



## Australian Academy of Science – Science education Interview with Professor Ralph Slatyer

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Professor Ralph Slatyer, ecologist, was interviewed for the Australian Academy of Science's *Video Histories of Australian Scientists* program in 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 of the interview.

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

List of edited transcripts.



### **Early gifts: curiosity, a love of nature and an enjoyment of work**

*Ralph, perhaps you would talk me through your early days. You were born in Western Australia in 1929.*

I was, and I grew up in Perth, although my parents had rural backgrounds. They both had a very powerful influence on me. I was the fourth of five children – spread out over about 20 years, so we all got rather individual attention.



*The fourth of five children – Ralph aged 9 (1938).*

I realise now that my mother, in particular, had a remarkable ability to arouse curiosity in people. She combined that with a love of nature: whenever one was out of doors with her, some sort of a biology lesson was going on about a garden plant or a bird or whatever it happened to be. She would always ask the question that aroused your curiosity and made you think about the answer. And my father was a bank manager who thoroughly enjoyed work. He got across to us as children that enjoyment of work, of paying attention to detail and doing something well.

### **A stimulating education**

*Where did you go to school?*

I went to the little local primary school and then began my secondary schooling at a selective high school, Perth Modern School – quite a remarkable school which took only 50 boys and 50 girls each year, based on IQ tests and things like that, in four classes of 25 children. Good teachers gravitated there and we had an excellent learning environment for all sorts of things. The science was very good (which later I became conscious wasn't always the case in those days) and gave us a very good grounding in physics, chemistry, mathematics. I went to Mod, as we called it, for two years.



*Doing homework at Perth Modern School (1942).*

When my Dad retired, my family moved out of Perth and I became a boarder at Wesley College (Mod was a day school only). Frankly, I thought that Wesley wasn't a particularly good school at encouraging excellence and achievement – although I should say that during those war years it was extremely difficult for the schools to keep good teachers when so many went into the Services or into other jobs. The redeeming feature was the science master, Vic Cooper. He was a remarkable teacher and the sort of person who aroused one's curiosity, rather as my mother did. He never gave you the answer but he gave you all the information you needed to go about finding the answer yourself. In that sense he influenced me more than any other teacher I had.



*Top dressing the family tennis court (1945).*

*You must have finished school close to the end of the war.*

Yes, in 1946, after the five years of secondary school.

*When you moved on to the University of Western Australia in 1947, did your experiences influence you towards research?*

Yes. The course was very good, very much hands-on. And the Dean of the Faculty, Eric Underwood, was another important influence on me. He was a really delightful person, a dynamic, influential teacher and very stimulating about science in general and agricultural science in particular. Andrew Stewart, another person on the faculty, encouraged me strongly to get into northern

Australia, having had some experience there himself.

So I have been fortunate: right through primary and secondary school – both Modern School and to a lesser extent Wesley – I had stimulating teachers. Later, as an undergraduate, I was again privileged in having good teachers.

### **Productive interactions**

*You had been surrounded at Perth Modern School by people who went on to achieve quite impressive things. Was there some special influence behind that?*

A number of people in that class of '46 did remarkably well, I think because we were the first group going on to university who encountered the enormous number of postwar ex-servicemen and women – people in their 30s and maybe 40s – in our undergraduate classes. They were all there because they knew they wanted to get an education. Without them we might have done the usual things that undergraduates do, with even less commitment than we had to the educational process! They were determined to beat us 'little kids'. So of course we were determined to beat them. It was a very powerful interaction, which I think was what made that whole class, around the countryside, do so well.



*Undergraduate days – with older friend, Roy Weston (1948).*

*Would you like to mention any particular figures from your class at Mod?*

Well, for example, the recent prime minister, Bob Hawke, was there. We knew each other, but the classes were divided alphabetically so he was in the A to Ms and I was in the N to Zs, and you mainly get to know your own classmates best. He did law at university; I did agricultural science.

*But you had a working link with him when he became Prime Minister.*

Yes. By then I had been appointed by Malcolm Fraser, the outgoing prime minister, as chairman of the Australian Science and Technology Council (ASTEAC), a part-time job. When Bob Hawke came in it was possible to see him on the first day he was Prime Minister, and he agreed to continuing ASTEC and to my staying on. That led to a very enjoyable and productive interaction with him – and also, might I say, earlier with Mr Fraser and subsequently with Mr Keating. The interaction with Hawke was very

satisfying. He was intensely interested in what you had to say and was a very good listener. And having been at school with him, you could say, 'Prime Minister, I think you should do this', whereas it is rather difficult to say that to someone you have never met. You knew that he was basically the Bob Hawke who was at school with you in 1942 or '43 or whatever it was. So that did make a difference.

### **The gift of a loving family**

*Your move to the University of Western Australia brought you a wife and subsequently a family life, didn't it?*

Yes. My wife is a wonderful person. We met as undergraduates at the University of Western Australia when she was June Wade, an educationalist. She is absolutely part of me, and vice versa, I think. She's a fantastic person: very clear-headed, marvellous to argue with, interested in a lot of things – really complementary to me, I guess. From the very start she gets the human dimension right, whereas if I do get it right it takes me somewhat longer. She is prepared to put more time into getting the best out of a book or a new experience, whereas I've been sufficiently locked into my career, and enjoying it so much, that I haven't spread myself as broadly as she has.

We have three loving and self-confident children – a boy, then two girls. They are all doing different things and enjoying them. We think they're fine young people.



*With wife June and daughter Judy in 1990.*

### **Agriculture: could we really feed the world?**

*You said you did agriculture science for your BSc. Why was that?*

As a matter of fact, it was a toss-up as to whether I did engineering or agriculture. In those days, boys in Western Australia who had a mathematical inclination did either engineering or science of one form or another. Doing medicine or law was a bit of a soft option! It is interesting to see how things have changed since then. But because of the prerequisites for engineering, for example, you had to decide in your fourth year at secondary school what level of maths you were going to take. And the teaching at Wesley was such that I

couldn't do the second maths – Maths B – at a level that I thought would be sufficient to do well in engineering. I was probably mistaken about that, but in fact with my parents and the headmaster I made that judgment, which led me to do agriculture rather than becoming an engineer – and I've not regretted it!

*Very early on, I have read, you were excited by the prospects and the challenges of feeding the world.*

Sure. It was very much the theme of the time. Indeed, one of the main reasons I did agriculture was that I saw a nice illustrated brochure with a person in a lab coat in a greenhouse, who clearly was going to help to feed the world. I regarded that as pretty stimulating stuff. I was very much dedicated to it. The general feeling, that science could address the enormous problem of the starving millions and that agricultural science was a primary vehicle for that, certainly drove me in that direction.

At the time, northern Australia was almost a blank map. It was thought to have enormous agricultural potential, but nobody knew. By the late 1940s, as an outcome of the war, there was an attitude here that we had to populate or perish – either we did something about northern Australia ourselves or somebody else would. So there was a lot of postwar activity to prove the rural potential of northern Australia.

*Did that influence your BSc honours degree in agriculture?*

It did. We had to spend the summer vacations either on a farm – in some area of agriculture – or on an agriculture research station. My interests in northern Australian and in feeding the world made the opportunity to go to northern Australia irresistible, and I spent my summer vacations on one or other of the CSIRO research stations. (One had been established at Kununurra, in northern Western Australia; one at Katherine, in the Northern Territory; and one at Alice Springs.) That gave me hands-on experience in those environments. In the wet season, with prolific growth of vegetation, one is emotionally convinced that it must be a very productive environment, and the idea of working up there and trying to assess whether it was a productive environment or not was very challenging.

### **An exciting first job**

*Didn't you later go into a full-time job with CSIRO?*

Yes. Actually, jobs were quite difficult to get in the early 1950s when I finished my degree. Fortunately, because I had worked at the CSIRO research stations I was offered a job with what became the CSIRO Division of Land Research, headed by Chris Christian – another remarkable person and very important influence on me.

*A man who let you have your head?*

Indeed he did, but he told me subsequently that he had his eye on me. And I had my eye on him, I might say. I wasn't altogether surprised when I got a job offer, but it did mean choosing whether to go straight into a job with CSIRO as a research scientist, for which the normal entry requirement was a PhD, or to do a PhD. I guess I was cocky enough to think I could do research without being trained particularly. Almost certainly that was wrong, but the temptation to get straight into a research career rather than marking time for three years persuaded me to take the job.



*Starting with CSIRO in Canberra (late 1951).*

My academic record looked quite impressive, and I could have had a job as a biochemist or as what was called an ecoclimatologist. It is a measure of the time, just as it is inconceivable now, that you could have a choice of jobs so far apart. I had done very well in biochemistry in my undergraduate course and Christian wanted to hire a biochemist to work on the downstream part of all this agricultural production. But I was very much attracted to the ecoclimatologist job, which was essentially to predict, from climatic and soil characteristics and other *in situ* characteristics, the length of the growing season for introduced crops and pastures – and, in the process, predict any other characteristics of the growing season which would make it either more or less reliable for a potential agricultural region. It was quite a challenge.

CSIRO had established a land research survey team and Christian had pioneered broad-scale survey techniques by which he mapped the whole landscape instead of just soils or vegetation or geology, as things used to be done. His approach, essentially geomorphological, became adopted throughout the world, particularly by the international agencies. It was all done with photo interpretation, with a field survey to validate the 'query points': difficult photo patterns which could not be interpreted accurately. I went along with those people occasionally for a few weeks at a time, but I wasn't actually involved with them. They were providing for CSIRO the information on the landscape, soils, geology and so on, and I was providing it on the climate and the growing season.

They were a great bunch of people to work with, and tremendously exciting.

They too were conscious that they were doing important things. For the first time, the physical and biological attributes of northern Australia were being surveyed. It wasn't easy. We all recognised we had to develop a technology for the growing season studies that could be applied to broad areas. It was first-approximation stuff, but in a sense all the more challenging because of that. You were conscious of actually crafting that big canvas, and so my first job was really quite exciting.

### **Win/win: collaborative research links**

For a start, Christian sent me off to Adelaide to sit at the feet of James Prescott, the head of the Waite Agricultural Research Institute, who had done some wartime climatological work. The great thing was that Prescott had two hats, one as Director of the Waite Institute and the other as Chief of the CSIRO Division of Soils. Indeed, the Division of Soils was contained almost entirely within the fabric of the Waite Institute buildings. As a young researcher during that visit, when I walked into a lab I didn't know whether the person I was talking to was a CSIRO researcher, a university researcher, a PhD student, a post-doc – there was just a group of people working together. It was a really marvellous collaborative atmosphere. Everybody won.

A few months later I went to the CSIRO Division of Physics, in Sydney. Exactly the same thing applied: university and CSIRO mixed up in the same lab, CSIRO people supervising graduate students and so on.

My whole philosophy in research, in any problem resolution, has been win/win – try and make everybody win to some degree. Unless you can do that, ultimately whatever you stitch up comes apart. And my experience with both Prescott and the Division of Physics made me realise how important it was to build collaborative research links between CSIRO and the universities. Rather sadly, those links fell away in the 1960s and '70s, but now we have set things in train to re-establish them very strongly with the Cooperative Research Centres program.

### **Linking plant/water relationships to climatology**

After that period with Prescott, I really set to on the task of predicting growing season attributes. Initially I had to work with broad climatological statistics, but very quickly I realised that I had to know more about soil physics and things like soil water-holding capacity, and about micro-meteorology and such things as factors that affected natural evaporation. So my career changed from ecoclimatology to a heavy involvement in micro-meteorology and then in environmental plant physiology. In the process, I developed what were probably the first agroclimatological-ecoclimatological models, and they have now been used quite extensively right round the world. They have been greatly improved, by others, however, from the early models that I used to crank out with a hand-calculator.



But to get the response surfaces to put into the models, one had to know the degree to which a certain reduction in water content of the soil reduced the transpiration rate, for example. One had to know whether there were periods of sensitivity during the growth cycle of a particular crop when a period of water stress would have been disproportionately disadvantageous to the crop. Looking back on it, I think it did follow a fairly logical path.

*Plant/soil-water relations has never stopped being an interest of yours, has it?*

That's true, it's still there. And that really started a wonderful research career for me. I was able to build a group with two other people, initially, which grew to about 10 in the relatively short time of seven or eight years. The team consisted of plant physiologists, soil physicists and climatologists – people who were properly trained in climatology. Christian was marvellously supporting. He would give you all the responsibility you could take, so it was my team to build. I was responsible if it went wrong; equally, I got the plaudits if it went right, and I could carry on my own work.

*You could extend your own research qualifications, doing an MSc in that time.*

Yes, and a DSc degree. I regarded plant/water relations as the hard core, looking for the factors that affected water stress and how those factors mediated certain physiological responses – fruit-set, flowering, things like that – and I got more and more involved in setting plant/water relationships in a full environmental context in the soil/plant/atmosphere continuum. I guess it was reasonably pioneering at that stage to see the plant not in isolation but as part of a continuum.

We very quickly picked up the fact that the permanent wilting percentage, which had long been regarded as a soil constant, in fact wasn't. It was biologically determined by the osmotic potential of the plants. Therefore, while for many crop plants it occurred at much the same soil-water content, for native plants and woody evergreen perennials, in particular – of which Australia has many – it meant very little. The plants simply didn't know about it. It was great to unravel all that, with its obvious implications for irrigation practice, watering frequency and things of that sort.

*And to publish a paper of immense importance on plant/water relations.*

It seemed to be regarded that way, yes. Writing the monograph was a pleasure. That pulled it all together, looking at stomatal control of transpiration and linking the whole soil/plant/atmosphere continuum. It was necessary for the climatological models too. Unless you knew how those things were linked together, you couldn't really generate the response surfaces for the climatological models. Anyway, that ran through the 1950s and into the '60s.

## Gifts to a scientific 'adolescent'

I should mention that in 1955 I was the beneficiary of what in CSIRO we used to call a Cook's Tour. In those days, after you had been in CSIRO a few years and if they were impressed with what you were doing, you would get an around-the-world air ticket (with travelling allowances and so on) and you could just write your own itinerary. You could do whatever you wanted to do for 12 months: you could visit 365 different labs around the world, or one lab for 365 days, or some mixture in between. That was a truly marvellous experience which let me match my own thoughts against those of the best people in the world – and to my delight and somewhat to my surprise I found that in many areas I was up with their thinking or even slightly ahead. For someone three or four years out of an undergraduate course it was powerful, heady stuff.



*About to depart Australia for his overseas experience (aged 25).*

*You must have met some impressive figures in that time.*

Well, I did a mixture of things. I went to see a number of agricultural research stations in developing countries in Africa and the Middle East, often working not just with indigenous people but with expatriates – who, in many cases, because of all the postwar idealism, were prominent researchers in their own countries. You would be aware of the British influence in cotton research in Africa, for example.

I guess the most important person I was exposed to was Paul Kramer, at Duke University in the United States. His text on plant/soil-water relationships, published in 1949, had been a very important influence on me, and I had a list of 100 questions I wanted to ask him! When this 12-month Cook's Tour gave me the opportunity to go and sit at his feet for six months, he suggested that I ran an experiment to prove my hare-brained idea that the permanent wilting percentage wasn't very important. He also wanted me to demonstrate a few other things which I had by then published on and which had created a bit of interest. So I did all that. It was a fantastic six months.

Kramer was a delightful person. He was marvellous to work with, a very avuncular figure whose door was always open. He was 25 years older than I was and for the first few weeks we had a sort of parental relationship, but gradually it changed: we became good friends and he influenced me powerfully. He was another very curious person, one who always looked for the simple explanation rather than the more complex one, but didn't walk away from more complex ones when necessary. He was a member of the US National Academy of Sciences and he was still getting competitive NSF grants when he was 85. There can't be too many people doing that at such an age.

I feel I was very privileged right through my scientific 'adolescence', until I became myself a scientific adult, in being exposed to these people who were so generous with their time, their resources and so on, totally dedicated to the scientific principle and the scientific process in what they did, and enthusiasts for what they were doing.

### **Looking further at arid zones**

*At about that time, I think, you started to get some rapport with UNESCO, which quite soon used some of your models.*

My relationship with UNESCO started in 1956 and then developed quite strongly, yes. They started a major program in arid zones, which internationally meant not only deserts but also sub-humid (seasonally dry) environments such as in northern Australia. Just after the war there had been an attitude that somehow science would 'make the deserts bloom' as important places for rural production and for people to live in. Australians already knew that idea was wrong; we knew how fragile the arid zones were. But UNESCO's program, more than any other single activity, really put the idea to bed.

Through a consciousness-raising exercise of symposia and reviews of various areas of research related to the use of arid zones, the program drew in many scientific people from developing countries and put them in contact with outstanding researchers from developed countries – these included Kramer, and Australians too were prominent in the program – and did a whole lot of bridge-building. It was one of the very best things UNESCO ever did.

### **Evaluating the rural potential of the Northern Territory**

*Your energy level was pretty high when you came back and built your team at CSIRO.*

Everybody's was. It just went zoom. As a whole group we were very productive, publishing hundreds of papers over those years, and it very quickly became a place for people to come on their sabbaticals. Who wants to come

and spend some time with you is a very good test of how well your lab is doing – and we simply had to ration people's time, and space too. We couldn't pack them all in.

*You made some strong recommendations about agriculture in the north.*

Yes. Our mission in CSIRO was all to do with evaluating the rural potential of northern Australia, which we showed was a highly precarious environment for agriculture. It was characterised by an unpredictable start to the growing season (which is always very difficult for farmers to cope with) and the prospect in every year of one or more significant periods of intraseasonal water stress – which would not infrequently hit determinant crops such as maize or sorghum just when their yield would be most affected. So the focus had to be on managed pastures, not agriculture.

The effect of those physiological attributes was compounded by the fact that the soils mostly had very poor water-holding capacity – which meant they were exposed more frequently to water stress than would otherwise have been the case – and, by and large, they were lateritic red earths and consequently very erodible. A mixture of erodible soils and an erosive climate, as we have in northern Australia, is not a good one. So the climatology reinforced our view that it was a very difficult environment for agriculture. People did take up our recommendations, and very sensibly went into pastures.

Those same technologies then became used around the world. As that CSIRO work ran on through the 1960s, my interest scaled down from the world to the community to the plant, until I was getting quite involved in the cellular aspects of plant/water relationships.



*Practical Microclimatology by R O Slatyer and I C McIlroy – translated into Russian (1963).*

### **Moving to a university base, but always within talking distance**

Then in 1966 the Australian National University (ANU) started the Research School of Biological Sciences. The first appointments were filled in '67, and I

became the foundation Professor in Environmental Biology. It was called initially Environmental and Population Biology but we subsequently created another group in population biology. I now had the opportunity for rather more freedom than in CSIRO; I could be a little more starry-eyed about certain things and get involved in some areas that would not have been consistent with the broad CSIRO mission. But they were not too far away from it. I always regarded that sense of mission very seriously, and felt everybody should.

I took with me several people from CSIRO – not wanting to weaken that group but aware that we had done our job in evaluating the biophysical basis of plant production in northern Australia. (Also CSIRO was always ready to take the opportunity to reshape labs when a group leader leaves. Most good lab administrations do that, I think.) And eventually we were able to strengthen the ties between CSIRO and the ANU, which was marvellous. Two truly outstanding people who came with me were Barry Osmond, who is now the Director of the School, and Ian Cowan, who has just retired. The group also included Graham Farquhar, an outstanding student who later came back and is now an outstanding professor. There were too many others to mention, but most of them finished up with professorial chairs in this country and elsewhere.



*Celebrating election to the Australian Academy of Science with Dr Tom Chapman (later to be Professor of Civil Engineering at UNSW), 1967.*

*I think the  $C_4$  photosynthesis story is one that you got into in an important way.*

Indeed so. At that stage we were very interested in water use efficiency, and  $C_4$  plants differ from  $C_3$  plants in their transpiration and photosynthesis ratio. Also we had the opportunity to work through a number of facets of  $C_4$  metabolism that were relevant to our broad interests in the water/energy interaction in plants. We gave the research into that a flying start, and it reached a pretty high level of momentum. Barry and his immediate colleagues played a major role in that.

*And there were funds at ANU for you to get the resources you needed.*

Yes, there were. It was great, fantastic. I was able to build another group at ANU, and again we were flooded with visitors and exciting young students from everywhere. The lab became known around the world and we had quite a bit of throughput during those years. In getting these things going – but with

other people very much leading the charge – I aimed for a group of people you can sit through a seminar with, people you can make some contribution to and gain some benefit from. If you get beyond that, it is hard to maintain and you have to form another little group. I always tried to keep everyone within talking distance.

### **‘Ecology was my real love’: investigating ecosystems**

*The exciting thing was that after coming from the great northern areas of Australia and honing down over the years to the plant, you began to spread out again to look at ecological succession. You became a really refined ecologist.*

Ecology was my real love. The whole thing was ecological, really, whether it was crop plants or natural, native plants. By about 1973, numerically about half the group – mainly the more junior people, post-docs and research fellows – were working in some aspect of ecosystem structure or function. The challenge that I set the group was to work at a community or ecosystem level. It is relatively easy to work at the population level and relatively easy at a single plant ecophysiological level. But because of the incredible complexity associated with both temporal and spatial variation, it is very difficult to determine the factors affecting structure and function at a community level. I thought we should have a go at it.

*Were you influenced by the enormous pressure at that time to deal with world ecosystem problems?*

Yes, very strongly. In about 1968 the UN decided to hold a heads-of-governments conference on the human environment – the first major UN conference of that type – and that took place in Stockholm in 1972. (Since then they have had such conferences in a number of fields, including another one on the environment last year.) That focused the attention of international bodies, both UN and non-governmental, on the environment. This gave extra momentum to the work which followed UNESCO’s Biosphere Conference of 1968, leading to the establishment of their ‘Man and the Biosphere’ (MAB) program in 1971. I was heavily involved in that program – together with a number of other Australians, but probably as the only one to have been involved right from the start – and I continued in it until just a few years ago when the Chief Scientist job was too demanding to have another interest.

International awareness of the importance of the environment was developing rapidly, and clearly the science-based contribution that people like me could make was in sorting out the important elements of ecosystem structure and function, enabling ecosystem management to be well planned.

### **Ecological succession simplified**

*One thing you looked at was how ecosystems recovered or failed after disturbances.*

Yes. There is plenty of good raw material for that in Australia. In ecology, frequently you just don't have the time to run experiments. You have to look for either natural, or human induced, perturbations which have done your experiment for you, or have applied the treatments. Then the answer should be there and the challenge is to see if you can validate it. So disturbances associated with overgrazing or the effects of fire were very important perturbations by which to test the theories.

That was the challenge I set the group. I got pretty heavily involved in it myself, with Ian Noble – a remarkable, very impressive person who came from Adelaide to work with us. (He is now leader of the ecosystems group at the Research School of Biological Sciences. The Environmental Biology group did get to be beyond talking distance and so two separate groups were established, but still with a lot of interactions between them). In the mid-1970s Ian and I began to develop what you might call a phenological basis for understanding whether a plant is likely to persist through a disturbance or to arrive at the site of the disturbance afterwards. And then whether it can reach reproductive age and produce viable propagulus. In other words, whether it is going to be in with a chance in the subsequent series of events that lead to the redevelopment of a community.

Also, I had established a close working relationship with Joe Connell, at Santa Barbara. In about 1973 or '74 I began working with Joe on major mechanisms involved in ecological succession, the temporal change in species composition. The work was intermittent, with ideas, manuscripts and so on going backwards and forwards across the Pacific – plus periods of time in each other's labs. We didn't even have faxes then and telephone calls were too expensive, and neither of us were terribly good correspondents, nevertheless we gradually got our ideas together.

We proposed that the complex array of mechanisms that were involved in ecological succession could be seen as just three basic mechanisms: a community acts in such a way as to facilitate the entry of a new species – and there is evidence for that – or to inhibit the entry of another, new species, or to tolerate the entry of a new species. That seemed to sum up, to a considerable degree, the controlling mechanisms for the temporal variation in composition. It took us two years to put together a paper on this proposition, together with the experimental tests that we thought people could apply in assessing whether these mechanisms were operational or not. In about 1975 we sent the paper to *American Naturalist*, which was rather slow in refereeing it but published it in 1977.

*Your proposition simplified a great problem area. People could now devote their attention to three mechanisms, instead of looking at a massive*

*multifactorial array.*

Well, it seems almost self-evident now, but it didn't seem so simple or obvious when we were involved in it.

### **A total commitment to cooperative research**

As you have probably gathered, I am totally committed to cooperative research. I know that a lot of people like to work away almost monkishly by themselves, but I've been so stimulated by interaction with colleagues through my research career that I feel quite unhappy about people depriving themselves of that opportunity and that great experience.

The paper that Joe Connell and I wrote became a citation classic, as did my plant/water relations monograph – that's always a bit of a thrill. When you get a citation classic you are asked to write a little essay as to how it happened. As Joe and I thought about our work, I realised that the successful ingredient was the interaction of our backgrounds. Joe is a great conventional naturalist who knows the names of all the shells on the seashore and stuff like that, and he brought a broad naturalist, community ecologist view of the world to our association. I'm the world's worst taxonomist, but I brought a more physically rigorous background. I am more mathematically inclined, more inclined to look at the physiological mechanisms underlying behaviour or performance rather than simply the manifestation of the performance itself. It was perfect that we should have worked together, although when we started we didn't realise that; it just became apparent to us that we always threw sparks off one another.

Incidentally, Ian Noble and I do the same even now, because we have different backgrounds. He comes from a community background interest; I come more from the specific physiological interest.

### **The World Heritage concept**

*The 1970s were exciting years for you, in a range of ways. You have mentioned being heavily involved in the Man and the Biosphere program, which had arisen from the 1968 UNESCO conference, and there was a range of other international activities.*

Yes. I was on the MAB executive bureau, in one role or another, from the very first meeting in 1971 through to '85. The bureau was an international coordinating council that ran the program. In the early days it was composed almost entirely of scientists: representatives of governments, but elected because of their scientific attributes, not because of the country they came from. (Although the US had a permanent seat, as it were, as did the Soviet Union, but they had good scientists involved.) For the period 1977-79 I was actually president of the program, and I was re-elected president for a second



term 1979-81. And I was then past president for a period.

*The World Heritage story was to have important repercussions in this country. Your involvement in it overlapped with your MAB work, I suppose.*

It did. The World Heritage Convention started in the mid-1970s. The concept was that in biblical times we had the Seven Wonders of the World, but if we were starting again, maybe not all of those seven would feature and anyway there would probably be more than seven. So the World Heritage List is a modern-day list of the wonders of the world; the task was to identify them and try to ensure their protection. And on the World Heritage List we could have not just cultural properties but natural properties as well, to ensure their protection also.

As with Man and the Biosphere, Australians were active from the early days of the World Heritage Convention. I was involved virtually from the start, and served on their executive bureau continuously for a number of years. I became president of the World Heritage Committee in 1981 and served for about three years – about five meetings. For what it's worth, former prime minister Gough Whitlam tells me that I have chaired more meetings of the World Heritage Committee than any other person!

Australians nominated various areas for the list but it has been a controversial process in this country. There has been much political posturing and a lot of extravagant comments have been made about giving up national sovereignty and so on. Frankly, such comments are ridiculous – but that's politics. Some of the nominations could have been expected to be controversial, but they have been unnecessarily so. The three most controversial have been south-west Tasmania, Kakadu and the Wet Tropics of Queensland.

My feeling has been that it is important that the state in which the property is located should want the property to be nominated by the Commonwealth government. It shouldn't be done over the resistance of the state. The group of properties which went through the nomination process at the start had been proposed by the states in response to an invitation from the Commonwealth. After a process of evaluation, some of those proposals (including the Great Barrier Reef) went forward and they were all successful. But south-west Tasmania, for example, was proposed by a state government which subsequently fell. The incoming government opposed the nomination but it had already been made and went ahead. That sort of thing has certainly made for difficulties in Australia.

As chairman of the international body I wasn't involved in the national politics of it. I had to stand off a bit. Instead, I tried to advise both the federal and state governments as to how to find some middle ground.

I like to think that in all those international programs Australians have not just

been active but have taken part in ensuring quality control, getting the terms of reference and selection criteria right – the rules of procedure, all those sorts of things that are very important for maintaining quality. With something like the World Heritage List, whenever you add a property that is slightly below the mark you raise the prospect of the addition of another whole group slightly below the mark. You must always maintain the standards at a very high level. It was very satisfying to do that work. It's certainly important for the benefit of future generations.

### **International scientific cooperation for the environment**

*You were also heavily involved in SCOPE. What is that, and when did it start?*

It is the Scientific Committee on Problems of the Environment, the environment program of the International Council of Scientific Unions (ICSU) which started in 1970. I was president of SCOPE from 1982 to '85.

For the record: the SCOPE program and the Man and the Biosphere program should have worked more closely together. I did everything I could to make that happen, but not all of my colleagues in SCOPE wanted it. A number of people, for a variety of reasons, wanted to be international and non-governmental, rather than getting involved in intergovernmental activities. I was a little disappointed it wasn't possible to build closer links there – the programs had different objectives, sure, but a lot of common ground.

When it didn't seem to be working, I just tried to encourage SCOPE to work on things that were seen not to duplicate MAB programs. This was needed in a role which involved preparing state-of-knowledge reports rather than with getting things done 'out there'. SCOPE still does that today.

The SCOPE involvement was very enjoyable, although I did drop the ball a bit in those years because I was also chairman of ASTEC (the Australian Science and Technology Council, which we can come to later). Although that chairmanship was only part-time, on top of my full-time job at the university it was sufficiently demanding that I don't feel I really did quite as much for SCOPE as I should have.

At the 1982 SCOPE general assembly, at which I was elected president, a motion from the floor was accepted that the incoming executive should look at the possibilities of doing a project on the environmental consequences of nuclear war. I was delighted with how we, as an executive, got around to doing that extremely important project on such a sensitive, controversial issue.

At our first meeting after the election, I went around the table and had everybody say how comfortable or uncomfortable they felt about the idea of SCOPE taking this on. I said it could mean the end of SCOPE if it went bad and led to international rivalries – it could even have led to some countries

withdrawing from ICSU, for example. Equally, there was no more important topic that could be considered. I guess the Soviet member of the executive, at least, had received some government briefing (perhaps they all had briefings from their governments) but everyone agreed we should do it. We set up a special task force under Sir Frederick Warner as chairman. It was such a big project that I had to go away and get the money: we raised about half a million dollars for it altogether, largely from various foundations. Ned Warner's chairmanship was marvellous and the task force ran as a self-contained entity within the SCOPE program.

*You published a classical report on the nuclear war consequences.*

It was the first international group to succeed in getting US and Soviet scientists working together. Those of us involved in SCOPE believe, I think, that it was a fairly important factor in a number of the developments associated with nuclear arms production. More than any other single report, it made it quite clear that nuclear war just could not be risked, and that there was massive overkill in the nuclear arsenals around the world. We launched the report in 1985 at a meeting in the National Academy building, in Washington – so we had all the right credentials for the job. The launch was very well attended by the US media, which helped considerably in getting the message around the world. It was extremely satisfying to be a part of that.

*You must have put a lot of your energies, enthusiasm and diplomacy into it.*

Mostly in trying to orchestrate it so it worked well. As I have said, Ned Warner really ran the project as a stand-alone exercise. I found my SCOPE association, which continued until about two years ago, very good indeed.

### **Using multilateral politics to benefit the world's environment**

*Something else happened in the 1970s. You had your big department, with a good team doing a lot of work. Then suddenly, it seems, you went off to Paris.*

That was quite a remarkable event. I had been involved with UNESCO right from the start of its environmental activities in the major project on arid lands, and also I served on the Australian National Commission for UNESCO, becoming chairman of that in 1976.

In 1978 the Prime Minister, Mr Fraser, appointed a previous governor-general, Sir John Kerr, to be Australian ambassador at UNESCO. The media arose with an enormous clamour that this was a payoff for Kerr's earlier sacking of Gough Whitlam as prime minister. There was such great controversy and deep feeling about Kerr in the Australian electorate that he had to live in the UK instead of Australia. Kerr did accept the post, but it became impossible for him to take it up, so someone else had to be found for the job. After all, Fraser had said when making the Kerr appointment how important UNESCO was.

To my utter amazement, the phone rang one day and I was asked if I would take the job of Australian ambassador at UNESCO. Well, looking back afterwards, it does make some sense. There I was, chairman of the Australian National Commission for UNESCO, having been involved with the organisation since the mid-1950s, involved in its science, educational and cultural programs, and with the sort of scientific credentials that I had accumulated since then. But at the time it just came out of the blue.

My response was to knock back the approach, saying that I had never considered doing anything like that and I couldn't see that I would, thank you very much. When I went home for lunch I told my wife about it, 'Hey, you'll never guess what happened this morning!' She said, 'If they ask you again, just talk to me about it, will you?' Anyway, a week later the phone rang again and I was approached a second time. This time I said I would need to think about it more seriously. That was the day before Good Friday, they gave me four days to think about it – and we took it.



*On appointment as Australian Ambassador to UNESCO (1978).*

That was a marvellous experience, something totally different, being in a world of multilateral politics. In the UN and the OECD, at least, that is a matter of looking for situations in which everybody wins, and so the whole process greatly interested me.

Most of the ambassadors there were career diplomats, for whom UNESCO is merely another multilateral posting. By and large they know nothing about its programs, but if you are going to be influential at UNESCO you have got to get the politics and the programs coordinated. While I had enough self-confidence that I didn't go there with any trepidation, I was conscious of being a babe in the woods politically. I had excellent support from the Department of Foreign Affairs and my own small team, but nevertheless at the end of the day the ambassador has got to front up, and do and say various things. My program knowledge became adequate quite quickly, not just in areas like science and education but right through the UNESCO program, and I

realised that consequently I had something to offer to my fellow ambassadors. It is always nice, in any sort of interaction, when you can say, 'Yes, what you've said is all very well, but how about these elements?' – things they would not have thought of.

*So you enjoyed being a member of a team, and you made professional friendships.*

Oh yes. Several of those ambassadors from other countries have since spent terms in Australia. We were very close friends with the German ambassador at UNESCO, Boff Fabricius, who subsequently came to Australia as ambassador for Germany. There were some very pleasant interactions like that.

Most importantly, as far as my career was concerned, it made me far more aware of the world as a whole, of the enormous challenges to getting things right internationally, and of the sheer importance of doing that in the interests of world peace and prosperity. I think if you are associated with UNESCO you become convinced that, for all its warts, it is an important organisation with a vital role to play in world peace – and in the meeting of people's minds across various cultural and other barriers. I became caught up in all of that.

### **Coming home to a transformed role**

*Ralph, I think you were asked to accept a second term as UNESCO ambassador, but you refused and came back to ANU to take up your professorship again.*

Yes, I returned in 1982. I had become conscious that I was dropping away from science, that if I stayed another three years I would have become another diplomat and not a scientist. So it was important for me to get back. Also, I think June was pretty keen to get back, and obviously that was a factor. I hadn't realised, however, that when I got back I would be through with science in terms of a personal research career. (One has these rosy expectations, I suppose.)

My work at UNESCO had the interesting effect of making me 'trivialise' science to some degree, because I saw so clearly that science was just one element in the future of humanity – very important, but just one among many other important factors. I came back conscious that really I didn't want to put all my resources back at the bench, as it were. In fact, in my case the bench was now the field. So while I kept ecological work going for a few years after my return, I suppose it was no longer my main driving force.

In 1984 I became director of the Research School of Biological Sciences, even though I had resisted doing it some years earlier, because by then it was much easier to be a senior administrator and involved in other things than to be at the bench and involved in other things. My feeling is that unless you are obsessed

about your science, it is very difficult for you to be a first-rate scientist. It has to be a whole commitment, essentially. But by now I was looking for broader responsibilities and a broader role to play.

*Indeed, by this time you had become involved in a great many national and international initiatives, an invaluable continuation of your interest in environment.*

Yes, they've all been linked to that. So becoming director of the School made a lot of sense. The other major commitment I had then was the chairmanship of ASTEC, to which Malcolm Fraser had appointed me in 1982.

### **Reviewing and guiding science and technology in Australia**

*Tell me about ASTEC. I think it was looking at where science and technology in Australia were going.*

ASTEC was a very interesting organisation which Prime Minister Fraser established in 1978 because he wanted a think tank on science, and the chairman of ASTEC had direct access to him – which was essential for effectiveness. If a body like that reports to anyone but the prime minister, it can't be effective. At that time government-sponsored think tanks were being appointed around the world to do various things but now, in the '90s, those days are past: a government goes to one source for this advice, to another person for that advice, and to someone else for some other advice. It's much more selective. In the United States, very dynamic think tanks operate independently with their own funds, but frankly I don't think that is realistic here. So a body like ASTEC may have outlived much of its usefulness. But at that stage it was very much the way governments operated. Both Fraser and then Hawke, in his early years, took ASTEC's views seriously.

As an example, during the time I was chairman of ASTEC the government introduced the 150 per cent tax concession for research and development, a major fillip to industrial R&D in Australia. ASTEC was instrumental in that.

Also, during my chairmanship I was responsible for a review of CSIRO that set future directions for the organisation. We pushed quite strongly for a sense of mission and of industry involvement. I think CSIRO at its best has always been characterised by a very strong sense of mission, but in the '70s and early '80s, in my opinion, it lost track of that a little bit, drifting into becoming more and more like a university and tending not to do such good work. ASTEC's review of CSIRO set directions for the organisation and, I believe, put it back on the right path. Virtually all ASTEC's recommendations were accepted.

In addition, ASTEC laid the groundwork for the establishment of the Australian Research Council as a truly national research foundation, absorbing several other granting programs and with the Australian Research Grants

Committee as its core. All those matters were ASTEC initiatives and one can take some satisfaction from that. But nowadays one doesn't find that sort of notice being taken of ASTEC.

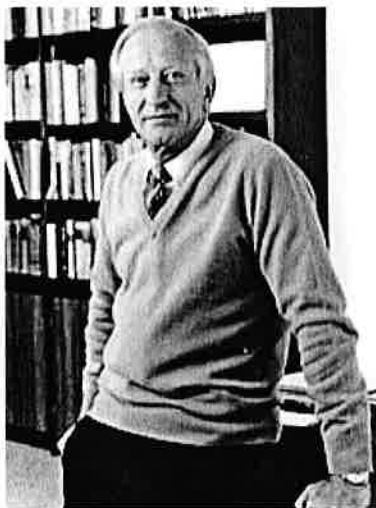
*You were still building a diplomatic brief in the '80s, weren't you?*

Well, my involvement with SCOPE and MAB was interesting, as was the World Heritage Committee – a labour of love, in that I see the World Heritage Convention as extremely important. And then the chairmanship of ASTEC was extremely interesting as part of a post-UNESCO ambassador role, when my interests broadened very much from what they had been. Anyway, all those activities have been great fun.

### **The emergence of the chief scientist**

*Your role as a scientific adviser to government then grew even more, didn't it?*

I finished my chairmanship of ASTEC at the end of 1987 and went back to the ANU, where I was still director of the Research School of Biological Sciences. In 1989, however, the government established new advisory machinery. The chairman of ASTEC was in a sense the prime minister's science adviser, even though the position was just a part-time one. Mr Hawke wanted to make that into a full-time post, to have an adviser 'on tap', and also to create a Prime Minister's Science Council and to establish a Coordination Committee on Science and Technology comprising officials at a very senior level. I was approached to fill the full-time position.



*Director, Research School of Biological Sciences, ANU (1989).*

This time the phone call didn't come altogether out of the blue. It was from Mike Codd, the head of the Prime Minister's Department. He and his predecessor, Geoffrey Yeend, were two outstanding public servants, people of total integrity and broad commitment to all facets of Australian life and society – delightful people to work with. People like that give you a tremendous sense of satisfaction and support when you are working in a job like the

chairmanship of ASTEC.

When Mike raised the possibility of the job as chief scientist I hesitated, saying that I had already been the chairman of ASTEC and I thought the new chairman ought to continue working the way I had. He said, 'No, this is going to be quite different. ASTEC is outside the bureaucracy; this is inside. The person in this job will have access to all the Cabinet papers and will be expected to be across all of them.' He said the new council would be very influential, with the prime minister and six other senior ministers involved. Also, there had been a great need for a coordination committee to bring the various bureaucratic elements together, avoid overlap and so on. 'It really is a challenge,' he said. 'Why don't you do it for three years?'

I was in Korea when I had that phone call – on 16 April, my birthday – and so I thought about it there. June said she could tell I was going to take the job, because on giving up the chairmanship of ASTEC 18 months earlier I had been left with so much energy but I hadn't quite expended it on other things. I did take the job, and it was great fun. And I really think we started something very important with the Council.

I was able to influence the character of everything that happened: the composition of the council, the working arrangements with the prime minister, the way in which the coordination committee worked. The prime minister himself chaired the Prime Minister's Science Council – now called the Science, Engineering and Innovation Council – and I chaired the coordination committee, so there was an almost seamless flow of information both ways.

It is early days, of course. How influential these bodies will be and whether they will last indefinitely is going to depend very much on the prime minister of the day and the person who is chief scientist. I am convinced – and I think everybody agrees – that we have put the necessary mechanisms in place to help science and technology, development and the environment, and it is really a matter of whether those things continue to appear valid and worthwhile.

*So it was a continuation of everything you were about?*

Yes. Being in a position to largely control the agenda of the Science Council meetings, through talking with Mr Hawke and subsequently Mr Keating, meant again that one was able to influence a very broad agenda. That has been good, and the relationships with both prime ministers have been good. They have both been very interested in science and very conscious of the importance of technological innovation in Australian industry sectors. I don't think one could have wanted better support at that level.

### **Truly cooperative ventures: the CRC program**

*Would you like to say something about the Cooperative Research Centres*



*program? That would be your biggest project since the establishment of the Science Council.*

I suppose I can't afford to be too immodest as I was largely responsible for it. I think people would agree it is the most significant development we have had in Australia since 1965 when the Australian Research Grants Committee was first established.

The seed was sown at the first meeting of the Science Council, when – not altogether unplanned – we had a parade of well-known scientists telling the prime minister and senior ministers that even in research areas where Australians had been leaders we were having trouble keeping up with the pace internationally, particularly in areas where you needed multifaceted teams to make progress. Individual research support was not too bad, and things were happening for the support of smallish teams, but there was no mechanism to build large multidisciplinary teams on the required scale.

Furthermore, during the 1960s, as both CSIRO and the universities grew rapidly, CSIRO labs that had been on university campuses moved off campus and that very important nexus – with the benefit of putting together researchers and students – was broken. Fortunately, in many cases the CSIRO labs didn't move very far, in effect only across the street; nevertheless, CSIRO and university people were no longer working in each other's labs. That affected both the research and the research training.

Even more generally, there is the reality that our Australian research enterprise is inevitably fragmented geographically because of the distribution of Australian centres of population, and also fragmented institutionally because of the separation of CSIRO and other government facilities from campuses. We have both arms tied behind our backs, in a sense, but because the institutional measures are not inevitable there was clearly a chance to do something about that aspect.

All of these ingredients added up to a case to the prime minister to create new centres that would be truly cooperative ventures – every one of them, I suggested, should have a university component because of the importance of the teaching element; and, wherever possible, researchers in centres should be together at the one location on a university campus, to recognise the universities' role as intellectual powerhouses in the country. In addition, we would try and link groups with common interests, even though they were geographically separated. So we were running a gigantic experiment in distance cooperation research as well. If ever a country needed to be very good at distance cooperation research – not just internally but internationally – it is Australia.

In the second part of 1989 I made sure I was up to speed with cooperative research ventures in the UK and other countries in Europe, the US and Canada

because everybody has tried this in different ways and some have worked better than others. Then in about November '89 I put a proposal to the prime minister, and to my absolute delight he smiled on it. Although ultimately it got caught up in the 1990 election, believe it or not we had it all signed, sealed and approved before that. But just when it was to go to Cabinet for formal approval the opposition parties did something or other which was politically rather inept and the prime minister decided the time was propitious to call an election, so the whole thing got into the election context. In the event, the government won the election and the opposition gave the program bilateral support, despite having initially opposed it. So now all 50 centres are up and running. It was an enormous effort.

*Has the funding been designed to facilitate the objectives?*

Yes, it has. This is a very big project – \$100 million a year. We wanted also to get matching funds from the participants, who would put up at least a dollar in either cash or in kind for each dollar the government put up. We in no sense regarded the contributions in kind as inferior; it is the contribution in kind – the people – that can bring with it a real commitment. Also, we wanted close links to be built with the user groups, whether they were government agencies, businesses or whatever, although I was a little apprehensive as to how successful we would be in building links with some of the businesses in Australian industry sectors that are not strong performers of R&D. (Some are very good performers of R&D, but most are not.) In fact, we have been overwhelmed with the response. Rather than one dollar of matching funds being contributed by the participants for every dollar of Commonwealth government funding, we have been getting an average of two dollars. And I am pleased to say a lot of experiments are under way, to do with better cooperation technologies.

*On that point of achievement, which in itself illustrates why you have found so much enjoyment in your career, we must close this interview. It has been marvellous to talk with you and it would be good to talk again, perhaps when you next visit Oxford.*

Okay, let's do that. It would be nice to talk about some broad global issues.

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