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Australian Academy of Science - Science education
Interview with Professor Frank Fenner

Professor Frank Fenner, microbiologist and virologist, won the Japan Prize in 1988 for preventive medicine and was awarded the Royal Society's Copley Medal in 1996 for his research on human and animal pox viruses. He was interviewed for the Australian Academy of Science's *Video Histories of Australian Scientists* program in 1992 and 1993. The interviews were 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 of the interviews.

You can order the videotapes from us for $65.50 each (including GST), or borrow them from Cinemedia.

List of edited transcripts.

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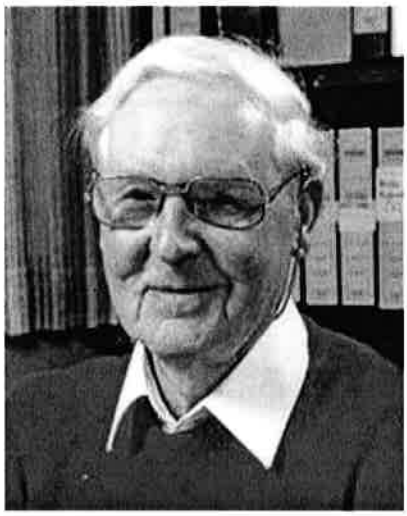


Family influences

Professor Fenner, it's a great pleasure to be talking to you today in the building of the Royal Australasian College of Physicians. Perhaps you would tell me about your early days in Victoria and later in Adelaide. It seems you

had very exciting parents.

I was born in 1914 in Ballarat, a former gold-mining town about 70 miles west of Melbourne. I was the second of four boys in my family, with one sister. Our parents were from Victoria, and when they married they were both teachers. My father then became principal of the Ballarat School of Mines, having come up the hard way, leaving school when he was 13. He had obtained a BSc degree at the University of Melbourne while at the Teachers College and later obtained a Masters degree in geology and physiography. In 1916, when he was appointed Superintendent of Technical Education in South Australia, we moved over there and I had the whole of my upbringing, to the end of a university course, in Adelaide.

Although for the whole of my father's life he was in the administration of education, he was a man of very broad interests. To eke out the income during the Depression, every two weeks he wrote an article on science for one of the national weeklies, under the pen-name of Tellurian. He carried out research in physiography – landforms – and set up a course in geography at the University of Adelaide, doing all this after hours. He was always writing papers, so the 'study' in our house was always littered with papers, but it was sacrosanct and my mother could never go in there and clean it up.

12 years old – taken at the time of the Qualifying Certificate examination.

He also took you out and about, far and wide.

Yes. Mother and Father used to drive from Adelaide to Victoria once a year, if they could, to see their relatives. These trips were quite an adventure, because on the long stretch of the Coorong there were no roads, only saltpans to drive on. My father was extremely interesting, a great expositor, in describing the geography, the geology underlying it, the trees, the birds and the white man's history of the area.

My mother was the anchor, the tower of strength and a very good foil for my

father. He was somewhat mercurial, whereas she was extremely solid and reliable – and a tremendous help as a mother. I was very fortunate to have a family like that. **First research: physical anthropology**

Your education took place in a rather interesting school.

Yes. My father was an experimenter in teaching methods but, being in charge of technical education, he could only introduce something revolutionary in a technical high school, the Thebarton Technical High School. So I went to that school, which taught according to the Dalton Plan. That became a well-known teaching system, but it was not common at the time. It was a great preparation for a university education because you had to do a lot of work on your own. For me it was great, although some people really needed a bit of a stimulus.

Because that school didn't offer Leaving Honours, the course from which you could get one of the 12 bursaries for a free education at the university, I went to the Adelaide High School to try for one. I failed, though, to my own disappointment and to my parents' disappointment because those 12 scholarships were the only assistance there was, and they then had to support me at the university. I'd been a bright boy at school, doing well in my examinations right through, but I just fizzled out at the critical time. It must have been quite a struggle for my parents to put me through university – I entered in 1933, just as the Depression was at its worst.

Frank Fenner (second from right) with his family, 1934.

Did you enjoy being at university?

Yes. I was very much involved in sport there and I had a great life.

You read medicine, but largely as a scientist wanting to do something in medical research, rather than to become a doctor.

I thoroughly enjoyed medical school, especially the contact with patients in hospital, which was fantastic compared with nowadays. We had no professors beyond the pre-clinical subjects – no professor of medicine. The Dean of Clinical Studies and Dean of Surgical Studies were both full-time medical practitioners. We had very small classes (there were only 17 graduates from

our year) so we all knew each other very well. Afterwards I practised as a locum for five days and in the Army as a physician for a little while in a casualty clearing station, but that was the extent of my medical practice.

At no time did I really think I was going to be a practitioner of medicine. I had wanted to become a geologist but this was before the mineral boom and the only jobs in geology were in universities. So my father dissuaded me, saying, 'That's a very narrow opportunity. If you do medicine, you can do what you like later on. You can become an administrator or a scientist or a physician or a surgeon – a very wide range of opportunities.' That is quite true, I think: it opens up a great range of possibilities.

Did any teachers make a mark on your thinking?

Not particularly. Mostly I got my interest in science from my father, and through him I was introduced to people in the South Australian Museum. As a university student I attended the meetings of the Royal Society of South Australia, a local scientific society, and from my second year, when I was doing anatomy, I did physical anthropological work with a joint Museum/Adelaide University group that used to go up into Central Australia for two weeks every September vacation and make various studies of Aboriginal people.

Third-year medical student, 1935.

I've read that you did terrific research, for a young man, and produced some papers.

Well, I was just lucky. I was catapulted not into assisting but actually into making various measurements – height, breadth of nose, and so on. The man who engineered the whole thing was a friend of my father's.

The more interesting findings came later, when I did some studies at the Museum under the influence of Frederick Wood Jones, who had been Professor of Anatomy in the University of Adelaide and then moved to the

University of Melbourne. He was a great figure and a very great help to me. I made a suggestion about what I'd like to do, and he wrote longhand letters in which he explained how I should do it. My xerox copies of those letters are in the College library here, but the originals I've given to Sir Sydney Sunderland, in the University of Melbourne. Wood Jones, by the way, wrote the introduction to one of the popular Australian science books by my father, *Bunyips and Billabongs*. **A wartime malariologist**

By the time you completed medicine at Adelaide, a war was around.

I was a resident on 3 September 1939 and our residency finished in February. I think only one man in our year didn't enrol in the Army or Air Force. I had the idea that the war would be fought in the tropics, so before I joined up I came over to Sydney and did a diploma of tropical medicine, using a £200 bonus that the hospital provided for residents who stayed the full 12 months. I have made very few decisions in my life, but that one really was sensible, leading ultimately to my being a malariologist in the Army, which opened up the possibility of my future career.

When malaria cropped up as an important disease in Syria, virtually none of my colleagues knew anything about it, whereas I knew what malaria parasites looked like. I got involved in lab work there, later being in charge of a laboratory at one of the Australian general hospitals in northern Australia and then shanghaied into Papua New Guinea as a malariologist.

My first Army service, however, was in the Middle East in a field ambulance. I went out on a practice operation in which the Army switched the officers from one field ambulance to work with the soldiers in another. The NCOs went off in trucks to put tents up for this field station, but I had been taught by the commander of our field ambulance that the officer always marched with his men, so I got up much later. Just as I arrived on the scene to find my men putting up a tent (with a red cross on its top) right on top of a hill, the general came along and blasted the hell out of me! My error of judgment was 'marching with the men'. That was all nonsense, of course. You should be in place and give some advice as to where they put the tent. I got transferred out of the field ambulance to Corps headquarters – they thought it might be safer to keep this man out of the field.

But that had some good repercussions, didn't it?

I was very lucky. The brigadier was W W S Johnson – later, I think, President of this College – who was a marvellous man, a great gentleman. I went up to Jerusalem with him, and through him I met Saul Adler, a very famous parasitologist who became a Fellow of the Royal Society. That again gave me more contact with malaria, which was an important disease even in the Middle East. When I came back to Australia it was a dominating disease in the Army in Papua New Guinea, and I spent the last two and a half years of the war as a

malariologist, with a free hand to go everywhere except beyond the Australian lines and back across Torres Strait.

Those were great years and very important for you, quite apart from the medical side. You had wonderful freedoms to grow as an administrator.

Yes. During that time, the behind-the-scenes influence was Dr Keogh, who went to the Middle East as a hospital pathologist. I became acquainted with him in Australia, not the Middle East, because he was moved from being a hospital pathologist to be Director of Hygiene and Pathology at Land Headquarters. He designed the Australian Army's operations against malaria, with malariologists who were different from the Deputy Assistant Director of Hygiene, who would ordinarily do the job at Division level – Ted Ford, who has made such great contributions at the library of the College of Physicians, was the senior malariologist; Jim English, a Sydney doctor, and I were the other two.

Keogh also was the original brains behind the malaria unit at Cairns, where Brigadier Hamilton Fairley acted as director. Two prominent physicians, Bickerton Blackburn and Rod Andrew, were officers-in-charge. Called the LHQ Medical Research Unit at Cairns, it was set up essentially to investigate the chemotherapy of malaria and how to control malaria in the field. The work – most of it with falciparum malaria – was done by experimentation with soldiers who volunteered to go there. My wife was involved, being a blood transfusion expert who had learnt from Julian Smith, a Melbourne surgeon, how to do direct transfusions. She used a machine given her by Smith to transfuse 200 millilitres of blood from a person who had just been infected by a mosquito, over to a non-infected person. Very early it was realised that in doing this you could transfer sporozoites by chance up to about half an hour after the mosquito bites and then you couldn't transfuse the disease until blood forms appeared a week later. Clearly, it got hidden. That really was the origin of the idea of the exoerythrocytic cycle of malaria.

Fairley once suggested that if I became a pathologist in the hospital I might have the opportunity of working in the LHQ Medical Research Unit. I'm very glad I didn't get that opportunity, because he did all the planning of the experiments and you were a slave, to some extent. I went to Papua New Guinea as a field malariologist and had a marvellous time, looking after not just malaria but scrub typhus, dengue, any arthropod-transmitted disease. It was very interesting.

The Australian Army gradually went from a stage where it was really collapsing from malaria to campaigns at the end of the war in which the malaria casualties were trivial. It was a great advance. In the first campaign I was involved in, the Lae campaign, there were a lot of malaria casualties. In the next, in Finschhafen, in northern New Guinea, there were very, very few. And after that, even fewer. I had under my administration some field malaria

control units and some entomological research units, so it really was a great opportunity. **Mousepox: working with Macfarlane Burnet**

You must feel that you played a large part in that amazing progress against malaria.

Yes. When the war was clearly coming to an end, I had virtually decided that I wanted to go into infectious diseases and that I would try for a fellowship with Macfarlane Burnet to learn something about it. But before I'd made any move I got a handwritten letter from Burnet, offering me a job at twice the salary at which I was going to apply for a fellowship. Keogh was very close to Burnet and must have recommended me from his knowledge of the way I was handling my job. Burnet wanted me specifically to follow up the fact that he himself had shown that a disease of mice called ectromelia – which subsequently, in a paper that I wrote, we called mousepox – was really smallpox of mice. It was closely related to vaccinia virus but this wasn't known when the group of Topley, Wilson and Greenwood, in Britain, wrote some classic work on the experimental epidemiology of ectromelia.

I was to use the knowledge that it really was a pox disease, but at the time it wasn't known that it produced a rash. One of the observations that my wife and I made together – she helped me in the lab as an unpaid assistant – was that there was a rash. And I got deflected to a certain extent from the epidemiological studies, the experiments on populations of mice, to investigate the pathogenesis of ectromelia and determine what happened in the incubation period. The paper was eventually published in the *Lancet* in 1948 and I still see it being used, all these years later.

That marvellous piece of research was a great development. And it took place at the Walter and Eliza Hall Institute, with Burnet around?

Yes. Burnet was a dominating scientist, in the sense that when he was working on influenza he wanted virtually everybody in the Institute – which was very small, about the size of a department in most places – to work on influenza. But he appointed me to work on this different thing and he gave me a completely free go. All the papers I wrote on that were under my own authorship, without Burnet as a tag-on. Our discussions were such that when I had a paper written I'd take it along to him, he'd go over it that night (he used to work in the lab all day), and we'd discuss it next morning, together with what to go on with next. So he gave me a very free go.

Did you form a good, lasting relationship with him?

Oh yes, we remained friends the whole of our lives. He and his wife – his first wife and then his second – used to stay with us. When he and Linda went to Britain for nine months we took charge of their children. The eldest son dropped out of university during our tutelage, and the eldest daughter got

engaged. So we had an exciting time in his family life.

How do you sum up the Macfarlane Burnet that you knew and who had such an influence on you – a great scientist, a hardworking man, a man of high perception?

Yes. He was a great 'lateral thinker' before the term had been invented, and had a tremendous memory. He worked enormously hard. I think every day he worked at the lab; every night he worked at home, writing papers. He was extremely prolific. I wrote three or four things in collaboration with him, one being a little book called *The Production of Antibodies*. He wrote the theoretical part of that, for which he got a Nobel Prize; I wrote the more mundane part. But he could write: his first draft was the last draft and he hardly needed to correct things. After his retirement he wrote 15 books, which is a lot to do – one a year. **The Rockefeller Institute: tubercle bacilli**

That Walter and Eliza Hall background lasted only two and a half years but with the mousepox work they were very influential years. Didn't you then, suddenly, have an opportunity to move to an even more exciting challenge?

Well, there was an intermediate stage. Again Keogh comes into it. He had realised that the generation of people who had been in the Army during the war – Ted Ford, Oliver Lancaster, Alan Jackson, Bickerton Blackburn, Rod Andrew, myself – had missed out on overseas training, which was regarded as essential for Australians wishing to undertake an academic career. We were very isolated, scientifically. He organised with the Carnegie and the Rockefeller people to make overseas fellowships available for us. Keogh and Burnet decided that I should go to a big centre where a lot of people moved through. They picked on René Dubos, at the Rockefeller Institute. That was a great opportunity.

New York was a great place in 1949. It was clean, you didn't get dust in your eyes, it was safe – when my wife came home from a trip to see a friend in Canada, she arrived at one side of town at 2am and just got an underground to come across to where we were staying. Even the locals would not think of doing that now. And the lab was an exciting place. Dubos was quite different from Burnet: he didn't work at the bench at all, himself. He had a small group of post-doctorals working with him. At the end of the day he would ask everybody what they were doing and erect an inverted pyramid of speculation on a point of fact. It would often collapse, as you can imagine.

He was a very inspiring man, who had another very good habit. At that time the Rockefeller Institute had a dining room to which all members of staff and all post-doctorals, went down to have lunch – at 25 cents it was heavily subsidised, and it was the main meal of our day. Dubos used to move with his post-docs from one table to the other, introducing us to all the notables. So we talked to Van Slyke, Tom Rivers, Frank Horsfall and all these great names; I

met Albert Sabin for the first time there. Dubos deliberately went round and exposed us to all these different people, so you really got to know a lot of them and were able to measure yourself against the great figures in the scientific world. Subsequently, of course, I travelled across the United States a lot and met many people there.

What research did you manage to fit into that time?

I had to work on tubercle bacilli. To give myself something original, I took over with me some strains of a bacillus which had been isolated in the Alfred Hospital in Melbourne from some patients from Bairnsdale. It turned out to be a very interesting mycobacterium which was present worldwide – slow-growing, but temperature controlled. It produced severe skin lesions, but nothing in the internal organs because it had a temperature restriction.

In addition, I developed an assay method for mycobacteria. Such a colony count had been very difficult, and my method was subsequently used quite a lot by Dubos's group. It was possible only because Dubos had developed a method of growing tubercle bacilli in a dispersed manner, by using detergents in the medium. This really made them very dangerous, because every time you used a mouth pipette – as we did in those days – you received a little aerosol of tubercle bacilli. There were several cases of laboratory tuberculosis but, fortunately, I never got sick even if I got infected.

I wrote four papers during that time, mostly work on BCG. Subsequently, when I got back to Australia and hadn't really picked up virology again as I wanted to, I wrote a very long review on the bacteriology of BCG. I had been by that time appointed to the new John Curtin School of Medical Research as Professor of Microbiology. **The founding of the John Curtin School of Medical Research**

I think that by the time you were coming towards the end of your Rockefeller Fellowship, Florey wrote suggesting that you might come to Canberra.

Yes. The fellowship ended in July. I had a one-year visiting visa, and just had to get out. But I got a letter from Florey in about March, offering me a position as Professor of Bacteriology. He asked me reply to him, and said a letter from the university would be coming later. I said I'd prefer the chair to be called Microbiology, but I would accept.

Did this exciting offer just come out of the blue?

Oh, there must have been people at the back of it. Anyway, a group called the Academic Advisory Committee – Sir Keith Hancock; Florey; an anthropologist, Raymond Firth; and Sir Mark Oliphant – were working in England to set up the new Australian National University, which was a totally postgraduate university, no undergraduates. Florey was looking after the

medical school side of things. They had already appointed Hugh Ennor as Professor of Biochemistry and Adrien Albert as Professor of Medical Chemistry, and mine was the third appointment.

Instead of coming back to Australia then, I went over to England and met Florey for the first time. I went straight up to Oxford, to plan the building that ultimately would be built for us to occupy in Canberra. We designed the H shape of that building.

What was Florey like?

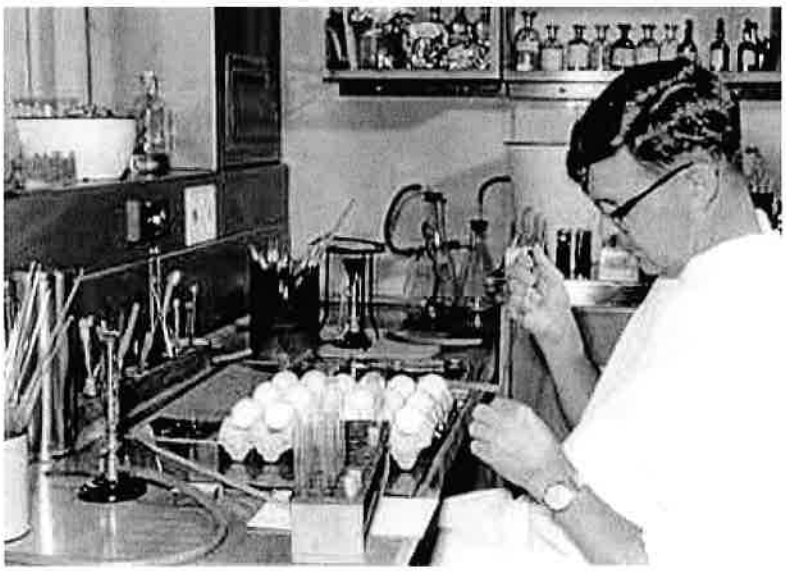
My dealings with him were all at that level, really, when he was adviser on the university. I found him very effective in his operations, very friendly, very nice to deal with. When I had returned to Australia and was working in Melbourne – Ennor was also working in Melbourne; Eccles had been appointed but was in New Zealand – Florey used to come out every year for about a month. We professors would all go up and live at the Hotel Canberra, where we had breakfast together, and dinner at night, for a couple of weeks while we tried to get the place together.

Since things were going slowly, Florey said, 'If you don't get together, this idea will collapse.' So he persuaded the university to build wooden huts for temporary labs and we went up there in 1952. Then he was extremely influential in getting the permanent building built. It had stalled midway because the university was unable to persuade the government to put enough money in to do it. Florey came out, he talked to the government, we got the money.

I think that aspect of his character that I saw was reflected in his work as President of the Royal Society: after 20 years of dithering, of trying to get decent quarters after Burlington House got too small, he was able to get Carlton House Terrace reconditioned to give them a splendid home for the next century.

Were you elected to the Royal Society while he was President?

No, two years earlier, in 1958. A few years later I happened to be in London at the time of the official opening of Carlton House Terrace, with the Queen and everybody else there in fancy dress. I remember being in the shower, having happened to arrive on that night, and without even a dinner jacket. Somebody lent me one and I was able to participate in that very fancy occasion.



Withdrawing inoculum from tube with graduated Pasteur pipette, to inoculate chorioallantoic membrane of developing egg. (John Curtin School of Medical Research, 1958.)

Myxomatosis: a second poxvirus

When you came as Professor of Microbiology to the John Curtin School, how did you find a problem worthy of you?

Well, as I mentioned earlier, I had started off by studying the experimental epidemiology of ectromelia – mousepox – a virus of mice which Burnet had shown was closely related to vaccinia virus and therefore to smallpox virus. Then, when I went overseas, I had drifted away to working on mycobacteria – various kinds of tubercle bacilli – because that was what Dubos was working on. In Australia again, having tasted virology and bacteriology, I wanted to get back into virology. We had no buildings in Canberra while the John Curtin School was getting going, so Burnet offered me space in two labs back at the Hall Institute. He suggested that I might like to take over from him and work on influenza virus genetics, but I felt it would be a mistake to follow on something that he was doing. He was too dominant a personality and I had to be independent. So for a while I carried on with work on the mycobacteria, especially *Mycobacterium ulcerans*. Then myxomatosis, a virus that had been introduced to control Australian rabbits, broke out.

It hadn't been very successful initially, had it?

Its introduction has a long history. It started in 1918, it was tried again in 1934, and finally a determined trial was made in 1950 with the setting up of the Wildlife Survey Section in CSIRO. But the initial releases were made through the winter, even though the Australian research workers had shown that it was mosquito-transmitted. It was about to be written off, when in December 1950 – when the weather conditions and mosquito breeding were right – it escaped and was found 10 miles from the nearest trial site. Then it spread all over south-eastern Australia in a matter of about three months. The case mortality rate, we later found, was over 99 per cent in the field, unbelievable.

There were rabbits dying in hundreds of thousands. And you just walked in on that.

Yes, this fell into my lap. Recently, in the Burnet archives at the University of Melbourne, I found Burnet's diary entries about it. There was no virologist working on myxomatosis, only zoologists. In his diary entries for 31 January and 1 February 1951, Burnet had written that Lionel Bull – Chief of the CSIRO Division of Animal Health, who had done the early work on myxoma virus in 1936-43 before war pressures and so on caused it to be dropped – thought there ought to be some virological work done. On 31 January Burnet wrote that he was thinking of talking to the ecologist Ratcliffe, the head of the CSIRO field section, who was working on myxomatosis, then on 1 February he wrote that I had approached him to say I wanted to work on myxoma virus. In brackets he had a note that he thought Dame Jean McNamara, a controversial paediatrician who had been needling the CSIRO to get onto myxomatosis again, had something to do with my request. But he was wrong there. I hadn't been prodded. I just wanted to get into virology and I saw this as an opportunity. And then I was delighted, as I dug into it a bit more, to find that this was a poxvirus – in line with my previous work with ectromelia. So that kept me in pox virology.

The second great poxvirus in your life.

Yes. I had this marvellous opportunity of a great experiment of nature, in which I was able to follow the way in which the virus changed to become less virulent and then some rabbits survived. And those that survived were genetically more resistant, so they could breed resistance. You got this interchange, this balance between virus virulence and host resistance. In parts of Australia which are valuable for agricultural purposes, the rabbit has now ceased to be a pest.

That period of study of myxomatosis and the myxoma virus lasted for quite a while. It covered epidemiology right down to genetics, at a molecular level. You must have played an extraordinary role in that field of research.

We did look at everything. I remember we started off with electron microscope observations of the particle and showed it was a poxvirus. Then I worked on the pathogenesis: the same things I had done with mousepox, I did with myxomatosis. We worked on mosquito transmission, doing what we call wipe-off experiments. That is, we had mosquitoes in little tubes, let them probe through a tumour and then saw how many positives of the different strains of virus would be taken along. That gave us the clue to why there was a selection for less virulent strains, because the very virulent one was very well transmitted but the rabbits died in four days; some intermediate ones were just as well transmitted but the rabbits lived for three weeks with infectious lesions. So there was tremendous selective advantage.

I had a small team – two research assistants, originally, one of whom became my first PhD student. And that became my virological team throughout the 15 years that I was working in the lab on it. But I had collaboration with all the zoologists, all the help that one needed on the zoological/ecological side, from the CSIRO group and also from various others, from electron microscopists to an entomologist, Max Day, who joined the team working on mosquito transmission. He was interested in the transmission of plant viruses by insects, and here was a chance to look at the transmission of an animal virus which wasn't an ordinary arbovirus. It didn't multiply in the vector; it was carried mechanically by it. We did a number of experiments and published two or three papers on that.

Looking back, I see that we published a paper on the pathogenesis, on the classification, on the morphology, on the relationship with other viruses of that group, the poxvirus group, on the immunity – passive immunity, active immunity – almost all aspects of all the things you could look at in the lab. But the most significant work was the study of the changes in virulence, which occurred very early and went on progressively, and the concurrent changes in the resistance of rabbits: the fact that when the mortality rate fell from 99 per cent, which it was originally, to 90 per cent, there were enough survivors for selection for genetic resistance to occur. For a number of years we followed both the changes in virulence and the changes in rabbit resistance. They have subsequently been taken up intermittently by other people, with some very interesting results. **The continuing rabbit story**

Perhaps we can stay for a while with the myxomatosis story, which has not gone away. You've stayed in touch with that.

Yes. The ecological modellers got very interested in it, especially Robert May, a very distinguished Australian in the Department of Zoology at Oxford. Myxomatosis provides about the only example of a long-continued study of changes in virulence and changes in genetic resistance, and there have been about a dozen substantial papers written on the model, comparing it with the results we got in the early days and with subsequent ones. There are some criticisms of these models, as there always are of models, and I think they are valid ones. But it just is very hard to get a disease where you can get this sort of data.

I believe you are to talk this week about the current resurgence of interest in myxomatosis.

Myxomatosis has done a marvellous job in controlling the rabbit pest in Australia. In many areas of the country it has virtually disappeared. But in the dry outback areas, where mosquito activity is minimal and you only get a mosquito plague every few years, the rabbit causes major problems in promoting erosion and destroying habitat for indigenous, native mammals of Australia. Either the land in such areas is not sufficiently valuable for the

landowners to put a lot of money into control, or else it is, say, national park. And in some areas the fall in virulence has been sufficient to allow a lot of rabbits to recover. Myxomatosis is not controlling the rabbit in those areas, and I am a member of an advisory committee to CSIRO which is looking at other methods of control.

The people in CSIRO Wildlife and Ecology are suggesting using an immuno-contraceptive approach by putting the relevant genes for zona pellucida into the myxoma virus and allowing it to sterilise those that it doesn't kill. It still would have to spread effectively in the presence of existing strains of myxoma virus, and that's going to be the hardest job, harder than molecular biology. But it's an interesting approach. I'm going to a conference at Geelong this week on another proposal: to see whether the rabbit haemorrhagic disease virus can be used as a supplement. It's a very lethal virus for rabbits, but not as lethal as myxoma virus was.

And there is New Zealand interest this time.

Yes. There have been several proposals to introduce myxomatosis for rabbit control there. A recent strong one was refused on the grounds that the flea which would have to be introduced at the same time to spread the virus – because there are not sufficient mosquitoes in that particular area on the South Island – might get on the kiwis. As a sop, I suppose, to the landholders who wanted to have the virus introduced, they were told that rabbit haemorrhagic disease virus might be introduced instead. I think that might cause some problems with the animal welfare lobbies there. We'll see. **Viral genetics**

Your study of myxomatosis was a great experiment in evolution, quite apart from its practical importance, and led you to viral genetics. Did you inaugurate viral genetics in Australia?

No. Burnet pioneered that with influenza virus. But I was lucky, I fell on my feet. Looking back, I would say I always had a second string. From 1950 to 1957 I was writing occasional papers on the mycobacteria. When I stopped doing that, I recognised that these changes in virulence were a genetic phenomenon, a virus genetic change. This was extremely interesting, but although the myxoma virus provided a lovely natural experiment, it was a lousy virus to work with in the lab. You couldn't titrate it readily: you couldn't get very high titres of virus. Moreover, the only animal on which you could do tests for virulence was rabbits, and groups of rabbits are expensive. So I turned to the model poxvirus, vaccinia virus, which was just the reverse. It is a lab virus; it multiplies to 10^{10} per millilitre of virus; it is easy to get; it grows on any cell culture; it produces different kinds of pocks on the chorioallantoic membrane so I could pick pocks, that is single clones of virus, readily. I embarked on a study of mutants, both pock-type and host-range mutants, of vaccinia virus, and then studied recombination between them.

It turns out that the poxvirus is extremely complicated. You have got about 200 genes, so it is not a simple matter. But I think I was the first to demonstrate that recombination occurred with an animal DNA virus. Burnet had shown long before that a kind of recombination occurs with influenza virus, where you have a lot of separate genes that shuffle themselves – reassortment – but this was one long DNA molecule with intramolecular recombination, of the kind that had been shown with bacteriophages long before, in the late '40s, but hadn't been really explored with animal viruses. From 1957 till '67 I worked increasingly on the genetics of vaccinia virus.

I finished up my lab work at that stage. I had had the idea of ultimately studying the genetics of virulence, but I never got to that. It is now being taken up, but by molecular biologists.

At the John Curtin School you moved on from Professor of Microbiology to Director.

Yes. In 1967 the then Dean, as he was called (but he was director in fact), took a job as Secretary of the Department of Education and Science, and I was appointed in his place as Director. I am temperamentally unable to do research without being personally involved, hands-on at the bench – I couldn't do it through assistants or students – so, since the Director's job was not a full-time one, I turned increasingly to scientific writing. But recombination and various other aspects of poxvirus genetics were to serve me in good stead when it came to the smallpox part of my career. **Smallpox: certifying eradication**

How did you move into that part of your career?

The smallpox eradication program was set up in 1967, and very soon D A Henderson, the director of the Smallpox Eradication Unit, realised that they had to be quite sure whether there was an animal reservoir of the smallpox virus. It was known from 1958 that another virus caused a smallpox-like disease in monkeys (it was called monkeypox virus, naturally enough). I was a member of the small committee of virologists that met for the first time in Moscow in 1969 to discuss whether this virus might consitute an animal reservoir of smallpox. I later became chairman of that committee, and from that I just got increasingly involved, not in the actual eradication program but in trying to ensure – to certify – that a country or a continent was free of smallpox. Ultimately I was Chairman of the Global Commission for the Certification of Smallpox Eradication – a long-sounding title for the commission and a very challenging role for me.

Was this working with the World Health Organization? You would have been their chief adviser in that sense.

Yes. The Global Commission had three meetings in three successive years and

organised some 21 international commissions that visited all the countries and continents where smallpox had been endemic since 1967. One nice thing about my job in Australia was that because there were no lectures to give, whenever there was a meeting I could get time off to go to it. I used to go about six times a year to Geneva and also I had some fascinating trips to China, Malawi, Mozambique, South Africa, all sorts of different places. Finally we got signatures from every country in the world that there had been no cases of smallpox in the last two years, and certified that smallpox had been globally eradicated.

Was the WHO happy then that there were no secondary animal reservoirs?

That was the main problem the virological committee had been set up to deal with. One is always suspicious that a disease in monkeys might be a reservoir of human disease. For example, it is known now AIDS is the result of a virus of monkeys that has got into humans. Monkeypox virus looked to be a reasonable candidate as a reservoir of smallpox but when the committee looked at it we decided – correctly, I know now – that it was quite a different virus. We now have the molecular biology, the genome.

But the Russian investigators, who were a very important team, claimed that they had isolated a virus identical with the smallpox virus from strains of monkeypox virus. They called these white-pock variants of monkeypox. I knew from my work with vaccinia genetics that you don't get a number of different isolates of an identical virus in these kind of mutants. They are all different. I had had 50 mutants of rabbitpox virus, white-pock mutants, and they were all different. They had about eight mutants of monkeypox virus that were all the same, and all the same as variola, the smallpox virus. I maintained from that that they were contaminants, and ultimately it was shown and admitted that they were lab contaminants. It is very hard to convince colleagues that they have contaminated their lab stocks.

Technically that was the most important thing I did, but administratively I felt a big responsibility in the final meeting of the Commission to get those 20 people, from 18 different countries – two representatives from the USSR, two from the United States and one each from a number of other countries – to agree to a series of 19 recommendations. They finally did that, a historic moment, and so we had champagne at 3.30 in the afternoon on Sunday, 9 December. The WHO don't often have champagne there, and certainly not on Sunday afternoon. But we thought it was a worthy occasion. **Linding biological, environmental and resource issues**

One of the great interests of your life, even broader than the microbiology story, has been environmental issues. Could we talk about your move in the mid-1970s from the John Curtin School to direct a new centre at the Australian National University concerned with environmental issues.

I was Director of the John Curtin School from 1967 to 1974, during which time the Vice-Chancellor of the university was Sir John Crawford, a close personal friend of mine – we played bridge and tennis together – and an outstanding, very imaginative vice-chancellor. He used the directors of research schools for other jobs than just running their research school, and he got me to be chairman of two committees on new developments. One proposal was for an undergraduate medical school in Canberra. Because the plan for the medical school was very detailed, it suffered by comparison with less explicit plans put forward by other universities and wasn't accepted by the Universities Commission, which made decisions on big funding proposals.

The other proposal was for a centre for natural resources – something not as big as a research school, but also not narrowly environmental – and in about 1972 that proposal was finally accepted for funding. Our first name was the Centre for Environmental and Resource Studies (CERS), pronounced 'curse'. We decided that was not a good acronym, so we just switched: the Centre for Resource and Environmental Studies, CRES, pronounced 'cress', gave quite a good acronym. I think it's the only centre in Australia where both resources and environmental problems are looked at in the one context like that. It is important not to go off half-cocked, saying, 'Don't use your resources.' You have got to use them, but in a way that doesn't despoil the environment.

By then I was coming to the end of my term as Director of the John Curtin School. I felt it was a bad thing to extend a term; it was better to do something else. After six or seven years, I had been out of the lab too long to want to go back to the bench in virology, and I'd had an interest in environmental affairs from my upbringing. From the late 1950s, as Biological Secretary of the Australian Academy of Science, I had taken a very broad view of things; my interest had grown in the mid-'60s and I had been on the international Scientific Committee on Problems of the Environment since '71; and I was Vice-President of the Australian Conservation Foundation for about the same time. And at Burnet's 75th birthday I gave a speech subtitled 'The three faces of science', one of which was environmental. So I'd had some touch with it.

Professor F C Courtice presents Professor Fenner with a book of photographs of the John Curtin School at the time of his resignation as Director in May 1973.

I indicated an interest in setting up the new centre, and was appointed as director. That was really quite good. Money had never been a problem in the

ANU in our early days, up to the time I left the directorship of the John Curtin School. Crawford, in his interviews with the Universities Commission, would lay down the law and say, 'The annual rate of increase in the gross domestic product is 5 per cent per annum. We must have 6 per cent per annum.' And that's what we got. With 6 per cent per annum you can do new things.

The first job was to get started. I was it. My secretary came across with me from the John Curtin School to this new job. To be Professor of Resource Economics I was able to get Stuart Harris, an excellent man. Although he didn't immediately succeed me, he did become Director of CRES for a while before extending his career elsewhere as a resource economist. And I got Peter Young, who was a systems analyst in England, because I was sold on modelling, a systems approach to environmental problems. He is now a professor at the University of Lancaster. Other senior appointees included Stephen Boyden, who was elected a Fellow of the Academy as an immunologist but then got interested in the broader problems of what he called urban biology, or humans in an urban setting. Having worked in a little unit in the John Curtin School, he then made major contributions in CRES. **Essential support, major outcomes**

Did you have strong political support when you got started, or was it just an academic venture that you hoped could win political support and funding?

We had Universities Commission support, which I suppose is conditioned by what the Commission perceives to be the political necessities. I remember it said, 'Every university has put up a proposal for an environmental studies centre. We can support only two, one in Monash University, mostly for undergraduates, and this one in the Australian National University.' There was a teaching component in the masters degree by coursework, and PhD students, but no undergraduate teaching.

I had a job to get adequate funds after the first few years. When we started, in 1973, everything was rosy: I had approval for 15 academic staff , quite apart from research assistants, computer programmers and all the rest. But I got only seven or eight, and university funding became tight. A new Vice-Chancellor saw that one way of saving funds for other things was to kill the half-grown chicken. That meant struggling rather hard.

You were really fighting to survive.

I received great help from 'Bob' – Sir Rutherford – Robertson, the Director of the ANU Research School of Biological Studies. He agreed to provide me with three research fellowships as the funding for three additional positions. Then the Director of the John Curtin School transferred Boyden, with his funding and a couple of support staff, to me. That gave me four positions which just made it viable, and there are no more positions in CRES now. If I hadn't got those positions, I don't think it could have been continued. The present

Director, Henry Nix, is now in his second term and doing a splendid job. He has been very interested in the effects of climatic change on the Australian environment from back in the 1970s. That is centre stage now. He has got very strong modelling, computer understanding and the centre is making a major contribution.

So CRES is there to stay?

Oh, it's there to stay, yes. It's good to have it established.

You had the satisfaction of addressing a number of really pressing environmental issues.

Yes, and they are not going to go away. When I began, however, I wasn't really an expert in any aspect of environmental resource matters. I could bring a broad overview but I never felt that I was an expert. I became an instant 'international expert': you are selected to these things because of the position you hold rather than necessarily the knowledge you have.

But you took it seriously and did a lot of research, approaching it with the viewpoint of a sensitive realist.

My writing experience was useful. I became the editor-in-chief of SCOPE, the Scientific Committee on Problems of the Environment, and went to all its executive committee meetings. That was fascinating. SCOPE is the environment body of ICSU, the International Council of Scientific Unions, and reports to the world, essentially. It writes reports: there is now a range of about 50, some of which have been very influential. One was the first comprehensive report on environmental impact statements. I was on the committee that drew that up, meeting in some ice-bound place in Canada for two weeks to do the final editing. **Taxonomy of viruses**

It seems that your smallpox work throughout the 1970s – leading to the great moment when, as chairman of a world committee on smallpox, you announced that eradication had been achieved – was the culmination of your career in virology.

I think that's true. When I went to CRES I resigned from a number of committees to do directly with virology and epidemiology, such as the National Health and Medical Research Council committee which I'd been on for donkey's years, because I thought I needed to devote myself to learning what my new job was and the topics were so broad. But I really found I couldn't give up the interest in smallpox. I justified this on the grounds that you could call it an environmental disease. Eventually I got involved with the hard problem of demonstrating to the world that the eradication of smallpox had been achieved. Probably I was acceptable for this because I was an Australian and therefore not from a country that was a big power; I was

native-born English-speaking (the World Health Organization conducts its affairs and writes its reports in English); I had been on WHO committees; and I knew a good deal about poxviruses. The high point came in May 1980, when I delivered a report at the meeting of the World Health Assembly, the governing body of the WHO, that smallpox was eradicated.

In England in 1983, at the gravestone of Benjamin Jesty. Jesty inoculated his children with cowpox virus in 1774, 22 years before Jenner made his first inoculation.

That indicates to me that your virology work never went away, no matter how many other things you were involved in. Tell me about the work you had done earlier on the taxonomy of viruses.

I was appointed in 1970 as the President of the International Committee on Taxonomy of Viruses. That meant writing the quinquennial report of the committee, which was an interesting job because the nomenclature of viruses came very late to biological nomenclature. In 1966 the committee had devised a number of rules, some of which were very good, such as that doing away with priorities. Up to that time, if you found that somebody had described a species 10 years before the existing name was given, you'd scrap the existing name and reinstate the first one. The committee's rules dismissed priorities of that kind, preferring a Latinised binomial system of genus and specific name. That was due to the influence of André Lwoff, a very famous French scientist, but it was opposed bitterly by plant virologists, in particular, especially British plant virologists such as Adrian Gibbs.

They said – for some good reasons – that it would tie virology into a system which was inappropriate for viruses. The threatening thing at the time I took over was that they were so upset that they threatened to get out of the system. Bacterial and animal viruses would be handled by one committee and plant viruses by another, under two systems of nomenclature. Since certain families of viruses have representatives in both plants and animals, that would be ridiculous. So I saw my major job as keeping them in the fold but not destroying the good things that had been achieved.

I did that by concentrating on the big groups – the families and the genera – and not worrying at all about Latinised names for the species. Instead of

always saying, 'the such-and-such virus group', it would be easier just to call them '*-idae*' for family and '*virus*' for genus. That was successful, and the last thing I did was to ensure that a plant virologist – Dick Matthews, from New Zealand – became the next President.

It sounds as though a lot of diplomacy was involved in that, resulting now in a very tight virological taxonomy.

I think it was a move in the right direction. The plant virologists have now come on board and have done away with their objections to using these more sensible names. That is now a very good committee. The taxonomy works very well, bringing order to the whole business. You can't have 3000 different agents floating round without any sort of system of nomenclature and taxonomy.

And you published the final report over your signature?

For that 1975 one, yes. But there are others coming out, including a very good one from the present chairman, Fred Murphy, who is one of the collaborators in the book *Veterinary Virology*. He was at the Centers for Disease Control and Prevention for many years and works now at Davis.

Before we leave taxonomy: what links the poxviruses together? What have they got in common that makes this a family?

The poxviruses are a tight bunch, a really closely related lot of viruses. They are similar in many ways: they have a few common antigens, they have a similar structure, a similar kind of genome, a similar cytoplasmic replication. But the poxviruses of vertebrates fall into eight clearly distinguishable families, one of which includes eight or nine different viruses – of which smallpox, cowpox and vaccinia are the important ones for humans. Myxoma virus is another group, with representatives primarily in South and North America. That's a virus of rabbits of different kinds and of squirrels. There is one that causes *Molluscum contagiosum* in humans, and so on. There are a number of different genera (*Orthopoxvirus*, etc.) within a well-defined virus family (*Poxviridae*). **Scientific writings**

You have mentioned your writing experience. Could we talk now about some of your books and reports – many of them quite significant.

Well, I have written a number of books. In 1960–61 I had a year's study leave at Churchill College, in England. Although that gave me the chance to go over to France and find the fascinating story of the French and myxomatosis – which I won't have time to talk about now – I spent most of the year writing a book, with Francis Ratcliffe, summarising myxomatosis. It was eventually published by Cambridge University Press in 1965.

I guess I had a guilt feeling early in my career about being a professor who had never in my life given a course of lectures – I went from the Hall Institute to the Rockefeller Institute to the John Curtin School. So I wrote a large technical book, in two volumes, on the biology of animal viruses. Then David White – an ex-student who was a very good teacher and Professor of Microbiology in the University of Melbourne – and I wrote *Medical Virology*, which Academic Press in America published in 1970 with some misgiving. They hadn't published a textbook before.

That became a classic.

It sold very well, 25,000 copies, and we are currently in the fourth edition of it. After the second edition, however, I wrote to the person whom I'd been dealing with in Academic Press, suggesting that there was room for a textbook on veterinary virology. So *Veterinary Virology* was deliberately designed as a companion volume to *Medical Virology*. I got four collaborators. David White came in with me again, writing some of the chapters of Part 1, the general principles, and four veterinarians – one from Germany, two from the United States (one of those, Paul Gibbs, was actually a British scientist), and an Australian man in Melbourne, Mark Studdert – to write the clinical chapters for Part 2. That first edition appeared in 1987. The publishers were a bit sceptical about its sales but for a veterinary textbook it sold very well, nearly 6000 copies. It got very good reviews and we have just done a second edition.

Was working with the veterinarians a good experience?

They were excellent, very good companions. Peter Bachmann, the German, died very tragically during the writing of the book so we dedicated that volume to him. To replace him, keeping the international flavour of the editorial group, we got another distinguished German veterinary virologist, Rudolf Rott. My function there was largely organisation and editing – everything went through my word processor. I was very proud that I kept the length of the second edition, in spite of the new material, to within five pages of the length of the first edition. But I'm afraid that *Medical Virology*, of which David White is now the senior author, has blown out by 200 pages. He doesn't control the writings so rigidly.

We come now to the smallpox story, on which you produced a vast book.

Three and an eighth kilograms – people would not be able to read it in bed. It is called *Smallpox and its Eradication*. It covers the virology, the pathology, the pathogenesis, the history of smallpox itself, the history of vaccination, then the history of eradication, country by country and continent by continent, and of certification, and finally the lessons for the future.

You had a major part in putting all this together.

Yes. I had been Chairman of the Global Commission for the Certification of Smallpox Eradication up till 1979, when it made its final decision. When I retired, aged 65, the Global Commission resolved – not at my instigation – that there should be a proper record of the eradication program. I was unoccupied, so I took it on, thinking I was going to do it on my own, and in a year. It took eight years, and I did it with several colleagues – in particular, D A Henderson and Isao Arita. It ended up an interesting book, beautifully produced with a series of coloured photographs of smallpox cases – it would be impossible for any ordinary publisher to afford to publish this in colour, but the WHO spared no expense. Some parts of the book are thrillers, especially the 100-page account of the eradication in India, which was really the hard nub of the problem, as that is the ancient home of smallpox. If you start reading that chapter you have to finish it before you put it down.

Frank, I think that in the mid-'70s you got involved in writing up the history of virology – a novel experience.

Well, it was a kind of history of virology. Adrian Gibbs, whom I mentioned earlier, came out from England for three years of working with me in animal viruses. (He is now Professor of Molecular Systematics in the Research School of Biological Sciences at the Australian National University and has just this year been elected to the Academy.) He suggested that we get some of the people who were studying the classical viruses – tobacco mosaic virus, foot-and-mouth disease virus, and so on – to write up the history of each one as they saw it. The resulting 'Portraits of Viruses' were published over about 10 years in the journal *Intervirology* and then we gathered them together as a volume, despite some reluctance on the part of the publisher, Karger, who couldn't see that he'd make any money on the sale. That volume contains a lot of very interesting accounts, portraits as much of the 15 virologists who wrote the histories as of the viruses. A classical example is the account of tobacco mosaic virus by Heinz Fraenkel-Conrat, who first demonstrated that the nucleic acid of TMV was infectious. Burnet wrote the one on influenza virus, Brooksby wrote the one on foot-and-mouth disease virus, and so on – all historical.

A classical text. And you wrote about the poxviruses.

I wrote a general one on the poxviruses, yes.

In retirement you have written another colossal text, this time on microbiology.

That was published as the *History of Microbiology in Australia.* The idea of it was proposed by my colleague David White, who was President of the Australian Society of Microbiology at the time, when I had just finished the book on the eradication of smallpox. Again I thought it would take a year, but it took three years. There were 320 collaborators, of whom 319 provided material. Unusually for a book like this, it has one blank page, on which I was

tempted to put, 'This page is where there should be something on …' – a particular subject, which I won't mention, by a person I won't mention. I left a blank page because when I was already indexing the book I was still waiting for his contribution. But it didn't come.

Writing the book was an interesting experience, because microbiology is a broad subject. As well as general aspects like international activities and microbiology in the two world wars, we included protozoology, soil bacteriology, water microbiology, viruses – of course – and institutions where microbiology is done and so on. **Happily married (though not always home)**

We must say something about a key figure in your life – Bobbie Roberts, whom you married in 1943, during the war. You have mentioned her transfusion work in the malaria unit at Cairns.

Having trained as a nurse in Western Australia, she went away in the first contingent of nurses to the Middle East. She was at the hospital at Kantara, in the desert, where Sir Ian Wood was a senior physician in charge of blood transfusion. He got her to move into that and she became a very expert transfusionist. Because the hospital couldn't get any saline, my wife undertook to prepare sterile saline – under those desert conditions. Bill Keogh, the pathologist there, did all the sterility testing and she came through with flying colours. She was very good and Keogh was impressed. She in fact got recognition as an Associate of the Royal Red Cross, which is very rare for a person who is not an administrator. Nearly always such recognition goes to matrons, not hands-on, working people. And then, because of her expertise in transfusion work, she went into direct transfusion in the research unit and also dissection of mosquitoes with Josephine Mackerras, the parasitologist there.

Bobbie Roberts, Jerusalem, 1940.

You've worked pretty closely, for example in the mousepox studies.

Yes. Firstly she worked with me in the lab, and then we had some children and so she looked after them. But also, especially when I was Director of the John Curtin School, she maintained open house for the staff and so on. She's been a

tremendous support to me, right through my career.

You have told me that you moved together from Vancouver to New York by train, when you were going to the Rockefeller Institute to work with René Dubos. I've often enjoyed that wonderful journey, with the arrival at Grand Central Station.

My wife has very vivid memories of that trip. We came across Canada on the Canadian Pacific, stopped at Lake Louise for a little while, and then got on the train. But I was under the impression that one got meals aboard. In fact, one didn't. And I had no money at all. I think I lived on a few oranges, but she almost starved to death before we got down to New York. So that coloured her memory of that trip.

Not all your travels have been together, but during your time with the Global Commission for the Certification of Smallpox Eradication you made some substantial journeys. This wasn't a sitting job – you were moving about an enormous amount.

I travelled for two reasons: to go to the meetings of this committee; and to the Commission and then on international commissions. Somebody had to see whether the Chinese had eradicated smallpox. At that time they were not within WHO and were very reluctant to receive anybody. Finally they agreed that one person from the WHO committee and I could go on a two-man Commission visit. We travelled fairly extensively in China for five weeks. I suggested we should go to Tibet, where the last cases were, but they wouldn't let us – they said it would take some weeks for us to get acclimatised. With the travel and after checking the extensive Chinese documentation, we satisfied ourselves that there were no recent cases of smallpox there. And I went to Malawi, Mozambique, Kenya and so on – very interesting.

You have written that during your career there was an enormous change in opportunities for science ambassadors to move about the world. When it had to be by ship, it would have been too slow to be possible.

Well, in the immediate postwar period we went to America by ship and came back by ship. The first time I went to America by plane – in 1953, I think – we stopped at Guam and a few more places as well. But the university at that time was pretty flush and professors were pretty important people who travelled first class. In first class at that time the airlines provided Pulman beds: they just put down white sheets on a bed, where I lay down and slept as the plane flew across the Pacific – very, very smart travel but not the way I travelled for most of the rest of my career. That was usually economy class. For a long time you could lie out flat on five seats across a 747, but since the tourist boom the planes have filled up and you can't do that any longer. **Extending the Academy link**

You have had a long association with the Australian Academy of Science. Can you tell me a bit about your increasing link with the Academy?

I was elected in 1954. The 23 foundation Fellows were Fellows of the Royal Society resident in Australia, plus a few senior scientists. One of the provisions of the charter, which was signed by the Queen in 1954, was that they had to have 50 members by the end of that year. I was one of the 30 who were selected in 1954. In 1958 I was asked to become Biological Secretary, in a similar arrangement to that of the Royal Society. That took me beyond medical research into the wide area of biological sciences. I enjoyed that job because it took me into new fields and led to involvement with committees that played an important part in setting up institutions in Australia. One report arose out of envy of the way in which physical scientists could get $10 million for a new telescope but biological scientists had to struggle to get $10,000 for a research assistant. So it was decided that the only way to get big money was to put up big projects.

One proposal was for a survey of the Australian fauna and flora, which was finally set up as a biological survey of Australia in a statutory body and has produced a new flora of Australia. The second was a recommendation to set up, within the Australian National University, a research school of biological sciences. That too came to pass, resulting in a very successful research school.

Later, because of my membership of SCOPE, I became a member of the Committee on the Environment, which was very active in the '70s. In 1975, when I was Vice-Chairman and a very prominent meteorologist called Priestley was Chairman, the committee produced a report on climatic change – so we were looking at the climate change problems quite a long while ago, with a whole range of quite influential reports on national parks, ecological reserves and so on.

There is another angle to all this. In the '70s the second edition of *Medical Virology* sold very well and because there were only two authors I got quite large royalties, but most of those payments went to the taxman. On top of my university salary they put me into the top tax rate of 60 per cent and I didn't like the money going that way. Knowing that there was tax deductibility for donations to the Academy, for a number of years I put the royalties into an environment fund for the Academy to use.

After I retired, my wife and I discussed our affairs and decided to set up a couple of endowment funds, this time specifically for conferences. One fund is within the Academy of Science, where colleagues had done the same thing. Sir Frederick White in particular, who had been the Chairman of CSIRO, set up an endowment with which conferences were held every two years. I have provided money over a number of years for a fund – adequate to be maintained indefinitely – with which they hold conferences on the environment. The seed money is put in from this fund, and then other organisations put in 10 or

20 times as much to get the things running. They have been extremely successful, with five or six very good publications and very influential meetings on such things as the preservation of the coastline and adjacent wetlands; the Murray-Darling Basin; the history and preservation of the high country, the Alps; biological diversity, as a forerunner for the conference in Rio; and, recently, trade investment and the environment, and the various aspects of GATT that impact on environmental matters and so on.

The other one is in the John Curtin School, for small conferences on various aspects of medical research, which I think have also been quite successful. So that is the last aspect of my environmental activities. As a person who I suppose is approaching elderly, I find it very satisfying to see how effective this small input of money can be as seed money for these conferences.

Tremendously influential. You and Bobbie must feel very pleased with the outcome.

Oh yes, we're very happy.

As well as going on with new editions of Medical Virology*, you wrote a history of the Academy, didn't you?*

Yes, covering its first 25 years. That was published in 1980. **Recognition, retirement, refocusing**

Frank, it would be nice to talk a little bit about your Japan Prize for Preventive Medicine, a remarkable but very fitting tribute.

The Japan Prize was set up by the Japan Science and Technology Council, in close consultation with the Nobel Prize people. It would not compete directly with the Nobel Prize, which devotes itself to fundamental science; instead, the Japan Prize would pick applied problems. The first award was in 1985 and two awards are made on different topics each year, one in the biological sciences and one in the physical sciences. In 1988 the subject was preventive medicine. Since smallpox eradication had just been achieved, it was a natural to be put up for an award. The real leader in that field was D A Henderson, an outstanding epidemiologist. If you picked one man you would have to pick him. But, fortunately for me, the names of two other people were added to his: Isao Arita, his Japanese successor as chief of the smallpox eradication unit at WHO, and mine, as having been Chairman of the Global Commission. We were fortunate enough to receive this prize of ¥500 million, which is a substantial amount of money. Even splitting it up leaves a pretty nice nest-egg. To have that sort of money come in has helped very much in setting up the endowment funds, of course.

It is as though, as you got into your role in addressing problems of the environment, your father's wisdom and that great early vision of the

environment were being put to new and better use all the time.

Yes. I'm sure that was an important influence. I carried on the smallpox work right through the time at CRES. I would go to a SCOPE meeting one month, the next month I'd go to Geneva on a smallpox meeting. And you can look on smallpox as an environmental disease. It is an infectious disease associated with close contact with infected people, and there is a means of controlling it.

You were an observer while it was being slowly pressed into a corner, finally into a little bit of Somalia and then out.

It was a marvellous thing to be associated with. And D A Henderson, who is now in the White House as officer of science and technology policy in charge of biological sciences, was an inspirational person to work with, a real leader, a great man. We respect and like each other very much. So that was a great experience.

In the 1980s you retired and you were going to do all kinds of things in a quieter way. But I don't think that happened.

The big relief of retirement was to have no administrative responsibilities, which I'd always had as director of a school or a centre, thinking all the time about money and about people. When those were shed I had an excellent job to do in writing the history of the eradication campaign, but in parallel, during quiet periods, I went on to write some other books. Whereas previously writing was my spare-time activity, it has now become my main, full-time activity. Since I became personally wedded to an Apple Macintosh computer, that's the only instrument that I deal with. I like it very much, especially for these collaborative works done with a number of other authors. It's so easy to alter, integrate and correct, stick things together, rearrange.

We're hoping to persuade you to write some more history of virology, with Tom Tinsley, who has joined us at Oxford Books.

I'll be interested in looking at it. I won't make any commitments.

Professor Fenner, thank you for going over your very exciting career with me. I hope that when next you are in London we can record some areas of that career in further depth. Thank you for talking with me.

Thank you very much. I've enjoyed it.

With wife Bobbie, daughter Marilyn and grandchildren Sally and Simon, at celebrations for his eightieth birthday in 1994.

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