



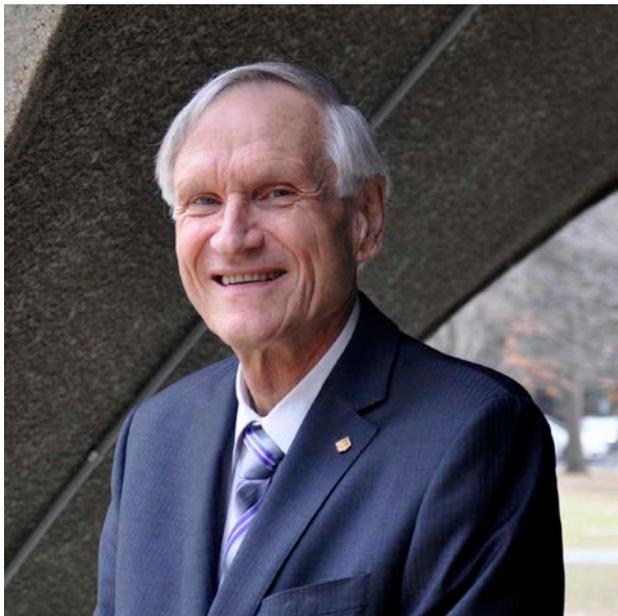
CSIRO Oral History Collection

Edited transcript of interview with Michael Barber

Date of interview: 27th October 2017

Location: Swinburne University of Technology (Melbourne, Victoria)

Interviewers: Tom Spurling and Terry Healy



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Emeritus Professor Michael Newton Barber AO, BSc (Hons) (UNSW), PhD (Cornell), FAA, FTSE, FAICD

Summary of interview

Michael Barber was born in Sydney on 30 April 1947. In the early part of the interview, Michael talks about his experiences growing up in an academic household in Sydney and Hobart. He is a second generation university educated Australian. He recalls some of the visitors to his parent's home, including Dr Jerry Price, a future Chairman of CSIRO. He also talks about his early interest in science and his mention in a 1965 'Heredity' paper by his father on selection in natural populations.

Michael attended Clarence High School in Hobart where the School Principal, Ed Smith, was also his science teacher. Michael remembers the long-term value of Mr Smith's insistence good English expression in science essays.

There ensues a detailed discussion about Michael's mathematical education, including the influences of his key mentors. Michael describes his experiences as an academic at UNSW and the ANU, the effect of the Dawkins' reforms and his growing interest in leadership roles. He talks about his recruitment to the University of Western Australia, and his contribution to the development of technology transfer policies there.

Michael then relates his experiences as Executive Director, Science Planning and then Group Executive, Information, Manufacturing and Minerals in CSIRO. This includes his reflections on the role of CSIRO in modern Australia and the relation between CSIRO and the tertiary sector.

In the final part of the interview, Michael talks about his time as Vice-Chancellor of Flinders University. He notes how the variety of his previous experiences contributed to his successes in that role.

NOTE TO READER

Readers of this interview transcript should bear in mind that some editing of the transcript, including additional material in the form of footnotes and endnotes, may have occurred at the request of the person interviewed.

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Introduction and early life in an academic family

This is an oral history session with Professor Michael Barber, and present in the room are Terry Healy, who is going to be recording it and also asking a few questions, and Tom Spurling, who's going to be the main interviewer. It's Friday the 27th of October 2017, and the recording is taking place at Swinburne University of Technology. So, thank you very much for agreeing to do this, Michael. You've had such an interesting career, that we're very interested to talk to you about today. We are going to talk a bit about your early life and experiences, what you did before you came to CSIRO, what you did at CSIRO, your views on the evolving role of CSIRO in Australia, and a bit about your career post-CSIRO, but in some ways, that's probably best left to historians of Flinders University, rather than us.

Let's start at the beginning. Maybe you could tell us about your early life in – presumably, the first few months in Sydney, and then in Hobart, and your very interesting parents.

Thank you Tom. It's actually quite a pleasure to reflect back. I think all of us who get towards the end of our career can become a little reflective. And I have to admit that I look back and can see a trajectory in my own career, but also one that I'd have to say was never really planned. However, certainly, the seeds of where I went as a scientist were planted, I think, probably in the womb of my mother. As you said, I had an interesting pair of parents. I am a second-generation university educated Australian. For someone of my age, that's a fairly unique perspective.

My father, to take that side of the family, was an Englishman originally. He was educated in the United Kingdom. He did a first degree at Cambridge, and then a PhD at the John Innes. , The John Innes is actually the first connection of the Barbers to CSIRO, because in London, he shared a flat with a future chief executive of CSIRO, and I know –

Yes.

– a colleague of yours, in Jerry Price.

Yes. *And they were both at the John Innes?*

I'm not sure Jerry was at the John Innes but they were certainly in London together. I will come back to the Prices a little later in the story. My father spent the war years – he was a geneticist –conscripted to fix radar sets, and he always maintained that the big burst in biology after the war from crystallography, came from the fact that biologists sat around North Africa, or the Far East, with physicists, and there was nothing to do at night after you'd fixed the radar sets, so you talked science. And so the collaboration of physics and biology that occurred after the war, he felt could be attributed to those days.

Anyway, he was demobilised back into what I gathered was a very depressing London of 1945, and there was a job advertised in Sydney in Botany. He always maintained he asked one question, "Did the sun shine?"

So, the John Innes was before the war?

Yes, the John Innes was before the war. His PhD, I think, was '41 and '42 but after that, they put PhDs to work in radar. Rather more civilised than in 1914, when the best use for scientists, like e.g. Moseley, was as fodder for the frontlines. Anyway, my father arrives in Australia to take up an appointment at the University of Sydney and meets my mother who was, again, university educated. Perhaps even more surprising for a woman in the '30s, she had done a first degree at Sydney, and had then a Master degree. During the war years, she ran –

At Sydney?

– the botany program at New England, then a college of the University of Sydney.

Masters degree at Sydney?

A: At Sydney, yes. She was educated at Sydney. Of Irish stock. Her parents had both come to Australia as children. So, on one side, I'm Irish. On the other side, English. My mother came back to Sydney in '45 to run the program for returned servicemen and, in early 1946, met my father. They got married, and she immediately was fired from the University of Sydney, because in those days, a married couple couldn't both work there, and there was never any discussion about which one went. Soon after, I was born in 1947 in Sydney; we went to Hobart where my father was appointed Foundation Professor of Botany at the University of Tasmania.

He was very young at the time.

He would have been – he was born in '14—so he would have been 32 or 33.

Digressing a little, but not unrelated to my scientific upbringing, he was the first modern trained geneticist ever employed in Australia. He was followed at Sydney by 'Spinny' Smith-White, and perhaps even more significantly for Australian science, the person who replaced him at the John Innes, when he left for Sydney, was a man by the name of David Catcheside, who went, ultimately, to Adelaide as the Professor of Genetics and then the ANU as Director of the Research School of Biological Sciences. So, I grew up in this small Australian biological community in a household that was very much about science.

It was also a household that was more cosmopolitan than the average one. My parents drank wine, which always amazed my friends when they came around. No-one else in Tasmania drank much wine in 1950s.

Cascade beer?

They drank Cascade beer if anything else.

Like, why did you need wine?

Possibly, true! But perhaps more significantly for me, it was a period in which my father was looking at the Australian environment— he’s probably best known for the work on the evolution of eucalypts—with new eyes. Indeed, I think he saw Tasmania a little like the way Darwin had seen the Galapagos, as a chance to understand evolution by looking at biotas and ecosystems which no one else had actually studied particularly with modern genetics. So, I spent most of my summers in the field around Tasmania. In those days, Launceston to Hobart was a day trip and we only very rarely until 1961 went to the Mainland. Thus, some of my early science was done helping him.

You have in the papers a recollection I gave at the University of Western Australia about those early days of genetics, and I only mention one point: the first time my name appears in paper, is in a footnote paper that my father wrote in *Heredity* in, probably, ‘64/’65, which drew a lot of his thinking together. My contribution was the outcome of a project, for which I won five pound in a science talent quest, that involved running a transect¹ over a dolerite hill in Western Tasmania, where the eucalypts evolve as the temperature decreases as you climb the hill. So, I went to school in Tasmania. Did quite well. Science obviously.

Clarence High School and Normanhurst Boys High School

So, you went to Clarence High School?

Yes, I went to Clarence High School. Like many of us, particularly at that time, I think teachers do stand out. I had a science teacher who was, in fact, the principal of Clarence High – Ed Smith. I don’t remember much science he taught me, but one of the things he made us do was write paragraphs. Each week, he would tell us to write a paragraph on something, and I still remember his distaste for a response, “is when”. For example, if you wrote a – as I can remember once—a paragraph on an eclipse, and you started with, “An eclipse is when”, you got this big, red cross. So, he marked us on English, and I think that, for me, was very important, because I really couldn’t bring myself to be creative in English literature; I hated writing essays on what I had done over the summer et cetera, et cetera.

But in science, there was something interesting to write about, and Smith forced us to do that. Many, many years later, I heard from him when I gave a Telstra address. He was then well retired, but I got this wonderful note from him saying he remembered me, that he always watched the Telstra address, and he was very pleased to see me.

So, what about mathematics?

Oh, mathematics at high school was easy. I don’t actually remember much mathematics from Tasmanian school. I just did it. I do remember a teacher at Normanhurst Boys High School where I did the Leaving Certificate in 1964. His name was Maurie Adamsthwaite. He took the honours maths class, and he must have seen something in me, because, in

¹ H N Barber Selection in Natural Populations *Heredity* **20** 551-572 (1965) “I wish to thank Mr Michael Barber for help in, making the transect illustrated in Fig. 1”

hindsight, he gave me an awful lot of first university year work to do. I didn't realise it at the time, but he obviously pushed me, and I did learn a lot of mathematics from him.

So you finished secondary school in Sydney?

Yes, I finished secondary school in Sydney. My father had taken the Foundation Chair of Botany at New South Wales.

The Orr Case and recollections from Hobart

Could you just go back to Tasmania for a short time? So, you were there, or your father was there, during the Orr case. Do you have any recollections?

An interesting time. And we talked about it many years later. Just before he died, he came through the US when I was a PhD student there. This was the 1960s, and I, sort of, said, "You know, I don't understand the Orr case? Sexual relationships between academic staff and students are probably not the right thing to do, but even then, not a firing offence." My father, sort of, said, "Well, it was never really about that."

So, we had quite a long discussion. And I've done a little reading. My actual recollections of the time—this was, what, 1956/7, so I was nine or ten—are limited. We used to come home from school, and the first question we'd ask my mother was, "Is it Prof Board today?", and if the answer was, "Yes", you knew it probably would be a good idea to keep out of the way of my father until he had a few glasses of wine, complained to his wife and unwound.

There's a book been written since then by.

Cassandra Pybus.

Yes, Cassandra Pybus. And for the most part, I think her record is quite a fair one. My father, in 1969, when I last saw him and we talked about it, maintained his view that the University was correct. Interestingly, he said, "He never sued me. He sued everyone else." And I said, "Oh", and he said, "Well, that might have been because I'm six foot six, and he was a little runt!" But he also was very adamant, which is where Pybus ends up, that the big crime of the University of Tasmania was appointing him in the first place, and that getting rid of him was the best outcome for the university. He was very much involved in that as Chair of the Professorial Board. But, as he said, he was never sued— and Orr sued almost everyone else around the place.

The other part of the Orr story, that I certainly only appreciated much later but which Pybus emphasises was the socio-political even geo-political state in Tasmania of the 1950s. It was a very small community with outsiders suspect and various complexities, even conspiracies, all around the place. However I also have to come to the conclusion that I think Cassandra Pybus and my father were probably right. I mean, how the university ever employed him in the first place is an interesting failure of governance.

Yes, that's so.

So, clearly my father was a figure in that and in other issues in that period that were typical of a small, isolated community with the University a very important part. But at the same time, he did a hell of a lot of science

Yes. And his student, W D Jackson, succeeded him as the Professor of Botany, and he was still the Professor of Botany when I went there in 1967. And it was a good botany department.

Yes. It was. In addition to the eucalypt genetics, Ian Murfet and peas, Geoff Sharman and potoroos made significant contributions. When I was on the Australian Research Council and chaired the Research Fellowship Committee—it must have been around 2000—I remember an applicant who wanted an ARC fellowship to go to the University of Tasmania to work with the then current professor, Jim Reid, who was one of Bill Jackson's students. The reason he wanted to go there was a eucalypt plantation that had been established by H N Barber, and he wanted to be part of the long-term trial that had started in the early 1950s.

Sorry, can I just go back to the Jerry Price connection? The University of Tasmania did have quite a good natural products program with John Polya and Ralph Bick, and they were closely connected with the CSIRO phytochemical survey. Do you remember any involvement of your father, as a botanist, in that enterprise?

A little bit, Tom. I've often wondered how deep the scientific collaboration between the two was. They did write a Nature paper together but that was in 1940 while they were both in London.. In Tasmania, the Bicks were certainly close family friends with two boys about my age. While my main recollections of the Prices are more from 1960 onwards, when we travelled quite often to the mainland, and would invariably stay with the Prices. Nevertheless, Jerry was, through the '50s and '60s, a very frequent visitor to our house. In fact, it was Uncle Jerry and Aunt Joyce to us, and conversely the Price kids referred to my parents as uncles and aunts.

So, I think that a scientific collaboration, which had started out of a friendship before the war, could easily have about. And, of course, later, some of my father's work at – towards the end of the time in Tasmania, and then at New South Wales—concerned the biochemistry of eucalypt waxes and the related genetics.

When you describe your family, you say "us". You had a sister?

I have a sister. She was two years younger than me, born in –

Born in Tasmania?

– born in Tasmania. Educated in Tasmania. She's still alive and retired. She lives out near Coonabarabran.

Was she a scientist?

No. She went through school, became a physiotherapist, wandered the world, somewhere met up with a real bush cocky, Wade Butler, who she married. Then very tragically, Wade, who was an experienced bushwalker, disappeared in the wilderness in Tasmania, probably 25 years ago. Left her with four small kids. So, bit of a tragedy on that side of the family.

Just before we leave Tasmania, you say that you came – you were in a, sort of, cosmopolitan household. My recollection of going to Tasmania was that the chemistry department that I went to was quite an interesting department, because it had John Polya and Ralph Bick. They were quite men of the world, and I was very pleasantly surprised by the sort of conversations that you had with people like them— particularly John Polya, who was a polymath, really.

I think that applied more widely across the University and back earlier. My parents were avid entertainers. My mother put on buffets and various people from right across the university – Gordon Newstead, an engineer, Polya, Bick and various other people – even from the humanities came. This was particularly post the Orr case, when the university had a black ban on filling the Chair of Philosophy, which my father always maintained was very positive, because they then had a visiting professorship, to which they invited all these very distinguished philosophers to come to Tasmania. And so, I can remember from early teenage a very eclectic mix of visitors to our home.

What was interesting was that my friends at school and the neighbourhood in which we lived was a different sort of world. But the university community, I think for Tasmania, at that time, was a small, but open, and very cosmopolitan society. The university certainly attracted a number of very well-regarded people to its faculty, and they laid the foundations for the university.

And one of those people that I got to know very well in Tasmania was Peter Boyce, who eventually came back to be the Vice Chancellor of Murdoch, and he.

I was going to say, there was a Boyce at Murdoch, yes.

Yes. And he was the person – he was in political science there--who told me much of what I know about the Orr, so, yes. Okay.

Another interesting aspect, coming back to the CSIRO story, was that Tasmania was a small research community as well. And again, in ways that were perhaps a little ahead of his time, my father reached out to the few CSIRO people. From memory, there was a close CSIRO person – Martin may have been his name—who was very often a visitor at our house. Similarly, the Forestry Commission. So, it is likely that the strong collaborations between the research agencies in Tasmania which is such a feature of the research scene there today, goes back to my father's actions.

And that may well have come from his contact with Jerry Price.

Very likely. But it was also because Tasmania was a small community and in the 1950's a rather isolated one. There were some big issues. For example, the Forestry Commission was doing a lot of work to understanding the ecology of Tasmanian forests for timber. Anyway, we can move on.

So you went to University of New South Wales to do your science degree. Why didn't you go to the University of Sydney?

Normanhurst Boys High School and the University of New South Wales

I got bribed. We came back to Sydney at the beginning of 1964 and I did the Leaving Certificate at Normanhurst.

Normanhurst High School?

Yes, Normanhurst Boys High School. I did quite well on the Leaving Certificate—was Dux of Normanhurst— and was obviously going to go to university. In those days if you lived, as we did, on the North Shore of Sydney, you went to the University of Sydney. I was actually offered a national undergraduate scholarship to go to the ANU, and I went down to check out the ANU. However, I really didn't know what I wanted to do, scientifically. I had no interest in anything else, so I was definitely going to do science, but I couldn't work out what sort of science. I liked physics and maths, I was interested in biology – or at least modern biology through my father—so at least, for first year, I wanted to do physics, chemistry, maths and biology.

At the ANU, and I was told I had to drop one of the four of them. Then the University of New South Wales – presumably my father having nothing to do with it – offered me a cadetship. In those days, if you recall your Australian university history, students paid fees.

Except at the University of WA.

Except at the University of WA. I had been awarded a Commonwealth scholarship– or something similar– so there was never going to be a fee issue. Anyway, the University of New South Wales had a very interesting program. They selected four students, gave them a princely sum of 500 –

Pounds.

Yes, and bonded you to complete a first degree and then become a tutor at the University of New South Wales.

I have never heard of that story.

I'm not sure how long the program ran—several years about then—but it did attract some of the top Leaving Certificate students to New South Wales was really beginning to develop. My father had gone there as the Foundation Professor of Botany. Again, in one of those interesting coincidences that intrigue me, the Dean who attracted my father was a man by the name of Bernard Ralph, a biochemist.

Yes. Bernard Ralph, a very important figure in Australian chemistry.

Yes. What you may not know, Tom, is that last April – April of 2017 – the Academy of Science elected an information theorist, whose name was Ralph. Turned out he is Bernard's son!

Okay, yes.

Anyway, back to myself. Yes, so I turned down the ANU, and took the cadetship at New South Wales. I did physics, chemistry, maths and biology in first year, but at the end of that year, I had been cured of biology. My big problem with biology was dissecting rats. My father gave lectures—interestingly delivered on closed circuit TV—on DNA and evolution, which I thought were fascinating. But, I couldn't dissect rats, and I couldn't stand a lot of the other stuff you do in biology, so I went off and did physics and maths for the rest of my academic career. My father thought that was, in the end, genetics, because Charles Darwin's son was also a physicist.

So you did mathematics – yet you say your major was in physics?

My major was technically applied maths. It's worth a little digression because New South Wales, at the time, and I think I only appreciated this much later, when I spent 10 years on the staff, was in a transformational phase. Some powerful and very influential people were appointed, like my father.. In mathematics, or the department of applied maths as it probably was then, was a man who has almost been forgotten in Australian science today. A man by the name of John Blatt. Blatt was Professor of Applied Mathematics at New South Wales. He was a Jewish immigrant, who had come to Australia via the United States.

Blatt had left Austria with the rise of the Nazis, moved to the US, got a double PhD, including part from Cornell. (I found his PhD thesis on the Cornell shelves once.) Blatt then worked at Illinois, but with the rise of McCarthy, John, who never saw anything except in black and white, saw in McCarthy the same forces at work as he'd seen in Europe in the '30s. So, he said, "Where can I go that's farthest away from everything that's likely to happen?". Picks Australia. Takes up an appointment at Sydney, where he teamed up with Stuart Butler and Martin Schafroth.

In many ways, Blatt was a polymath himself. He was interested in a range of subjects and very wide reasoning. Originally a nuclear physicist—wrote a land mark textbook with Weisskopf at Sydney he worked on SILLIAC—the first computer—turned to

superconductivity with Butler and Schafroth and in John's words, "Got robbed of the Nobel Prize for superconductivity". He always believed that Leon Cooper had pinched his work.

By the time I knew him, he was at New South Wales. He taught me in a number of courses. A brilliant lecturer. I still can vision him walking in and dividing the blackboard – it was chalk in those days – into three. He would carefully fill each bit, and then you would leave having learnt.

So he was a major influence on you?

To some extent but probably the biggest influence at the time was someone that John had appointed, Barry Ninham.

Oh, yes.

Who you know very well. Ninham had come back from the US as the first Australian to get a PhD in the United States. A West Australian. He'd come out of the University of West Australia.

I didn't know he was the first to get a PhD in the U.S.

I'm pretty sure of that.

Okay.

At UWA Barry had been influenced by Bob Dingle (in Physics) who encouraged a number of students there at the time to go to the US instead of the traditional UK. Barry went to Rochester, worked with Montroll, came back to Sydney and UNSW, and took me and a few other people on as honours and postgraduate students.

So, he came back to the University of New South Wales?

Yes, probably, as a senior lecturer. He was an associate professor when I knew him. I did my honours project with Barry.

Oh, did you?

My honours thesis was on random walks, polymers and related applications of stochastic processes. It got published as a book. Unfortunately, by Gordon and Breach, who went into Chapter 11 and cancelled all the royalties. So, I've never got any money out of them. But Barry, very importantly, taught me some mathematical techniques that made the next phase of my scientific career successful. So, Barry had a big intellectual influence. He also convinced me, and this is now '68 when I finished that I should look to the US for a PhD.

Can I just interrupt there, and ask about your personal motivations? You were very clearly a top student in this environment. You probably could have done anything you like. These

days, you probably would have gone into medicine or law, because if you talk to students, that's where the money is. But back in those days, people pursued their interest. Is that how you saw things?

I actually, to be honest, Terry, I don't think I ever thought about it. It wasn't – inevitable is perhaps the wrong word – but I never really thought that I wouldn't go to university. I'd grown up in a university household.

Yes, so you'd become a professional of some sort.

Yes, but I probably didn't realise how – well, I realised I was probably smarter than some people, because I would regularly get, 100 percent on maths tests and, as I said, at Normanhurst, in hindsight, I was really pushed a bit more than would normally have been the case. I got to university and for the most part university was easy. Later, of course, when I was on the staff and we were going to continuous assessment, I used to think, "Oh my God! I would have hated this system", because my approach was to have quite a good time. Enjoy my science but enjoy a few other things. I'd often get into the exam and sit there and thinking, "Oh Christ! – I probably shouldn't use that word in an oral history -- "If I get out of the exam, I'll do some work next semester." I usually did quite well and forgot the resolution!

And so yes, university was easy and generally interesting. Biology was out, physics was interesting, applied maths was interesting. However a lot of the 'applied maths' was really theoretical physics because Blatt, while he was in applied maths department, was a theoretical physicist along with Ninham, Ian Sloan and several others.

Well, Ninham was a genuine applied mathematician applying mathematics to colloids.

No, not at that time. Ninham's background was many-body physics. He had done his PhD with Elliott Montroll at Rochester on the electron gas. Blatt had built a de facto theoretical physics department in the department of applied maths. In the department of physics, you did experimental physics. If you wanted to do theoretical physics, you basically were in applied maths department. And many of the courses we took were standard theoretical physics: Electromagnetism from Blatt, quantum mechanics from Ian Sloan, who later, of course, got a big reputation in numerical analysis, but that came out of trying to do many-body nuclear calculations. So, theoretical physics was where I ended up but with a solid grounding in mathematical techniques.

And you didn't have to worry about money, by the sound of it?

My five hundred pounds was very useful. And, of course, I was growing up in a family, supported by a professorial salary which in in the '60s –

Was okay.

– weren't, you know, that bad. I don't ever remember a sense of financial stringency.

Or worry, yes.

Worry-no. I could save my five hundred pounds, and bought a car in third or fourth year. So, in hindsight, I have to admit it was a very fortunate –

Comfortable existence.

– upbringing. Comfortable, but intellectually challenging.

Yes, that's right. You could concentrate on that.

But we hear you say that in those days that you only studied in third term, what were your extracurricular interests at the University of New South Wales?

I played a bit of sport. I wasn't a bad tennis player in those days. I wasn't a bad hockey player as well, until someone decided I was hitting the ball the wrong way and my grip wasn't right and decided to correct with the end result, of course, that I couldn't hit the ball the way I liked, and therefore lost interest..

And you stayed at home?

Yes, I stayed at home, except for one year when my father went off to Nigeria, and I spent the first term in Baxter College. At the end of the first term, I said to my parents, "I can't stand this. I'm going to go and live at our place", as they hadn't rented it. With a bit of trepidation, I think, my mother agreed. But other than that, I lived at home. I didn't find that confining.

I enjoyed talking science with my father and would often go home with him in the evening. I can even remember one occasion when I had had a less than successful biology lab, and some snooty tutor had found the piece I was meant to dissect in my scrap dish. I was going down in the lift with my father who, at that stage, was, I think, head of the school of biology at New South Wales, when she got on the lift, took one look at the two of us, and suddenly realised who she'd been chastising in the lab. I have to admit, there was a bit of me that quite enjoyed that.

But I think you're right, Terry. I mean, on one side it was very comfortable, easy and I never felt that the next step was a choice. It was– inevitability is the wrong word – just the right thing to do, including the decision beyond my first degree.

Summer vacations in CSIRO

But again, I never questioned that I would do a PhD; It was what you did if you were in an academic community. However, CSIRO played a big role the final decision because I spent two summer vacations in CSIRO. One with John Collins at Chippendale.

That was before they'd gone up to the new labs?

Yes. Now this was – in fact, not my first encounter with CSIRO, other than through the Price connections, which actually shaped my physics, because Don Price showed me Feynman’s lectures when I was probably 13 or 14– he was a couple of years older than I was, so probably the early ‘60s. I was fascinated by reading them. But we lived in Sydney next door to a man– Kennett, I think he might have been named – who was a tech officer at North Ryde in the Division for Food Research. So I got a vacation studentship – it must have been at the end of my first year at university– in the food tech lab at North Ryde. I used to tease Ron Sandland that I’d actually worked for CSIRO before Ron did.

Then at the end of third year, I spent a summer vacation job with Collins at Chippendale. And they sponsored me for a CSIRO pre-doctoral fellowship.

So, Chippendale – that was on the campus at the University of Sydney?

Yes, on the campus of the University of Sydney – the old standards lab. It was where Giovanelli’s Division of Physics was based. I was in Guy White’s group.

Well, that’s another West Australian connection, because John Collins was a graduate of the University of WA.

It was like a mausoleum over there.

Sorry?

It was like a mausoleum in that place. It’s a wonder it didn’t put you off for life.

It was – yes, I guess you’re right. However, I found it intellectually interesting. I’ve never really talked to John about what John saw in me. I should do that at some stage. He clearly saw something, because he was very supportive. Guy also was very supportive, and they gave me quite a challenging job to write a computer code to calculate things called Grüneisen constants. The Grüneisen constant or parameter describes the effect that changing the volume of a crystal lattice has on its vibrational properties, and, as a consequence, the effect that changing temperature has on lattice dynamics.) At that time White was very much involved with thermal expansion of solids. Guy maintained that they used my code for many years after it in various ways. So, like Ninham, they asked “what are you going to do?”, and, “What do you want to do?”. I guess my own view would have been a PhD—mainly because it was just the next step. And probably stayed at UNSW with Barry. Would have been a big mistake. However, they encouraged me to look overseas and John helped ‘fix’ a CSIRO pre-doctoral fellowship to take me to the US.

Cornell University on a CSIRO Studentship

Okay. So you were in the US on a CSIRO studentship. Why did you pick Cornell?

Yes. Well, I had to do a couple of things. One, I had to get myself out of the bond at the University of –

Of New South Wales.

Yes. I mean, it was less my bond. It was my parents' bond. I think I'd been bonded for a couple of thousand pounds. Anyway, New South Wales were quite gracious. They postponed it and then a few years later waived it. And of course, in the end, I went back anyway.

Yes.

CSIRO, as you probably well know at that time, and even before then, had been quite strategic about looking at people, looking at their potential area of work, and then thinking that, "We need to skills". Guy himself had been sent to the UK to work with Ziman to help build the foundation that led to his group on thermal expansion of solids. They were now very interested in developing expertise in the theory of liquids.

And John Barker was part of that group.

Yes. So, there was this was the emerging interest in what was then the Division of Physics, in liquids and they wanted to send someone to Chicago to work with Stuart Rice. I have to admit, that I wasn't a) particularly interested in the theory of liquids, and b) too interested in going –

To Chicago.

to Chicago. My honours thesis had got me interested in statistical mechanics, in particularly the work of a man by the name of Michael Fisher at Cornell. Fisher was one of the leaders of an emerging understanding of phase transitions, in particular. This had a very strong element of combinatorics and he had done some work in self-avoiding walks, which is where I'd got to know his work. I was quite fascinated by that, and also by a course I'd had in honours from Colin Thompson, the a QE II at UNSW and later a professor at Melbourne.

Colin talked about critical phenomena and our understanding of the theory of phase transitions. Well, perhaps at the time, the lack of an understanding of phase. So, I was quite keen to work in this area. CSIRO wanted someone to work in liquid physics. So I, wrote this little spiel about the importance of liquids and in particular the physics of when liquids turn into solids! Whether it was through the influence of John or Guy I don't know, but, CSIRO graciously decided two things. One, I was doing the theory of physics of liquids, which later knowing CSIRO probably ticked off on some box on legal requirements at CSIRO, and agreed to Cornell.

The other important reason I picked Cornell was because, unlike most American graduate schools, it had a mid-semester early year intake, so I didn't have to wait for four months.

And that agreement had no obligation to come back and work for CSIRO?

No. There was an expectation that you might work for CSIRO, but I don't remember being formally bonded

My point. When I got mine, it specifically said, "I understand that CSIRO has no obligation to employ me."

Yes, mine may have said something like that as well. It was clear that if I had wanted to, it was quite likely that someone like White would have me back at the drop of a hat. However the intent seemed more to build Australian capacity and, hopefully, you would come back to CSIRO. As you know, they did but 44 years later! So, I left at the end of '68, went to the US, and started at Cornell in early '69.

And that was a big – some of the papers that you produced at Cornell are some of the most highly cited, high maths papers by an Australian scientist.

Oh, thank you, Tom. Yes, a combination of luck, timing and some mathematical techniques Barry had taught me. Indeed, if I'm honest, I have had a fortunate scientific, as well as personal, life. The fortunate part came from seizing opportunities that were given to me. Fisher is a brilliant mathematician, as well as a very deep-thinking physicist. At the time, he held a position in chemistry and for some reason couldn't supervise students in the physics program but could in the applied physics program. So I joined the applied physics program but worked in Department of Chemistry.

But the most significant thing was that Barry had taught me a mathematical technique called "Mellon transforms", and it was one of the few parts of mathematics that Fisher didn't know anything about. Because I had a bit of independence with my own CSIRO money, I didn't have to do teaching or other sorts of things to pay my way as most graduate students. So, I had enough time, even in my early qualifying years, to do a bit of research. Michael gave me a paper of a former Post-doc of his, and he said, "We got to this point, but we didn't really finish it." I looked at the problem. It involved the analysis of a product, and Barry had told me what to do with products.

He had said, "When you want to analyse a product, Michael, take the logarithm. Turns it into a sum. Apply a Mellon transform." So that's what I did. I think Michael had given the paper to me on a Thursday/Friday, and on Monday, I walked in and gave him the solution. That led to the first of my early little papers and Mellon transforms were the secret to much of my work at the time. The focus of my work was on finite size effects at phase transitions. I analysed—with Mellon transforms—a number of solvable models and we extrapolated those results to a general theory of finite size effects in phase transitions. The importance of that was it allowed a very sophisticated analysis of numerical simulations of finite physical systems, eg by Monte Carlo, and not only in phase transitions. Hence, gratifyingly it has stood the test of time and is still used today. So, the combination of Barry's technology with Michael's thinking, I think, shaped that period for me.

QE II Fellowship at the ANU

And so you finish your PhD in 1972 and you were then quite a well-known applied mathematician from these publications, weren't you? So, you got a Queen Elizabeth II fellowship at the ANU.

Yes.

You came back to work with Barry Ninham?

Yes. And there were a number of reasons, political, scientific and personal. For a young Australian in America in the early 70's was fascinating politically. In fact, I'd arrived in America on the day Nixon was inaugurated in 1969, out of a pretty apolitical university, well before the moratorium marches in Australia. The University of Sydney was more of a radical place than UNSW and I was certainly not politically active. However, thrown into the cauldron of late 1960 America was a fascinating opening of eyes. By the end of '72 I'd had enough fortune to have finished much faster than most American PhDs do.

That was partly because of the good education I'd had at New South Wales. I got fed up at the beginning of my induction interview at Cornell when they wanted me to do all these courses. They finally suggested, I think to shut me up, that I could sit the qualifying exam the following day – the day after I had arrived at Cornell. The exam was usually taken 3 semesters into a PhD program but I sat it. Most of it was about stuff I'd done in honours programs at New South Wales, so I passed and leapt about 18 months.

Scientifically, and particularly in the context of statistical mechanics and phase transitions, the early 1970's were revolutionary. This was because in 1971, Ken Wilson, who won a Nobel Prize, introduced a concept called the "Renormalization Group", and a lot of the problems we thought about in the early '70s, by the mid '70s were solved by the Renormalization Group. So, there was a real paradigm shift, and I was lucky enough to ride that wave through it. And finally by '72, I was married. I'd married an American graduate student.

Did she want to come back to Australia?

Well, Jeanette had grown up in upstate New York. She was a maths teacher. I'd met her at Cornell. Her father was a dairy farmer in the Hudson Valley, not far from West Point. Mark Kac at Rockefeller University (in New York City) offered me a post-doc, but Jeanette said, "I'm not going to live in the city." Unfortunately, there weren't many post-docs in the US at the time due to Nixon's cuts to scientific funding. Barry wrote to me and said, "What are you going to do?", and I said, "I don't really know", and he said, "Do you want to come back to the ANU? I've set up this very exciting new program in, sort of, everything. We'd love to have you here". I agreed and he said, "Right. I'll fix you a QEII!" And a little while later, I got the offer! Rather easier than getting one today! So I came back to the ANU in '72.

With or without Jeanette?

With Jeanette, yes, yes. I nearly lost her when we first got back. Do you remember there used to be flats up Northbourne Avenue?

Yes, yes.

Ooh, yes.

And there was a petrol strike on at the time, and it was winter in Canberra, and those flats were never very appealing or heated. I think she nearly thought this was the end of the world. But, she survived and we've stayed since then.

So, we've been talking for an hour. We'll have a break.

Yes. We'll have 10 minutes, yes.

[music]

We are back on recording and Michael has left Cornell and gone to the ANU as a Queen Elizabeth II Fellow in Barry Ninham's department. But did you work with Barry there or did you set up an independent research group of your own, or research topics of your own?

I obviously went back with some applied work but again, whether it was just fortunate or whether it was part of my innate ability, I kept my eyes open for opportunities. Barry had set up quite an amazing group—Snyder, Mitchell, Perram, himself, Richmond. There was even degree of competition and even a bit of open warfare within this group at the time. There were two sub-groups that only spoke to each other by writing papers which criticised each other!

So, it was quite an exciting period to go there. But sitting quietly in the Theoretical Physics Department was a remarkable man, and that man was Rodney Baxter.

Yes, he certainly was.

From the point of view of my own immediate interests in statistical mechanics, Baxter had written a paper on a model called the Eight-Vertex model that upset the applecart. So, I was interested to understand more about what Rodney's model.

There's not time to get into details of that area but what is perhaps important to realise is that, at that time, the theory of phase transitions had been re-shaped by the emergence of the Renormalisation Group. In particular, the Renormalisation Group explained an old observation that at a critical point most physical details irrelevant. To understand critical points in phase transitions you only needed to know the dimensionality of the system—two or three dimensional—and the relevant symmetry group. This 'universality' explained why

phase transitions in different systems were quantitatively similar, eg liquid gas transitions were in the same universal class as a simple model of interacting spins—taking plus or minus values—the Ising model. Thus by analysing the Ising model—much simpler computationally than a real liquid-gas—you could make predictions about fluid gas systems.

Baxter's exquisite analysis of the eight-vertex model appeared to violate that now well-established—or accepted—universality. In Rodney's model, some quantities that according to universality should have been simple numbers became functions of a physical variable. Universality appeared to be breaking down.

I was fascinated by this so I started talking to Rodney and, a little like my introduction to Fisher, was incredibly lucky. I went to talk at the University of New South Wales by a friend of mine, Jaan Oitmaa. Jaan was investigating—by conventional tools—a different aspect of the eight-vertex model than Baxter had calculated. The result was a complete mess. I came back to Canberra and described Oitmaa's attempt to Rodney and Rodney said, "You know, if you do this and express it in these different you'll probably get something interesting." I took him at heart, did the calculation, walked in and gave him the solution on Monday. It was quite a significant result—showed among other things that some aspects of universality still held. Actually what Rodney and I published was a conjecture rather than a mathematical proof. It took Rodney 10 or 15 years to prove it!

Again, that example, for me, confirms my view that one should keep one's eyes (and mind) open and size the opportunities that arise.

So, I had probably did have more independence as a researcher in my QEII post doc days than most – and was encouraged by Barry. With my recent American experience, I also knew some of the new ideas in statistical mechanics that weren't yet widely known. For example, I gave at the ANU the first lectures in Australia on the Renormalisation Group. Barry, I think would have liked me to have been closer to his developing interest in colloids and physical chemistry but has remained a very strong supporter of mine with one exception. I suspect he doesn't think I really did my talent justice by becoming an administrator and a vice chancellor!

Lecturer in Applied Mathematics at UNSW

We'll come onto that later. So, you were at the ANU until 1974 and then you went back to the University of New South Wales as a lecturer in applied mathematics. What was it like going back to your alma mater?

It was interesting. The QEII was a two-year position so I had to find something else. Blatt advertised a lectureship, wanted someone to work with him on something or other. I even forgotten what it was. I applied. He gave me the job. John had a view of the world which I found interesting. He would often give me a lecture which would go along the lines of you've really got to do something new. You've got to do – this is a great idea—with me,

Michael, but of course it's going to take us 10 or 15 years to get recognised, which for someone who interested in more immediate recognition not that appealing.

However I did listen to John – because the other thing John did was ignore the literature, and even attempt to destroy the literature. His view of writing papers was to demolish someone else. I didn't think this was necessarily quite the right way to go. But, while he would have some really strange ideas, sometimes, more often than not in combination with the literature that John deliberately scorned, these would turn into an interesting paper. So I wrote a few of those with him.

At the same time, I built my own interests through that period of the 70s with a few students. Most of my work was still statistical mechanics but I was beginning to have a much more computational focus. I'd always done computer calculations and was a pretty good programmer– but now I was beginning to realise the full power of modern computers. And that's where I probably broke with Blatt.

Blatt had been an architect and pioneer of Australian computing. He'd been appointed initially at Sydney to assist with SILLIAC. And he'd been innovative. He pushed SILLIAC hard, and introduced computing and particularly optical card readers to teaching.

SILLIAC, yes.

But I went for a sabbatical to Santa Barbara in probably '77, '78, and came back an avid fan of UNIX, of VDUs and insisted we got rid of card readers and we moved on to more 'modern' computing. Blatt wanted nothing to do with this. For Blatt, this was a passing fad. Fortran and card readers were the way you communicated with computers!

But I wanted a VAX 750 because I wanted to do some big calculations. So, I pushed for that, and ultimately got that machine and by the early 1980's, I suspect to John's chagrin, we were teaching students on VDU's running Unix editors.

My portrait of John is rather unfair. There was much to admire in John and, computing aside, he was very progressive. I am not aware of anyone with such a wide research span. Originally a nuclear physicist, as I mentioned earlier, he switched to solid-state physics when he came to Australia and was nearly the first to crack the puzzle of superconductivity. When I joined UNSW in 1974, his interest was non-equilibrium statistical mechanics but soon after he switched to optimal control theory and then economics. And, he always asked big questions. He also built and led a great department. Under his leadership the department changed from a de facto theoretical physics department to a modern applied maths department. An amazing number of people he appointed went on to chairs. I certainly learnt a lot of mathematics from John and he, despite our occasional differences, strongly supported me including for accelerated promotion to associate professor. That he

has now faded from the history of Australian science is rather sad. But, that's a story for another time.

Researcher or teacher?

Tom, can I just ask on this period can you give us an idea as to whether you thought of yourself primarily as a researcher who had students or primarily as an educator who was doing some research?

That's an interesting question, Terry. And the answer to that is I probably hadn't – to me university, academic life was teaching and research. There was no need in those days, and that was again fortunate, to get research funding, per se. A bit later ARGS came in. (Australian Research Grant Scheme—the predecessor to the Australian Research Council.) Blatt was very, very protective of his young staff. We were not required to do much administration. We were in the early days of continuous assessment.

I quite liked teaching but New South Wales also had a very interesting system. It still had a very large part-time cohort. And if you taught them in the evening, you got time-and-a-half against a set contact hour criterion. So, I used to teach differential equations to second year engineers from 7:00 till 9:00, with most of them asleep and that knocked off a large chunk of my teaching requirements. I did first year calculus to another group of tired engineers. That almost cleaned the whole slate.

We lived on the North Shore so I didn't go out to the university every day. In a way, teaching was necessary but I had plenty of time to do research in a way that I certainly do not think the current generation of academics have. Even later in life as the head of a department, I could not protect the staff from administrative intrusions the way I had been.

The other important thing is that I used that freedom to shift directions. So, I started new things. And again, I that's something I worry about when I look at people today; too often you can see their PhD continually through their career. I broadened. I've left things behind. Often ideas that other people would publish.

And did you have a lot of students, PhD students?

I had several PhD students at that time.

Who helped you with this or -

A: Yes, one of them with an interesting connection back to CSIRO. Peter Hornby – who later worked in exploration and mining in Perth. Murray Batchelor is probably the best; the only person ever to win the Maths Society Medal for mathematical research under 40 and the Pawsey Medal for research in physics under 40. Now a mathematical physicist at the ANU.

However, I didn't think my life was unusual. This is what I thought the academic world was like. Now, I suspect most of my contemporaries didn't see it that way. I did what I wanted to do and had the freedom to do so. We had kids and Jeanette was a very supportive wife, which I think again I underestimated, as she never objected as I started to move.

And did she work herself?

She was a teacher. She taught when we first came back to Australia and then after we had kids she went back to teaching part-time.

And you kept being promoted at the University of New South Wales. You very quickly became an Associate Professor. You got a few double increments and -

Again, it says something of a very different time. I was appointed on a tenure track appointment and, very early on, I can vaguely remember the head, the acting head, I think John was away, saying, "we should fix your tenure". So, I got a nice letter saying I had tenure!

John was supportive but I remember he said to me when I wanted to go for a promotion to Associate Professor, somewhat jumping up the ladder. He said oh, that's going to be a tough ask. He said, who are you going to use as referees? I said I'd use Baxter. He said oh, I'd worry about that, Michael. I said, why? He said, Rodney will call a spade, a spade. And I said well, I'm happy to be called a spade!

So yes, it was an easier time. For me it was a time of opportunities to do new things. But importantly I had almost no administrative responsibilities beyond the administration of my student cohorts. In fact, I think even as an Associate Professor the only administration I did was to organise computing in the place. But that was largely because I wanted to use the machines and I wanted access to them.

So you weren't on any university committees or -

I don't remember being on any university committees at all at UNSW. In fact, I'm 90 per cent sure I wasn't because when I went to the ANU one of the questions they got very worried about was I had no administrative experience!

Professor of Mathematics at the ANU

We will go to 1984 now. And you become the Professor of Mathematics at the ANU in what was then the School of General Studies. And Barry was still there, presumably?

Barry was still there but in the Institute of Advanced Studies. You've got to also remember that in those days if you wanted to be a full professor you couldn't stay in your own institution. There was no promotion to full professor so if you wanted to be one you went somewhere else.

A couple of things had happened at the ANU at that time. Archie Brown, who was my immediate predecessor, had retired if not died. And Neil Trudinger, who was the other professor in the Maths Department in SGS had got one of the first ARC Centres of Excellence – the Centre for Mathematical Analysis. So, they advertised a chair in the then Maths Department in SGS (a teaching department) which included applied mathematicians and pure mathematicians.

And statisticians?

No. The statisticians were in a different department and a different faculty in the School of General Studies, and they were in a different school in the Institute of Advanced Studies.

I'd actually been going back to the ANU for a while. I had a very strong collaborator there in Chris Hamer, who was the son of the Victorian Premier, and of course Rodney Baxter. By that time I suppose I was beginning to be one of the emerging leaders of statistical mechanics in Australia and Canberra was a really hot bed of interest in statistical mechanics.

After I had been in the US on study leave in 1981, I had been offered a Senior Research Fellowship with Rodney. However, in the end I turned it down, I think, in part, going back to your question, Terry, because I actually quite enjoyed the educational side of what I was doing. I didn't really want to be a pure researcher. But then the chair came up I thought that was an interesting opportunity. So, I applied, got interviewed by an illustrious committee headed by Ian Ross (then Deputy Vice Chancellor) who became quite an important figure in my subsequent life.

In many of our lives.

Yes, in many. Like all good academic committees it took forever and a day and I learned later from Ian the problem was that they had two problems with me. One, a mathematician on the committee who didn't think I'd done any mathematics! They didn't believe anything done in physics literature was mathematics and I only ever published one paper in a maths journal, so they were very sceptical about me as a mathematician. And then they were even more sceptical because I had absolutely no administrative experience.

In those days when you went to a chair did somebody in the university take any interest in giving you any training at least or was it all learning by doing?

Very much by doing. After I got appointed to the ANU I had a meeting with Ian in which we discussed a few issues.

He was the head of the School of General Studies, was he?

He was DVC of the whole institution at that stage. The VC was Peter Karmel. But Ian was certainly the Chief Operating Officer for the faculties in quite powerful ways. At this initial meeting, I went in with a shopping list of things I wanted both for me and the department

and I walked out with \$1,500 to bring a collaborator who was due to come to the University of New South Wales from Germany to Canberra, and nothing else.

So, no, I wasn't really told anything. I'm a classic example of an academic that somehow has managed to succeed at various levels with almost no formal training in anything administrative or managerial.

And were you given an assistant, an administrator who actually knew what was happening and what to do?

No, not really. However, the Faculties in those days operated in an interesting way. There was a body called Resources Committee, which consisted the deans of the faculties and chaired by Ian. If you were a head of department and wanted something like an appointment you approached your dean, wrote a submission to Resources Committee. Later, when I was Dean of Science I learnt what Resources Committee did: it made decisions on 'academic grounds' with no consideration of finance then at the end of the year the Faculties' Business Manager found enough hollow logs to square off the budgets. Very different from today when universities, and even faculties, have strategic plans and departments or schools operate like little business units. And of course, for the ANU all of this fell down with the Dawkins revolution. By that time I was Deputy Dean and about to become Dean of Science at the ANU.

Dean of the Faculty of Science at the ANU

Why did you agree to become Dean?

I guess I'd taken that step for two reasons. One, a bit of an expectation that a professor should do it and secondly, because, I have to admit, I was getting fed up with the nature of the decisions that people were making and decided, that I thought I could do better. So I agreed to stand and ultimately get elected—deans in those days were still elected positions—to the job.

By that time Dean Terrell was Deputy Vice Chancellor and I when I was still Dean-Elect and he summonsed me into the office and said, "You'll become Dean next week, Michael, and the first thing you have to do is stop that chair in molecular biology." I said, "The one we're interviewing for next week?" He said, "Yes, we can't afford it." And I said, "But we're interviewing for it." And he said, "No, got to kill it."

This was all a consequence of the 'Dawkins Revolution' which hit the ANU and particularly the Faculties, very hard. For the first time I think in the ANU's history, and certainly in the faculties, people began to realise that finances mattered. Aitken (Don Aitken) had just done a hard-hitting review of the Institute so that was also in a state of near crisis. This was the mix into which I was thrown with nothing probably more than a fairly open mind, an analytical ability and a view that some evidence on which to make decisions wouldn't be a bad idea.

So, Tom will tell you that as chief of division he needed and relied upon a good personnel officer and a good finance person to look after the money and the people. How many people were you looking after as dean?

Well, first of all as Professor of Mathematics. But how big was the department?

The department would have been not huge--ten, 12, 14. And the Science Faculty was not huge. There was good HR support.

Across the whole university?

Across the Faculties. There was an HR officer in the faculties. The Business Manager of the Faculties handled the resources. And when we got into the new regime—post Dawkins—that was fortunate. Scardonis was his name, he was quite a strategic thinker and I think probably welcomed a -

He was the business manager?

Yes, and- welcomed a dean who was prepared to think analytically. I was the first non-experimentalist to be dean of the Faculty of Science. There was almost a push to stop it because some were scared that I was going to do it from a different perspective. And to some extent I did but what pleased me about that period people seemed to appreciate my style and approach to the deanship. I still remember, the comment that the Head of Chemistry, Jack Elix made at the end of my term as I was about to go off to West Australia, , In my farewell, he said, "Michael should have been someone that did things that we didn't like and he did to some extent, but we rather liked him."

I guess what I quickly learned, and perhaps didn't even know I had to learn was that it was best to get out. I'd go talk to people. I'd try and understand what they were doing. But at the same time no one had told me I couldn't do things. And so, while the dean was an elected role, I did things and I think people were surprised at that.

So, the dean was an elected role?

Yes, and you didn't have delegated authority to do much.

You had to do it by talking to people?

Persuasion?

Yes. Or occasionally making a decision! I remember the Head of Chemistry, Ron Warrenner, left and went off somewhere else and the Chemistry Department went into a bit of a huff and said they weren't going to nominate a new head to me. So, I simply said, "Oh, that's right. If I don't have a name from you I'll have a head by 2:00 this afternoon." Ben Selinger rang me up and said, "Who's going to be head, Michael?" And I said, "I will." And by 2 o'clock I had Ben as head.

It was a different time but I have to admit with no training I somehow muddled my way through it to the extent that when I went back to Santa Barbara on study leave in, during my deanship, I was bored. People were telling me, you must be pleased the phone is not ringing, but I was bored and missed the phone ringing.

Research work continues

Michael, just lets go back briefly to your period as Professor of Mathematics which was a decade from 1984 to 1994. Did you do, were you involved much in research then?

Yes, oh yes.

So, that was a period of activity of research activity?

Very definitely. I was adamant that I was going to keep research going. I'd moved out of equilibrium into non-equilibrium processes. I'd spent 1987 at the Institute for Theoretical Physics at Santa Barbara, part on study leave, part on leave without pay, supported by the Institute for Theoretical Physics. The Institute for Theoretical Physics (ITP) in Santa Barbara was the first big National Science Foundation (NSF) institute set up, at that time under the directorship of Walter Kohn. The idea was that they have a small number of staff members, some post docs but focus on year-long programs that brought people from across not just physics but other disciplines. In the original NSF competition which had been won by UCSB and ITP, a competing bid had been from the University of California, Santa Cruz led by Mike Nauenberg who I knew very well. If that bid had come off, I would have gone to the U.S. as Deputy Director of the Institute.

It didn't come off but Nauenberg moved to ITP and they invited me to come for a year. There I was involved in a program looking at 'emerging structures"—structures that come out of non-equilibrium systems, eg snowflakes and things like that. I also got very interested in pushing computers in ways we hadn't thought of doing before. So, with a computer scientist and another physicist we built, or they built, and I helped analyse, a special purpose computer to do Monte Carlo simulations, incidentally using finite scaling from my PhD. Unfortunately it didn't work as well as we hoped because we ran into the problem that random number generators aren't really random if you use them enough.

So, I got very interested in where computing was going. At that time, my own research was in two rather disparate areas but with the common feature that they were vociferous users of computing. One, at the interface of statistical mechanics and quantum field theory required the determination of eigenvalues of as big a matrix as one could handle. The other coming out of my more recent interest in solidification concerned calculating moving interfaces—a notoriously difficult computational task. Thus, I was an avid seeker of more computing power. Santa Barbara, at that time, was one of the three or four sites for ARPANET which became the internet. There were people there very interested in emergent computer technology. So, I came back to the ANU with a broad knowledge of trends in computational science.

Ross had put me onto a review he'd commissioned of computing at the ANU, chaired by himself. This would have been before I went to Santa Barbara but somewhere around the same period, '85, '86. That was the review that led to the acquisition of a Fujitsu VP 20, the first supercomputer in an Australian university.

And soon after that, which would have been after I'd come back from Santa Barbara, we had a deal with Fujitsu which was one of the first big computing deals that Ross pulled off. They gave us hardware and software systems. However, Fujitsu had a major problem because they just settled out of court with IBM having swiped their software codes. I remember going to a Fujitsu meeting and coming back to the department and saying, "They'll give us some money to develop some parallel codes for doing vector matrix multiplication." Not very challenging but a good introduction to parallel coding. The department looked at me and said, "What on earth would we want to do that for?" And I said, "Because if we do that well, we might get a little more money." But they didn't want to do it.

However, I was now very interested in what these machines could do so that led to my last period of, if you like, pure research, an interest in parallel computation, particularly for boundary value problems, moving interfaces, quite a lot that never got published.

So, was Bob Watts around at that time?

Yes. Bob was around at that time.

So, did you ever interact with Bob? Because he was interested in many similar things.

Yes, Bob was head of the computer centre at the time at the ANU. Denis Evans was another big computer user. In fact, Evans together with Michael McRobbie (later President of Indiana University), Robin Stanton (then Head of Computer Science) and myself were a small cohort of people across the university who were seeing this where science was likely to go. Leo Radom in computational chemistry was another. Ross' genius was to realise that this was a place that the ANU could lead.

So I got more involved in university affairs through computing, being asked to solve various problems by Ross, in the computing space, particularly in the Faculties where I chaired the Faculties Computing Advisory Committee.

And during that period was there any interaction between ANU and CSIRO in your field? Your field sounds to me pretty academic. Was the CSIRO work in this space different?

No, I don't remember much, if any. CSIRO did soon after get a supercomputer which I think at the time was slightly faster than the VP20, and they made a call for users. But they didn't ever tell us how to use it. So I had an account but could never access it! Showed me that technical support was as important as computing cycles.

For me, the bigger interaction at that time was some emerging cross institutional structures within the ANU. When I joined the ANU in 1984, the ANU consisted of the School of General Studies and the Institute of Advanced Studies.

They were totally separate?

Yes, and with an almost impervious wall between. In fact, one of the first committees that Ross put me on was the Interface Committee. Its brief was to explore how you might penetrate this wall.

Mathematics turned out to be an interesting test of what could happen with a bit of new thinking. This came about because by '92, Neil Trudinger and I were both Fellows of Academy of Science but in the School of General Studies, while in the Institute of Advanced Studies were a number of other Fellows including Derek Robinson and Leon Simon. Moreover, Leon and Neil worked together and I was still very closely involved with Baxter. So, there was more of a commonality in mathematics at the ANU than elsewhere and importantly, excellence was not the preserve of the Institute. The end result was that I was one of the leaders who created the first genuine cross institutional body, of some scale, in the ANU—the School of Mathematical Sciences. With a bit of finesse SMS acted as a single entity within the organisation of the ANU. In many ways SMS was a predecessor for the college structure that Ian Chubb introduced 20 years later.

There was quite a bit of interaction between CSIRO Chemistry and the Research School of Chemistry.

Well, that's quite interesting because to leap forward, off that remark, one of the jobs I had in CSIRO was to respond to Nelson's inquiry into collaboration between CSIRO and ANU. The problem was that there was more interaction between Clayton in Melbourne and chemistry at the ANU than there was between ANU and CSIRO in Canberra other than with Plant Industry where there was strong CRC. So, I was aware of that but much later.

Deciding to be a University leader- PVC at UWA

So Michael, in a sense you had become – decided that you wanted to become an administrator.

Yes.

So why did you choose the University of Western Australia? And what was the job of Pro Vice Chancellor (Research and Innovation)?

As I've already mentioned, I had taken a sabbatical while I was Dean; I guess it would have been 1991. When I told Dean Terrell I was going to take sabbatical, he said, "Deans don't take sabbaticals." I said, "I've got a good Deputy Dean. The faculty's not going to fall apart.

I'm going to take my six months." So, I went back to Santa Barbara and the Institute for Theoretical Physics.

Excuse me, was the Deputy Dean also elected?

Yes. At the time, it was a geologist, Mike Rickard. Again, I suppose in hindsight I've been lucky by some of the people who've served with me. There was no reason for Rickard to want to support me since I shifted the Geology Department out of their building into another one to make way for Engineering and they got really put out. But Mike was one of those people who could see bigger things.

Jim Langer was then Head of ITP, and I'd known Jim since I was a graduate student – he was a close friend of Fisher's. Jim gave me some wise advice which was think about what my career and whether I really wanted to leave research and be an administrator, rather than – as he had done suddenly wake up one day as a and realise he didn't really want to be a provost. He'd been lucky and got back to research and the directorship at ITP. However wise advice that I did take to heart.

So, I came back to Canberra more or less convinced that I did want to move up in administration. At the ANU, Dean Terrell was Vice Chancellor, or at least VC-elect, and it was probably fair to say I was likely to be his deputy – I'd acted on occasions as Deputy Vice Chancellor. And, then Fay Gale (VC at UWA) rang me up from Western Australia and said they were looking for a Pro-Vice Chancellor (Research).

Fay had advertised a DVC role 12 months before which went to Alan Robson. I'd applied for it, gone out to West Australia, and had quite a good and interesting interview. Alan was clearly the right person to appoint but obviously I had impressed Fay so she rang me up 12 months later and said, "I'm looking for a Pro-Vice Chancellor (Research)," to which I answered – possibly not the best response – "I had never thought about being one of those." However, that was case, since having been dean, a DVC was the natural step my interest in education. I hadn't really thought about being a DVC Research. Fay said, "Well, come out and have a talk to us." So, I went out to Perth and gave one of the most abysmal interviews I've ever given in my life! At the end of it, I sat in the hotel feeling rather disappointed. Fay rang me up and said, "That was hopeless, Michael. But we'll give you the job."

And was Alan on that panel?

Alan?

Alan Robson.

Alan was at that time Deputy Vice Chancellor.

But did he interview you?

I can't remember whether he interviewed me but he certainly knew me.

Yes?

Back to your comment whether I had any training. Just before the WA interview, probably in anticipation that I might become DVC under him, Dean sent me to a workshop at the Mount Elizabeth –

Management training at Mount Eliza

Mount Eliza.

Mount Eliza Conference Centre. One of the people I sat next to early on was Alan Robson so he knew me but I don't think Alan was involved in the actual interviewing. Anyway, Fay built this little team, Robson, myself, Martin Griffith, who was Bursar, and Malcolm Orr, who was the Registrar. And it was a small team in a university under a massive change agenda. UWA had been shattered by the failure of the proposed merger with Murdoch. It cost Smith his Vice Chancellorship. Fay had been appointed from outside as VC and came into a university steeped in tradition but facing the immense seismic shifts occurring in the higher education system. As I said, somewhat shattered by the failure of the Murdoch and challenged by the rise of Curtin in ways that I think UWA didn't realise at the time. A fascinating time to join an executive team bent on change. It was also a very good move for me with new challenges and an opportunity to enhance my national profile and engagement in ways that may not have occurred if I'd stayed at the ANU even as DVC.

What was attractive about you to them, do you think?

I'm not entirely sure. I obviously had research credibility. I was a Fellow of the Academy of Science with a good track record of grants. As we discussed early I was well across computing and I'd shown, as Dean, that I could strategically respond to change and carry people with me. I was also an outsider that probably helped.

I think Fay may have seen more in me than I saw in myself and was prepared to take a bit of a punt that I could quickly learn what I didn't know about research policy, etc... She hadn't played safe with Robson but been very strategic because I discovered later that UWA was full of brilliant ideas, coming out of what was then the Faculty of Agriculture, with Robson's name on them, that went nowhere. So, she had a deputy who knew where the bodies were, knew which ones should be exhumed and, perhaps more importantly, which ones should be buried even deeper.

A loyal lieutenant, as Fay had with Alan, is immensely valuable if you wish to drive change. I was fortunate later at Flinders to have that sort of person as one of my DVCs, as did Geoff in CSIRO with Ron Sandland.

So what was the job?

Sorry?

What was the job, Tom was asking, was it –

My job?

Yes.

Initially it was designated Pro Vice- Chancellor (Research). Later, under Deryck Schreuder, about 1999/2000 ‘innovation’ was added to reflect an emphasis not just research policy and performance but also translation and innovation. When I was appointed universities—including UWA—were just beginning emergence to think more strategically about the research and more effectively evaluate the research that was done. The PVC(R)’s role was to lead the relevant policy development.

Did you have deal with businesses? Attracting research funding from industry?

Not initially but towards the end of my term definitely. CRC’s were important.

So, were you internally focussed or externally focussed?

Fay encouraged an external engagement both in WA and nationally. Soon after I got to Western Australia and I think, in part, because I was in WA Max Brennan rang me up and said, “I have to balance the ARC, would you like to join the ARC?”

She’d come from somewhere else?

She came from Adelaide.

Adelaide, yes.

She was also beginning to get engaged in the AVCC (Australian Vice-Chancellors Committee). She became President of the AVCC just post the Dawkins period. One of the big issues at that time concerned the development of criteria on which to base research funding for universities. Indeed, the first big moves around research performance including research quality audits came under Fay’s chairmanship of AVCC. She needed some backup so I did a certain degree of work in that space and gained some national profile for that work. I was on the ARC and soon after, probably two years before I finished at UWS, I joined the Council of the Academy of Science as Secretary, Science Policy.

My thinking was certainly evolving and there was an increase in sophistication in our planning and strategy development. Robson and I led the development of the first formal Operational Plan for the University, and we began to think more sophisticatedly, of how we should position the University, what was the story we would tell. I can remember we always used to say – UWA was doing international research that benefitted West Australia. It was a simple but powerful message building on the importance of UWA to WA.

When I said external, did that include West Australia industry and the various parts of the West Australia Government's economic development department?

It did and increasingly so. Some of the first cross-institutional initiatives in WA came from that period. A few were before my time but not very successful. I had to help get rid of a body called the West Australia New Materials Centre which was almost going broke. But then in partnership with, particularly, Curtin –

Working with CSIRO in Western Australia

And CSIRO was particularly around the minerals area.

And CSIRO, yes, and I worked closely with Bruce Hobbs and Steve Harvey. Together we established iVEC which is now the Pawsey Supercomputer Centre and, along with Barney Glover, Curtin, we were the architects of the beginnings of what later became the energy cluster. Bev Ronalds was Professor of Oil and Gas Engineering at UWA at the time and in some ways followed me into CSIRO, as chief of the Division of Petroleum and ultimately a group executive.

Bruce Hobbs was a close collaborator on a number of projects. It was position in the CSIRO executive that I ultimately filled. One of the initiatives that Bruce and fostered along, again, with Barney Glover from Curtin was the John de Laeter Centre and I think it is a good example of how I tried to see the bigger picture and rise above petty institutional rivalries.

The John de Laeter Centre was—and I think still is—a joint centre at Curtin but involving UWA, Curtin, CSIRO and I think the Geological Survey of WA. Its objective was to develop and deliver high quality mass spectrometry for the geological and material research. It operated on a basis of institutional shares and not too surprising UWA had the biggest demand that resulted in people at UWA getting put out because they reckoned that other people's use was 'lower quality'. So, when a new SHRIMP came up they said, we've got to have the shrimp here. But I said, "No, we're going stay with the Curtin site" because that made most sense particularly with regard to technical support."

Can you just say what a SHRIMP is?

A SHRIMP is a special sort of high resolution mass spectrometer—an ion microprobe. Those earliest days showed me that you could work across the divides. But you could also see the emergence of tensions in the West. I'm someone that always believed that the failure to merge Murdoch and UWA was a significant opportunity lost not just for UWA and WA but for in fact Australia I think.

And I think that my discussions with Peter Boyce indicate that Peter thought that too. And he was very disappointed.

I think a lot of people were. The merger had been so close and opportunities were lost. Agriculture is a great example. At the time, the Waite Institute (in SA) was just beginning reorganise itself. If the new building that was ultimately built on the UWA campus had been instead built on the Murdoch Campus as envisioned, Perth would have really been the major Australian and a global centre of Mediterranean agriculture.

Can we just get back to, briefly, about your activities within the University of Western Australia? Were you involved at all in developing plans for the research, or developing directions for the research within the departments and the areas, or was that still pretty much left to investigator curiosity?

I think the strongest drivers were individuals but at times there were opportunities that you could foster. It was also the beginning of programs such as the Key Centre Program and the ARC Centre of Excellence Program which required a more coordinated university response. But the power was still very much with the researchers.

What interested me, particularly, was the way that locality was driving some of the best research. Mark Randolph is a classic example. Randolph and the civil engineers who were working on the North-West Shelf were working on a globally significant project. I remember saying to Mark, when I first visited his department, that if he'd told me they had nothing to do with North Sea Shelf then I would have said nothing else would have convinced me they were an internationally significant civil engineering department. But they were and primarily on the back of research relevant to the North-West Shelf projects.

So, and without being conscious about it, UWA was showing to me the power of high quality research to deliver national even though that may not have been necessarily the intent. However, by the late 90's it was clear we had to do more. So, under Schreuder (VC from 1998 to 2003, succeeding Fay Gale) Pro Vice- Chancellor (Research & Innovation) and set up a unit, the Office of Industry & Innovation (OII), under Andy Sierakowski to more proactively translate UWA's research and lift skills in the University to do so.

So, he was the sort of technology transfer officer?

Yes but with a wider brief to develop relevant skills and assist me formulate effective policies. However, there was a lot of learning by doing. The story of Paul McCormick and Advanced Nanotechnology is a great example and one, I think, with some lessons that remain relevant.

Paul McCormick was Professor of Material Engineering and Director of an ARC Centre of Excellence in advanced materials—RCAAMP. I first met McCormick when he walked into my office probably about a week or two after I was there and said, "You're coming to Melbourne with me next week, Michael." I said, "Who are you and why am I going to Melbourne with you next week?" He said, "We're going to get a patent back from Rio."

Paul's interest was mechano-chemical processing using ball mill technology, a bit like Terry Turney in CSIRO. One of his patents had been assigned to Rio and Rio wasn't doing anything with it. Paul wanted it back, and so we went to Melbourne had an interesting discussion with Rio and ultimately got the patent back.

That patent and a few others became the basis of a spinoff—Advanced Nano Technologies (now ASX-listed Antaria.) But again we did that by developing the policies and procedures as we went along. McCormick, Street, and an external collaborator – Frank Honey was his name – had set up a little company and -

Frank Honey was a contemporary of mine.

I thought he might have been. Anyway, they'd set up this company and they approached me about a licence to this company of McCormick's ball mill technology patents. I couldn't see any reason not to so we signed a simple agreement that if they got financing UWA would assign the patents. They then had some discussions with a local businessman, John Poynton, who came to see me and said that it was really interesting technology but we needed to spend some money to do a proper business plan and various other bits and pieces. So, I said to him, "What's that going to cost?" He said, "Oh, two or 300K." He said, "I'll do it for you but I'd want 30 or 40 per cent of the company and that would be the dumbest thing that the university ever did."

So, I now had to find \$300K. I went and talked it over with Fay who told me of some funds that UWA had got from a sale of some IP –I don't remember what—to Telstra. She said, "I'm happy to let you have it but please give me a policy that will allow me to spend it the way you want." So, I wrote a policy out for her, took the \$300,000, commissioned Poynton, but I took 15 per cent of the equity in the Honey company.

To cut a long story short at one point that was a very, very profitable outcome. Later they floated it as Antaria and it went nowhere except down with the board making some very silly decisions. It is still around with a share price around 2c! In fact, it's suddenly gone up again. But it was again, learning as I responded to individuals coming to see me.

So, with the creation of the Office of Industry & Innovation and the appointment of Seirakowski who came to UWA from Kodak, we began more proactively to facilitate technology transfer and commercialisation.

Did he precede you or did you appointed him?

Andy was my appointment. As I said I had come to the conclusion that we had to do more in that space

That's a huge learning curve to go from being an academic mathematician to knowing about all that stuff and not getting screwed by the first business person that -

I was a bit lucky, and John Poynton on the McCormick deal was very supportive and we had a really good lawyer that believed that -

A lawyer employed by the university?

Yes, She was the legal officer for the university but believed her job was to enable what I wanted to do and keep me out of trouble, and not necessarily -

Not block?

block it. And so, we had a good working relationship. You might as well hear one of the more interesting little sidelines of that time that ultimately became the Sirtex case in court. I was PVC Research and I did sign a letter assigning to Bruce Gray the University's interests in his liver cancer technology but on a draft, found in discovery for the Sirtex case, an insertion in my handwriting "on the evidence you have given me". Later in the court case, before Justice French, I was asked why did I write on this draft? What advice was given to you? Who told you to write that?

And I said, "For crying out loud, that's 10 years ago. I have no idea why it was written." But of course, it was quite important since it turned out there was quite a lot of things -

You'd been misled.

A: - Yes, well certainly not told everything by Gray and quite a lot of that misrepresented. But typical of the time.

Did Andy Seirakowski report to you?

Yes. At that time, we were beginning to move to a more aligned structure. I think the Research Office still reported to Peter (Curtis, Registrar) but I took the Office of Industry and Innovation and we took some of the income we were finally generating and set a Pathfinder Fund to assist the earliest stages of commercialisation. And Andy brought a broad industry background that was very useful. As a result, we spun out quite a few companies in that period.

So roughly how many staff reported to you, maximum, at that time?

In those days – I had the animal laboratory guy, which is another long sad story. I did cut my teeth on some big problems. Probably three or four. It wasn't a big exercise.

So, looking back on that UWA experience what do you see as your achievements there?

At UWA, I think a number of my initiatives persisted which is pleasing: the Office of Industry and Innovation, a more strategic approach to research planning and a Graduate School. UWA, like most universities at that time administered PhDs in the faculties with someone who spent two per cent of their time on that administration so things started to go wrong.

So, I pushed, cajoled, influenced and ultimately organised a cross-university graduate school with again, some hiccups. The first dean wasn't very good. The next dean was Cheryl Prager followed by Robyn Owens, both of whom went onto other significant leadership roles; Robyn as DVC(R). And so I think part of my legacy are the people who began to emerge at that time.

I take Antaria as a successful example of commercialisation notwithstanding its subsequent performance on the ASX. I would also point to a number of important cross institutional initiatives, for example, the founding of the West Australian Telecommunications Institute, IVEC and the beginnings of the energy alliance. I like to think I was looking beyond UWA to what we might do in WA if we're a little more open and collaborative than we were.

Was Alan Robson ever the Vice Chancellor when you were there?

No.

That was later?

Alan was DVC and most of us actually thought he should have become VC, when Fay finished. But Council appointed Derek Schroeder and Alan served effectively as -

As the operating officer?

- Yes Chief Operating Officer or Provost and – the record's pretty clear. One of the great things I learned from Gale, and I'd put Gale as one of my mentors, is that she expected openness from her executive team. It's probably not an appropriate description but there was a little bit of a mother hen in Fay, for us. Certainly, Alan and I were younger than she was. She had no partner at that stage. So, we were in some ways family.

She wanted us to be open in the same she was open with us both about her agenda and I think she genuinely liked us. She was a great believer in acting with some certainty. She would never act if she had 51 or 52 per cent support, but if she had 70 per cent support she would act. And only once in my recollection did all the 30 per cents coalesce and Fay said, "Don't worry, they're only together because they hate me. They'll get over that."

She was an action orientated person but politically aware and very open. We knew what her agenda was. We could question it. She didn't want us to be "yes" people. We were perhaps, at times, subject to 'group think'—we all saw the world the same way. Later VCs I worked with were not as open as Fay so you got into a train smash because you thought that's the way they wanted to go, supported that and suddenly it all fell apart.

So, you were at UWA from '94 to 2002.

Yes.

So Deryk Schreuder was the Vice Chancellor towards the end of your period?

Yes, Deryk was VC towards the end of my period, yes.

And did you involved at all with Council, the higher-level politics?

To a certain extent, yes. There were things swirling around the university at that time. The Rindos affair. There was a scurrilous magazine called *Rumpus* (that I think Alan finally worked out in hindsight who the author was), that believed that the university was going to the dogs. I was part of that change so that I was often pilloried, although at one time there was a letter written to the *West Australian* by a senior figure in the Department of Political Science, Paddy O'Brien – who I actually knew quite well. I quite liked Paddy. But he wrote this letter saying he knew what was wrong with the university and the way he would fix it would be to get rid of the Deputy Vice Chancellor, the executive deans and make the VC a part-time position. The only person in the senior executive that he didn't mention was me and for a fleeting second, I thought Paddy actually thought I was valuable and then I realised he probably didn't even know I existed. So, you couldn't avoid -

Politics?

Yes and the forces that reacted to change. UWA, in that period was still a somewhat macho society. Fay had had a crisis with the Weld Club because the Weld Club had an automatic membership for the Vice Chancellor of the University of Western Australia but the Weld Club also didn't take women members. They solved it by creating a special membership for Fay: you can imagine what Fay probably told the Weld Club!

But it sounds as though the Council was not actually something which was actively engaged in university business.

The Chancellor when I was appointed was Justice Kennedy who'd actually led the WA Inc. inquiries. And yes, you'd be right – councils did not, I think, in those days really act as a governing board and I'd had some interactions with the ANU council. Both councils, in contrast to the South Australian universities, such as Flinders, consisted of a mix of government appointed members and members elected on a representative basis.

So, they were not really configured in a way that encouraged them to be really strategic. The influence of government appointees both at ANU and UWA was mixed and depended very much on who was appointed. I can remember at the ANU Ruddock was there for a while and was good. In contrast we had Senator Colston who got into trouble over travel. At UWA, I am aware that the government of the day balked at certain people that were suggested

On the other side, the staff elected members certainly saw themselves very much as staff representatives. In fact, at UWA one of the staff representatives on Council was in the Industrial Commission (or whatever the appropriate body was), challenging the university about his study rights. So, it was not a body that was designed to be a -

A governing body?

Yes- a governing body. That is why one of the interesting things I learned when I joined CSIRO and interacted with the CSIRO board, was the strategic role that board could play. It also helped at Flinders that South Australia is the only jurisdiction that does not have government appointees on university councils so that council could act much more like a board. However, at UWA I would not have seen the council as particularly relevant to the governing of the university in a strategic way.

In fact, Schroeder at some stage wanted to make some changes to Council. In addition, I can remember Robson and I saying to Derek, "They're not stopping us doing anything, Derek. So, why waste our time doing it." Not surprisingly, trying to transform Council consumed an awful lot of time and effort and didn't really solve anything.

Didn't matter, yes?

Yes – and certainly not compared with where Councils are today.

Thank you.

Also in Western Australia, most of the people who were on the council tended to have been graduates of that university, so it was a smaller community.

Yes, a small community but also a very powerful community when harnessed well. The Business School drew on people like John Poynton, Mark Barnaba and other people. The way that Frank Honey and John Poynton acted in the APT arose in part from the fact that they were UWA alumni. Indeed, there was a very significant coterie of people in WA who believed in the value of education because of their own transformation. As I think I mentioned to earlier when we were chatting before the interview, we once had a reunion of the Hackett Fellows. These were graduates of the University of Western Australia who had been given a postgraduate scholarship to go somewhere in the world. It was an amazing collection of West Australians—justices, lawyers, business people, academics—most of them first generation people to university. The sense of what a university did in a community certainly was something I appreciated greatly from my West Australian days.

So, in 2002 you became the Executive Director, Science Planning at CSIRO. How did that occur?

Probably a couple of events led it. In 1997, I had gone onto the ARC and began to take an interest in research policy and science policy.

Who was the head of the ARC at that point?-

- Max Brennan but soon after I was on the Council and chair of the Research Fellowships Committee he was replaced by Vicki Sara. Following Vicki's appointment, Ross Milbourne, an economist who chaired the Research Grants Committee, Peter Robinson and I worked

very closely with Vicki on the transformation of the Australian Research Council to its current structure as an independent entity within the education portfolio.

Engagement with Science Policy issues

So, I had become involved with those sorts of issues. Then I joined the Council of the Academy of Science and then was approached in 2001 to be its Secretary, Science Policy. I took that on and that led, in particular to leading the Academy's engagement with the 2002 National Research Priorities, which was the attempt by the Howard Government and the then Science Minister, Peter McGauran, to impose national research priorities across the nation.

This engagement led me to meet people like Joanne Daly who was leading the CSIRO response, and I came to the attention of the new CEO of CSIRO, in Geoff Garrett. I even gave a fairly well-regarded Telstra address (although I have never had the nerve to watch it), as part of that research priority exercise.

I rather liked what Geoff was saying about CSIRO. He'd come out to Western Australia in his early days as chief executive, where I'd already been interacting with Bruce Hobbs and other CSIRO people in WA. So, I was somewhat surprised but flattered when he, or someone from him asked if I interested in succeeding Bruce on the CSIRO executive. I think they advertised it as a Deputy Chief Executive, Science Planning & Strategy or something like that. Anyway, I remember in the interview and Geoff said, "We've decided to change the name, does that worry you?" Names had never worried me particularly and the focus science policy remain, so I got the job.

I was also I have to admit I was a bit bored. I didn't really quite know what I wanted to do. I'd been a DVC research for nearly 10 years. For some reason, I didn't actually think a VC was quite what I wanted to do, at least if I had to take a route which took me out of a research intensive university into a second tier institution, which seemed to be the route that people had gone. Schroeder, for example, had come from Western Sydney to UWA. I probably could have had another term at UWA if I'd wanted to but as Jeanette said at the time, "You want a change." So the offer from Geoff to become what was finally called Executive Director, Science Planning came at an opportune time.

Executive Director, Science Planning at CSIRO

So, you shifted house to Canberra?

Yes, we came back to Canberra. It was made pretty clear that the expectation was for the position to be based in Canberra. Bruce had done the bulk of the work out of West Australia but I think there was a sense that you had to build relationships with the Canberra bureaucracy.

So, were you succeeding –Bruce Hobbs, did he have a similar job to that before you?

My understanding at the time was there was a bit of reshaping of the role (possibly Terry may even know more) but certainly in his role in Western Australia I had been interacting with him around the strategic scientific planning for CSIRO, where the big issues might be, and the emerging issues of science policy spaces and things like that.

The characterisation of the role as first put to me was a little like a 'CSIRO Chief Scientist' in a championship type role, although Geoff walked away from saying that explicitly. There was also the explicit expectation that the appointee would engage in the science policy debates of the day, which was a role that I think Bruce had to some extent played. But I've never really sat down and analysed precisely what Bruce did. Anyway, Bruce went off the executive team and I came on.

So, in 2002 you came to that position. That was after the Backing Australia's Ability report and the "crisis" in the organisation that CSIRO didn't get any money, or not very much money anyway, allocated through Backing Australia's Ability. What was your impression of the effect that that had on the organisation, and can you just reflect a bit on that time in CSIRO?

Not too surprisingly, most of my colleagues in the university thought I'd gone nuts. Why on earth were you joining this organisation— the best thing you could with CSIRO was the view put to me from most people outside the university was to carve it up.

Most definitely, yes. Outside of CSIRO?

Yes, carve up CSIRO. For example, in Western Australia the only debate would have been whether it was just Curtin or a joint venture between Curtin and UWA to take over the Mineral Centre and the Energy Centre while UWA took over Floreat.

However, for some reason, and to be perfectly honest I don't know why, I found Garrett's view that he was strongly articulating around the country, that CSIRO had an important role to play in the nation but that it had to some extent lost its way. Backing Australia's Ability was, to him, a wake-up call or should be a wake-up call, but it was up to the organisation to change itself or change would be thrust upon it, and it would be thrust upon it in ways that we in CSIRO, wouldn't like.

Or perhaps put more positively, in ways that would not advance Australia. I'd come out of the National Research Priorities debate with a view that it was worthwhile Australia setting some national research priorities. I was worried—and if you're interested I can send you my Telstra address – that setting those priorities should not prevent high quality basic research being achieved. However, I think I'd seen enough in Western Australia with people like Randolph and McCormick and others that if you tackled big problems for Australia you did research that no one else could actually do.

So, I had come to a view that basic and applied research were not in any sense opposed to each other, nor did I believe that basic and applied should be ranked. There was good and

there was bad research. You could see in the work of McCormick how basic mechano-chemistry lead to Antaria. So, I think I signed on to a view that this organisation which I had known for quite a lot for a long time-

Since you were a little kid.

- was worth actually worth trying to save. There was also an intellectual challenge in joining the organisation at that time of its journey. I'd met Graham Harris and been to some meetings around the emerging Flagships and I liked that sort of big picture thinking that was beginning to come into the organisation.

And so did Geoff recruit you then?

Yes, Geoff definitely recruited me. Although, as I learned later, there were various processes in Geoff's recruitment style: a few dinners, a few meetings, you're told to see X, you're told to see Y. Someone from some consulting firm meets with you in a Qantas Club somewhere and asks you a whole string of questions, and somewhere along the line they come up with a decision to offer you an appointment.

In a sense I can't say, I actually even applied for the job in CSIRO but as I said I was certainly by 2002 beginning to look at other opportunities, but certainly Geoff was very much the influential figure in recruiting me into the organisation.

So, in your job as Executive Director Science Planning how did you interact with the, first of all the flagship programs, the divisions, the research capability, and what sort of external role did you have with the Government, the chief scientist – Robin Batterham was still the chief – what was the sort of scope of your role?

It evolved over the period running up to 2006 when I became a Group Executive. But it had probably three significant streams to the role, and possibly four. There was a sense of championing science within the organisation. I think that it's a fair assessment of the CSIRO that Geoff inherited so say that, with a few exceptions, science for science's sake had been subsumed and therefore celebrating science for science, encouraging high quality science to be published – with due respect for commercialisation opportunities – was not something that was seen as imperative to the organisation as I believed.

The counter example, of course was Plant Industry under Jim Peacock, but that was in many ways the outlier amongst the divisions. A more typical situation was down at Clayton where fascinating research in materials was being done, particularly within the Boeing collaboration. I can't remember who led the program—a young Chinese woman, I think—at the time. Interestingly, and I think it said something about the culture of the time; they forced me to sign a confidentiality disclosure agreement before I went and talked to a science group inside CSIRO. I thought was a bit odd but I signed it, and she described how this material was setting new standards for flammability, because one of the issues which

Boeing had was flammability. And so, she described this test on which these new CSIRO materials were showing tenfold improvements.

I said, "Have you published any of this?" And she said, "Oh, no. Boeing wouldn't like us to do that." I said, "Have you ever asked Boeing whether they would like you to do it?" And she said, "No." And I said, "Why don't you ask Boeing?" Of course, the end result was Boeing loved it to be published because it added credibility. What also amused me was that the next year in a typical CSIRO response she made as the key performance indicator for her group to produce N publications. In the end they produced something like 2N publications!

But I do think a mind-set that to do research which was important for business, etc, meant you couldn't publish, was far too endemic inside the place. So part of my role -

What you're describing is not the CSIRO that I was a part of in the '90s.

No.

So it must have occurred, in fact, must have devolved in some way.

The external earnings target and particularly how that was implemented and flowed down too often to individual scientists was a bit part of that evolution. I remember talking to one of the chiefs about this problem— he was in exploration mining. There was a ton of stuff sitting on this person's shelves that could have been published. But the real issue was there was no time and capacity to do take these reports and translate them into scientific papers.

At the same time a big pressure on the organisation was the perception from outside – particularly the university sector – that it was a recipient of \$600 million-odd without going through competitive process, so ipso facto that was lousy science. But anyone who'd been in the institution, and even as short time as I had, realised the true depth of the science that was in CSIRO. The challenge of 2003-4 was to demonstrate this and to do so in a way that supported the emerging strategies of CSIRO particularly the Flagships.

The first challenge was the McGauchie review into collaboration between the public-funded research agencies (PFRAs) and universities. Under Geoff's typical very direct interest, I led CSIRO's response. This was an important review. However, probably the most important response to the criticism that CSIRO science wasn't as good as everybody else was the science assessment process. I have to admit that I regard this as probably my most significant legacy on CSIRO. On completion this demonstrated very clearly the high quality of CSIRO science.

We have the board paper that went in 2005 outlining the science investment process. So you were the, one of the instigators of science investment program?

Well, the instigation of the science investment program (SIP) was something that Ron Sandland led-

Ron Sandland was the Deputy Chief Executive?

Yes.

But you were part of that?

I was, but SIP was separate from and built upon the science assessment process that I led and developed. Personally, I do not think the importance of the science assessment process that I established can be understated. It came at a time when research quality was a hot topic in the research sector and particularly the university sector which was looking at a formal Research Quality Framework (RQF). Indeed—in whatever year it was – that was my key performance indicator to Garrett, or rather to keep CSIRO out of the RQF which had an emphasis on university-type ‘investigator lead research’ and not the more mission directed strategic research which was where CSIRO was strong.

The RQF had its antecedence back in my days in the university sector, when a publications index entered funding formulae. The problem was that there was no general acceptance of the index, or at least the raw data that went into it, and the universities kept telling the Government that the data needed to be assessed for ‘quality’. Not too surprisingly, the Government decided to assess it and set up a working group to help design a Research Quality Framework to do that. I was the CSIRO representative on the Working Group but Geoff’s brief to me was to keep CSIRO out of it.

Yet it was pretty clear that we needed a more substantive response and that led to the Science Assessment Process. It wasn’t that CSIRO didn’t do some assessment, but from a governance perspective there was a) no expectation on every chief to do it, b) there was no expectation that if the chief did it he or she would do anything with the report, and c) there was a great risk that if the chief didn’t like what the review said it would get appropriately buried and no one would take any action.

So, I was resourced and given a few other people in a small group to sort out what might be best practice in this field. By that time, I knew enough from my ARC background about research assessment, to look at models around the globe that might suggest what we should actually do. I do feel rather proud of what we achieved because, I think, we found the right balance in the science assessment process between the two dimensions of CSIRO’s science activities.

One dimension was a genuine assessment of how significant, scientifically, the work of the group was—and we focussed on the divisions. At the same time we wanted to have an analysis of the significance of that research from an impact point of view. More importantly, and this also got picked up later in the SIP Process, that while the commercialisation value, if you like, was an indicator, we were much more interested in how partners actually saw the significance of that research. So, for a group to get a level 5 on that impact side, partners had to effectively testify that the research was mission critical to them.

For example, I can remember talking to people at QCAT about iron ore processing and the way that dumping iron ore into Chinese blast furnaces wasn't easy to do because of the high purity. So, you had to do research to enable them to do it. So, you had the iron ore companies saying that the CSIRO research really enabled them to do sell the iron ore successfully to China. That group got a 5 on impact and from memory a high score for science excellent because it required very innovative science.

Having set up the Science Assessment Process I then commissioned external reviews of the all divisions over a three-year period. I would then take the review reports to the board and every one went to the Minister then. I think they had a real impact on the organisation and certainly politically dealt very effectively with the criticism of CSIRO's science quality.

Leading CSIRO's information, manufacturing and materials research

So, Michael, that process occurred between 2003 and 2006 and then you became the Group Executive, Information, Manufacturing & Minerals.

Yes.

Was that a smooth transition?

Yes.

Did you succeed Rod Hill?

Yes, I succeeded Rod. I think it was a smooth transition. I had been involved at ET-level with many of the key strategic issues impacting the groups. We were beginning the environmental scanning processes at ET so, with people like Ian Elsum we produced these annual reviews of emerging areas of science that we might invest in.

And science, had you started the science health report?

A: Yes, that was my initiative -

So Jack Steele's group still does that.

Good to hear! Its origins are worth a comment. When I joined ET, the only indicator that went to the board of CSIRO on the science health was citations per head. I remember one year we all lost part of our bonus because we'd said we were going to have X citations per head and it turned out to be X minus a (small) delta X. I said to Geoff, "That's the most ludicrous indicator because the only way I can fix that is to give me a time machine so I can go back and stop some papers!

So I advocated for the science health report, which still included citation data. And, I have to admit the year that after I left CSIRO I was rather pleased to I read in the CSIRO annual report that for the first time the citation index for CSIRO had exceeded the ANU. And so

again, there were indications of the quality of the science in the organisation but until the science reviews and the annual science health report, we were never demonstrating it.

So, the science health report was really an attempt, again, plagiarised to some extent from what other similar organisations were doing, to give a broader assessment of the quality and the health of the science in CSIRO and help make decisions to improve it. The responsibility of acting to improve science operationally fell on the Group Executives. So, when Rod left I said something to Geoff like, "It would be quite interesting to be Group Executive," because I was quite interested in how you actually made it work. Moreover, I said there were some issues in the group that I thought needed someone who had enough knowledge of the relevant science to help make some intelligent decisions. To give Geoff credit, he decided to take a punt on me as a Group Executive.

I still remained part of ET but took on the operational side of trying to see how these big strategic moves could be made to work in the day-to-day demands of staff and dollars of external learnings, bloody legal people, and so on and so forth.

And did you enjoy that?

Yes, I did. Not for the first time I found people that seemed to both accept of my vagaries and at the same time broadly commit to what we were trying to do. There were some very big issues in the group. The Square Kilometre Array (SKA) was in the portfolio at the time and beginning to emerge as a critical issue. Again, I could see people beginning to emerge. Indeed, if I look back on my career I'm pleased with many of the people I've touched in various ways.

Calum Drummond is a good example. The Chief of Industrial Physics (CIP)- SIP became vacant and Geoff said, "The person you should get is Calum Drummond." I said, "I have thought about him. But Calum had said 'no bloody way'." Geoff told me to have another talk with Calum. In the end I got Calum to be chief and I think— a little like me — he found there was something interesting to do in trying to drive these things. So, I think I had the view that my job as a Group Executive was occasionally to reign people in but more often to try and create opportunities for them to develop as the next generation of CSIRO's leadership.

I brought to the position a pretty broad knowledge of the organisation by then because I'd done all the science reviews. I'd been on the Flagship Implementation Committee and I understood what the Flagships were trying to do. I don't think that was always understood by a lot of people. So the role did bring a range of issues from science, people, external partners and commercialisation. I particularly enjoyed working with Raj Rajakumar and the Light Metal Flagship around what I thought was the very exciting initiative in titanium.

Were you well supported from a staff point of view in that second function?

Yes.

Who do you recall being important -

I recall a series of good, effective EOs that helped me in various ways— I've forgotten some of their names— but they played an essential. There was a small secretariat of which the most significant ones were probably Ian Elsum, Les Rymer in the science policy space.

So that was in Canberra?

That was in Canberra.

Did you do the Group Executive role from Sydney?

Yes, in the Group Executive role I moved to North Ryde but then with just an EO who for some of the time was still down in Limestone Avenue. That was in part because the role really wasn't in North Ryde. Most of the Group wasn't in New South Wales. It was in Western Australia or Queensland or at Monash. So, I spent a lot of my time on a plane.

Was it your wish to go to Sydney or did the CSIRO impose that on you?

Geoff said I could have a choice of going to Clayton or to Sydney. We really had no connection to Victoria -

Melbourne.

Yes, so I said we'd go to Sydney.

So, you shifted house to Sydney?

Well, at that point, we'd bought a house at Port Macquarie thinking about post-CSIRO retirement and Jeanette said, "When you get settled we'll find an apartment in Sydney. Meanwhile I'm going up to Port Macquarie." So, for all of my period as Group Executive I actually lived at Port Macquarie. But really, location didn't matter too much and we were beginning use technology more, such as video conferences. However I did spend time in Clayton or Western Australia or wherever the Group operated.

I have never believed that you manage people by sitting in an office. You must get out – particularly if you've got a difficult message—and talk to them. So, I wanted, say Vicki Tutungi (Chief, CMIT) to do something, particularly if a bit challenging then it's best to talk face-to-face rather than sending an email which may get misrepresented.

Changes in CSIRO

Can I just ask you a question from my own reflection on CSIRO when I joined the board in 2008. It seemed to me that two things had changed. One was that the organisation had become highly bureaucratic, so it seemed to be a process driven organisation much more than I can remember it from when I was a chief. Secondly, there was this discussion

amongst the officers of two classes of research, CSIRO research and research for our partners, which I, both of those things I found to be quite odd. How did that happen? How did the notion of some research being ours and some being someone else's fit with the notion that CSIRO was doing research for the benefit of the nation? And secondly, did this process that you were involved with which in a sense was meant and did help the organisation, how did it become so bureaucratic? Or am I wrong?

No, I don't think that you are wrong on the bureaucratic aspect. Although, I'm not sure that it would be fair to say that the science assessment process itself was bureaucratic. I think to some extent what you might be referring more to, and I will talk about that a bit in a moment, was the emerging of what was called SIP, or the Science Investment Process. I think that the science assessment process, the science reviews, were – and I think this was genuinely fed back to me – was a real opportunity for the quality and the significance of the science that was done in the organisation to be both promoted and even brought together.

Now, how that was handled in the individual divisions could at times begin to become more complicated. I remember one division, Exploration and Mining I think, from memory, produced a massive volume of paper that the poor review team had to deal with. But for the most time this was the first time that many of the divisions, in my assessment, had actually stopped and thought about the science they did on the two dimensions that we deliberately wanted them to look at. And I think having done so the findings surprised people.

I think the organisation even found things that surprised them. Paul Cleary's work in Clayton on particle methods is a neat example. This had gone from modelling ball mills and flotation plants through graphics to beer frothing and a video on sinking of the Titanic. This diversity (and Cleary wasn't the only example) of application of a core scientific capacity was I think a rather amazing discovery for the organisation.

However, I would have to agree that one of the things I learned from my period in CSIRO is that, if you're not careful, in an organisation, like CSIRO, which probably had some tendency to become overtly bureaucratic, a real risk is you begin to be process driven and lose sight of what you really want to come out of the process. In the science reviews, the detailed and lengthy face-to-face interview, even conversation, between the expert review team and the divisional scientists was vital. This allowed the review team to look behind the paper work and focus on the science.

In the case of the Flagships, life did get more complicated. I actually think the flagship implementation plans were a great concept, particularly the idea that you differentiated between scientific milestones and impact or engagement milestones because you needed to understand the steps in getting effective translation. Translation is certainly not just chucking ideas over the fence. So, thinking about how this might occur ahead of doing the research and building some specific indicators and associated milestones indicators into project planning, I think was some of the most radical thinking I'm aware of in science.

Potential problems arose, I think, if the requirement for that sort of thinking was propagated too far down and if it was formalised too much. At the same time, it was important to work out how you created some capacity for people to do something slightly out of the ordinary. This led to a debate on capabilities, basic research and its timescale of basic research. Those issues got too codified in my opinion in the processes that flowed through the latter part of the SIP process. As a result we lost to some extent, what I think was the most significant aspect of what we were trying to do.

Your second question – I understand—but I would never have thought of that until you mentioned it. However I can possibly see the seeds of that being planted in the way the organisation thought about the different types of research—particularly the so-called CSIRO house—but it is not a distinction I made in either of my roles.

I think that is correct because, there was a tension within the organisation between people in the group that you led who had a much clearer understanding of the link between the impact and science than in some other parts of the organisation.

I think that's true. What was interesting to me was some of the emerging thinking we had around the social side and the policy side. I can remember a fascinating discussion, probably as part of SIP, comparing pathways to adoption on the social/public policy side of the house with more conventional commercialisation pathways. I was part of the discussion but I don't think I was primary author. Indeed, I don't really know who the primary author was. In commercialisation, we have always recognised that one of the differences between incremental innovation and more disruptive innovation is the capital required to scale up.

When you started to think about the policy side and/or the public good side there is a parallel construction which I now tend to talk about as the social capital required. If you're working within existing regulatory policy frameworks to deliver incremental innovation that's one thing, but if you want to do something that potentially disruptive you've got to change regulation or change policy. And, that can be as tough a task as getting say \$100 million for commercial scale up. I admit that much of my thinking here has also come out in my own work post-CSIRO.

And that actually came out in one of the ACOLA reports that you and I were both involved.

Yes, I now think it's a very important part of the mix required for effective research translation.

Can I just sharpen Tom's question a little bit. My recollection the primary person responsible for that dichotomy in thinking about CSIRO research versus somebody else's research was Mike Whelan. Was Mike Whelan active when you, or influential when you were there?

Yes, Mike was an ET member when I joined and was still there when I left and had a number of evolving you. To be frank I don't recall us ever talking about it as explicitly as you have said. Certainly, Mehrdad Baghai argued very strongly that CSIRO did own significant

intellectual property that we needed to deal with better than we have done in the past. And, and as you would well know, we did in the 802.11 case.

But no, when I left one of the main ‘research debate— and this may have in part led to the dichotomy—concerned the whole notion of capabilities and how did you maintain and develop the capabilities CSIRO would need in the future. In my initial role as Executive Director, Science Planning I had been responsible for the Emerging Science Program and had written some of the reports led to the first attempts at platforms of various sorts. Both of these initiatives were aimed at building capabilities and did recognise that they might generate IP which was CSIROs, like 802.11. In contrast, the Flagships were very much focussed on partnership and recognise that partners would often ‘own’ the IP. At the same time there was a tension being emerging between the flagship program and the divisions as to the extent that the Flagships should contribute to capability development and to what extent did long-term aspirations of a Flagship dictate the sort of capabilities we had or needed.

But, I wouldn’t have said that the issue was put quite as bluntly as you have done in any great extent in 2006/07.

That’s fine. There was one extra question that I’d like you to comment on and that is Tom and I are managing this history project and it’s occurred to me as we’re talking that that series of reviews that you did when you were in charge of Science Planning could actually form a very good snapshot of CSIRO over those years. And so if you wanted to get a picture of what CSIRO had been doing, what its thinking was, and what it decided to do that we could put somebody, maybe a PhD student onto having a look at all of those reviews as a group and saying well, this is pretty good evidence of what was happening actually and what the thinking was in the CSIRO at that time.

That would be fascinating. They’re all in the public record. One little indicator of their durability: I was invited to back to chair the second review of the Division of Mathematical and Informational Sciences (CMIS). I was VC at Flinders at the time so that would have probably been ‘08/’09. Louise Ryan was the chief of the division at the time. Frank de Hoog was still in the division too. Two things amused me about it. One was the letter I received was exactly the same letter I used to send out. The only thing that had happened was my name was removed from the bottom and Jim Peacock’s name had been put on it. The mechanism and the methodology were quite the same. Although Jim did actually think that I should write the report and I told him I was too busy and I wanted an exec officer. So, I got Marcus Zipper. My experience had been that a good exec officer was essential if you wanted to get the report done in time and Marcus certainly helped me.

I can’t remember who the members of the review team were but one of them was deliberately chosen from the previous review. What was interesting to me was how seriously CMIS had taken the original review, and secondly, how positive the person who had been on both reviews reflected upon what had happened. Not everything said in the first review had been accepted but within the CMIS case I would have to say that that first

review which I'd orchestrated and was chaired by Terry Speed, had had quite a significant influence both on the culture and scientific performance. While there was a new chief—Loise Ryan having replaced Murray Cameron—Frank de Hoog provided great continuity.

So, for that division my assessment would be that the review process had released latent scientific potential and in particular helped profile the scientific excellence of what they were doing. For example, the division was doing some amazing stuff on risk management that was not well known. So, it would very interesting generally to look across the reviews because most divisions would be reviewed twice in the cycle.

I think it would be a good idea to stop at this point.

[music]

Reflections on CSIRO and its place in the National Innovation System

So, Michael we're now back from having had lunch and in this last session of the interview, we'll talk a bit about your reflection on your role in CSIRO. Your achievements as the Vice-Chancellor of Flinders. And reflection on looking back now that you're a sort of Elder Statesman of Australian Science. So, in thinking about the CSIRO that you were a part of, how do you think that the structures within the organization inhibited or helped getting out most of its scientific challenge? What about the challenges of building and changing scientific capabilities in the organization as the needs of the nation changed? Did it have the flexibility? And reflecting back on all of this, what do you think were your main achievements in the planning role?

Okay, if I look back on the nearly five years I spent in the organisation, I think the significance is to me has two dimensions. One personal, in the sense of both development, learning and understanding of leadership and governance, with a sophistication that I had not seen before. I'll say a little bit more about the relevance of that in the latter discussion about Flinders.

The second dimension is really a sense that I was part of an amazing organization; an organization that genuinely is a significant asset to Australia. I was part of a leadership team that was privileged, may not be quite the right word, to be the leadership team at a time of dramatic change, even crisis about the role of CSIRO in Australia in the 21st century. I am a very strong believer that you don't need to get hung up on things that could have been done better. Instead one should recognize that in 2002/3 if CSIRO hadn't been given a massive kick, it would have either withered the way of the Canadian Research Council or have been broken up like the crown research institutes in New Zealand. So, I think history will judge Geoff Garrett as the man that potentially saved the organization from either a death by a million cuts or a be-heading.

That's not to say that everything we did was done perfectly. It wasn't. The Flagships, themselves, were experiments in their own right. I think one of the lessons I learned personally, and I think it took us a while to absorb in CSIRO, is that culture is a very powerful force. If you want to drive change, it is necessary to engage with the culture of the organization. I took that message very squarely into Flinders. I think communication, along with stories, are really an important part of the driver of change. And I think again, that's something that I took away from CSIRO. As Executive Team members we heard Geoff talk about Wayne Gretzky and why he was so good—because his eye was on where the puck would be—very often. We would grimace, yet you'd go out into the organization and someone would say, "Oh my God, I get what strategy is about now." So, you know, I don't think one can never communicate too much if you want to embed a change agenda.

At a more personal level, I do think I helped position the organization through the turbulent times of 2003 and 2004. I think the Science Assessment Review Program would stand up against the best in the world. Partly, because we modelled it on some of the best of the world. I think it was then delivered with a transparency, openness and an engagement that helped people. I think that the beginning of recognition of the importance of capabilities and their health was important. As was the work we did in the run-up to probably the 2006/7 funding process around the emerging importance of materials, synthetic biology and ICT. Those ideas were the first steps and later got reinvented—one of the great things about CSIRO is the ability to reinvent things—as Future Science Platforms, I think they're called. I still think that engagement around the flagships was personally an exhilarating engagement.

Engagement, what do you mean by engagement? Engaged with the outside?

Engagement in a couple of dimensions. One engagement was inside the organization as a member of the flagship oversight committee. As ED, Science Planning I had a role of deliberately going around the Flagships to energize scientists intellectually to think through what a flagship project meant in ways that we hadn't done before. And then as Group Executive, particularly with regard to the Light Metals Flagship, external engagement, which in the end became, I think, a disappointing story with industry, for whatever, reason, seemingly unable to engage at a scale that was really necessary to drive that project to completion.

Can you expand a bit on the culture issue? And so, give us a bit of an assessment of the interaction of the executive with the different cultures within the organization. Let me just interject one Victorian note here, "Richmond won the AFL premiership this year and won one of the leading players in particularly the preliminary final was Maurice, who was Daniel Rioli, who's wearing number 17. The number of Jack Dyer who a long time before Wayne Gretzky summed it up, "Don't be where the ball ain't."

Yeah, I know. Some reflection on that, which one of the things I have thought about a lot. And to give Garrett a bit of credit, he did send us to a couple of courses. One up in Sydney with John Kotter, the great guru of change. I do think thinking through change programs in an analytical way is a very desirable way of going. And that, I think, means understanding

the culture of the areas that are going to be impacted. It doesn't mean that you can't change them but it's at least worth thinking about what the existing culture and its apparent values.

Peter Shergold, when he left the CSIRO Board to become Secretary of PM&C, gave a farewell address in which he talked about culture. Two things I took away particularly from that address. One, quite profound: At times organizations need to celebrate history as history. I think one of the interesting aspects for an organization like CSIRO, is at times, the history is not regarded as history. However, to move on it is important to celebrate past accomplishments but as history.

The other message from Peter was a little soliloquy about the need in driving change to recognise what you can change and what you should not change because it doesn't really matter. He recalled how at one time (and I don't recall what department it was), he realised that he really should change all the parking spaces for the senior executive. However, that would impact so much on the culture that he'd get nothing else done. It's a great message for driving change: work out what really needs to change.

I also found Peter's story rather amusing because, on my arrival in CSIRO at Limestone Avenue, Geoff had given me a little speech about the importance of the change process occurring in CSIRO and how he had decided to abolish car parks for the executive staff at Limestone Avenue. I wasn't fussed by that and had more important things to sort such as an office assistant, even order a computer. All took longer than they should have but out in the parking lot, close to the front door, appeared by the end of my first day a brand new parking spot labelled very clearly 'Michael Barber'. In hindsight, that story probably says something about the limits of a CEO's control!

So, I think that culture is important to CSIRO and there is a definite CSIRO culture. There's also a number of micro-cultures within the organization. I was always very surprised that people would see the way they did business, whether scientifically or engagement, as the only way—"Why doesn't the rest of the organization do it my way?"—without considering the differences between say the chemical industry, the existence of the agricultural R&D Corporations for agricultural research or the financial industry. And, it did surprised me at the strength of some of these micro-cultures.

Consequently, when you started to drive an agenda that was deliberately about building the capability of CSIRO as a unified organization, this was a big guerrilla in the room. I don't think we ever considered that cultural motives of the distinct divisions as much as we should have.

So, what were the challenges that you saw in changing, stopping some things and starting new things?

A: I actually believe that one of the strengths of the Science Investment Process, so-called SIP, when implemented the way that I think it was conceptually set u, was to help this

process. As I saw it, SIP required an assessment of not only the quality and impact of the proposed research, but also an assessment of the obstacles that lay in the pathway to adoption. If those pathways had barriers which were too difficult to circumvent, that, for a research organization like CSIRO, in contrast to a University for example, was a legitimate reason to actually discontinue that research even if, from a pure science perspective, it was very high quality research.

On one occasion, I suggested to Jim Peacock that if we couldn't get rid of the moratoriums on deploying GM varieties, we should stop doing GM agriculture research. Not too surprisingly, Jim didn't agree with that view! And to be fair, Plant Industry put a lot of effort into the effective regulation of GM crops.

One program that I recall we did stop using this type of argument concerned a program looking at automatic traffic management systems. This involved some quite exciting mathematics and operations research. However, it was also very clear that there was little interest from Road Traffic Authorities and almost impossible to see the ideas deployed at scale because of the need for 'smart roads'. Looking back from where we are now, no one, as far as I can recall, thought about what happens if all of the required compute power sat in the car and you had an automated vehicle of the type Google has been developing. So, in a way the group in CSIRO was ahead of its time but didn't actually see where the technology could go.

However, the critical issue for CSIRO was how could you stop research programs without necessarily terminating the people involved. In the early days of Flagships, there was often talk of 'fast failure'. That was meant to imply fast failure of projects, not fast failure of people.

Unfortunately, I think CSIRO over the years has had too much financial pressure put on it with redundancies becoming the main response to the financial stringency. That is very, I think, non-constructive if you want people to change and close projects. Too often, I think the first reaction of management was: "I can get rid of those people because that is the easiest way and they have no project to cover their costs."

Thus I strongly think that a research organisation like CSIRO must find the capacity to fast fail projects without necessarily losing the people involved in those projects. That also means that the scientists involved need to have flexibility. They need to keep up with the times. They need to have an engagement with their own research community. Publication is an important part of maintaining scientific credibility and ensuring science remains contemporary.

We did a science review of food technology. They have, or had, a fascinating project, in which they had heavily instrumented a pantechicon container. They did some great work some of the major transport companies and often repeat business. So, that's a good tick. But companies weren't very expert scientific users. So one of the CSIRO scientists came in and described his mathematical modelling to the review committee. I listen and

immediately thought, "That is the mathematics I taught 30 years ago. The world must have advanced." One of the external reviewers picked that up and asked if any of the work had been published. The response was: "Oh, no I'm too busy doing work for industry". That meant that very capable scientist didn't involve with the science and ironically delivered less value to the industry partners!

I felt was an issue that we were beginning to stress with the science reviews. It became an expectation on chiefs that they were conscious of the need to publish because without their scientists evolving, then fast failure of projects becomes fast failure of people.

And that's one of your examples of different cultures across the organization. You wouldn't have heard that story in Jim Peacock's division.

Yes although to be fair to other chiefs the existence of the R&D Corporations helped but Jim also spent a lot of time helping e.g. the Grains R&D Corporation, understand the need for long term basic research.

So, you now, let's go -

Sorry before we leave that, I had a question about the Backing Australia's Ability problem that was created for the organization and CSIRO's response to it. Did you have any visibility to the government? So, in other words, could you see, or can you tell us at all about how CSIRO was viewed by the government at the time by the Minister, by the Prime Minister, by Cabinet, and how that changed during that period?

Not with Backing Australia's Ability directly since that predated my engagement.

Predated you, yes?

I did have some engagement with the Batterham review that led to BAA wearing my hat in academy. From memory, and I probably have a copy somewhere in my records, I helped authored the Academy's response to that review. And, I guess my actual recruitment to CSIRO could be attributed to the impact of BAA. Geoff Garret, on his own appointment, clearly recognised that BAA, and particularly its view of CSIRO, was something we had to deal with if CSIRO was to continue. More effective relationships with key departments was an important element of our strategy. In fact, for a while I was—Geoff divided up the departments between his executive team—I was the point of call for people in Education, Science and Training (DEST as it was then) if they had an issue with the organization. Sometimes, that was, "Geoff has sent us this letter, could you please get him to retract the letter or rephrase it! Such informal communication pathways I think were essential through that period as we rebuilt the relationships which led ultimately to the success for quaternary funding in 2005/6 of whenever it was.

And did you have to sell the flagship concept to DEST?

Yes, particularly in the run up to the 2005/6 Budget which was 'The Big Sell'. The previous budget was a little bit of a trial which resulted in the Flagship Collaboration Fund to expand collaboration with others, particularly universities. For 2005/6 and later there was a concentrated campaign of engagement, which often changed the language. For example, when I was IMM Group Executive, we were pushing Minerals Down Under, as a new prototype flagship. I remember a meeting probably led by Mike Whelan, with I've forgotten which government departments. We came back with a really interesting reconfiguration of the message. Some of the proposed Flagship research was to enable new difficult deposits to be mined with a general argument about new opportunities. The message after the meeting changes to: "The Government has built into the forward estimates all these revenue, coming from BHP and RIO. You know, you are going to have a big political issue if those companies don't remain as productive and profitable in 10 years' time. Hence guys, here's a solution...."

It sounds like Treasury or Finance might have been the department?

Probably. And it was typical of a smarter engagement that came through that period. Indeed, one of the lessons I took away—and Garret was good at it was: If you can convince people that you have a solution to their problem, rather than, "I want you to solve my problem," then you actually get a lot more traction. And, I give Geoff a great deal of credit for encouraging that that sort of discussion with everybody, across government.

And the other question we would like is a brief reflection from this time in your impression of CSIRO's interaction with the Co-operative Research Centres (CRC) program?

It was troubled. Not all of the problems were CSIRO's. However, there was a tendency to, I think, overregulate the CRC program. -

A tendency for who to overregulate? The government or the CSIRO?

By the program itself, to some extent but CSIRO then layered on top of that, with due respect, legals and a reluctance to share governance. On the other hand, the University sector did not understand, and to some extent CSIRO didn't understand even when I was in the organization that CSIRO and universities operated in two very different funding regimes. So, for CSIRO we had a billion-dollar expenditure, of which six or seven hundred million dollars was appropriation. We had to get some money from the CRC to pay for our contribution. Universities, on the other hand, put in their contribution and through their block funding formula got you resources for the future. The clash of those different cultures, if you want to use that word, was something that was not understood.

Nevertheless, some of the CRC's were, I think, brilliant successes in their great contributions even for CSIRO. It's a program which has matured over a couple of generations. While CSIRO engagement was often problematic, many CRCs delivered even for CSIRO.

Vice-Chancellor at Flinders University

So, we now have come up to 2007 and you leave CSIRO to go to Flinders University. How did that what happened Michael? Who rang you up then?

A search firm with whom I'd done a bit of work before rang me up. She said, "I want to talk to you about the VC at Flinders." To which I responded: "Oh, you want me to nominate some people." And she said, "No, I'd like you." I have to admit that I was surprised. I was coming up towards the end of my term. I hadn't had a serious discussion with Geoff about what the future would be, but I was 12 months into the GE role and quite enjoying it. We were beating expectations. We were still a group in a negative position but I'd promised that would get no worse. In fact, I returned a bottom line which while still negative was not as negative as initially forecast. Thus, I wasn't expecting any difficulty if I wanted to renew.

So, I spent some time over December thinking about it and reading about Flinders. I got intrigued because I thought that it would be interesting to take some of the ideas that I had been exposed to inside CSIRO into a university setting. And South Australia was at that point an interesting development. Rann was the Premier. There was a lot of focus on the resource industry. So, my experience as Group Executive seemed to be relevant. Manufacturing was under a challenge. Luckily, Rod (Rod Hill, my predecessor as Group Executive) had shut down CSIRO's manufacturing operations in SA before I became Group Executive, or I might have had an interesting time in South Australia being responsible for the shutdown of Woodville.

So, I agree to go forward. Indeed, my little spiel to the Council selection committee was around the view of integrating disciplines to focus activities and exploit the new trends in SA. The Chancellor, Sir Eric Neal, intimated that he thought the institution was complacent and needed a bit of a kick and lifted up in various sorts of ways. He clearly thought that I could have some capacity to do that and I knew a bit about change agendas.

So, I joined. I'd assumed that Flinders would be a simpler more unified organisation than CSIRO. However early on one of the Executive Dean said to me, "You realise Michael, Flinders is four semi-autonomous republics under a loose alliance to a king." Thus all the lessons I had learnt in CSIRO from Geoff's so called 'One CSIRO' agenda became very relevant. I needed, as we had in CSIRO around particularly the Flagships, to get Flinders to think of itself more as a single institution. Culture again mattered. Indeed, I've often said: if I was a halfway decent Vice-Chancellor, it was because of what I learned in my CSIRO days; including about trying to give government solutions to its problems, rather than assuming they are going to fund your problems.

Can I just go back briefly to interactions between research organizations and governments. You've had experience both of state governments in Western Australia and South Australia and the Commonwealth of Australia. So, how did you get on at Flinders in interacting with the industry and economic Development programs of the Government of South Australia?

The best example I can talk about is Tonsley, because to a certain extent that's probably my biggest legacy at Flinders.

That was the Chrysler site?

Yes, Tonsley is an ex-Mitsubishi car site. It's almost adjacent to Flinders. Two weeks after I became Vice-Chancellor, Mitsubishi announced that it was cancelling car production on the site. The SA government had lent them \$34 million or so, and Mitsubishi somewhat generously decided to give it back. In return, the government acquired the site.

I'd been reading a lot about how universities had to engage with their community. This seemed to me a massive opportunity for the University. So, I rang the minister that been assigned to deal with Tonsley and its consequences, John Hill, who became quite a close friend of mine. I said, "Look Minister Hill, if I can assist, please ask and I'd be happy to join any working party or planning group." I think I surprised John because he asked, "Why would a Vice-Chancellor want to do that?" My response was that Tonsley was in our neighbourhood and Flinders might be able to help in some way.

To cut a long story short, the Government decided to revitalise the site as an 'advanced manufacturing hub' and Flinders ultimately made an investment of \$120 million of Flinders money, with no money from the Commonwealth, to build a new building on the site for our School of Engineering, Computer Science and Mathematics. We also established on the site our outreach activities including the New Venture Institute designed to facilitate start-ups.

All that took a significant cultural change. Flinders had to 'come of the hill'. We had to engage with and listen to the needs of our community more. So, I joined the Southern Adelaide Economic Development Board. And, we had to reinvent engineering at Flinders. My predecessor at Flinders had made the decision to close down Engineering at Flinders. She'd got some money from DEST to delay the process for a little while and allow a sensible wind down, However, the Chancellor, Sir Eric Neal, who of course is an engineer, made it up very clear to me that I'd better find a reason to reverse that decision!

That took a bit of creative thinking and a lot of credit for the leadership of the school in making the necessary changes. The program wasn't viable. While people said they loved Flinders graduates, we didn't produce enough of them. We had to find different ways to do engineering. Some people didn't want to move but we spent three years talking to them about the new building, industry engagement, new opportunities, building on Karen Reynolds and her medical devices program. So, my message back to government was simple: this is a program that will deliver your agenda. Flinders research and education, particularly what we will move to Tonsley, will make a difference to the state. I think that's a powerful message to any government. Certainly, the SA Government has pumped money into Tonsley for things like autonomous vehicle trials and things like that.

For the benefits of people who aren't as familiar with South Australia geography, how far was the Tonsley Park campus from the main campus?

Flinders and Tonsley are about a three or four-minute drive.

So, not very far.

Not very far. There's a bus now that circles between the main campus and Tonsley. But for Flinders, it was again a very symbolic move off the hill. The message I got from talking to people when I arrived in the state was Flinders is down in the southern suburbs, is halfway to McLaren Vale, it's on the hill, it's disconnected, the city's up there, we're down here. We were inward looking. In fact interestingly, most of the buildings looked inward instead of outwards.

So, I moved some operations into the city. We took a presence on Victoria Square at a time when rents were down. So, we grabbed a building and the naming rights and as the head of EY said to me, every time we walk out onto our balcony with our clients, they look across the Square and they see Flinders in big lights. I have to admit that I hadn't thought of that degree of advertising when we put up the lights! But it did say we were back in the City!

Some people say you can't change organisations like universities. I strongly disagree. I believe you can lead universities and you can change them. But you need to get out. You need to challenge people. You need to listen to them. But in the end, as Geoff used to say, a leader's job is done when the people say they did it themselves. If I now look back at my time in Flinders, there was some massive change that we did but where the people would claim now that they did it all by themselves. And I take that as an accolade.

So, Flinders had a medical school?

In the end we had two! We had a School of Medicine but I split it in half. Everyone told me that was impossible. You wouldn't get doctors to move. While the school had a great reputation, it was becoming too unwieldy. We split it in half. They now think they did it themselves.

When you say split in half, what does that mean? Geographically in half or?

No, it was more on a disciplinary side. It had become more than medicine. It included allied health, paramedics, and audiology. Nursing wasn't in the school but almost everything else related to health science. It had regional and rural health in South Australia and ran a program up in the Northern Territory.

So, I commissioned a review to get some evidence. It's a nice example of my view of how to initiate change, get some evidence, conduct a focussed review with expert people to do the review. The review made 22 recommendations with recommendation 22 to split the school into three: Medicine, Allied Health and Public Health. I sent the review down to the executive of the school and said, "Please disregard recommendation 22. Deal with all the other 21 recommendations and then we'll deal with 22." So, what did they do for six months? Worried about recommendation 22!

I finally walked down to them and said, "Look, I'm going to have a meeting with the school next week to tell them what I think about the review." I said, "I don't agree with Recommendation 22 (to spit into 3) but you've all told me the school doesn't really work because it's become too big so something has to change." The review's recommendation of three didn't make much sense because Public Health was too small to be a stand-alone school. Two was the right number but how then to organise the disciplines was the challenge. The obvious way would have been medicine with everything else in the second. That was rather conventional and I thought we could be more innovative so suggested that we have one school focused primarily on hospital-based medicine (and the important MD program) and the second more community and population focussed. The division wasn't to be absolute which would encourage cross-fertilisation and of course teaching into the various degree programs. The reaction was interesting: "That's ridiculous. How are you going to get doctors to move from school of medicine?" Six months later, they told me that the new School of Health Science focused on the community (and involving doctors) was the best idea they had ever had! So, you can change organizations, but you don't do it by email. You get out, talk and debate the tough issues. Again, something, I learnt in CSIRO.

Did you learn that at CSIRO by contrary examples of things that didn't work or by the way that you worked?

There are always contrary examples and you can learn from them too. Some of my emails have produced the wrong reaction because people often tend to see what they want to see or fear what they don't know. That's why face-to-face meetings are so important. Initially I was a bit surprised at the amount of travelling, that Geoff did and, in fact, encouraged us to do in that change period. He would spend a lot of time out talking to people about the issues. Occasionally he would talk to one community about something in a slightly different way, but that's a separate issue. Keeping communication messages straight, but, you've got to engage with people. Let them hear you directly, listen to their fears, and, perhaps, change your mind. I tried to merge two faculties but, in the end, I decided it just was going to be too counterproductive to do it and pulled back. Then I slammed by all the people who wanted it in the first place but that's a separate part of the story. No, you've got to listen to people. Geoff was right: change is spelt 'CCC'—communicate, communicate and communicate!

So, did you have a choice of being the Vice-Chancellor of other universities or did Flinders come out of the air, as it were?

As I have already mentioned, Flinders came as a bit of a surprise. Before I moved to CSIRO, I had been approached by a few and even threw my hat into one or two rings. Post Flinders, I wasn't really interested in another VC role. And I have to admit looking at where the system is now, I'm glad I moved on. I get up to interesting things now with less stress!

Can you just reflect a bit on the role of the universities like Flinders or in Victoria, like La Trobe. It's the second university. So, in a sense, it's a bit like Monash but Monash is in a

Group of Eight while Flinders isn't. So, what is the role of these Universities and that university, to some extent?

I actually agree with some comments that were made by one of my senior executives at Flinders, a woman who retired a couple of years before I did. At that point, she was a Vice President but had started to life as the Executive Assistant to the VC. She gave a fascinating account of the history of Flinders, including the momentous changes around the Dawkins period when Flinders could have easily merged with the then South Australian Institute of Technology. If that had occurred then, South Australia would have had a very powerful higher education with the second institution perhaps a bit like a smaller Monash. I don't think it's a bad system now and I don't agree with the idea of a single institution because that can limit competition and innovation.

Having made that decision—or non-decision—Flinders, like all universities, had to find its niche and I think it went back to its founding and the principles articulated then particularly by its inaugural VC, Peter Karmel. Flinders, and La Trobe in Victoria or Griffiths in Queensland were all set up in an interesting time in Australian history in the sixties. They were often a little ahead of their time even for that period, had a strong commitment to social equity and an interesting balance between teaching and research that I don't think exists in the group of eight today.

So, Flinders had a good commitment to teaching, is that what you're saying?

I think Flinders' rhetoric was always probably a little stronger than the reality, but I do think that Flinders cared about its students. We spent quite a lot of money improving the student environment. Regional education, particularly in medicine, but more generally was important. During my time, for example, the School of Medicine worked out how to teach a cohort of medical doctors in Darwin with consequently great impact on the Northern Territory. That sort of innovation, I think is at the core of universities, like Flinders.

The other thing that I really liked about Flinders because it came back to my management style was its size. You could, in a year, visit every school and visit them for long enough to have a serious discussion with the issues. If something blew up, as in the medicine review, you could get out personally and relatively quickly. So, I think scale matters in organizations if you want to keep a human touch. A university is a human enterprise.

My impression is that Flinders is pretty well endowed when you walk around that site, that's a pretty, I hesitate to use the word, rich, but there's a lot of very expensive buildings there.

The new ones? Yes, but what's important about that Terry, is that we were not, a massively endowed institution. That investment was done by a very careful stewardship of money. We spent \$120 million on Tonsley, another \$67 million reforming the centre of the university into a student hub. Almost none of that was Commonwealth money. And, I do think it came back to a fundamental commitment to the student cohort, to student engagement

and listening to students. It also took some great people to make these things work at very cost-effective ways.

So, when you were at Flinders, how did Flinders University get on with CSIRO and also the CRCs?

We had a couple of quite important programs that engaged CSIRO in various ways. Probably the biggest one was the National Groundwater Research and Training Centre for which Craig Simmons of Flinders was director. The Division of Land and Water was a very substantial partner with Peter Cooke as Deputy Director. This was an ARC Centre of Excellence but co-funded with the National Water Commission which had some discretionary money. There was a pretty good working relationship between the Centre and CSIRO. Importantly, Peter was very clearly in the Groundwater Centre's leadership team and I think managed the engagement with Land and Water which for the most part then ran very smoothly.

We had another significant engagement through a group in chemistry with people in, what was used to be, Molecular Science and the manufacturing flagship.

Through Greg Simpson?

Yes and David Lewis on our side who had worked in CSIRO and was a joint appointment for a while. That one got into a hiccup when CSIRO yanked the plug and on very short notice. I did protest Megan about that one. At its core was misunderstanding, I suspect on both sides, of funding arrangements, the nature of appointments and strategic priorities.

Having worked in CSIRO I could understand their logic to some extent. Water and South Australia's position in the Murray-Darling Basin made water and SA strategically important to CSIRO. Hence the relationship with the Groundwater Centre was strategic and consequently strong. It wasn't that manufacturing wasn't important to CSIRO. It was but the focus was Clayton and South Australia was very peripheral.

Without that strategic national priority or a need to access critical expertise as occurred with the p-Health Flagship and Graeme Young's work on colorectal cancer, CSIRO's relationships tend, I think, to be more transactional. Consequently, if financial stringency arises they, as occurred with David Lewis, are expendable on a relatively short timeline and with little regard for consequences in their partners.

Following CSIRO's decision to cut support I had to step in and support his appointment from my discretionary funds. It wasn't too difficult a decision, if a bit annoying, because his work and leadership were vital to our engagement around Tonsley.

Indeed, I was disappointed that, despite trying, I couldn't get interest from CSIRO in the Tonsley redevelopment. I suspect we failed to really sell the vision. Tonsley was potentially much more than another 'technology park' or even the 'advanced manufacturing centre' that the State Government liked to talk about. Instead it was, I believe, an Australian

example of an 'innovation precinct'; a concept that, at the time, was gaining considerable carriage in the US as a driver of innovation and regional development. Tonsley brought together education (there was TAFE College on the site in addition to the Flinders presence) plus a range of start-ups and bigger companies. R&D and innovation opportunities ranged from advanced manufacturing, to medical devices and assistive technologies to low-carbon housing. There was even an innovative retirement village nearby to act as trial site and housing planned for the wider site. I think the State government got a bit tired of me pushing this vision although after I left they started talking about the Tonsley Innovation Precinct!

I felt that this transformation agenda was one with which CSIRO could have engaged more. But, they have to make their own choices and within their own resources. So, an opportunity that could have accelerated development but for whatever reason wasn't taken up.

Was Flinders part of many CRC's?

Yes it was in three or four of them in various ways. And, they were relatively successful and in important areas, e.g. Indigenous health. However, they weren't, put it this way, they weren't up on my radar of problems!

Oh, okay, so that means they were probably were working okay. So, can we now go to your post Flinders work. Therefore, you have retired from Flinders in 2014 and you had been very much engaged with various activities through the Academy of Science and other things that you and I have met up on together. So, what's your impression of a couple of things. One is, what do you think now is the role of an organization like CSIRO in a National Innovation System? You've commented on that briefly as we've been talking. So, what do you, if you were starting from scratch in 2017, would you still have CSIRO as part of Australia's National Innovation System?

I suspect the answer to that would be, no. But that doesn't, imply that having got CSIRO, it should not exist. I guess my no is because I just can't quite see the political context that would drive such a development. We have a much more mature university sector with a very strong research capacity than when CSIRO was established. To establish a CSIRO like body, today, would be such a significant policy initiative that probably needs a crisis of some scale. Well, put it this way: if Australia faced such a crisis, we'd probably have bigger problems than just what was happening to the Science system.

That said, CSIRO is still, from my perspective, an organization of considerable assets, of considerable skills and with a remarkable history; not all of which should be assigned to history. I think some, may be a lot of it should be assigned to history, but not all. Finding its role at the moment, I think, is a challenge for both CSIRO and other institutions.

[music]

One model might be what I think was perceived by the Flagships that it would give not only CSIRO a coherent view but position CSIRO to lead a national challenge of scale. The other possibility, which I see bits of since I chair the board of the National Computational Infrastructure, is the role CSIRO plays as an infrastructure provider.

A manager of National Facility?

National Facilities. That is a genuine role but it needs a degree of scientific sophistication and deep understanding of scientific trends to do that role as well as it ought to be. It's probably too topical for me to talk about NCI specifically but I'm not always sure that CSIRO has that understanding.

I think that the risk is that CSIRO thinks it's doing things which are unique or particularly distinctive. At times they may do them with more scale than others but rarely as distinctively as they think. Support of start-ups is a good example. I think CSIRO has taken a while to realise that a significant amount of research that is done that will be led to start-ups comes out of universities. And it's not necessarily a skillset of CSIRO to successfully manage that process as much as perhaps at times they think it is. So, I do think the organization is still, in a sense, trying to find its niche in a much more complex ecosystem than when it was established or even 60 or 70 years ago.

The economy of South Australia depends a lot on Defence industries. So, I need to ask you about your relationship both with CSIRO and at Flinders with the Defence Research Organization, which is now called the DSTG, Defence Research Group and Alex Zelinsky, one your former colleagues probably, is a head of that?

A very interesting question because the engagement was quite strong but in relatively different ways. There was a small group in Chemistry at Flinders that was interested in energetic materials. For lay people that's explosives.

Explosives, yes.

They were the only group in an Australian university authorised to deal with explosive materials. A very significant partnership with DSTO or DSTG over several periods because it was seen by Alex and his predecessor as a way to bring DSTO technology into the private sector through the development, for example, of focused explosives.

The other area of collaboration was really interesting and was in the social sciences. For two years, we ran a workshop, involving DSTO people and others from DFAT, that I used to call, the Failed State Program. The focus was on the various interventions in which Australia had been involved: Solomons, in Bougainville, in East Timor, in Afghanistan. The idea was to understand the lessons learned from those engagements. They were really fascinating workshops involving DSTO people, DAFT and military people talking with social scientists, historians and other academics.

The interaction of Social Sciences with Science is of course, something that interests me, a lot.

Me too, but we probably haven't time to go through that one.

We're winding up.

Okay I just need finally, to get you to comment from your position now on the state of Australian Science and Innovation and what would your advice be to an incoming Minister for Science?

I've recently gone on the Council of the Academy of Science and through our Fellows and others; I see the amazing scientific talent we have in Australia. What also strikes me are the opportunities that the new generation is seizing in areas that we would never have seen before. However, I again come back to something that I took away from CSIRO, and tried to practise at Flinders and I still think is a challenge. The great opportunities we will seize in the next 10 or 15 years are going to come where disciplines collide, where new technologies, particular data and data science, go out into new areas. For my own field of the mathematical sciences, I think this is a great opportunity today. But I worry that it will be lost because mathematicians need to have a deep conversation with ecologists, material scientists, and even social scientists.

There's no time in people's busy lives to have the conversations that I can remember having 30 years ago. And I think that's the challenge: the system has to free people up to allow them to explore. But, they also need to be willing to explore. Too many people that I've seen on professorial selection committee are still pursuing ideas related to their PHD. I'd much prefer to see different threads. One of the advantages I had was to be able to do very different things. Today, I think we don't give them that capability. Some do it, at quite a risk that their own careers and opportunities and even family relationships. Particularly some of our women colleagues who are some of the most exciting thinkers. But you know, that's the opportunity, that's the challenge.

Well, that's a very good place to stop Michael and thank you once again very much for spending such a lot of time with us, and we hope you've enjoyed the experience.

Thank you.

[END OF TRANSCRIPT]