was that he would prepare myosin in the laboratories of University College and that he and I would measure the amount of heat produced as myosin went through the cycle of splitting ATP. We had various ideas about how you could divide that into processes and this indeed is what we did. We had probably a three-year project, but it might have been extended a bit, I think, in which we measured the heat produced by myosin interacting with ADP, which is like going backwards in the cycle, and the heat produced when inorganic phosphate comes off from myosin, which is one of the forward steps in the cycle. So we were able to decompose the cycle into various steps and to measure the heat of these different steps, which turned out to be large. Now that was the point of finding this out because if the heats of these reactions had been small they couldn't have made a noticeable contribution to the total heat produced by muscle during contraction, but they were large and so they could, and so they then became a candidate for explaining what at that time was unexplained.

[…] We haven't said anything about measuring chemical change in muscle and you know when did you and I start doing that?

NC: Well the chemical change stuff really started before I came. It started when Marty Kushmerick came to do a post-doc in Wilkie's lab and they set up to measure phosphocreatine splitting.

RW: And that must have been at the time when you were being a PhD student.

NC: Yes because Marty was ahead of me […] three of four years ahead. '68 to '72 I did my PhD.

RW: And in those years Marty Kushmerick was in London and he was working with Wilkie, not with me, and he was measuring chemical change.

NC: That's right, yes. Then I came in '72.

RW: When was Merlin Kretzschmar there?

NC: Well Merlin was already there when I came and they published his papers in *The Journal of Physiology* about the energy gap and...

RW: That's right, I was an author of that paper because I had measured the…

NC: Had you done the heat measurements?

RW: I have done the heat measurements not as part of the same experiment but we did some parallel experiments which I did the heats and they did the chemistry.

NC: And they invented this smasher.

RW: They invented this smasher which is supposed to be terribly important in getting the right amount of chemical change, so it's a rapid freezing method.

NC: Right so that was all set up when I came and that was all going on, so in parallel you must have been getting your thoughts together about the calorimetry.

RW: Yes that's right. I suppose being involved in those experiments must have had a big influence in developing the thoughts about the calorimetry.

NC: Yes, I think so because as you say you have to get all the numbers right and if the ΔHs from the calorimetry aren't right, then the energy balance isn't going to work, so that all made sense, all hung together.

RW: That's right and the first experiment that we did together was it the time course experiment?

NC: No it was relaxation.

RW: Yes.

NC: And we got to do that because Merlin was on holiday so we could get our hands on the hammers, so we quickly did this thing while Merlin was not looking.

RW: Yes that's right. Anyway it was a perfectly sensible scientific question which was if you freeze the muscles as Merlin Kretzschmar had been doing just at the

end of contraction before they relaxed, does that make a big difference to what you see comparing to heat and chemical change to an experiment in which you wait a little bit longer, the muscles have relaxed and maybe they split a lot of extra ATP and maybe they produce a lot of extra heat during that period but maybe during that period they're sort of putting matters straight which had got unstraightened by the start of the contraction. So that was the point that Nancy and I were testing in that experiment. The experiment was extremely clear and the answer was no, that if you looked at the end of contraction after relaxation you saw just the same thing that you saw earlier on. What happened during relaxation was just a little bit more of the same thing that happened during contraction, just went on a bit longer, so that was nice. We then decided… […] to do this more elaborate time course […] chasing up the relations between chemical change and heat production, experiments which we are still doing.

NC: Well I think that we got to the point where we did everything we could do with the technology we had at the time. And the reason we returned to it now 20 years later is because there is some new technology so we can look at things in a different way. But in between times… we went back to Plymouth!

RW: Dogfish.

NC: And that all came about because Vic had to get a PhD because he wanted to go and do some work with Brenda [Ritchie] in the United States […] and he couldn't get a grant to do that unless [he had a PhD] because it was being paid for by some bunch in the US; so it was probably being paid for by Brenda's grant and that required he had the PhD. So he came up to UCL to have his PhD viva and we talked to him about measuring […] heat production in nerve, and we got the idea or I got the idea that if you could measure heat production from nerve you could measure from single muscle fibres and Vic said, 'yes, yes I'll make the thermopile,' and he did.

RW: And so we went down there.

NC: With a bunch of frogs and we were scorned by the locals.

RW: I think they were objects of enormous fascination.

MR: Because they weren't sea creatures?

NC: Absolutely. They said what's this?!

RW: We had this tank of frogs at Plymouth and everybody came and looked at them and said what's this?

MR: Weren't aquatic enough.

NC: No but we did those experiments. First heat measurement on single muscle fibres, great fun but what really happened was we thought Plymouth is wonderful and we have to go back here.

MR: The town or the lab?

RW: The lab… well both really. But the setting.

MR: Were there a lot of teething troubles getting that to work?

NC: No Vic loved it and you know we did too, didn't we?

RW: Yes we did. I mean Vic is an absolutely brilliant experimenter and trouble-shooter who can make things work.

NC: And fun.

MR: What was the critical thing about getting heat production in a muscle fibre?

RW: Self belief.

MR: Magnitude of the heat or…?

NC: Well the magnitude of the heat was not small compared to heat production from nerve, which Vic had measured already, so from that point of view it didn't seem to me like there was any technical problem.

RW: And no there isn't. You know the important thing is recognising that measured temperature change; if you have a smaller piece of tissue the temperature change is almost as big as it is in a big piece of tissue. The only reason it isn't quite as big is that it gets diluted by the apparatus and if you make the apparatus suitably small then that doesn't happen.

NC: Which we could do.

RW: Which we could do, and in fact the signal from a single fibre is about a quarter of the size it is from a whole muscle; so it's not a thousandth of the size, it's a quarter of the size, it's not proportional.

MR: It's a situation where you know if you wanted to do it, you will do it?

RW: Believing it could be done … is important.

MR: And that's the whole of that kind of science I think.

RW: And yes and Vic had that belief and had that confidence…

NC: Yes I can remember the first trace that it came up of the oscilloscope and Vic smiled…

MR: When did computers enter into things? Well it must have been sort of gradually.

NC: […] Yes gradually, yes, and we used to use a mainframe computer at UCL for a while.

MR: But did you see the possibilities immediately even with punch cards or were you sort of...

RW: Yes we did see the possibilities we were writing programs from the earliest time from when there were computers to write programs on. We used punch tape machines.

NC: And our needs always exceeded the capabilities of the computers.

RW: Yes that's right, and certainly we've used it all along and have we benefited or not? It's hard to tell.

NC: Oh yes I think so.

RW: Certainly the experiments you now do you couldn't do without computers, but when I look back at my pieces of paper with graphs drawn on them, it seems like I managed to get pretty good experiments done in spite of not having elaborate ways of…

MR: It was more satisfaction then; I mean its so easy now to just…

RW: Yes but the challenge is much bigger now and so what you can do expands to fill the mental capacity that you have, which is enhanced by having the computer to do the addition so…

NC: So if it can add up the numbers then the thing is you're thinking about some more elaborate interpretation, which then the computers can help you do, which you wouldn't even have been able to consider doing, you know, 40 years ago because you were too busy adding up the numbers.

RW: Yes exactly, and you know I can remember not being able to calculate things I wanted to calculate which required just basic forward integration, you know, numerical models. I remember trying to do those on squared paper and finding that it was too slow to be possible to get the answer whereas now I do that routinely as a method for modelling.

MR: You can be satisfied with a day's work back then when all you have done is arithmetic or you worked out a standard deviation.

RW: That's right. But the good thing is because we did it that way we know what a standard deviation is and you know what a convolution or a de-convolution is because of having done it that way. Another way to put that is our mental picture of those processes is linked into a certain way of performing them but at least we do have a mental picture of them which is not just a function. We had a mental picture of what's happening behind the scenes of when you are doing convolution or de-convolution so not having computers gives you a particular mental view of what's happening which you probably have a different mental view if you had been brought up on computers. It might be better or worse I don't know because I haven't got it, but certainly in must colour the way you look at everything.

NC: […] OK so lets say looking back on the perspective that you have now about science or physiology or muscle physiology or whatever, what are some of the important changes or evolutions, your perceptions of what the important changes or evolutions have been?

RW: Yes that's a hard question isn't it, because the evolutions in my own way of thinking are that I now think about it much more mechanistically than I used to.

NC: Rather than black box?

RW: Rather than black box, yes. So that I would like to be able to build a model which is a cross-bridge model which has in it such concepts of the elasticity of the cross-bridge, the elasticity of the filaments, the rate constants for the different processes involved. That is something that I certainly didn't think of years ago, not because it couldn't have been thought of years ago – because that type of way of thinking comes back to Huxley's paper in '57 so you know perfectly well you could have thought along those lines in the sixties, but we didn't get to it.

NC: But that mechanism, though, was just mathematically descriptive; I mean it had diagrams but it didn't really try to relate to say ideas about structure in muscle or biochemistry in muscle particularly; I mean it wasn't sort of latched on to that kind of data.

RW: No it wasn't and in the 50s of course that kind of data didn't really exist did it. But that type of approach which we have come to take an interest in over the last I don't know last five to 10 years, whatever, that's a big change from our perspective, I think, approaching it from expecting to be able to explain the results in those type of terms whereas we never could. Hill's view was that you should not try to do that; his view was that you should describe it in a phenomenological way and that was it. So he discouraged me from speculating about what the nature of thermoelastic effect is; it was just the thermoelastic effect and you put a number on it and that's the bottom line. You don't ask, why does muscle have a thermoelastic effect?

NC: Do you think that's a change in science or is that's just the difference between A.V.'s approach and what's satisfying to you?

RW: Well I don't know the answer to that question; I mean I know it's a change in what satisfies me. Whereas previously I was satisfied with that, I'm now no longer satisfied with that purely empirical approach. I want an analytical approach in which I feel we are looking at underlying mechanisms. You aren't looking directly at underlying mechanisms but there is a process of modelling which is important by which you have an interpretation of your results right or wrong, but it's an important part of the process, whereas previously that was seen as something you might do privately but you wouldn't put it in your paper. It wouldn't be part of what you would put in your paper.

NC: Yes well there certainly was a time where *The Journal of Physiology* wouldn't publish any sort of model at all, but they've come around quite a bit – it just has to be testable model.

RW: So that's a big change; obviously techniques have changed.

MR: Has your approach to paper writing changed? I don't mean your language abilities, I mean the kind of points you want to make?

NC: So it must have, in a sense that the end point as being a description evolved to…

RW: …to being an analysis? Oh yes, certainly, I mean I want to put these models into papers now and I want to put a lot of interpretation in the paper, and it's hard to get it in there because referees often don't like it. You have to be very careful to not muddle it up with the results, a perfectly valid point that you want to have the results seen plainly and then the interpretations separately. I find it very hard to get my favourite stories sort of accepted. People aren't on the whole challenging the results but they challenge the stories. I mean that's fine, there ought to be dialogue about that; but there can't be dialogue if you don't get it published, so you do need to get it published. So therefore you need to get past this first barrier of getting it refereed and like everyone I am often exasperated by the difficulty of doing that.

MR: You probably tried all this already, but what do you see as the future of muscle physiology? I mean that's a big question; what are the big questions? It's a sort of journalist question, isn't it?

RW: What are the big questions in muscle physiology? Well recently we have seen in the last few years a lot of the big questions being quietly answered. One of the big questions is does it move and 'it' being part of myosin molecule? So Huxley supposed, however long ago it was, almost 50 years, that part of the myosin molecule moved with respect to another part of the myosin molecule. Only in the last few years has there been really convincing evidence that that in fact happens.

MR: But that was his idea?

RW: It was an idea, it was a challenge, if you like, and people have taken up the challenge in looking for and finding that movement in recent years, and they have found it.

NC: Yes and there have been several sort of, what seem to me like A.V.'s Trials and Trails, that there have been some false trails and there have been some methods applied which didn't see the movement.

RW: Because of methodological limitations.

NC: But of course the people who were championing those methods weren't going to give up easily were they?

RW: No they were saying there is no movement, because they couldn't see it. But now we know that there is movement, and so I suppose that's one reason that I feel involved in trying to look at the consequences knowing that this movement exists and understanding the flow of energy in muscle, what controls the flow of energy in muscle. What's the challenge in our field? – our field is the flow of energy in muscle contraction and recovery – so what controls that, what are the key factors which control the rate at which energy is turned over and can we build a model of that using these ideas, old ideas but recently validated ideas of which part moves with respect to what part? We know a lot of very useful things in modelling like how stiff various parts of machine are, so it looks as if we have a consensus from a number of different experimental approaches, not ours, about what goes on if you like – what the

machinery of muscle is. Our speciality is measuring energy flow and we now have the opportunity of understanding that in these terms using the mechanisms which other people have proved and now of making a quantitative model. If you say making model is the objective that sounds as if it's nothing to do with experiments any longer. Well that's not true because you have to make the model in concert with doing the experiments, so you prove a little bit of the model by doing a certain set of experiments designed for that purpose.

We haven't said anything about the dogfish. Dogfish turn out to be a very good tool for doing that, and so what Nancy and I will be doing in the next three years is working on the corner of that using our dogfish preparation, which goes back to our links with Plymouth, to try to find out about that.

MR: You can go a long way do you think with the current methodology? I don't mean technique I mean method, or maybe technique as well, what is needed (a) to technically to improve things and (b) is there another kind of method that might be approached? This is the ignorant asking the question.

RW: We have used a fair number of different methods in our time, not a huge number, but we've used magnetic resonance spectroscopy, sometimes we use measurement of oxygen consumption. We have a number of measurements like that. Is there anything we kind of wish we had? Well yes I guess there is, I mean it would be nice if we had something that could measure phosphocreatine in un-skinned muscle cells. We work with Mike Ferenczi on fluorescence methods, which measure the release of inorganic phosphate from ATP. This is a technique which is only applicable to skinned muscle fibres, ones which don't have a membrane, and therefore a lot of our work is concerned with bridging the gap between one kind of measurement, which has to be made on the live muscle fibres – which is very important that we do measurements on the live muscle fibres because all the bits are there – and we can piece that together with some bits of information obtained from skinned muscle fibres, that if we could measure the inorganic phosphate release or the ADP release in the intact muscle fibre that would be certainly a technical breakthrough.

NC: So what one could possiblly foresee there would be NMR that had better resolution, better sensitivity is what you are saying. Because that you can do on a live muscle, but the trouble is it isn't at this stage sensitive enough to measure the kinds of changes that are occurring in a single contraction or in parts of contractions.

MR: So that doesn't exist in the world?

RW: No it doesn't exist in the world.

NC: But the NMR sensitivity is improving all the time.

MR: So that will come?

NC: Well I don't know; the thing is you are talking big money.

MR: But that's what you would like to have?

NC: Yes.

RW: Well as Nancy says you are talking big money.

NC: And so the action in NMR is towards imagining on human subjects where the medical applications where…

MR: Well we are not talking money, I mean we are talking what is possible?

RW: Yes well that's what's possible. I mean that would be nice if you had that. That would be perfect if you had this super NMR machine that put in a few fibres and get out the phosphocreatine and the inorganic phosphate concentrations in real time.

NC: ADP would be great.

RW: ADP in real time, but we are not going to live to see that I don't think.

MR: But that will come?

RW: Well I think it wouldn't.

NC: Think it won't, why not?

RW: I just think that it seems like that's too many orders of magnitude above where we are at.

MR: Is there another way of approaching that?

RW: I think this fluorescence signal is going to be the way of doing it. I think there will be florescent probes you can use inside the fibre.

NC: Inside intact fibres, like some of these calcium ones that you can get [or photophores].

NC: […] And then they are non-esterified inside or whatever.

RW: And then maybe you can have some that you can put the gene in and the animal makes it inside the cells, so that would seem a possibility wouldn't it?

NC: Yep. […] Nanobiology is the latest rage, looking at single molecules again. This is isolated even more isolated than skinned fibres, I mean that is the most fashionable thing at the moment, but that's getting away from the physiology.

RW: Yes it is. I think our interests are in what controls single cells, what can controls cells, you know, what's the mechanism? You know you can find out about that a bit by breaking the mechanism and looking at the bits, but we are not fundamentally interested in the bits, we are interested in the whole, and so we need the bits to understand the whole, so therefore we are a bit interested in the bits but I can't imagine that we would say forget the whole and we will spend the next 20 years studying a bit. That's something else, that's where the divide between the biochemistry and physiology comes, I think, and if you are a physiologist you want to put the bits together, still.

MR: Does it have to be physiology? This is an intellectual problem.

RW: Well that's right it could be anything. To interested me it has to have a physiological component to it. I can't get interested in a crossword or a Soduku puzzle because it doesn't have any physiology in it.

NC Or a single myosin molecule in a…

RW …in a cuvette. You know I'm interested in reading someone else's experiments about it but I'm not interested in the level of saying OK I'm going to spend a few years trying to get this to work. If I'm going to get interested in spending years getting something to work, there's got to be a muclse, there's got to be an intact cell in there, something where you give a stimulus and it contracts.

MR: And there has to be a hole in an animal to show where it came from.

RW: Yes, I suppose so. I wouldn't mind if it was produced by cell culture. My criteria for it being the real thing is that it has an action potential and when there is an action potential it contracts. If it does that it's a muscle cell as far as I'm concerned, and therefore it's interesting.

MR: So you see everything in relation to its physiological role. You are studying muscle because you want to understand how muscles work in people and what they do?

RW: Well I mean I am interested in how muscles working in people and that's a separate strand of my scientific life. That's not why I'm studying muscle. I could easily say that, but it's actually not true. I'm interested in a problem *per se*, I'm interested in how muscles work and I just find that an interesting question especially the control aspect because that's what is different about the whole compared to the pieces.

NC: So now you are actually retired and you've come back and you're doing this science and that's actually what you are doing?

RW: Yes on Mondays.

NC: On Mondays!

RW: But it's not what I do on Thursdays; I do something else on Thursdays and that's much more to do with whole animals and people and movement, but that's something else.

MR: Well thank you very much, but when you've read the transcript if you feel you want to have another session to expand things then there is no reason why not.

RW: Well we can go on forever as you can see.


Roger Woledge and Nancy Curtin photographed by Martin Rosenberg.

The Physiological Society
Hodgkin Huxley House
30 Farringdon Lane
London EC1R 3AW
United Kingdom

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