Australian Academy of Science - Science education
Interview with Sir Rutherford Robertson

Sir Rutherford Robertson, a plant physiologist and former president of the Australian Academy of Science, was interviewed for the Australian Academy of Science's *Video Histories of Australian Scientists* program in September 1993. The interview was conducted by Dr Max Blythe of the Medical Sciences Video-archive of the Royal College of Physicians and Oxford Brookes University in the United Kingdom. Here is an edited transcript.

You can order the videotape from us for $65.50 (including GST), or borrow it from Cinemedia.

List of edited transcripts.

Contents

Family background and early life

Perhaps you could start by telling me about your early life.

I was born in Melbourne in 1913. My parents were living there because my father was a Baptist minister and he had churches in Melbourne. My parents were strictly religious people, as you might expect in the Baptist denomination. While my father was busy converting people, my mother, who was well-educated and well-read, had a broader perspective. Certainly I owe a good deal of my early upbringing to her wisdom.

About 4 years old, with his mother

What kind of person was she?

She was very investigative and really got me interested in science, particularly when we settled down after World War One. During the war, my father was a chaplain to the Australian Forces in France and was away for two years. My mother, who came from Queensland originally, took me to Brisbane and we stayed with my grandmother. My father came back in 1919.

By that time I'd succumbed to an epidemic of polio (or infantile paralysis, as it was called in those days). When we went by train from Brisbane to Sydney, I remember limping along the platform to meet my father – a forbidding figure in full uniform – Captain Chaplain Robertson. So those are my earliest memories. We then went on to Melbourne, where he was going to a church in the suburb of Canterbury.

Talking about your family...you have an interesting Scottish and English background.

Yes. The Robertson name comes from a family in the Shetland Islands. The Shetland Islands were in a pretty bad way in the middle of the 19th century, and there was a great deal of emigration. Our branch of the family migrated then and went to settle in Victoria. My grandfather Robertson married another Scot, Miss Cairns, whose mother's maiden name was Rutherford; that's how I got the Rutherford in my name, because it was a family name given as a Christian name.

On my father's side there must have been genes for preachers, because almost every man was a preacher. In the 19th century they conducted the sort of tent-missions which travelled around Australia singing and preaching. On my mother's side my grandfather (whom I never knew, because he died before my birth) was a very successful businessman in Brisbane. That family too was well-read and well-educated, but none of them was encouraged to take tertiary education in those days; they went into business.

Just thinking of that line of preachers – tell me more about your father.

He was a great influence as far as I'm concerned. While he did believe it was necessary to save souls, he had more than just a conversion instinct. In addition to doing theological courses, he studied history and social studies. He preached what he himself would have termed a 'social gospel', which meant that Christian people had to do something for the community as a whole and not just look after their own heavenly futures. That was unusual in those days.

You mentioned being ill as a boy and winding up with a limp. That must have been hard to cope with.

It wasn't easy, though my parents encouraged me to do everything to strengthen my weak leg and to take part in sport as far as I was able. If you recover from poliomyelitis, you seldom have pain subsequently, just weakness, and I didn't find that too much of a burden. The worst part was being dragged around to various people who thought they might be able to cure it. There was always the possibility that the next person I was taken to might have a better form of massage or something of the kind.

First interest in science

What stimulated your early interest in science?

My mother. She did not have any specialised knowledge, but from reading and from occasional lectures, she became interested in what was happening in science and what scientists were up to. This was in the early 1920s in Melbourne.

And she'd talk to me about it. For instance, I remember when Ernest Rutherford visited Melbourne, she went to listen to his lecture and to learn something about what such people were up to, without knowing any of the details. She encouraged me to have a similar interest in science. When I began to see that chemistry was an interest, my parents didn't buy me a toy chemistry set. Instead, they went to a friend who was an industrial chemist and said, 'What sort of things could a boy do a few experiments with?'

So you built a bench and you got to work.

Yes, that's right. I didn't discover anything very vital, but I enjoyed it.

Education

And your early schooling in Melbourne?

It was mostly enjoyable. Because of my inability to get about very well, the first school I went to was what would have been called in Britain a Dame's school. It was a school mainly for girls up to about 14, but which took a few small boys. It was very near where we lived, and was run by three sisters, the Misses Hester. I learned not only what I was supposed to in my own class, but because there was really one big room with all ages in it, I also learned what was going on in the other classes. I really enjoyed that period.

Then I went to what was the beginning of a primary and secondary school, called Carey Grammar School, which had a Baptist foundation. I was there for three years before my family moved to New Zealand. I enjoyed my time at Carey, although I began to learn what it was to have hard knocks with people with whom I didn't see eye-to-eye.


The start of a lifelong interest in riding (about 1926)

Did Carey contribute towards the scientific career that was to follow?

Only a small amount. Science was a subject that was taught, I think, only in the last year that I was there, by which time I was about 12. As far as it went, that added to my interest, but it wasn't very exciting science. Science classes in those days tended to start with mensuration, so we spent our time looking at rulers and protractors and one thing and another. This didn't seem as interesting as chemistry, which I wanted to get involved in.


With his parents in 1925

In the mid-1920s your family went to New Zealand for your father's posting?

Yes, it was a fascinating experience. We went to the South Island of New Zealand, to Christchurch. The advantage of the South Island from my point of view and from my parents' point of view was the proximity to the Southern Alps. I had several opportunities to go to Franz Josef Glacier and to the high mountains in the vicinity of Arthur's Pass, which was one of the ways through the mountains in those days. That was a good experience.

What kind of school did you go to?

It was a Presbyterian school, a boarding school for sons of farmers, but I went as a day boy. The social life was fairly demanding and enjoyable. I had to turn up on Saturday afternoons to watch the first 15 play the rugger match, and I had to take part in sport, which I enjoyed to the best of my ability. And I think the teaching on the whole was good. Certainly the ethical outlook of the school was very good, and I enjoyed the teachers I was associated with.

Do you remember anyone in particular teaching at that school?

Well, I remember the mathematics teacher, Leadbetter, who was very good indeed. He was something of a hero to the boys, because he was the sprint champion of New Zealand at the time he was our teacher. And in those days, anybody who could run 100 yards in 10 1/5 seconds was automatically a hero.

Leadbetter went on to become headmaster of one of the big and famous New Zealand high schools. I think he went to Waikato, and he then did an unusual thing: on reaching retiring age, he took holy orders in the Presbyterian Church and became a minister. I was pleased to call on him in later life when he was a retired preacher.

And what about science teaching in the school?

The science subjects were mostly restricted to chemistry and some very elementary physics. We didn't really have a scientist as a science teacher. I remember that I had a little trouble when it was said (and I suppose I'm talking about 1927 or so) that we were now going to talk about the atom, which was the smallest indivisible particle – that there was nothing smaller than an atom. And I said, 'But, Sir, hasn't...?', and went on to suggest that someone had split the atom. The master at the time was embarrassed and said, 'Yes Robertson, come and see me afterwards'. So afterwards he said, 'You're quite right, Robertson, but that's not in the syllabus'. So that was about the level of science we were getting at that stage. But I didn't lose my enthusiasm for having more.

You came to Sydney next, and went to university there?

Yes.

It must have been quite difficult going to Sydney University from that kind of background.

Yes, it was, and in some ways it was a mistake. My father, consulting me, toyed with the idea of my going to a school in Sydney for another year or even two. I was only 16, but I had a matriculation qualification, which Sydney University accepted. A friend of the family who was a school teacher said, 'Why don't you let the boy go to university? Even if he fails and repeats, it won't matter very much, and it would be better for him to start getting university experience.' In fact, that wasn't quite so, because I was ill-prepared and had to work so hard to keep up. I didn't get much chance to grow up – I was too busy working in the first year or two. That all changed with a couple of years' maturity.

And what did you study?

Chemistry, botany, physics and geology in the first year. Then it split down to more chemistry and botany, and I did zoology as a first year subject in second year.

But by the time you got to third year, you were really beginning to find that botany was coming out on top?

Yes, that's right. The botany that I was doing then – under the leadership of Professor Osborn who subsequently went from Sydney to be Professor at Oxford – was more interesting than chemistry. It was the way it was taught; that was largely because there was a lot of memory work in chemistry, which I found less challenging than thinking out the solutions to problems that I was getting in the biological courses. Subsequently I made up for that, because my interest in chemistry didn't decline. I learnt much more in graduate years than in the undergraduate years.

And Osborn?

He was a very good lecturer, sparkling and lively. His lectures were appreciated by first year students. He was not so good for third year students, because he didn't give us as much of a feeling for the research atmosphere then as successful teachers of senior students do now.

On graduating BSc (Honours Class I), 1934.

Entering scientific research

In your fourth year, you had an option to do a research year?

Yes, that's right. I chose to do the honours year in botany, which consisted of a great deal of reading – a long reading list - over the whole range of the subject, including its history. Osborn suggested the research problem, and then left the honours students alone to work out what to do, though he gave advice from time to time.

But he set you a rotten problem?

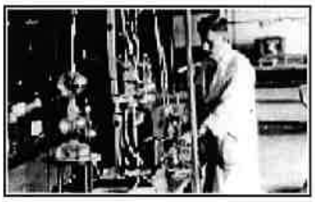
Well, it was partly my own choosing. It was to work on the way the pores in leaves, called stomata, open and shut. It was quite fascinating, and I did manage to get a few results. In fact, the fourth year students in the different subjects were invited to compete for a prize based on the thesis or paper they had written, and I got a student prize for the work that I was doing. I also got a first class honours degree, which showed that I was enjoying it and making something of it.

Looking at that work, you made all kinds of apparatus.

Yes, one of the physical chemists helped me. I learned how to make apparatus by blowing glass. I did that primarily so that I could analyse for carbon dioxide and oxygen, which we believed going in and out of the stomata had something to do with controlling their opening and closing.

You had to make a high vacuum?

Yes, in attempting to get analysis for very small quantities of gas. I didn't use it throughout the research, though, because I found I could get rather bigger volumes of gas than I had been thinking of. I then made an apparatus which was based on the Haldane apparatus, which was a gas apparatus. That worked for what we were doing at the time.



Working with high vacuum apparatus for gas analysis at the University of Sydney, about 1934.

But you didn't resolve the problem you initially set out to resolve?

No, so I talked to Petrie in Adelaide about it during the summer holiday. I should explain that this was a plant physiological problem and there were no plant physiologists in Sydney at that time – none at all. The only two of any stature were Petrie at the Waite Institute, University of Adelaide, and Wood, who was Professor of Botany, also in Adelaide. I got an opportunity to discuss the problem with Petrie, and he made some suggestions, which I worked on. But they weren't terribly successful either, at least not in my hands, with the equipment available at that time. But the important thing about the contact with Petrie was that he said, 'If you want to be a plant physiologist, you must get to Cambridge.' This was good advice.

How did you meet Petrie?

My father took a busman's holiday by going and preaching every Sunday for a month in a church in Adelaide, so we were living in Adelaide. He did that in two successive years. On the first occasion, through friends of his, I was taken round the Waite Institute and met Petrie. And Petrie, being a Sydney graduate, took an interest in me, and we had some discussions. The next year my father was going over again, and Petrie said, 'Why don't you come and work in my laboratory for the three or four weeks that you're here.' That was a great experience in itself.

How did you find Petrie?

He had a very sharp intellect and was very critical – so critical at times that he offended the people that he was giving helpful, critical advice to. But I got on very well with him.

As a graduate student, about 1935.

From there you went to Cambridge?

Yes, I got a scholarship. Some of the money that was raised in the Prince Albert Exhibition of 1851 was used to set up scholarships called the Exhibitions of 1851. These scholarships were mainly responsible for taking colonial or dominion students to England. Most of the students went to Oxford, Cambridge or London University, but not necessarily. When I was fortunate enough to get one of these, I went to Cambridge on Petrie's advice.

I was wondering whether that's where the term exhibitioner had its origins.

I think it might be. The list of people who went and became famous since is long. I think Rutherford had one of those scholarships when he first went, and certainly Mark Oliphant had one, going from Adelaide in his case.

So you went from Sydney to Downing Street and St John's College. What a botany school! When did you arrive there?

I arrived at the beginning of October 1936. I went in to see Briggs, of course, and gave him the work that I had been doing on stomates. He was not too uncomplimentary, but it was obvious that he had something that he really wanted his next research student to do. He suggested that I might put this stomate work aside and work on what was really interesting to him. That seemed to be good policy – to do what your supervisor wanted done – so I had no difficulty in doing that and was completely fascinated with the problem.

What work was that?

The best way to start is to say that all cells – plant and animal - take soluble constituents (nutrients and so forth) into them. They do it in such a way that quite often these constituents are being moved against their natural tendency, against the concentration gradient which makes them diffuse back out, but there is something that spends energy on taking these constituents into cells. Not at the time I started with Briggs, but later on, this came to be called active transport, meaning active in the sense of work having to be done to make these things move. We called it accumulation in those days, because it resulted in salts such as sodium chloride or potassium chloride accumulating in plant cells. If some work is being done in living organisms, the energy for that work comes from the process of respiration.

Briggs invited me to combine the measurement of the respiration with the measurement of the salt accumulation. We used slices of ordinary carrot roots, which were living cells. As the salt went into the tissue, the electrical conductivity of the solution that it had gone from would decrease because there was less salt in it. We had electrodes in the external solution, and we could measure the amount of salt that was going into the carrot discs.

The respiration resulted in carbon dioxide being given off, and we developed a technique for measuring the amount of carbon dioxide given off, which also depended on electrical conductivity of a solution. We bubbled air past the respiring tissue, and the carbon dioxide given off was taken into the air stream. We passed it through sodium hydroxide, in which the carbon dioxide combined to make sodium bicarbonate. The sodium bicarbonate has a lower conductivity than the sodium hydroxide, so we were able to get conductivity shift again. So it was a double conductivity shift that we were looking at.

Did it correlate the respiratory rate with salt uptake and accumulation?

Yes, it did. The system we were working with was very advantageous, because after we cut the tissue and put it in aerated water for 24 to 48 hours, the respiration rate dropped very low. When we put salt on it, it rose again and – to make a long story short - the increase in respiration was proportional to the amount of salt that was taken in. A good deal of my work went into establishing that close relationship with aerobic respiration.

And that was the core of your thesis?

Yes. I finished the thesis in December 1938, and I was examined orally by Maskell, who was a famous physiologist and my internal examiner in Cambridge; and Bennet-Clark, another physiologist who was the external examiner. They give me a viva, as it's called. Nobody told me whether I'd passed or not – nobody at all. There wasn't a hint. I was on the ship going to Australia, somewhere near Cape Town, when I got a telegram from my tutor in Cambridge saying that my PhD had been passed.

A marriage of scientists

I want to go back a bit before the doctorate to your marriage to Mary Rogerson. Could you tell me about her background.

Yes. My wife is of Scottish parentage. Her father came out to settle in Australia in 1911 I think, and his fiancée came a year later. He sold a farm in Dumfrieshire, which had been in the family for a long time. The name Rogerson is quite famous in that area. They were either farmers or doctors.

Indeed, there was one John Rogerson who was medical adviser to Catherine the Great because of the connections between Scotland and the Russian court in those days.

When Mary's father came to Australia, he bought land first near Canberra at Gundaroo, and then in 1920 at the place where we now live – Binalong – which is about 95 km from Canberra. Mary was a science student at Sydney University when we met, and we were engaged to be married before I went to Cambridge. We were then facing a separation of a possible two years, but fortunately, with some financial help from our families and finding that I could do better with my scholarship than we feared, she was able to come over at the beginning of the '37-'38 academic year. We were married in September 1937 in All Saints Church, Jesus Lane, Cambridge – which sounds great.

And your marriage has lasted all these years?

Fifty-five years and still going strong. Mary has been an enormous support. In our Sydney days she used to do part-time demonstrating, which was practicable. We had a son who had to be looked after, and a full-time job would have been difficult. She continued that interest until we went to Adelaide, when we made a choice of our own that perhaps a professor's wife being on the same staff might not be a good idea. That might have been an error, looking back. She could probably have continued demonstrating part-time there, too. But she not only helped me with experimental work from time to time, but also edited a great deal of my writing. She criticised my papers, and did all the typing before word-processors came in.

Activities at Cambridge University

I'd like to spend a bit more time talking about Cambridge in those years.

Cambridge was a very exciting place in the 1930s. The Cavendish Laboratory, with physics led by Rutherford and a collection of stars around him – Cockroft and others – was doing exciting things that were reported almost every month. It was a great privilege to be able to go and listen to the great Rutherford give an occasional lecture. Plant physiology, of course, connected up with biochemistry, and the biochemistry school under Sir Frederick Gowland Hopkins was especially strong in those days. Among the people that I knew there were Dr Joseph Needham and Dorothy Needham, and Robin Hill, Pirie (who worked on viruses), and Dick Synge (who subsequently won the Nobel Prize for work on paper chromatography in collaboration with others).

And Briggs?

Briggs was one of the most imaginative plant physiologists that I met. He placed a strong emphasis on the importance of physical chemistry and mathematical analysis to the basis of living processes. He was highly critical and appeared offensive when not meaning to be. It's said that once, just before my time in Cambridge, he was asked to comment on a lecture given by the Professor of Botany from either Edinburgh or Glasgow – I'm not sure which. The professor was a very distinguished figure in his day, but in his older years, he wasn't presenting things with the rigour that they deserved. Briggs said so in the audience. The gentleman, when asked to reply to Mr Briggs (as he was then), said that he didn't think he wanted to waste his time on some up-start of a young man who didn't know what people were doing. The story goes that the poor chap wouldn't eat his breakfast the next morning. He was staying with Sir Albert Seward, and Lady Seward had to ring Briggs and suggested that an apology would be appropriate. Briggs was very upset about having done this; he had no idea that his trenchant criticisms were in any way unfair, and he had no idea that he was upsetting anyone. When he made those sort of criticisms, they would be pretty sound.

Briggs was very supportive of your work...

Yes, he was one of the two great influences in my career.

And so, Cambridge. Cambridge was under the influence of the Spanish Civil War at that time.

Yes, it was. In 1936-37 we were concerned about the way things were developing in Spain, exemplifying, a lot of people thought, what was going to happen in Europe generally if Hitler cut loose as he was threatening to do. And, as a person interested in the social relations of science (perhaps due to my father's influence), it was inevitable that I took an interest in the sorts of groups that had that in mind.

One of these was the Association of Scientific Workers, which tried to get scientists to acknowledge their social responsibility and the contribution that science could make. I became secretary of the Cambridge branch. At that time, not surprisingly, such bodies tended to attract a number of people who were on the left side of politics, including some people who were communists - a few card-carrying communists, others secret communists – and I found that atmosphere very interesting. I was never a communist myself, because I tended to be suspicious of things that some of these people took as acts of faith.

The other body that I was associated with was the Cambridge Scientists Anti-War Group, and this was the one that related particularly to the way things were developing in Spain. We were concerned that the conservative British Government was using some technical knowledge to prepare people for the possibility of Britain being invaded – like advice on how to gas-proof rooms in houses, should there be a gas attack as part of the war. At that time we published a little book called *The protection of the public from aerial attack*.

You were a contributor?

Yes, I was one of the junior contributors. Somewhere down here in the list of the editorial committee, it says R.N. Robertson BSc. This gave me an interest in meeting a lot of people that were seriously concerned about what was happening. At this time the great J.B.S. Haldane, an acknowledged communist, was writing daily articles for the communist paper, the *Daily Worker*. He gave an unfavourable review of the book, which surprised my colleagues, because they thought that Haldane should be on our side. So I was deputed to go and see Haldane, who was in London at that time. I called on this great man, who really was a great scientist. He was just back from helping the government in Spain against Franco, and he gave me a very good hearing. He agreed in the end that in one of his criticisms he may have been right, but in the other he was wrong and he apologised. So, as far as I was concerned, that was a good experience.

I have to say that I was surprised at the devotion of some of the people on the left, including Haldane, to things that obviously were not as rosy in the USSR as these people believed. As far as I was concerned, when Hitler and Stalin made a pact, that was the last straw. I couldn't believe this stuff anymore, and was surprised that some of my friends remained with it until later years. Most of them *were* put off by the invasion of Czechoslovakia, I think.

So that was the atmosphere of Cambridge, and one of my personal experiences relating to the atmosphere at the time. When summer came in 1938, I went to Leiden for a couple of weeks for further experience in glass-blowing.

That was something that you really got into?

Yes. Knowing that I'd have to make equipment for myself when I got back to Australia, I was keen to do that. After Leiden, Mary and I went to Germany, but with some misgivings, because by this time it was early September 1938. We went to Munich, because a friend of ours was working there, and we were surrounded by Nazi uniforms. We had a very pleasant visit, except when we got into an argument with one of the German soldiers in the Hofbrauhaus. We had a friend with us who was fluent in German, and it was quite clear that what they thought they were going to do was very different from what we thought they should do.

We returned to England in that September, and under strict instructions, the first thing we did was go to the local school in Cambridge and collect our gas masks, because it was thought that war would break out within a month. As it happened, Chamberlain went and talked to Hitler – what became known as the Munich crisis – and the war was put off for a year.

Teaching science

And so you returned from Cambridge to Sydney University and the Botany School you had left...

That's right. Osborn had left and early in 1938 Eric Ashby, who subsequently became Lord Ashby, had taken over the professorship. This revolutionised the outlook and the way things were done. I went back to Sydney University as an assistant lecturer. Ashby gave me responsibility for the practical classes in plant physiology and some of the first year practical classes in botany, and this was a good experience. First of all, I found that I really enjoyed teaching. Secondly, Ashby said that the important thing about teaching is not to give students a lot of facts but to make them **think**, which suited my approach to science. Ashby encouraged us to make every practical class like a mini-research project, where the students were encouraged to investigate for themselves, draw their own conclusions, and discuss it with us when or if they got stuck. I enjoyed that...

But different to your time as a student.

Yes, it was. It was different from organic chemistry, which was all memory work, and from lots of botany, which was also memory work.

So you settled in well?

Yes, but then the war came in September 1939, after I'd been back for about eight months. Ashby was called on to do war work. Towards the end of the war he was asked by Australia, with the connivance of Great Britain, to go as a counsellor to the Australian Embassy in Moscow to further relations between the allies and Russia. By that time the USSR was fighting Hitler.

I was put immediately on the reserved occupation list, which meant that I couldn't leave. I took over some of the things that Ashby had been doing, even while he was still in Australia, because he was busy setting up a Science Liaison Bureau.

Solving food storage problems

I also got involved with CSIRO, with problems that arose out of the war in relation to two food products. One was apples. All of a sudden there were no ships to take our export apples from Australia to other parts of the world. We needed to store them for as long as possible to spread the marketing in Australia, so that there'd be minimum loss for the growers. We worked on things like oily layers on the surface of the fruit to try and restrict the respiration and hence make the fruit live longer.

The other food product was wheat. There was so much wheat that every silo in Australia was filled, and there was nothing to do except put water-proofing on the ground, make a huge stack of wheat and then build a roof over the top.

They still do it these days with excess wheat. The stack would be about 10 feet deep on the edge and 40 feet deep in the centre, and the size of the structure would be as large as 5 or 6 tennis courts.

The problem was that the wheat was getting hot. It was racing up to 40 degrees centigrade, and the question was, 'Why is the wheat getting hot?' Dry grain has a very, very low respiration rate, and we didn't think it could produce enough heat to do this. With the CSIRO people, we discovered that there were grain borers and grain weevils boring into the wheat stack, and it was their respiration that was bringing the temperature up. So, those were the two things that I was doing part-time.

You made quite a contribution to apple storage, though, didn't you?

Yes. That led me on to what happened at the end of the war, which was a change of jobs. In 1946 I moved over to head a section of CSIRO. That gave me staff who did experiments on the practical side, like how to store fruit. It also gave me some extra research assistance for continuing the work that I was doing in fundamental ion movement and respiration, which was encouraged. That was a very happy period, especially as CSIRO agreed to my continuing to run the practical classes in plant physiology at Sydney University. So I had the best of both possible worlds. I did that until 1958.

And you confirmed something about fruit storage that was important?

Yes. With the staff that I had in the fruit storage work, there were experiments going on in cold-rooms, cold-rooms with modified atmospheres and so on. I had very competent people to do this routine work. But, in the physiology of fruit, we followed up an observation made by Martin, a CSIRO colleague in Tasmania, that large fruit could be of two kinds: one kind would keep reasonably well in storage, but the other would keep badly.

We were able to show that if the fruit grew large because it had produced a lot of cells (in other words, there'd been a lot of cell division) but the average cell size wasn't very large, that fruit was a good keeper. If the fruit grew large but had produced few cells, so that the average cell size was large, that was a bad keeper. This was important information. We sent advice to growers and people storing fruit about how to divide their batches of fruit to keep the bad storers for a shorter time.

This would have had widespread economic importance.

Yes. Martin had most of this information, but he asked us to find out the explanation for it. So that was the fruit storage work. At the same time we were doing work to try and explain why or how the respiration rate of fruit changes and the time it needs for ripening – technically called the climacteric rise in respiration. We tried to determine what controlled the climacteric rise, whether it might be due to the balance of the free phosphate and bound phosphate in different constituents of the fruit. If there was a lot of free phosphate, there was plenty of opportunity for the respiration to rise. If the phosphate was bound, the respiration was going as fast as it could, because it couldn't send compounds away.

Was that a good hypothesis?

Yes, except that in the end, like a lot of science, it didn't turn out to be universally true. There were some varieties of fruit for which the explanation was different.

Was it true for apples?

Yes, it was true for apples, but it became more complicated to fix all the details.

Investigating active transport and respiration

And at this time, you were still excited about the active transport?

Yes, in the 1950s we were investigating the active transport and its relation to respiration at a more biochemical level. Respiration produces energy, but it distributes the energy through a phosphate carrier that is called ATP (its longer name doesn't matter for our purposes). We found that if respiration was proceeding and the active transport was proportional to the respiration, the respiration could be knocked out with various inhibitors and the active transport would be knocked to zero.

One of the things we found was that an inhibitor of the formation of ATP would also knock the active transport out. That was a very important observation, because it led on to later aspects of my work that were to connect up with the brilliant work of a man called Mitchell. But the way in which the respiration was affecting the ATP made us wonder whether the active transport was directly dependent on the respiration, because we could knock it out with respiration inhibitors; or whether the ATP needed to be formed first and the active transport depended on the ATP, because we could knock it out with ATP inhibitors. We did a lot of work to separate those two possibilities.

We were bothered about this through the 1950s. In 1959 I went to the University of California at Los Angeles as a Visiting Professor for the academic year, and while I was there, I attempted to follow up the literature in my own previous work, about whether the active transport was dependent on the respiration or the ATP. As early as 1940 we had shown that the respiration and the active transport were stoichiometrically related (that is, the number of oxygens being taken up was quantitatively related to the amount of salt that was being accumulated). At that stage, we decided that it was mainly a respiration feature, and we believed that the respiration was producing a separation of positive and negative charge – the negative charge being an electron going one way, and the positive charge being a proton going another way.

Causing a polarisation of the membrane?

Yes. If that was the case, you could swap a positive proton for another positive ion like a sodium or a potassium, and you could swap a negative electron for another negative charge like a Cl- or Br-.

An uptake by a kind of anionic or cationic trading?

Yes, that's right. That was the hypothesis that I developed in the 1950s.

At that time work was being done in Kreb's laboratory in Sheffield by a man called R.E. Davies on the way hydrochloric acid is secreted by the gastric mucosa (in your stomach, my stomach, a frog's stomach, everybody's stomach). Davies and I got together in the late 1940s and decided that the hydrochloric acid secretion was possibly another facet of charge separation. There the proton was going straight off and the negative charge joining it was in the form of a chloride minus. That was a pretty exciting idea.

Now coming back to later in the 1950s: While I was at UCLA, I pointed out that possibly this charge separation had another function; it might be what activated the phosphate to go on to the ATP. So the charge separation was the basis both of ion movement and the formation of this energy-carrying substance, the ATP. I published this in 1960, and as far as I am able to judge, I think this was an original suggestion: that the charge separation could be used either for ion movement or phosphorylation.

Was the polarisation controlling which things could move?

That's right.

You were getting close to the cytochromes, in a way.

Yes, indeed. The separation was taking place at the cytochromes in the respiration story. The next step was in 1961 when Mitchell, the great star of this part of the game, came up with what he called the chemiosmotic hypothesis. This hypothesis, which he arrived at in parallel with my thinking and perhaps with some influence from me, made the point that this charge separation was likely to be both the active transport system and the ATP forming system. That was a great leap forward because he recognised something that I didn't have enough nous to see: if you reversed ATP and broke it down, you could get a charge separation back from it (that is, a positive and negative charge) across a membrane. That was where Mitchell and I had a very high degree of agreement starting in 1961 when we exchanged correspondence for the first time. He was then in Edinburgh.

Another influence in my thinking, which I haven't mentioned in sequence, was the great Swedish plant physiologist, Lundegårdh. The work that he was doing through the 1950s was very similar to what I was doing, and we exchanged information and were helpful to each other. I saw him in 1948 and then again in 1956. He had invited me to go and work with him in the late 1940s, but CSIRO thought I'd had enough leave and overseas trips and should stay in Australia.

Overseas studies

Could I just ask about those overseas trips.

Yes, I had some wonderful opportunities to travel. In those days CSIRO was aware that we could easily be isolated in Australia, so they gave us the opportunity to travel for six or nine months to other parts of the world. In 1948 I went by ship to South Africa and travelled overland to see the fruit work in South Africa in particular. Then I went on to England where I saw plant physiologists and fruit storage, and got the chance to see Briggs again.

You stayed on and worked with him a bit?

Yes, I had about five months, a lot of it in Cambridge, and we produced a paper together. That was when I met Lundegårdh for the first time. Lundegårdh was not only a great scientist but an unusual man. He had enough money to build himself a private laboratory – which he did, adjoining his quite luxurious house some distance from his apartment in Uppsala. Incidentally, he was married to the daughter of Bruno Liljefors, a very famous Swedish painter. The Lundegårdh house and laboratory were hung with Liljefors' paintings, lovely things like a hare in the snow. In his scientific work, Lundegårdh was one of the greatest plant physiologists of the century, and he and I had similar ideas about the charge separation phenomenon and the quantitative relations between respiration and ion movement.

And you also travelled to the United States?

Yes, the opportunities to go to laboratories in America were splendid. My first contact on arriving in America was with David Goddard, who was a plant physiologist at Rochester, but he soon moved to the University of Pennsylvania in Philadelphia. He was very influential, and very good to me personally. He made contacts with the other plant physiologists in America, and told me whom I should go and see. So I met Kenneth Thimann, then at Harvard; Folke Skoog, who was at Wisconsin; and then across to the West Coast, which was the real stronghold of plant physiology in those days, with the Hoagland Laboratory. James Bonner was at the California Institute of

Technology, and he was doing the remarkable work that took him into the biochemistry of plants. Also there was Fritz Went, the growth substances man. That was a great broadening experience that led subsequently to some collaborative work.

Promoting scientific research

What about the CSIRO work in the late 1950s?

I mentioned that I went to UCLA in 1958-59. I came back because I was asked to join the Executive of CSIRO. CSIRO had about 4000 employees, and the Chairman, Sir Ian Clunies Ross, had died in office. The new Chairman, Sir Frederick White, asked me to join the Executive, which was a four-man body in those days.

I had misgivings about doing that, because I didn't think a full-time administrative job for the rest of my life was what I should do. First, I was still interested in research. And second, you do well at administrative jobs if you take on somebody else's problem and make it yours while you solve it. I found I could do it, but it took a great deal out of me and I didn't know whether I'd want to keep doing it for very long. So I took the administrative job in the CSIRO for up to three years, but in the event, I left after about two and half years. The Chairman didn't want me to leave, but he agreed that it would probably be better for me to take the next step, which was to become a Professor of Botany at Adelaide University. So that brings us to 1962.

You were better off in Adelaide?

Yes, it was like coming home – doing more plant physiology in a proper way, and I got the opportunity to make a couple more appointments. One of them was a man who had been working with Briggs, Professor Michael Pitman, who later became Chief Scientist in the Prime Minister's department in Canberra. The other was a former student of mine from Sydney who had been in America, Dr J.T. Wiskich. I brought him back in plant biochemistry, and he is still in the Adelaide department as a reader. So we had a strong physiology group there.

Did you still have a chance to do research at this stage?

Yes, I did, especially between 1962 and 1965. Then something happened that took away all my research time. The Australian government decided to do something it had never done before - namely, give individual grants to research workers or teams in universities. The minister in charge was Mr Gorton, who later became Prime Minister. At that time he was Minister Assisting the Prime Minister in matters relating to education and research. Some people, including the Chairman of the Universities Commission, thought that the money should be given in block grants to the universities. Others thought that there should be a system whereby the best work was supported; let it be competitive.

Mr Gorton sent for me from Adelaide to come and see him in Canberra, and I remember that when I walked into his office he was riffling some papers on his desk. He looked up and said, 'Oh, hello. Thank you for coming over. I wanted to ask you to be Chairman of a research grants committee that I'm setting up. It will be a bloody awful job and I wouldn't advise you to take it, but I'd be tremendously grateful if you would.' Who could resist such an offer?

So there you were, dealing with research grant applications.

The money was already available and there was a good deal of unhappiness in the universities because it hadn't been distributed at all. It had been delayed so long, we agreed that we should get going as fast as possible. Gorton had the idea that I should be able to do it with a committee of about a dozen people and, in fact, we started with ten. Those ten were specialists in different fields, but the system was based on confidential reports in writing from referees both in this country and elsewhere: 'This is Joe Blow. Here is what he is doing. He will be known to you through his research papers. How do you rate this man?' We brought in that system and distributed the available money. There were some disappointed people, but there were no rows. The system that I set up and operated as chairman for four years went very well and hardly changed in the next 25 years. It's changed now because the whole exercise has got rather bigger.

It was an important development that lasted for a quarter of a century.

Well, it was important to me; I felt it was something that was worth doing to help science in the community. That is probably why someone recommended me for a CMG [Companion of the Order of St Michael and St George], which I got at the end of that period. But it meant my research time was shot to pieces.

After the Adelaide professorship and your chairmanship of the research grants committee, you got an invitation to come back to Canberra to the Australian National University?

Yes, that's right, I became Master at University House. The mastership is like an Oxford or Cambridge mastership. The Master has a senior position in the university, is expected to be scholarly or scientific in his pursuits, has time to do so (or did in my time), and has some research assistance. I set up experimental work in the Research School of Biological Sciences and I had an assistant. I enjoyed being Master of University House because it was even more academic then than it is now. Indeed, we wore gowns to high table and had dinner in hall every night of the week in those days. My wife and I always stay there if we're in Canberra overnight.

It's a bit more commercial now, I suspect.

Yes, it has to be. In those days it was using some of the grants provided by the government. These days that's no longer possible.

Was your research a success in that time, because you would have been quite busy running University House.

Well, yes and no. The work was going quite well, but it wasn't University House that interfered with my time. Very soon after I came to Canberra, the President of the Academy of Science, Dr David Martyn, died in office. I seem to be fated to succeed people who had died in office.

You were captured again!

I was asked to be President of the Academy of Science. That, of course, is an honorary part-time job which becomes all-absorbing. The next four years, the four years of my tenure as President of the Academy, were at the expense of my research. I was able to do very little.

President of the Australian Academy of Science (1970-1974).

But you were busy for the Academy.

Yes, I did a lot of things. I suppose the most exciting, indeed the most straining, was when we entered a protest through the Australian government against the French atmospheric testing of nuclear weapons and suggested that there was a circulation of radioactive material. In 1973 the French asked if they could send four scientists to come and discuss this with representatives of the Academy. The discussion lasted over three days, during which time we agreed on all the technical details; we had a very happy atmosphere of discussion with our bright French colleagues.

We disagreed in the end because, while they didn't quarrel with the figures, they thought that the amount of radioactivity released into the atmosphere was negligible and likely to have negligible effects. We said that there was no way of knowing when a given dose of radioactivity was negligible, and we thought the prudent view was to stop atmospheric testing. That was a very interesting experience. It was run, I think, by the Department of External Affairs, and everything was done with strict protocol. Our French colleagues were such good chaps that it was an enjoyable experience, even though we differed in the conclusion. There were so many protests about atmospheric testing that the French stopped it soon after that. We may have made a very small contribution to that.

During your career in Canberra you moved to a new job.

Yes, I was appointed head of the Research School of Biological Sciences. I was there until retirement.

Where you facilitated a good many developments, rather than doing a lot of bench research, I believe.

Yes, that's true. I was asked by one of the great vice-chancellors, Sir John Crawford, the same one who had asked me to come to University House. He sent a message, saying the directorship of the Research School of Biological Sciences was coming vacant. That was in 1971, and he asked whether I would like to consider that rather than continue at University House. He asked Frank Fenner to come and discuss it with me. I think Frank was then the Director of the John Curtin School.

I was in hospital at the time Frank came. I was a very keen horseman, and I have the dubious distinction of having broken two legs on separate horses in one year – which was a bad thing to do, especially with the sort of break that requires some eight weeks of traction. But while I was in this state, the matter was discussed. I decided I would like to get back to a greater involvement in science and lesser involvement in administrative details which, though not enormous, were still significant in University House. So I accepted.

You didn't take long to decide I guess. Frank Fenner is terribly persuasive.

Yes, that's right. So that was how I came back to the Research School of Biological Sciences. I was still President of the Academy, which meant that I didn't start with a lot of time for research. I enjoyed the association with the research school people, an absolutely first-rate lot.

Even when I ceased to be President, yet another of these part-time jobs came along. This time it was the Prime Minister of Australia, Malcolm Fraser. I'd known him for some time, ever since CSIRO days when he'd come to visit us as a politician to see if we were doing our job properly. He wanted to set up something that the Academy had recommended: a science and technology council. The idea was to bring together ten or twelve people – bureaucrats from departments that had scientific activities, businessmen who had scientific or technological interest, and academics. Under Malcolm Fraser's persuasion I consented to be Deputy Chairman, and a former colleague from Adelaide University, Sir Geoffrey Badger, was Chairman, and that was no sinecure. I think my contributions through that (which ran into my period of retirement) might possibly have been the reason that I got recommended for the honour of Companion of the Order of Australia.

Well, you certainly put a lot into that job. We're into the last minute of the interview, Sir Rutherford, but there's a lot that you haven't talked about. I hope that we can meet again for a further interview.

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