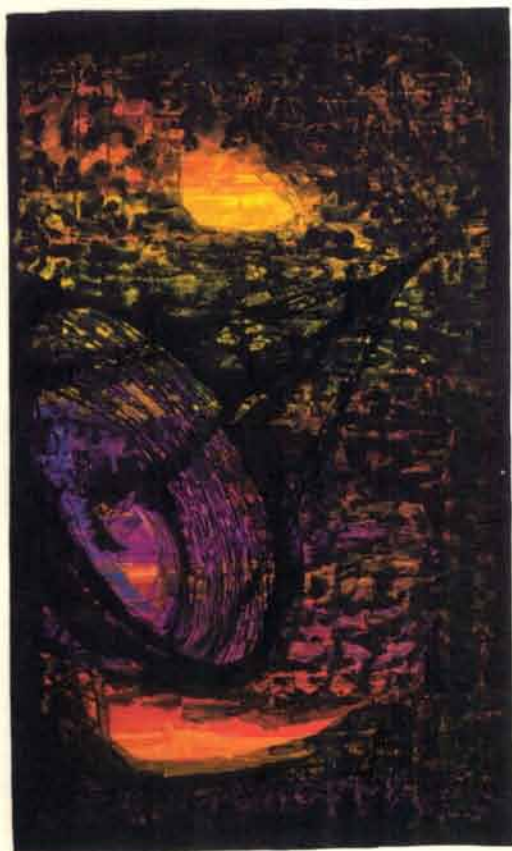


Portraits IN SCIENCE



Compiled and introduced by
ANN MOYAL

SCIENCE IS ONE of the most important intellectual and cultural forces of the twentieth century, yet surprisingly little is known in Australia of the lives of this country's key scientific men and women and of the contributions they have made to enlarging the boundaries of scientific knowledge.

Among these interviews compiled and introduced by Ann Moyal there are two physicists, Sir Mark Oliphant and Professor Harry Messel, and three medical researchers in immunology, human genetics and the brain—Sir Gustav Nossal, Professor Susan Serjeantson and Professor Peter Bishop. Others interviewed include an animal geneticist, Dr Helen Newton Turner; ecologist, biologist and Chief Scientist, Professor Ralph Slatyer; Dr Elizabeth Truswell, a palynologist; Dr Paul Wild, a radiophysicist; and Professor Ted Ringwood, physicist. There are also lively interviews with science communicators, Robyn Williams and Dr Michael Gore.

Together the group encompasses the innovative application of scientific knowledge since the 1930s; the foundation of scientific research institutions; high-level Australian representation in international science; and policy development and public education in science. They reveal their formative influences, diversely rewarding experiences and outstanding commitment to their demanding and challenging work. This is an impressive record offering evocative reflections and inspiring achievements.

*Cover: 'The Great Australian Desert by Night
(with the world famous giant telescope at Parkes, NSW);
tapestry by Mathieu Matégot in the
National Library of Australia.*

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Library of Science
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Foreword

Since the late 1950s, the National Library of Australia has built one of Australia's great institutional collections of oral history recordings comprising detailed interviews with distinguished Australian men and women in a variety of occupations and disciplines. In recent years, through its publishing program, the Library has begun to share the wealth of this collection with a wider community of Australians. So far, two books have been produced—*Self Portraits* and *Artists' Portraits* in which David Foster and Geoffrey Dutton respectively edited for publication a selection of interviews first with Australian writers and secondly with artists, drawing mainly on the work done over a twenty-five year period by the late Hazel de Berg, one of this country's pioneer oral historians.

With the success of these two publications, a third has now been compiled in which Ann Moyal, a distinguished historian of science, uses the discipline of oral history to offer a perspective on the development of Australian science through interviews with a cross-section of men and women who have won high achievement and recognition in their various scientific disciplines. Unlike the two previous books which drew on an existing body of material, Ann Moyal has constructed her book largely from interviews which she herself conducted with the subjects of her choice. The interviews of course vary in length and quality and have been edited for publication by Ann Moyal. The complete recordings and supporting transcripts are held in the Library's oral history collection.

In a publishing program which has largely been concerned with aspects of the humanities in Australia, the National Library is pleased to give recognition to the singular contribution which scientific endeavour has made to the national life and which has brought international recognition to Australia. Ann Moyal, recently a Harold White Fellow at the National Library, has brought keen and incisive judgment to bear in offering readers a sense both of the individual careers she has explored and of the nature and variety of Australian science itself.

Warren Horton
Director-General

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Speaking of Science: An Introduction

Science is one of the most important intellectual and cultural forces of the twentieth century. Yet surprisingly little is known in Australia of the lives of this country's key scientific men and women and of the contributions they have made to enlarging the boundaries of scientific knowledge and to the application of science to human development. It is curious that the scientist, despite a key role in peace and war, is often seen as an obscure or exaggerated figure—the laboratory experimenter in a long white coat, the absent-minded professor, even the wild-haired fictional 'Dr Who', intrepid, often boring, and well outside the bounds of everyday life. Stop anyone in the street and ask them to name one or two famous Australian scientists and you will almost certainly draw a blank. Ask for a sporting figure and the names will flow. Yet science is all around us and those who work at its frontiers have fascinating stories to tell. It is the link between the probing and creative mind and the physical and natural world that has made the scientific and technological civilisation in which we live.

Significantly, Australia moved into a scientific age when Cook and the *Endeavour's* naturalist, Joseph Banks, traversed the eastern coastline and carried their rich collections of specimens and scientific data back to Britain. Across two centuries, scientists in this unique 'fifth continent' have built on scientific knowledge and won considerable prominence for Australia on the world's scientific stage.¹ In the last decades of this century, science and its applications have assumed greater importance for this country than ever before.

This collection of oral history interviews with leading Australian scientists and science communicators has been fashioned with this point in view. In the last fifty years Australia has established special excellence in a number of scientific fields. At the forefront of these are radiophysics, neurophysiology, medical research in immunology and molecular genetics, geology, various areas of physics, agricultural genetics and ecological research.

There are, of course, many more. In bringing together an introductory set of interviews with a group of Australian scientists who have leading reputations in these fields, the aim is to convey something of the variety, excitement and accomplishment of science, to explore its work and processes, reveal its challenges and perceive its relationship to society. In so doing, it is hoped to shed light on a crucial area of Australian culture and through the intimate reflection of individuals, open an enlightening view to readers, to students of science and society, other scientists and to young Australians who may wish to contemplate a scientific career.

Oral history offers an invaluable route. The oral record is now widely accepted as a significant primary means of recovering the past.² In the arena of science, its value is evident. Few scientists are given to autobiographical writing;³ few keep personal diaries and a significant part of the history of the last fifty years of science in Australia is in the memory of living scientists. Since a scientist's work is often complex involving unfamiliar terms, explanation afforded by oral discussion can assist in conveying and clarifying many themes.

These informal stories reflect Australian science in the second half of this century. Little enough has yet been written by historians on these important years. The period, however, was one of the blossoming of Australian science. New institutions were established, major organisations reshaped, while World War II—'the Physicists' War'—gave impetus to the national growth of our scientific training and education, scientific infrastructure and research in both the physical and biological sciences.

Retrieving history through the recollections of individuals also offers other distinctive benefits. It can yield new and often elusive material about both a key scientist's role and the collective social system of science. On a personal level, it offers a persuasive and illuminating way of perceiving formative influences, clarifying education and training, revealing mentors, defining scientific networks and identifying the creative thrusts and innovations of individuals who, throughout their lives, are involved in attempts to explain the workings of the universe and to devise models and applications for the extension of scientific knowledge and its use.

In its broader context, the oral records of scientists furnish insights into the sociology, institutions and political dimensions of science. How for example, do creative scientists act towards each other? Do they have individual, collaborative or team approaches? What are their reactions to breakthrough and discovery? Do they share a belief system of science? What is a routine working day in the life of a research scientist? How do they communicate their findings and how do they participate in the reward system of science? Is women's experience of science different from men's? What ethos in our scientific

institutions has been most conducive to scientific research? What part do scientists play in the advisory system of national science? And how, in a geographically remote country like Australia, do scientists stamp their reputation and at times Australia's official interests, on international science and its affairs? We also need to know with some concern and urgency, how in a world increasingly dependent on scientific understanding, we communicate science to the public and educate and attract new generations to science.

Oral history takes many forms—ephemeral recordings for media interviews; 'in-house' oral history pursued by organisations for records on particular aspects of their affairs; oral interviews conducted by historians and sociologists for defined research projects, captured by cassette. The present records, however, form part of a technologically advanced and permanent collection of the National Library of Australia, pioneered in the first instance by Hazel de Berg⁴ and now encompassing a wide diversity of fields. It was Hazel de Berg, cutting a swathe through a huge territory, who coined the concept of 'the thinking voice'. Particularly attuned to her literary and artistic subjects, she adhered to an oral history technique of allowing her interviews to develop in a largely intuitive way, withdrawing herself from the discourse by pushing the pause button for her questioning. She believed that the participant should be allowed to speak directly to future generations without interruption from the questioner, and her tapes are designed to be heard.

In 1992 the National Library invited me to add to their collection by undertaking a series of interviews with leading scientists. As a historian of science, I take a different approach to oral history. I believe that the historian can enhance the quality of the record through a partly structured interview in which subjects are encouraged to recall significant information about their careers and backgrounds, expand on aspects of their experiences and, by interjecting questions and related points, trigger memory of events and attitudes that illuminate the social and political dimensions of science. The outcome of this technique presents a record very different from Hazel de Berg's and becomes in part, a conversation interspersed through the interviewee's reflective comments, a form that the National Library's Oral History Officer, Mark Cranfield, has described as 'speaking of science'.

Again, perhaps differently from de Berg, I attach importance to the interview's transcribed form. Ideally, of course, listening and reading should go hand in hand. The character of the voice, the pause, the thoughtful answer and the laughter can tell much of the respondent's character and mood and there is an intimacy in the revelation that does not always stir the printed page. But for researchers, the transcribed interview is the record from which they can readily draw. Both approaches characterise the use of the Library's collection, while recent evidence from the pioneering Oral History Program at Columbia University, New York,

suggests that generally requests for transcriptions outnumber those for oral records by almost a thousand to one!⁵

The following edited transcripts represent for the most part well under half of the full interviews held in the National Library Oral History Collection. Full transcripts and recordings of these interviews are available through the Oral History Section of the Library. In editing, the aim throughout has been to mark the influences, the passages, and the milestones of these lives in science. Care has been taken to preserve the character of the speaker's style and each version is published with the subject's consent. As comparisons are instructive, I have included one interview, Harry Messel's, conducted by Hazel de Berg in 1972 edited by me, and have also used, in edited form, Hazel de Berg's interview with Sir Mark Oliphant, made with him in 1967 (at the age of sixty-five), as a prelude to my interview with the distinguished 'elder statesman of science' at the age of ninety-one.

The sociology of science

These twelve scientists, nine men and three women, span ages from forty-seven to ninety-one and their careers traverse a period from the 1930s through the post-Second World War period to the present day. Two are physicists, Sir Mark Oliphant and Professor Harry Messel; one a radiophysicist and inventor, Dr Paul Wild; three are medical researchers—in human genetics, the brain and immunology, Professors Susan Serjeantson, Peter Bishop and Sir Gustav Nossal. One is an ecologist and biologist and the Commonwealth government's first Chief Scientist, Professor Ralph Slatyer; another an animal geneticist, Dr Helen Newton Turner. Two are earth scientists, the geochemist and geologist, Professor Ted Ringwood and Dr Elizabeth Truswell, a palynologist. Robyn Williams is Australia's foremost communicator of science, and Dr Michael Gore is a science educator and Director of the National Science and Technology Centre.

Together the group encompasses key fields of Australia's basic research, the innovative application of scientific knowledge, the foundation and administration of scientific institutions, policy making and national planning of science, top-level Australian representation in international science, and public communication and education in science. It illuminates a remarkable landscape of science in Australia.

In a society where creativity draws on many backgrounds, the scientists' geographic origins are mixed. Seven are Australian-born, Oliphant, Bishop, Newton Turner, Serjeantson, Ringwood, Slatyer and Truswell, with backgrounds in South Australia, New South Wales, Victoria and Western

Australia; three are British, Wild, Williams and Gore; Messel is from Canada, and Nossal was born in Austria.

All have led distinguished careers that have propelled them into the reward system of science. Six are Fellows of the Royal Society of London, eight are Fellows of the Australian Academy of Science, and four are Fellows of the Australian Academy of Technological and Engineering Sciences. Helen Newton Turner has a medal named in her honour; Peter Bishop is a winner of Australia's high scientific accolade, the Australia Prize; Susan Serjeantson won a recent Clunies Ross National Science and Technology Award for outstanding vision and personal tenacity in a scientific field; Michael Gore (with his Shell Questacon Science Circus) won the Eureka Prize for the Promotion of Science, and their civil and scientific honours shine widely in Australia and abroad. Each in a distinctive way, is a prominent spokesperson for science.

Through these autobiographical studies, fascinating light is shed on the formative influences that shape men and women towards a life in science. A majority of the group were born or grew up in country towns or fringe suburban locations where they developed a lively early interest in natural science. Their fathers filled the ranks of surveyor, engineer, civil servant, country banker, miner, teacher, railroad foreman and for most, there was a strong encouragement from their parents towards reading, observation and education.

The vital influence of the mother rises powerfully from the pages. Strong, forward-looking women, most of whom were denied educational opportunities in their own lifetimes,⁶ they poured their hopes and ambitions into a clever son or daughter and spurred them to a creative career. Such women have a clear historical provenance in Australia. Ralph Slatyer remembers, 'My mother was extremely influential on me. She had a love of learning and inculcated that ... particularly of the natural world,' while Mark Oliphant's mother, he recalls, 'above all others, formed our taste in reading'. Helen Newton Turner and Elizabeth Truswell enjoyed maternal influence and support. Farther afield Michael Gore in Britain, and Harry Messel, brought up by immigrant Ukrainian parents in Manitoba, Canada were also encouraged. 'Education,' Messel records, 'used to be almost the sole topic of conversation in our household' so determined were both his parents that he should be better positioned than themselves.

Schooling and the presence of an inspired teacher proved a further shaping force. In Adelaide, the young Oliphant had his interest stirred at school by good teachers in physics and remarks that he 'took it up like wildfire'. Messel, rising at four and learning to hunt and trap with the Sioux Indians, exulted in schoolwork and took a great delight in examinations and 'probing into the unknown', while in Sydney in 1939, seven-year-old Gus Nossal, fresh from Vienna and unversed in English, meeting what he calls 'a noisy wall of silence' at school, set himself to rise swiftly from the bottom of

class to the top, and relished the discipline of the Jesuit brothers and the practice of weekly exams at St Aloysius College.

But success at school was not an essential prerequisite for a key life in science. Some testators were 'late developers'. Peter Bishop found his impulse to reading and interest in science and technology in one of those nineteenth-century institutions, the Mechanics Institute of Armidale, New South Wales, but as a boarder in Sydney at Barker College in the early thirties, he sat the Leaving Certificate twice to get a scholarship to Sydney University and a more forward start. Gore at an 'abysmal' secondary modern school in Britain was, self-confessed, an educational flop. He managed to fail the Eleven Plus twice, and in the scholastic division into sheep and goats, 'I was a goat', he says.

In the field of university education, what emerges from the reflections across the spectrum of age and disciplines is a clear picture of changing educational patterns and opportunities over four decades and an insight into how able men and women could be happily nurtured in, or could transcend, traditional training experiences as the foundation of innovative careers.

For the senior of the scientists, Mark Oliphant, traditional university teaching in physics at the University of Adelaide taught him 'the extraordinary exhilaration' that existed even in minor discoveries in that field and he won First Class Honours and a coveted 1851 Exhibitions award to the Cavendish Laboratory at Cambridge as a result. In wartime, contrastingly, Peter Bishop in Australia and Paul Wild in Britain both had their university training compressed. Wild, at Cambridge, completed only five terms of university study in his whole career, graduating with a 'wartime BA' with Second Class Honours in physics and mathematics before joining the Royal Navy in 1943. And while Wild would soar in post-war Australia as a highly original and innovative scientist, he notes with humour, 'I don't think that [his scientific education] would ever do in the British world!'

Bishop, doing his medical studies at the University of Sydney, was working as an anaesthetist and wartime registrar in pathology before his studies were complete. But as a student, he had held a human brain in his hand and, marvelling at its contours and the human life it marked, he made a committed decision to devote his career to brain research.

For the three women scientists, their training experience swings from the unorthodox of the thirties to the sixties and seventies norm. Helen Newton Turner took her university degree in architecture and her career path into statistics and human genetics demonstrates the application and adaptive skill that could in earlier times form a remarkable scientist. By any gauge, she was unusual. In contrast, by the time Elizabeth Truswell and Susan Serjeantson—both from the baby boom cohort of the 1940s—emerged with First Class Honours in their areas of science, it was accepted practice for

talented candidates to prepare for a research career with a PhD degree from a university overseas, though even then it was exceptional for a woman.

The PhD degree was itself not introduced in Australian universities until the late 1940s and Newton Turner, Slatyer, Bishop, Nossal and Wild figure as highly productive scientists who secured the higher degree of Doctorate of Science on the basis of published papers.

Educator and communicator, Michael Gore, had a markedly different ascent. Battling in secondary schools in Lancashire, he had the good fortune to find a 'superb' mechanics teacher who had an indelible effect on his life, both in giving him 'a great love of mathematics and technical things' and in imparting by example, that teaching was an honourable profession. Yet his own route was fraught with challenge. 'Clawing his way up' the academic ladder, he suffered the ignominy of being told that he was not fit for university education, and when, with effort, having made it, he learnt that he was an experiment from whom his professors expected only a Third Class Honours degree. But he reflects buoyantly, he was thus spurred to acquire Second Class Honours, a PhD, a post-doctoral position at Brown University, USA, and appointment to the Australian National University (ANU).

No one, perhaps, exemplifies as clearly as Robyn Williams the direct relationship between a university curriculum and his career in science. Williams attended a 'rather drab college' in North London where he did 'a very old-fashioned BSc degree'. T.H. Huxley and various people, he relates, had put this degree together 'and it was virtually everything that you could mention to do with science ... taxonomy and anatomy ... and as the century had changed, they discovered genetics, physiology, biochemistry, and all this stuff.' So he was awarded a University of London honours science degree that, he adds, 'we all knew was bonkers'. Yet the degree clearly furnished one of the most versatile science communicators with a pertinent base for his probing and ranging journeys around the scientific brain.

These narratives also give us vital insights into the role of mentors in a scientist's life. Sociologists stress the point that creative careers grow from early support and encouragement to develop in a particular field. Mentors introduce the young scientist to the development of scientific codes and standards, to networks, and to career and publication opportunities. The latter, in turn, provide access to resources and facilities that enhance the quality of performance and subsequent recognition. There is an accumulation of advantage in science, and a reward system—the 'Matthew effect' ['to him that hath shall be given', in the Gospel according to St Matthew]—which leads to a scientific élite.⁷ Moreover, in the growing literature on science and gender, mentors of either sex are now perceived as making the difference between the anchoring of women in the lower level of the scientific enterprise and lives of real accomplishment.

The lives recorded here are rich in mentors. Oliphant's great inspiration and encourager was the Nobel Laureate and atom breaker Sir Ernest Rutherford. The Australian immunologist and Nobel Prize winner, Sir Macfarlane Burnet, succoured Nossal; while a cluster of medical professors at Sydney University combined to send the young Peter Bishop, with no strings attached, to his overseas research opportunity. At the Council for Scientific and Industrial Research (CSIR),⁸ pioneering radio astronomer J.L. Pawsey proved a mentor for Paul Wild; in land research other scientists provided models and support for the youthful Slatyer, while the famous European physicist, E. Schrödinger and several others at the Institute for Advanced Studies, Dublin, were to prove vital influences for Harry Messel.

Helen Newton Turner's shining mentor was the veterinary scientist and first CSIRO Chairman, Sir Ian Clunies Ross. For Newton Turner, Clunies Ross was always 'enthusiastic about helping anybody who had any sort of ability or skills'. 'I don't think,' she adds, 'I would have got anything like the career I've had without his help.' Serjeantson's and Truswell's stories also illustrate the high value of Australian and international male mentors for their early careers. As graduate students, both drew direction and support from leaders in their disciplines, Serjeantson in human genetics and Truswell in the relatively new field of palynology, the study and dating of microscopic fossil fragments of pollen in rock sediments.

In interviewing these scientists, I wanted to find out—particularly for younger people contemplating a life in science—something of the diverse nature of scientific work. Clearly the group were impelled into science by teaching, reading, an innate feeling for the natural world, opportunity, contact with inspired leaders and a decision to commit to a research career. But how do they work? What is a routine day, week, or month in a research scientist's life? Their answers are lively, full of range and interest; their involvement and output intense.

Team work, the part played by technicians in assisting the technological developments, the role of research assistants spill from the reminiscences, with the rarer appearance of the solitary researcher. The stories mirror the march of twentieth-century science from often very sturdy and versatile independent endeavour to expanding team and collaborative research and, increasingly, towards sophisticated instrumentation and technology.

Certainly for the older researchers, much of the challenge and interest of science was harboured in more primitive technologies. There was a need for skill and inventiveness in what Oliphant calls the 'string and sealing wax days' of the Cavendish Laboratory in the thirties where scientists made their own equipment from odds and ends and did their own glass blowing. Grappling with electronics, Peter Bishop's initial research revolved more than most around the relentless challenge of devising equipment for his task. Unedified

by colleagues, he appears as a solitary yet tenacious figure at University College London from 1947 to 1950, making 'many mistakes' and false starts before he found his mainstream techniques for brain research.

For each scientist, the nature of their work is made accessible through their words. Ralph Slatyer, perhaps, sums it up for them all: 'If you are going to be a successful researcher,' he says, 'it has to be almost an obsession.'

And what of women in science? Do women do science differently from men? Is science, as some feminist writers suggest, feminism's 'dark continent'? Deep cultural forces survive that continue to locate most women in the profession's lower ranks, and the place of women in science leadership is conspicuously small. How, then, do successful women scientists see the question of gender and science? I chose three women, all with fascinating careers, whose experience spans from Dr Helen Newton Turner's highly productive career from the 1930s to 1973 and beyond, to the 'baby boomers', Professor Susan Serjeantson and Dr Elizabeth Truswell, in their prominent contemporary scientific careers.

Newton Turner rejects any notion of discrimination in science. She felt, she insists, no sense of it and was delighted to be regarded by her colleagues as 'one of us'. Of her brilliant career, she says characteristically, 'It was all luck, all the way.' She repudiates the idea of difference and discrimination of women in science and of special feminine qualities she may have brought to her work. She has, nonetheless, been an inspiration to women scientists in related fields.

Women's texts, however, are different; their narratives diverge from the stories of men. Women have different attitudes to success: they see it in collaborative terms. All three emphasise team cooperation in their research. Susan Serjeantson underlines the participation of a wide cast of students, research assistants, technicians and clinicians in her work. Her career, founded on skill, ambition and determination, has been upwardly mobile all the way. She notes some failure on women's part to 'put themselves forward' for advancement. They wait to be invited to apply for higher jobs, and wait in vain. As Head of the Human Genetics Group of the John Curtin School of Medical Research, ANU, and the first woman science professor to be appointed, she attracts, and clearly acts as a mentor, to a high number of women PhD students in her field. She believes that the situation for women in science is changing. 'Some doors,' she observes, 'will close but other doors will open.'

Both Serjeantson and Elizabeth Truswell see women as oriented largely to organic science and to a sense of usefulness in science. 'Women,' says Serjeantson, 'are happier working with problems with which they can identify ... and somewhere ... is this extrapolation that it's going to do the world good in the long run.' Truswell perceives some difference in women scientists' views. From a leadership position at the Bureau of Mineral Resources (now

called the Australian Geological Survey Organisation) and as one of the few women Fellows of the Academy of Science, she expresses some sense of isolation from a male conditioned organisation and socialisation of science, and finds that any call for 'humility'—one of the newer norms of science—does not find resonance among male colleagues.

The company of scientists

Mark Oliphant had made a prominent name in Britain after his Cavendish days. At Birmingham University he had contributed to early radar research; he was a member of Britain's famous MAUD Committee on atomic development and, crossing to America to inform the United States government on atomic affairs, spent several years at the Lawrence Livermore Laboratory in California working on the Manhattan Project. In the post-war euphoria following 'the Physicists' War', he belonged to the priesthood of the physical scientists, in demand, and looking to pursue a distinguished research career in Britain when his own country pressed him to return to found a Research School of Physical Sciences at the newly-established national research university in Canberra.

The need for such a school was explicit. Despite past pockets of excellence,⁹ physics was in a doldrum in Australia. Before the war, university departments had concentrated on undergraduate teaching. But the war had generated new demands for physicists, and, in its immediate aftermath the Commonwealth government contemplated the field of nuclear research.¹⁰ In addition there was a clear need for the training of Australian researchers to build a cadre of talent for the growth of the physical sciences in the universities and burgeoning government bodies. Mark Oliphant's advent was seen as a critical part of these developments.

I interviewed him in my Canberra home. The most interviewed of scientists, I wanted to pursue questions of how, at ninety-one, he saw his contribution to Australian science, how from his perspective across almost half a century, he felt about his transfer from the high centre of British science to the young Australian National University; and how he now judged his attempt to build the world's most powerful synchrocyclotron at ANU to put Australia on the international physics map.

His narrative traverses this major venture, his influential role in shaping the contours of the ANU's scientific staff, the formation of new bodies—the Australian Atomic Energy Commission—which he helped to sire, and the founding, under his strong impetus in 1953, of the Australian Academy of Science as an élite body of eminent researchers,¹¹ which he regards as his major national accomplishment.

Throughout his career, Oliphant emerges as a prominent public spokesman on science. In the previous century, science in Australia was notably served by the patronage of an interested corps of colonial governors, Sir Thomas Brisbane and Sir William Denison in New South Wales, Sir Henry Barkly in Victoria, and Sir John Franklin in Tasmania. In this century, Sir Mark Oliphant has the distinction of being the first major scientist to be appointed as a State Governor in Australia. As Governor of South Australia, he continued to espouse his views on science (if chafing somewhat under gubernatorial forms) and to encourage scientific endeavour by young students. Rosy, thoughtful and ever forward-looking about the consequences of science, his views on its practice hold firm. 'I think science is something like weeds,' he says, 'it just grows of its own accord and if you've got the right atmosphere, the right situation within universities or within places like CSIRO, then it grows and develops of its own accord ... You can't plan science.'

In another part of the physics spectrum, Professor Harry Messel, outgoing and dynamic, moved eclectically around the social system of science. A maverick; a scientific entrepreneur; energetic, creative, outspoken, he arrived in Australia at the University of Adelaide in 1951. Joining the University of Sydney as its Professor of Physics in 1952, he made a striking contribution to physics research and training, science education and publication, and built one of the liveliest and most diversified physics schools in the country. Melding 'town and gown' and drawing on huge donations from the private sector, Messel created his Science Foundation for Physics at the university, raising over \$34 million for outstanding research at the school,¹² and 'gave academic physics a new image throughout the country'.¹³

Vivid and flamboyant, Messel has remained outside the scientific élite and has not been elected a Fellow of the Australian Academy of Science.¹⁴ Hazel de Berg's interview takes his career only to 1972 but it catches the essence of the man. Although we do not know what questions she asked, the narrative exemplifies one aspect of oral history: men at the height of action leave different records from the more detached reflections of men who have retired from the centre of the stage.

While physics found strong roots in the post-war period in Australia, the new field of radiophysics (developed from radar activities in war) located its dramatic and rapid development at the Council for Scientific and Industrial Research and later CSIRO in the Radiophysics Division. A creative subgroup led by Australian wartime radar expert, Joe Pawsey, turned their equipment towards the radio heavens and gave Australia an early international leadership role in radio astronomy.¹⁵

Dr Paul Wild found his opportunity for a striking Australian research career in this arena. Serving with the Royal Navy battleship *King George V* in

Australian waters, he became engaged to an Australian girl and, eager to quit the bleak scene of post-war Britain, joined the Radioastronomy Research Group near Sydney where he contributed to exciting pioneering discoveries. Wild's research centred on radio emissions from the sun for which he designed the swept-frequency or dynamic radio spectrograph which first demonstrated the great complexity of burst and storm phenomena and enabled him to classify these into universally recognised 'Types'. He subsequently devised the radio heliograph, the centre piece of a radio observatory near Narrabri, NSW, then the most advanced radio observatory in the world and through this device developed a technique for taking radio moving pictures of the sun. Later, it was his conception that lay behind the Interscan landing system for aircraft using microwaves which won him the Gold Medal of the Royal Society of London, and Australia, high recognition on the international scene.

Paul Wild's boyhood fascination with the great nineteenth-century rail, bridge and ship building engineer, Isambard Kingdom Brunel, circled his days and he became the creative force behind the concept for a Very Fast Train in Australia. His story, with its humorous shafts, moves through research, invention, politics, and his period, perhaps least to his taste, as Chairman of Australia's science colossus, the CSIRO.

The impetus to medical research in Australia began quite strongly in the nineteenth-century universities of Sydney, Melbourne and Adelaide. But private benefaction was to provide its seed-bed across twenty years from 1916 with the founding in Melbourne of The Walter and Eliza Hall Institute of Medical Research and its steady rise to national prominence. In 1944, 'the Hall' came under the directorship of the outstanding Australian immunologist, Macfarlane Burnet, later a Nobel Laureate, whose reputation advanced the standing of the Institute as a centre of international repute.¹⁶

Sir Gus Nossal, his protégé and deputy, succeeded Burnet at the Institute in 1965 at the age of thirty-four. Nossal, a high-profile experimental immunologist, medical administrator, international and national policy figure in science, came to our interview superbly organised and his life would seem to echo much of the small Austrian boy's determined cry at an English-speaking school in Sydney—'I will show them!'—in the large gulps of scientific life he has taken across his career.

We held our interview in a room at the Australian Academy of Science. The spectacled bust of Sir Macfarlane Burnet, his mentor at the Hall, was a friendly presence weaving into the recollection of Nossal's zestful scientific life. We talked of his and Burnet's differing approaches to research and to the growth of the institution within their care. As a theoretician, Burnet's ingrained resistance to advancing medical technology was a point of real concern. Nossal, as an experimentalist, however, has presided as director over

a crucial shift in the range and sophistication of the Hall's equipment and has extended this major international Institute into new organisational, funding and research forms.

In some contrast, Peter Bishop found his introduction into medical research in the essentially teaching environment of the University of Sydney. By the early thirties, other medical institutions were rising. In 1933, the Kanematsu Institute was formed at Sydney Hospital and four years later attracted the Australian neurophysiologist John Eccles, as its director, to work on neuromuscular transmission to the brain. Another future Australian Nobel Laureate, Eccles found the academic isolation of Sydney 'severe' and later expressed criticism of the University of Sydney Medical School as 'a very dim place, being little more than a teaching institution'.¹⁷

Bishop was to return to the University of Sydney after training in Britain and inaugurate his own career in brain research. Together Eccles and he have been characterised as 'the two great father figures in Australian neurophysiology'. But while Eccles explored the synapse—the nerve cells—and synaptic transmission to the brain,¹⁸ Bishop concentrated on the brain and the visual system, notably binocular vision, with research leading to extensive publication and the award of the Australia Prize in the field of sensory perception in 1993. From 1967 to 1982 he succeeded Eccles as Professor of Physiology at the John Curtin School of Medical Research at the ANU.

Three disciplines mark the women scientists' work. From 1956 to 1973 Dr Helen Newton Turner was Leader of the Animal Breeding Section, Division of Animal Genetics of the CSIRO, and throughout her working life has contributed notably to the Australian wool industry and to the dissemination of research findings on breeding to conferences, agencies, international organisations and sheep breeders at home and abroad. In office and as Honorary Fellow at CSIRO, she has served the international Food and Agriculture Organisation (FAO) and the Australian International Development Assistance Bureau (AIDAB) and has trained graduate students in her field. Her story is framed with genuine modesty and throughout the whole interview, often expressed refrains, 'I can't take all the credit!', 'It wasn't only me!'

Professor Susan Serjeantson, human geneticist, led the first team of researchers in the world to define transplantation antigens at DNA level. Through her early experiences in Papua New Guinea, as a geneticist with the PNG Institute of Medical Research, she was concerned to take this work to the peoples of Oceania, where her investigations made a vital breakthrough in defining transplantation antigens and applying them to donor-recipient matching. Her research on the molecular basis of disease susceptibility has both expanded our understanding of inherited disease and, through interrelated disciplines, has contributed to the genetic pre-

history of Pacific people. She publishes across a wide spectrum of journals and is frequently invited as a speaker at international forums. As a parent, she emphasises the help her mother has given in travelling with her young son to enable her to participate in vital conferences on science.

Elizabeth Truswell follows a line of distinguished Australian women in the geological sciences, a pattern that grew from the universities of Sydney and Melbourne in the early years of this century. Yet in palynology she escaped the curbing restrictions once placed on geological women in the field, and from early palynological work for Western Australian Petroleum Consortium, moved through research in Antarctic geology, the biostratigraphy of Australian systems and polar plant life to become Chief Research Scientist, Environmental Geoscience, at the former Bureau of Mineral Resources in Canberra. On an earlier Deep Sea Drilling mission to Antarctica, she experienced the excitement of pinpointing, from the sedimentary evidence, the precise location where the cold diatom-laden [microscopic algae] Antarctic waters join the warmer, carbonite-rich waters from the north. As the Bureau's representative on the Cape York Land Use Strategy group she is closely involved in the processes of communication between scientists, pastoralists and Aboriginal communities. We talked of the question of intellectual property and the rights of those participating to access scientific knowledge about areas in which they are involved.

Professor Ralph Slatyer represents three aspects of the scientific estate. A research scientist in biology and ecology whose early work on climate, soil and plant responses at the CSIRO led to fundamental findings on growing season attributes adopted worldwide, he moved from Associate Chief of the CSIRO Division of Land Research to become Professor and Head of the Department of Environmental Biology at the Research School of Biological Sciences, ANU, in 1968, where he developed research on ecological succession and ecological responses to perturbation.

From 1978 to 1981 Slatyer, with his environmental background, was appointed Australian Ambassador to UNESCO, the first scientist to fill the post. His three years in office marked an important period in Australia's international role. As Chairman and skilled facilitator, Slatyer participated in the critical development of the World Heritage Convention in whose character, he notes, 'Australia had a very influential role.' He also played a leading part in UNESCO's Scientific Committee on Problems of the Environment (SCOPE) and the Man and the Biosphere (MAB) program. His oral reflections provide perceptive insights regarding these roles.

At a national level also, Ralph Slatyer represents a key link between government and science. For many years, despite financial support for science, the Australian government had adopted a noticeably *laissez-faire* attitude to its science policy development. Malcolm Fraser as Minister for

Education and Science in the Gorton coalition government summed this up in a famous statement in 1969 in which he declared that the country was best served by 'a pragmatic, evolutionary approach, seeking advice from different people as different projects arise'.¹⁹

Evolution did occur. Starting with Prime Minister McMahon in 1972, and running through the Whitlam, the Fraser coalition and the Hawke governments, a succession of Australian Science and Technology Councils (ASTECS) has emerged, largely with scientific appointees, to provide the Federal government with advice and recommendations on scientific and technological matters both on request and on the independent initiative of ASTEC. By May 1989 however, political perceptions were shifting. Science could no longer be seen as external to national policy that involved industry, productivity and development, education and training and increasing demands on resource allocation for science. Prime Minister Hawke, spurred by his Minister for Science, Barry Jones, established the Prime Minister's Science (and later Engineering) Council, a Coordination Committee on Science and Technology and the new post of Chief Scientist to link science policy directly with the Department of Prime Minister and Cabinet.

In the implementation of these developments, Ralph Slatyer, Australia's first Chief Scientist from 1989 to 1992, played a critical part. His interview offers an insider view of these processes and of politicians' reactions to the new and closer relationship between government and science. In his role as Chief Scientist he also initiated what he perceives as a major change to the research culture and organisation of Australian science, the foundation of the Cooperative Research Centres scheme. His hopes are conditional—can Australians, geographically scattered and with a long history of less integrated efforts, he asks in his full interview, sustain the measure of collaboration and cooperative input required?

Science communicators

While the research scientist is essentially 'a person viewing the state of knowledge in his time as something to be constantly improved',²⁰ a key role in our scientific and technological society is undoubtedly also that of the science communicator and educator. 'Being without a science background,' Robyn Williams firmly predicts, 'will be disastrous for you from now on. Whatever your job, an understanding of science has become absolutely fundamental.'

For the past twenty years, Williams has set himself the task of contributing to and stimulating the public understanding of science. Already, at fifty, something of a legend in his own lifetime, he has built

the broadcasting of science in all its aspects in Australia into a prominent art form and is a recognised leader around the world. Wheeling across the spectrum of disciplines and environmental, political and social aspects of science with the ABC's 'The Science Show' and television interviews, he has led, self-described, 'a rich and complex life'.

The morning I interviewed him at the ABC, he had in his workaholic way just come from a four-hour early morning session compering 'Daybreak'; he was to go on to make a television program; he did a telephone interview with a Melbourne scientist; and he gave me an hour. Travelling to conferences, guest speaker at innumerable functions, Chairman of the Commission for the Future and of the Australian Museum, a regular columnist for numerous newspapers and journals, the author and editor of many books, he has poured information and discussion with national and international scientists into the public's ear. I remember in the 1970s his coming to the phone to interview me for 'The Science Show' and asking cheerfully, 'What are we talking about today, Darwin or nuclear politics?'

Despite his achievements, Robyn Williams is still concerned that there are in Australia no obvious role models in science for boys and girls and argues that 'until a scientist becomes a central figure in a popular film or sit-com, we are not getting school children in'. His central message is clear: 'Either we go one way and consolidate the reality of science and our world, or else we become ... a southern supermarket.'

Even so, during weekends and school holidays, children of all ages, their parents and teachers may be seen in Canberra streaming towards the National Science and Technology Centre—Australia's hands-on exhibition building for public education in science. Its Director, Dr Michael Gore, has a fascination for communicating and teaching science. Coming to Australia in the late 1950s to teach Physics at the Faculties of the ANU, Gore revealed a love and flair for expressive lecturing—'there's always been a touch of the performer behind what I've done'—a skill that led to his setting up Questacon in an old Canberra school building and to the establishment of the National Science and Technology Centre as a bicentennial event.

In his own school life in Lancashire, Gore learnt directly that 'you could make a big difference to people if you were a good teacher'. Later he would discern that such skills are too often underrated in academia. He also held a strong view (not always shared by university scientists) that popularisation is not 'demeaning' to science. His enthusiastic work in creating a science centre for people of all ages, training young graduates in science communication, and sending outreach mobile science exhibitions and 'science circuses' around the country is an invigorating expression of

his creed. And his own career, as he tells it here, is an epic story of how he turned the many 'ill winds' that blew, into 'considerable good'.



From this company of leading scientists, several important themes emerge. One is the need to preserve the base in Australia of fundamental, curiosity-driven research and the concomitant need for scientists to develop and innovate. As Elizabeth Truswell suggests there needs to be 'a certain amount of freedom in science ... but on the other hand I see Australia as a very large continent with a very small population and with enormous problems, problems in the way we use our land, and I think that we don't have too many choices but to try and channel our best scientific expertise into solving some of those problems.' A second theme is the importance of not undervaluing science, of training the young for careers in science, and through education and changing social values, to bring science into the mainstream of national affairs. Our future, as Peter Bishop insists, 'is very much involved in science'. For the physicists too, training remains at the core of our progress in science. 'In the conditions of modern life,' says Messel (quoting A.N. Whitehead), 'the rule is absolute: the race which does not value trained intelligence is doomed.' Finally Sir Mark Oliphant, reflecting on the perils of economic rationalism in a self-centred world, sums up a positive view for an intelligently clever country: 'What mattered in this world was people and skills, skills plus people, that with skills plus people one can do anything, and ... money is merely a sort of lubricator, or should be just a lubricator of activity by people.'

As this collection was being prepared for press a decision was made to include one more oral record—in this case incomplete—that of Professor A.E. Ringwood. When I invited him to make an oral recording in 1993 I was not aware he had been ill, and as we taped it in his ANU office in March, he was buoyant and forward looking. The record did not entirely meet his high expectation: he was a perfectionist, and he planned to tidy loose ends in our projected second hour.

To our sorrow, Ted Ringwood died in November 1993 after a determined fight with cancer. Our decision to include the incomplete transcript, with his family's permission, is made on two counts. First, little was known of Ringwood's early background, although his reputation as a geochemist and earth scientist circled the world and he had brought great international standing to Australian science. Secondly, although incomplete, the discussion reveals the mainspring of his creative scientific life and of his pioneering forays into a far-ranging research career.

Ted Ringwood stands as Australia's most renowned earth scientist. Trained initially as a field geologist, he turned early to the pioneering application of new geochemical concepts to an understanding of the Earth's interior. After a formative postdoctoral year at Harvard, from 1958 he centred his career at the Australian National University where he was Professor of Geochemistry and from 1978 to 1983, Director of the Research School of Earth Sciences. Extending at various times and, sometimes simultaneously, across six major fields of research, Ringwood made fundamental discoveries on the phase transformation of minerals at high pressure and their bearing on the constitution of the Earth's mantle; on the composition of the Earth's core; on the origin of basalt magmas (the petrology of the upper mantle); on the chemical evolution of the planets and meteorites, and on the composition and origin of the Moon. From the late 1970s he brought his geochemical knowledge and his high pressure research work to bear on the safe disposal of nuclear waste in the development and patenting of SYNROC. More recently, he had extended his work on materials technologies based on his deep Earth research to developing and patenting ultra-hard cutting tool materials including diamond composites for industrial use.

Of his numerous proposals on the nature of the light component of the Earth's core, Ringwood, it was claimed, 'has just about covered the waterfront'.²¹ Ringwood as Principal Investigator for Lunar Samples, NASA, was also a key figure in moon rock research, while his landmark work on the phase transformations, which he describes in some detail in this text, marks a decisive contribution to one of the fundamental advances in man's knowledge of the Earth.

In 1991, Ringwood was presented by the National Academy of Italy at the Corsini Palace in Rome with the Feltrinelli International Prize awarded in a five-year cycle in the fields of Science, Medicine, Art, Literature and Humanities. He was the first earth scientist to receive the prize since 1966.

Professor Ringwood's truncated record has further resonance in its poignant reminder of our transience. It emphasises the timeliness of oral history and the importance of ensuring that we secure such illuminating reflections for posterity.



In making these recordings, I have experienced a sense of personal privilege that close access affords and have greatly appreciated the sense of stewardship, candour and perception that each speaker brings. Most importantly, as Hazel de Berg was aware, I have participated, I believe, in building a new kind of

literature. Reviewing her pioneering work in the mid sixties, this remarkable oral historian wrote, 'As the transcripts began to arrive ... I saw a new type of literature emerge. It is unedited, unplanned but real. Since it comes from people who have all paid the price of achievement ... it is basic and uncontrived ... I wish everyone could share my moment of recording'.²²

Ann Moyal

Personal reminiscences invariably include the names of many people. Explanatory notes are given only for key participants and have been added where the name first occurs.

Quoted text relates to the edited transcripts except in a very few places where it is excerpted from the full oral record.

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The Library gratefully acknowledges permission from the CSIRO Division of Radiophysics to use the photograph of Dr Wild at the Parkes radiotelescope during the Apollo 15 project in 1971. The photograph of Professor Messel is courtesy of the *Gold Coast Weekend Herald*. Professor Serjeantson and Dr Truswell were photographed by Loui Seselja of National Library Photographics.



Sir Mark Oliphant,
AC, KBE, FRS, FAA, FTS

Interviewed by Hazel de Berg
Canberra, 24 July 1967

Hazel de Berg Collection
National Library of Australia
Tape no. deB276

and

Interviewed by Ann Moyal
Canberra, December 1992

National Library of Australia
Tape no. TRC-2890

MO: I was born in Kent Town near Adelaide in 1901 and grew up in various places in South Australia, including a little town called Mylor in the hills above Adelaide. I enjoyed my schooling, and after primary school went to Unley High School, where I was fortunate in having teachers who really made me interested in what I was doing. I'd never found this in primary school, I'd always looked upon work as a bit of a chore there, but in high school I began to learn that learning was fun for its own sake. My father and my mother were both keen on reading, on cultural things in general. My father tended to have his head in the clouds a bit, my mother had her feet well on the ground, and I and my four brothers, I think, learnt a very great deal from her. She, above all others, formed our taste in reading and our taste for literature.

I went on from high school to the University of Adelaide. Unfortunately I was unable to pay my way there and didn't gain a scholarship, so I had to take a job as a cadet in the Physics Department of the university and intended, when I went to the university, to do either medicine or chemistry, which were the two things that appealed to me most. I think medicine, because of parental influence and general influences round me which had made me what in America is sometimes called a 'do-gooder', a characteristic that's been with me all my life, and has sometimes got in the way of other things. However, I was lucky in having a very good teacher in physics in Adelaide, a man named Burdon, Dr Roy Burdon, who started me off with enthusiasm on this subject and who weaned me away, I think, from my ideas of being a chemist or a doctor and taught me the extraordinary exhilaration there was in even minor discoveries in the field of physics.

Looking back, I realise that perhaps the genesis of this desire of mine arose from my mother's influence. We had always been great walkers, lovers in the country, and interested in the countryside and everything that went on in it, and I did a great deal of walking as a child. We were not a well-off family by any means, my father was a civil servant, and it was necessary for us to make our own amusements. I spent much of my time pottering about with a few odd tools, trying to make things like clocks and so on which would work. In this I was encouraged by my mother who was quite an artist, and one of my brothers has followed her as a painter and etcher of some merit in South Australia, so that I think that perhaps we all owe a great deal to our mother for this gift of curiosity, curiosity about the world in which we live.

I graduated from the University of Adelaide with First Class Honours in physics. I think I was rather lucky to get this distinction, because I'm quite sure now that I was nothing like as good as the students that I've taught in the many years that I've spent in university teaching since. But still, I did get this great uplift that came from earning something for myself,

and went on to do a little research work in the spare time that I had from my duties as an assistant in the laboratory, as a result of which I won an 1851 Exhibition which enabled me to go to Cambridge to study with Lord Rutherford²³ in the famous Cavendish Laboratory. This scholarship was founded by the husband of Queen Victoria, Prince Albert, with the monies that were accumulated as a result of the 1851 Exhibition, the first great Exhibition held in London in the Crystal Palace.

I was married shortly before going overseas and, on reaching Cambridge, I went to the Cavendish Laboratory to see Lord Rutherford and find out what I was supposed to do. I found Rutherford sitting behind a desk, surrounded with paper, and in an atmosphere that can only be described as a thick London fog, tobacco smoke. I found out afterwards that he smoked a pipe, smoked it incessantly, but unlike most men, he dried his tobacco out in front of the fire before smoking it, so that when he put a match to his pipe it behaved a little like a small volcano with ashes and sparks flying out of the top of it all the time. He received me with immense kindness, and from my first moment of meeting him I felt at home with him, a feeling that lasted throughout my association with him, which was until his death in 1937.

To proceed to a degree at Cambridge—I was after a Doctor of Philosophy degree for research work only—I found it was necessary for me to join a college, and through the good offices of Rutherford I was able to join Trinity College, which was his college. Most of our time was spent in the laboratory and it was necessary only for us to dine in a few times a term in college and, of course, it had other compensations. College life was interesting, as it always is, particularly to an Australian who finds himself in an atmosphere four or five hundred years old, with discipline and activities focused round the past rather than the present. And my wife and I became very close to both Sir Ernest, as he was then, Rutherford and Lady Rutherford, and we spent much time with them in their country cottages. Rutherford was the most inspiring man I have ever met, unassuming, determined always, an extremely hard worker, and I found myself in very congenial company because I like to work hard as well.

After two years in the Cavendish Laboratory, I obtained the degree of Doctor of Philosophy from Trinity College, and was able to get more money as a result of getting some further scholarships from the Royal Society and from other places, to help me to carry on with my work. I went to Cambridge with a very clear idea of the sort of research work that I wanted to do, and Rutherford was very kind in allowing me to carry on with this because it was not exactly the sort of work which he himself was interested in. Then, in 1932, following the discovery by [John] Cockcroft and [Ernest] Walton [physicists and Nobel Prize winners in 1951] in the

Cavendish Laboratory of the artificial transformation of chemical elements one into another, Rutherford invited me to work personally with him in order to extend this type of observation.

My original work had been associated with the properties of positive ions, that is to say, of atoms of matters which were charged positively with electricity, and this was useful in that, for the research work that I was to do with Rutherford, we needed intense beams of positive ions of hydrogen which could be accelerated and made to hit targets of various kinds. In this work, which we did together, we were able to discover two new kinds of atomic species, one was hydrogen of mass 3, unknown until that time, and the other helium of mass 3, also unknown. These new atoms were produced as a result of atomic transformations induced by our ion beam hitting targets of lithium, beryllium and other materials. Incidentally, at the same time, we were able to show that heavy hydrogen nuclei, that is to say the cores of heavy hydrogen atoms, could be made to react with one another to produce a good deal of energy and new kinds of atom. This particular reaction, which we discovered at this time, is the basic reaction in the so-called hydrogen bomb. Of course, we had no idea whatever that this would one day be applied to make hydrogen bombs. Our interest was just curiosity about the structure of the nucleus of the atom, and the discovery of these reactions was purely coincidental.

Rutherford's assistant director of the Cavendish Laboratory was Dr Chadwick, later Sir James Chadwick. He was most helpful to me in every way. He was the man to whom one had to go in order to get pieces of equipment bought and so on, and the Cavendish Laboratory in those days was extremely poor, so that we had to make do with coffee tins and sealing wax, with bits of paraffin wax and with all sorts of odds and ends, in order to carry out our experiments. There was no luxury about it whatsoever.

In 1935, Dr Chadwick went to Liverpool as Professor of Physics, and I was asked by Rutherford to take his place as assistant director of the laboratory, which flattered me a great deal. In 1937, I was invited to become Professor of Physics in the University of Birmingham and head of the Physics Department there. And somewhat against Rutherford's wishes I accepted this invitation.

In Birmingham I set to work to try to build up nuclear physics. The department had not done anything of this sort in the past and it was necessary for me to collect money, which was not an easy task at that time when Britain was just recovering from the Depression years, but thanks to the generosity of Lord Nuffield, who gave me a very considerable sum of money, we were able to set to work to build a large cyclotron and to move into the exciting world which was opening up in high energy nuclear physics.

While these preparations were going on, war began to loom in Europe and some of us were asked to spend some time at the coastal defence stations round Britain, learning what radar was all about and to endeavour to do what we could to improve it. When war broke out, I was invited by the Admiralty to take charge of a team working in my laboratory, to endeavour to develop radar working at very much shorter wavelengths, so-called microwaves with a wavelength of only a few centimetres or an inch or two. We were rather successful at this, and were able to produce devices which would be flown in aircraft for detecting other enemy aircraft, which could be installed in ships for detecting other ships and submarines, and which from aircraft could even detect cities on the ground. These radar developments, which were described by Vannevar Bush [the Director of the Office of Scientific Research and Development, Washington during the Second World War] in his book about the American war effort as the 'greatest in reverse lend-lease',²⁴ we were rather proud of, but by the year 1943, early in that year, we came to the conclusion that we had done all we could in the development of radar, and returned once again to the nuclear work. I spent a considerable period in America and one short period in Australia, bringing news of the latest developments in radar in Great Britain.

Immediately before the war, there had been discovered in Germany a process known as nuclear fission. It was discovered by an old colleague of Rutherford's, Otto Hahn, and we were immensely interested in this process of fission as a possible source not only of energy, as a substitute for coal and oil, but also as a possible high explosive. In order to exploit this possibility, it was necessary to separate the isotopes of uranium, that is, the two kinds of uranium which exist in ordinary uranium and which can not be separated by chemical methods because they're uranium and both have identically the same chemical properties, and it was necessary therefore to devise a physical means for separating these atoms, just because they possessed slightly different mass, one being 235 times as heavy as hydrogen and the other 238 times as heavy as hydrogen. We decided to pursue the possibilities of separating them by forming beams of uranium ions—you'll recognise my old love there—and accelerating these, passing them through a magnetic field where the ions of different mass would describe circles of slightly different radius and consequently could be collected separately at the other side of the magnet.

While in America in 1941 on radar business, I'd had discussions with American scientists about the possibility of nuclear energy.²⁵ They were not terribly interested at that time but interest developed, so much so that their project for doing this—which became known as the Manhattan District Project—was established and all communication between Great Britain and America on these subjects was then cut off. However, in 1943, soon after

we started work again on the problems in Birmingham, Roosevelt and Churchill decided that it was absurd to carry on with such a project, an immense project, in both countries, and it was more sensible to transfer the whole effort to America. So in September 1943, we moved our work to America, I leading a team of British scientists who worked in Berkeley, California, and at Oak Ridge in Tennessee, on the electro-magnetic separation of the isotopes of uranium. The sequel to this work in America is now well known.

When I returned to Birmingham in 1945, we had fairly clear ideas of a scheme which I had cooked up [while in America] for the acceleration of particles to very much higher energies than we'd been able to achieve with the cyclotron. This device, called a proton-synchrotron, was thought of almost simultaneously by two other people—by Edward McMillan in America and Veksler in Russia. There was greatly awakened interest in Great Britain after the war in the development of British science, so that it proved possible to get the considerable amount of money required to build a machine of the sort which we had developed. This has done, between that time and now, very good service. But in 1950 I was invited to come to Canberra to become the first director of the Research School of Physical Sciences.



AM: Sir Mark, could we begin by going back to your return to Australia in the formative days of the Australian National University and your decision to leave your prominent career in Britain and become Director of the Research School of Physical Sciences. What motivated your decision to join this new venture of an Australian National University?

MO: The persuasiveness of Nugget Coombs, who played upon our nationality in order to interest us in this new university and, from the very beginning, the offer of the directorship of one of the schools.²⁶ But what made me say yes in the end was, first of all, that I felt I owed something to the country of my birth, and secondly, I felt that it would be a better place for my children to grow up. So I decided to go. Howard Florey²⁷ came to the station to see us off. And while we were waiting he and I walked up and down the platform, he trying desperately to persuade me not to go, to turn round and stay, saying to me, 'You know, Mark, you're committing scientific harakiri. When you get there all you'll find is a hole in the ground and lots of promises.' But I persisted, and came; and I found exactly what he had described. They had dug a hole where the school now stands and there was no building at all.

AM: One of the important developments (and it was really at a very early stage of science policy and major funding for Big Science) was your capacity to persuade Prime Minister Menzies, to back the development of a world-class particle accelerator, the synchrocyclotron. Can you describe Menzies' attitude to this major development in science policy, and to science in general?

MO: He was very cooperative always. But he wanted to know why, he wanted to have reasons. And he was very ready to listen, and I found him most sympathetic and very keen on the university, very keen that it should be good. And he kept on saying, 'Look, I'll back you if it's good. I'll back it if it's good.'

AM: The synchrocyclotron was a very major venture. And you yourself, from your record in Birmingham had a gift for attracting very handsome sums of money for Big Science in nuclear physics.

MO: Well, I wouldn't call them handsome sums. They were what was required but nothing more. In England I got £60 000, which at that time of course was quite a lot of money, from Lord Nuffield.

AM: The sum that Menzies allocated for this world-class accelerator, was something like, I think, £1 million, and it was a very important start to the university. Looking back on this project, how do you appraise the venture now?

MO: Well, I think we were too little too late. The concept was all right. The concept was to produce ten billion electron-volt hydrogen ions at a time when the biggest aim in America, for instance, was the Bevatron in Berkeley for eight billion electron-volts. So it seemed to be in the same class. But it was to use an electro-magnet without iron. Because we couldn't afford the sort of money that the Americans could spend on the huge equipment, we tried to make it smaller by making the magnetic field much larger and using a lower repetition rate, a lower number of pulses per minute that one could get from the equipment, and using a homopolar generator to provide the power which was required—which was hundreds of thousands of kilowatts that it needed for generating the field without iron.

This was a very major operation for us, and indeed it turned out to be too big really for Australia. And, while we built the homopolar generator, in the end we abandoned the idea of the accelerator because the Bevatron in Berkeley got going and it was quite clear it was going to take the cream off the particular energy we were working in. And so we decided to concentrate on the homopolar generator and the production of very strong magnetic fields with it. Which we did, for a time. What I wanted was to

see *something* in Australia in the top steps in the development of nuclear physics, a development of the physics of particles—which, after all, is the physics of the universe and of the whole of matter. And there was nothing going on in Australia, so I thought we should at least have a foothold in that.

AM: In Stewart Cockburn and David Ellyard's biography of you—they refer to some of the controversial aspects of the synchrocyclotron in chapters which they call 'The white Oliphant' and 'The light that failed',²⁸ It's important to have your comment, whether in retrospect you see that undertaking as an impractical venture in a country like Australia which may not have had the right industrial and technological back-up.

MO: Yes, that's right. It was more than Australia could really do from the engineering point of view. Also it was more than the team that I was able to collect together was able to handle properly. Yes, it was a mistake. But it was an attempt to put Australia on the map, as it were. And it did to some extent, the general development of magnetic fields without iron and shaping the magnetic fields was a useful contribution.

AM: At the ANU, as a founding father of the university, and during your Directorship of the School of Physical Sciences until 1964, you were seen as a highly influential figure in the university and in the appointments to various of the Science Schools. It would be of interest to know how you conceived of the best appointees for this innovative new university.

MO: It was really rather a personal thing, that when it came to an appointment to a professorship or even a directorship, names were bandied round, names that people knew of, or knew about.

Everyone of us who was concerned with the university had wide connections in Europe and England and America, people whom one could ask about certain people. And a real endeavour was made to get whatever was good—but all the time with an eye on the fact that, other things being equal, one would prefer an Australian to be attracted back again to this country.

AM: From the perspective of this almost half a century on, do you think that the ANU has met the objective of the planners as a training ground and centre for the best Australian scientific research?

MO: Yes and no. The first thing that the ANU did was to appoint, or advertise, a large number of research scholarships which sent Australians abroad to train. And this was highly successful. People who later became

heads of schools, like John Carver²⁹ for instance, were scholars under that scheme. And it did train, and then bring back to Australia to jobs in the ANU, quite a large number of Australians. And I think it contributed, in a sort of lump-sum way, to the development of science in Australia at that time. It was a time of real blossoming of science.

AM: Now over forty years since those early days, the whole attitude towards science and scientific accountability has changed quite dramatically. With this in mind, do you believe that the structures and procedures which were conceived by the ANU founders should be sustained in the way that they were set up then, or should we be ready to contemplate new funding and institutional arrangements which are more in kilter with the times?

MO: Well, there you've got my head on the block, as it were; because I believe that the ANU should have remained a research school, a research university with definitely research as its major objective and the training of research students. These institutions exist all over the world—the Max Planck Institute in Germany, and similar institutions in other parts of Europe, the great places in America like the Princeton Institute of Advanced Studies or the California Institute of Technology and so on—which have never had undergraduates and have remained in the forefront of the development of what one may call the 'core' of physics throughout the years, as a result of being not mixed up with duties other than doing research and producing new ideas. I think it is a pity that that was dropped, and that first of all the local College of Melbourne University, which served to train civil servants in things like economics and so on, was incorporated into the ANU. It was a shotgun marriage, mind you. It was opposed very bitterly by Florey, Hancock and me. But it came about as a result of government decision and there was nothing we could do about it.

AM: At the ANU an early tendency developed to establish departments with a lot more tenure than would be the case in Princeton. Do you think perhaps that was a weakness in the ANU?

MO: No, because the number, for instance, in the Research School of Physical Sciences, the number of departments as it were, was small; and they were such that, in order to start, in order to build them, in order to get things going, one had to offer (at any rate to the first head of the place) some sort of tenure, some sort of ability to spend years building up the department. For instance, let us take mathematics. It took me, I think, nearly three years to find a mathematician of the quality that was required. Mathematics was in a real mess in Australia. And I didn't want to have

anybody who wasn't absolutely first-class as the mathematician in the Research School. And it was a strange year, I visited all sorts of people, and particularly Australians who might be attracted back, but they hesitated and hesitated and then decided that their future didn't lie in this country. And I think they were probably wise. But I did in Manchester find Bernhard Neumann, who was interested. He was a Professor of Mathematics there and a Fellow of the Royal Society and a very distinguished man. But there was a problem. Professor Neumann's wife [Hanna] was also a mathematician, and as Neumann said to me, 'Wherever I go, she comes too. It's like the butler and the cook-housekeeper. We go together.'

He [Neumann] made an immense contribution to Australia. He lifted mathematics from just some subject which was taught to undergraduates to become a subject in all universities with some research content, some endeavours made to increase knowledge of mathematics, and to introduce into Australia some of the more recondite types of mathematics. And this has happened and has borne great fruit. I believe that Neumann has been a very great benefit to this country. The University Council couldn't conceive of the simultaneous appointment of man and wife. Hanna became the Professor of Mathematics in the Faculties when they were established and was a tremendous success as a teacher.

AM: You were also associated with the foundation of the second largest institution of science in Australia, the Australian Atomic Energy Commission. At Birmingham during the war you were working on uranium, and I think you advised the Australian government to get control of uranium at that point.

MO: What I did was to tell them that if there was uranium in the country that it would be wise not to let it go overseas unless they decided that they didn't want to use it themselves. At that time of course we didn't know that uranium was virtually running out of our ears, we knew we had a little bit (which had been discovered, by the way, by Sir Douglas Mawson at Mount Painter in South Australia), and for a time it was very strange, people's attitude towards these things at the time of that discovery. For instance, there is in the Flinders Ranges, quite close to Mount Painter, I think one of the only hot springs in Australia, and this has gases from the interior of the earth bubbling up through it as it comes up. It's not a big spring, it doesn't produce an enormous amount of water. But it is highly radioactive. And not only was it thought at that time that the radioactivity was good for you, but an attempt was made to put up a spa there so that people could go and bathe in these waters that were radioactive. In addition to that, a small company was proposed by (I understand) Sir

Douglas Mawson and Sir Kerr Grant together, to bottle water from there and sell it as an elixir. [*a laugh*] This sounds a strange concept of the use of radioactivity today. But there were all sorts of misconceptions of that sort in Australia about radioactivity, and they had to be taught that this wasn't something to be foolish about.

AM: With your background in atomic work in Britain and America, Prime Minister Chifley in 1949 appointed you to chair the newly-formed Industrial Atomic Energy Policy Committee which was to consider potential development and industrial applications of atomic energy in Australia.

You came as a visitor from England to chair that committee. It had all the people who had any know-how in this field on it—Professor Philip Baxter,³⁰ who had just come as Professor of Chemistry to the University of New South Wales, Professor Leslie Martin at Melbourne, Dr Harold Raggatt the head of the newly-formed Bureau of Mineral Resources, and Sir Frederick White the executive head of CSIRO. It was that committee's recommendations which led to the formation of the Australian Atomic Energy Commission by legislation in 1953. At that time you said

The application of science and technology, including atomic energy, could bring Australia prosperity and fruitfulness as few nations have experienced.

MO: I was still away till 1950. I remember the discussions, but I don't remember at all what actions were taken by government or by anybody else. All I know is that two people, Baxter and Titterton,³¹ took command as it were of the situation. And I let them go ahead as they wanted to.

AM: They were very persuasive advocates for nuclear development. Baxter almost got to the point of getting a nuclear reactor at Jervis Bay, [NSW] in the early seventies.

MO: That's right, that's right. The site and everything was chosen, but it was decided not to go ahead. And one of the reasons was that I, who had been in favour of nuclear energy for generating electricity (I thought it would be helpful for Australia to join in that work because it had uranium), I suddenly realised that anybody who has a nuclear reactor can extract the plutonium from the reactor and make nuclear weapons, so that a country which has a nuclear reactor can, at any moment that it wants to, become a nuclear weapons power. And I, right from the very beginning, have been terribly worried by the existence of nuclear weapons and very much against their use.

I was a member of a group that was led by Niels Bohr, after the test in Alamogordo, that was very much opposed to the use of this new weapon on civilian cities. Niels Bohr, who was our spokesman—which was a pity in some ways, because his English wasn't good and [*laughs*] his wife told me that his Danish was almost as bad—but he became our spokesman and was very very good and persistent in his approach. And he wrote a long memorandum to President Roosevelt, which I helped him with the English to some extent. And as a result Roosevelt gave him an audience which lasted for an hour and a half (at that time in the war an hour-and-a-half audience with the President was something); and Roosevelt said that he was very interested but wanted further information, wanted more documentation. Niels Bohr came back again, and I happened to be in the office at that time in Washington, the British Office, and he kept bringing me versions of this attempt of his to enlarge the knowledge of the President of what was involved really in the use of the weapon, and to persuade him to use it in some symbolic way, like blowing the top off Fujiyama or dropping it on a naval base, a military base or something, but not on a civilian city. We thought that once it was dropped on a civilian city that it just became a weapon of war, and we would rather that it never became a weapon of war. And of course we were supported very strongly by people, by Americans who, many of them, felt the same way. But by and large we were in a minority, but a rather distinguished minority. But the trouble was that this second memorandum to Roosevelt went off to him but he never read it, he died before he read it. And Truman, of course, was a different kettle of fish.

AM: And then in Australia in the fifties when various scientific institutions were rising, one was the Australian Academy of Science in which you played a fundamental part.

MO: I think, if I'm proud of anything, it's the founding of the Academy of Science. It happened in this way. [Soon after my arrival in 1950] I realised that there was only one body which was representative of the whole of science in Australia and that was the ANZAAS [The Australian and New Zealand Association for the Advancement of Science]—which was of course a body set up specially to bring the public into the picture, and not really a scientific institution. I realised Australia had no voice, no international voice, to speak with the academies of Russia and America or the Royal Society of London and so on, which were powerful bodies advising their governments and others and ensuring that science in its best forms was developed and that people were recognised in some way.

Now, I discovered that several attempts had been made to set up some such body, but they'd failed always because of the differences between Sydney

and Melbourne. Sydney wouldn't accept it being in Melbourne, and Melbourne wouldn't accept it being in Sydney. But I discovered that there were eleven Fellows of the Royal Society resident in Australia. And I thought, now, if I can get those to cooperate and get those together, make them the nucleus of an Australian Academy of Science, then nobody in Sydney or Melbourne can quarrel with us—because these people would have been chosen not by people in Sydney or people in Melbourne but by a completely disinterested group. So I wrote round to them, and everybody was happy to join in except for Michell in Melbourne, who was the man who developed [in 1905], you remember, the high pressure bearing which is used on the propellers of ships.³² The Michell bearing is one where the oil is automatically drawn into the bearing in a very clever way that he invented. And he was a Fellow of the Royal Society. But—he was an old man then—and he wrote and said, 'No, he wouldn't take part, he didn't want to be associated with still another failure.' [*a laugh*]

But I persisted. And we had a meeting, as a result of which we decided that there were several people in Australia who, if they had their deserts, would be Fellows of the Royal Society but, being in one of these outlandish colonies, had been overlooked. So we decided to get them to join us to make a body big enough to petition for a Charter from the Queen to form an Academy. And we did this. David Martyn was a CSIRO man who worked in radio and he agreed to act as a sort of secretary, and he and I together then drew up some documents. We got our Charter in the end, and this Charter was presented to me, as the first President of the Academy, at Government House in Canberra in 1954 when the Queen made her first visit to Australia.

AM: The motto of the Academy (and I think it was David Martyn who thought it up) was 'We are whom we elect'. It enshrines the idea that the first motive of the Academy was to recognise distinction. The general point is often made that there is perhaps a problem with all scientific élites, or any body of distinguished people, in that there are always people outside who are as well qualified as those inside. The French Academy of Belles Lettres with its forty members has a name for it—the 'forty-first chair'. The argument is that there is always a forty-first chair for someone who should be in and who is not.

MO: This is true, this is true. But it's bound to be the case, just as at the Royal Society there are people who perhaps should not be in. The Academy has increased its numbers. But it ceases to be an Academy if one has all and sundry in it; and one's got to preserve a faith in a body like that, faith on the part of the government, faith on the part of the academic community, that this is a body of reasonable reputation and attainment, a body fit to represent science for Australia with the academies of the world, to send representatives

to international gatherings and so on, and in other words make Australia part of the world scientific community. The Academy doesn't exist to represent anybody. It exists to honour people who have distinguished themselves. I don't know how many members there are from Queensland for instance at the moment, but at the time that the Academy was formed there was only one person anybody could think of from Queensland who was good enough to be a member of the Academy, and that was a woman.

AM: Dorothy Hill, a geologist.³³

MO: Yes, a geologist, who had really established herself as a person of consequence. There were, strangely, one or two from Western Australia who had already made a world mark; like [E.J.] Underwood, the Professor of Agriculture.

AM: One of the prime aims of course in establishing the Academy, in your own words, was that it should be able to assist the formulation of scientific policy for the country as a whole. Do you think that in the last forty years this has been a function the Academy has been able to provide?

MO: Well, I think the development has been unfortunate for two reasons. First of all, I've lost any belief I ever had in scientific policy. I don't think you *can* have scientific policy. I think science is something like weeds, it just grows of its own accord [*laughing*], and if you've got the right atmosphere, the right situation within universities or within places like CSIRO, then it grows and develops of its own accord. And I believe that science is best left to scientists, that you cannot have managers or directors of science, it's got to be carried out and done by people with ideas, people with concepts, people who feel in their bones that they want to go ahead and develop this, that, or the other concept which occurs to them.

AM: There is a problem of the allocation of scarce resources with so many competing claims on the national purse, it forces a government to at least look as if it has a policy for science, or a science policy.

MO: No, I think that's wrong. I think that what it should do is just to see that bodies like CSIRO or like the Academy have people at their head that they trust. Rivett, you see, did a magnificent job in the establishment of CSIR[O], and I think that both Clunies Ross and Fred White did well.³⁴ And interference by government has been *disastrous*, both with CSIRO and with the National University. I don't believe that one can possibly have a science policy. One can have a policy for applied science, a policy for the

use of science in various ways, but you can't have a policy for science any more than you can for medicine for instance. You don't know what's round the corner, you don't know what's going to come up tomorrow. And it's utterly impossible to plan science in the way the word 'policy' implies; because, whatever you do, in scientific work you find yourself going along a path and suddenly diverging completely away from it because something turns up which is of interest, which is new and so on.

AM: But on the decisions about whether you allocate large sums of money to particular areas of science is it not incumbent on government to have some concept of where they wish to put their resources? You would agree that it has become more complex since the days of David Rivett and Fred White in the CSIRO?

MO: I don't think it's become more complex. It's become—it's this emergence in the world of—what's it called?

AM: Economic rationalism.

MO: Yes, which has taken over.

AM: What about the transfer of scientific results into inventive processes and to technology? One of the pressures on the CSIRO has been to swing from the sort of organisation Rivett founded, where you channelled the money to the key scientists, and to bring it into a more mission-oriented and strategic-oriented climate of work. Is this not inevitable?

MO: Well, I think governments would like it to be inevitable. But it is impossible, it just can't be done. You can't plan science like that, you can't plan the development of scientific ideas to the practical stage. This is a process that either takes place, or doesn't take place, naturally. If there is a sufficient novelty and promise of a market in a new tin-opener, then somebody will make the new tin-opener that's been invented by somebody. But this is what business should be doing, what its task is, what the manufacturer's task is, to be aware all the time of new ideas and new knowledge which might be used by them and take it up. It is not the job of the scientist to sell science to industry. You can't transfer what doesn't exist or what is not of any immediate practical use.

AM: Now, Sir Mark, could we turn to a more personal note to your period as Governor of South Australia. In the nineteenth century Australia had quite a tradition of scientific governors, but it is very

exceptional in the twentieth century. Was science latched into your life very much in that period?

MO: Well, I tried to make my position as active as possible in the promotion of knowledge of nature (I prefer that to the word science, myself, because it's what science is all about, trying to understand how nature works). And I had close relationships with the University of Adelaide and with Flinders University and still have very close relationships with both those universities. But, on the other hand, there were parts of the job that I disliked intensely. I disliked the fact that I was not my own master, I had to do what was expected of a Governor. In other words, to a very large extent, what one does each day is determined by other people, and that didn't sit easily on my shoulders. But I endured it. And I think I was able to help the universities, and technology, and in particular to help science in the schools in South Australia.

Indeed, the Science Masters Association there has established a competition which they hold each year which is named after me and where science students in the schools take up a project and then submit it to the Science Teachers Association each year, where it's judged and placed on exhibition and prizes are allocated (or rather distinctions are allocated) to people. And the prize which is most valued is a silver thing that I made myself and which I presented to them, on which there's space for several dozen names of holders to be engraved and which is held for a year by the winner. Now, this has been extremely successful, so much so that this year they've had 1726 entries for the prize. So it shows you that if you can build up enthusiasm amongst the science teachers and give them a role to play other than just the blackboard, that you can produce enthusiasm and build something that didn't exist.

AM: How important is the teacher in stimulating the scientific men and women that we need? Do you have any perceptions which can be helpful? There's no doubt that many of the great science teachers who've been in Australia across the century have come from very bright people who didn't have the opportunities to do PhDs. Now the bright people may well be going into higher degrees and being lost to teaching. Do you think that's a problem?

MO: Well, it's worse than that. The year before last I was asked by the science masters to present the prizes of the Oliphant contest and by far the most brilliant of the students was a tall fair gangling boy who really shone at mathematics and science, he was out of the top drawer. So afterwards I sought him out and I said, 'Now, I'm interested. What are you going to do now? Are you going to the university to seek a doctorate in mathematics or in a related subject like theoretical physics or statistics or something?' 'Oh no,' he said, 'I'm going to take

a business course. That's where the money is.' And that was the reception I got from this boy, you see—who, despite what he had been exposed to and what he'd achieved, had caught this objective, this monetarist objective, which is now inculcated into everybody in Australia. And that's what I'm against. I think that that is what is destroying education, that's what is destroying the whole motive for doing anything, that unless there is money at the end of it then you rule it out. And that's why I think the future looks bleak for pure science.

AM: You have been always very future-oriented, very positive, a man of ideas and a stimulating contributor to many discussions of science and technology, the environment, and the common good in Australia. You have expressed some reservations about the future. But at ninety-one, and at a critical time of recession in Australia and a search for national solutions, do you have ideas that you've thought about in this way, which are still positive future-oriented?

MO: No, I'm a Keynesian. I attended some of his lectures in Cambridge in the early 1930s and was tremendously impressed with him, except that he was a very bad lecturer. But I felt that his ideas were so sensible, his ideas were so good, his idea that what mattered in this world was people and skills, skills plus people, that with skills plus people one can do anything, and that this thing called money is merely a sort of lubricator, or should be just a lubricator of activity by people in doing things, in producing things, all the way from good music down to better roads. And the whole of our approach therefore, through money always, is wrong.

AM: You speak of John Maynard Keynes. I wondered who in your life have been the most influential figures, intellectually and scientifically?

MO: The man who has influenced me to the greatest extent in life is Rutherford of course. [In 1925] he gave a lecture at the University [of Adelaide] in which he talked about the work going on in the Cavendish Laboratory. And I absolutely fell in love with this man. I just immediately decided that this was the man I was going to work with, if possible.

AM: Coming round full circle—from your life with Rutherford, your contribution in the war, and all the very important parts that you played, then coming back to your own country—I wonder if you now feel, looking back across the vicissitudes of life, whether your Australian life was one you would have chosen anyway?

MO: There's a strange dichotomy in my thinking about this. I think that Florey was perfectly right in saying that by coming to Australia I was

committing scientific harakiri. The dominance in Australia of management, of writing reports, of justifying everything that one does, just in the end got me down. That's why I gave up as Director two years earlier than I needed, in order to settle down to research work again. But of course it was too late. I did some useful work, published a number of papers, but still it was too late to make a real mark. Whereas, if I'd stayed in England, I think I would have achieved far more because I was in an atmosphere where science was respected, science had a special meaning. I'm not at all sure that that's the case at the present time. But, at the same time, I feel that I've managed to do something, one or two things that were useful here, like setting up the Academy. The birth of the Academy I regard as my greatest contribution to science in Australia, because at last Australia became part of international science.

AM: The vicissitudes, the dark and the light. From the record it seems you have had a most remarkable career, and one of enormous interest to Australians, and one that has been an inspiration for many people. Thank you for this very revealing and absorbing interview, and for all you've been able to share with us. Thank you very much.

MO: Thank you for treating me gently.



Dr (John) Paul Wild,

AC, CBE, FRS, FAA, FTS

Interviewed by Ann Moyal
Canberra, December 1992

National Library of Australia
Tape no. TRC-2892

PW: I was born in Sheffield, England. My father was a cutlery manufacturer living in a rather grand house with, I think, three cars including a Rolls and a Daimler and something else. However, just about the time I was born, which was 1923, there was a depression and he went bust. And when I was three months old he left the country to go to America in an attempt to sell his patents and so on for cutlery manufacture, and in the event he never returned. I met him eventually when I was thirty-three years old.

We moved to Croydon. I was brought up in Croydon, south of London, and we went from riches to rags and we were absolutely struggling, the family was really struggling, but eventually finished up in reasonable circumstances when the financial dealings had come through with my father.

When I was seven years old, one definite impact was when my mother bought me a Hornby train, and the locomotive had written on it the words 'Great Western'. And that began a lifelong sort of love affair with the Great Western Railway, and I soon began to follow up the history of the Great Western. I read about the great man Isambard Kingdom Brunel and all his works, which were not only railways but the extraordinary ships that he built at the time. Well, I suppose he was the first source of inspiration to me.

I've had ambitions from about the age of seven onwards, but at different stages, different ambitions. The first was to be an engine driver to drive one of the King-class locomotives from Paddington to the West Country. Another one was to open the batting for Yorkshire. And the third was to become a Fellow of the Royal Society. And, in the event, I only managed the last one of those.

I developed a love of mathematics from a very early age, and when eventually I matriculated at school, it was the system then that you could go on and spend two or even more years in the mathematical sixth form where you spent almost your whole time doing mathematics, just a bit of physics and current affairs, and free periods when we used to play bridge (we were meant to be working, but we played bridge, under the chestnut trees in summertime). We had three specialist mathematics teachers covering analysis, calculus and modern geometry, and I think I owe a lot to them. I think I remember especially the year 1940, which was spent in this very pleasant environment doing mathematics, because at the same time the Battle of Britain was going on overhead and added a lot of excitement to life, real excitement at that age. There was no sense of danger, it was all marvellous fun. Croydon was right in the thick of it, and we used to watch the air battles going on. We were very close to Croydon Aerodrome, which was a Spitfire and Hurricane base.

Well, in early '42 I went up to Cambridge. And the first year I did Mathematics Part 1, Mathematical Tripos Part 1, but I could only stay on at the time without going into war service if I did something more useful to the national war effort. So that's how I became a physicist. I went straight into Part 2 Physics, which is fairly tricky because the great majority of people in

Part 2 Physics had already done two years of it before, so it was a real challenge. But I enjoyed it very much, and I was very inspired by the sort of grandeur of the approach, the wonders of quantum mechanics and relativity and that kind of thing. It was hard work, it was six days a week.

In the event, I left after the second year and had the choice of joining one or other of the three services or going to research, radar research, or industry. And I joined the navy, because I'd always had a great interest in ships and the sea in general. It turned out, since I went up one term late when I went to Cambridge, that I only spent five terms at a university, and I've only spent five terms in my life at a university. After a year away, they give you a wartime degree, which was a Bachelor of Arts. But a few years later, one paid £5 and it became a Master of Arts. That's the way things happen at Cambridge. And after ten years of research, I collected a whole lot of papers up and sent them to Cambridge and, after a two-year deliberation, they gave me a Doctor of Science at Cambridge. And that's my very limited university career.

AM: And what of your wartime career?

PW: I joined the navy then, in July 1943, and I can remember my title was 'Probationary Temporary Acting Sub-lieutenant (Special Branch RNVR)', which was a fairly lowly kind of creature in the navy. I also remember I had to go there in uniform, buy a uniform from a London tailor, and I set off to go from Croydon to Portsmouth. I got to South Croydon Station and then encountered a Grenadier Guard who cut me off a tremendous salute and I had no idea how to salute. So I gave him a Home Guard one. And he looked a bit strange! And when I arrived at the Royal Naval Barracks at Portsmouth I walked up the steps, and there to greet me was a three-badge sailor, an old salt (a 'three-badge' is not for promotion, it's for long service), and I told him who I was and that I'd come to join the navy, and he said, 'Thank you, sir. But haven't you left it a bit late?' And such was my entry into the navy. I did six months on a training course at Portsmouth as a radar officer. The course at Cambridge had included a special radio course, which had prepared me for a lot of this.

Then I was given an appointment, a sea-going appointment, which was the grand battleship the *King George V*, and that was my home for the next two and a half years. I joined at Scapa Flow and spent some time there, and then came down to Liverpool for a refit. They were fitting us out to go to the Far East, and eventually we did in fact sail for the Far East and we became flagship of the British Pacific Fleet, having done odd attacks on the way through the Mediterranean, and at Palembang oil refinery where the carrier forces bombed very heavily, we went on to Sydney which was our rear base.

Then we took part in the Okinawa campaign, a hundred days at sea, which was made possible by a big 'fleet train' of supply ships. Then we came back to Sydney and then did another assault, actually on the Japanese mainland, including a night bombardment of Tokyo. In both campaigns the fleet was frequently attacked by Japanese suicide bombers, but they tended to concentrate their attention on the aircraft carriers, and our ship was not hit.

One story I've given the name 'How I won the war'. We were ploughing through the Pacific and the telephone rang in my radar office. It was the officer-of-the-watch saying the Captain wanted to speak to me. And this was fairly unusual. The Captain said, 'Is that Wild?' And I said, 'Sir.' He said, 'Hold on, the Admiral wants to speak to you.' And this was unheard of. And the Admiral said, 'Is that young Wild?' And I said, 'Sir.' He said, 'They tell me you're a bit of a mathematician.' So I said, 'Sir.' He said, 'Tell me, what does "normal" mean?' And I knew about six different meanings of the word 'normal' but I knew that with admirals you don't confuse them with options, so I made a choice, and I said 'at right angles to, Sir.' And he just said 'Good God!' and put the phone down. Then I went into the darkened radar room where the plan position indicator was there in front of me, and of course your own ship is automatically in the middle of that. Astern of us was a sister battleship, the *Howe*, and we were surrounded by four huge fleet carriers, and outside them there was a ring of eight cruisers, and finally twenty-four destroyers in a huge circle round the fleet. And that's how we were proceeding through the Pacific. And I just gazed at the screen, and within the next twenty minutes or so the whole fleet turned, tick, tick, tick, slowly, gradually it turned through ninety degrees. It made a deep impression on me!

Well, we finished the war by entering Tokyo Bay just after the surrender. And we were with the American fleets, we were parked quite close to the *Missouri* where the peace treaty was signed.

AM: Australia came into your life, when calls into port at Sydney were made?

PW: That's how it came into my life. I returned to England and then left the 'KG5', and I finished up teaching radar to permanent naval officers until about February '47. During the Pacific operations, the Sydney-based operations, I had become engaged to be married to an Australian girl, Elaine Hull. And first thing on leaving the navy I went back to Sydney as fast as I could on the next ship. I'd already lined up in London a job with the Radiophysics Laboratory at CSIR. It was a tremendous contrast, between the wonderful sunshine of Australia at that time and the miserable winter and conditions in England in 1947, and I was glad to get out of it.

My appointment was a humble one really. It was simply to maintain and develop test equipment. But after a year I wheedled my way into radio

astronomy research. Taffy Bowen was the Chief [of the Division of Radiophysics]. But Joe Pawsey was the sort of father of radio astronomy in Australia.³⁵ And that was a tremendously exciting time, round about 1948, '49, '50. Really the only two really powerful groups in radio astronomy were the Australian one and Cambridge; and it's a matter of argument but we all thought we had the edge over the Cambridge group. Pawsey was a wonderfully inspiring leader, very self-effacing and taking no credit for himself, and he was a delight to work under. And he did something which I appreciated greatly: he left me alone to do my own research but I could come to him at any time and get his advice; and also there would be regular meetings with all of us research workers under his chairmanship.

The Sun became my field, and that's been my life work really, solar research. I began, at Pawsey's suggestion, by building a radio spectrograph, which was the first one ever built and which actually looked at the spectrum of bursts of radiations from the Sun over a wide spectral range for a wide range of frequencies and produced some spectacular results. We [Wild, a Technical Assistant, and later a Technical Officer] set up a field station for observing, by the railway line at Penrith in the foothills of the Blue Mountains. We had a wooden aerial which we pulled round with ropes with the aid of a winch, and every twenty minutes we changed it so that it pointed towards the Sun. Anyway, at the end of four months of observations we had so much data that we packed it all up, returned to the lab, and I started to analyse it. And in the first paper that I wrote I remember writing that 'we have identified three distinct spectral types of burst'—and this is all for the first time—'and for the purpose of this paper we shall call them Type I, Type II and Type III.' And nowadays that is the international classification, the accepted nomenclature.

We next built a much better engineered and more powerful spectrograph. John Murray was the electronic engineer. And for the next ten years or so, at a new field station at Dapto, south of Wollongong, we went from strength to strength. And I don't want to be immodest, but there's no doubt that we led the world in this kind of research.

Perhaps I should explain something about this work. The radio spectrograph had a mechanically tuned receiver; that is, the received frequency was continually swept between two limits (actually in the metre wavelength band, around 100 MHz). The results were expressed as a two-dimensional picture or contour diagram, one dimension being time, the other frequency. The contours or picture brightness showed the intensity of radiation being received from the Sun at each instant of time and each frequency. The radiation from the Sun came sporadically in bursts. Sometimes you would wait for days, even weeks, with nothing happening. Then suddenly all hell would break loose and a huge outburst would occur. It was very exciting. We recorded all manner of patterns, but the most

fascinating was the appearance of mottled bands which were *inclined to the time axis*. That is, the peak emission gradually changed frequency; always the high frequencies came first, gradually decreasing in frequency.

Through a long series of observations, involving high-precision directional observations, as well as frequency-sweep, plus a lot of theory, we were able to prove the meaning of these slanting bands. Around the Sun is a huge atmosphere (the 'corona', seen only at total eclipses) of ionised gas. This is where the radio waves originate: different frequencies originate at different levels, high frequencies closer to the Sun's visible surface, lower frequencies further away. This meant that the slanting bands were due to the source of radiation *moving outwards through the solar atmosphere*. With the aid of models based on eclipse data, we were able to work out the height in the solar atmosphere corresponding to each frequency; and so from the slant of the bands we could work out the speed at which the source was ascending. It turned out that the speeds fell into two very distinct categories. One was around 1000 kilometres per second (km/s); the other at least a hundred times faster, or about one-third the speed of light.

The slower velocities were associated with great outbursts at the time of large solar 'flares'—these events are seen optically as sudden brightenings on the face of the Sun and are the biggest outbursts of energy in all the solar system. A disturbance moving outward at this speed would take about thirty hours to reach the Earth's orbit. Now, thirty hours after a major flare one sometimes sees a display of the aurora in the night sky; we had discovered the answer to a century-old riddle—What was the agency which conveyed the disturbance from the solar flare to the Earth? This agency showed up on our spectrograph as the 'slow' velocity bands; we concluded (and later verified with the aid of the radio heliograph) that the agency was a steep-fronted shockwave travelling at some 1000 km/s.

To finish the story, we identified the 'fast' velocity agency as bursts of electrons, emitted from flares and at other times. Travelling at 100 000 km/s, the electron bursts would take less than half an hour to reach the Earth's orbit. There remained a few sceptics about this interpretation until, a decade or so later, American physicists using satellite data regularly detected bursts of electrons twenty-five minutes or so after solar flares. This was just one component, but to me a very exciting one, of a much wider research program.

We'd been studying the Sun by indirect means all this time by looking at its spectrum, and what we really wanted to do was to 'see' the Sun in radio, really to get a radio image and get moving pictures of the Sun. And that was a very considerable challenge. To get the same resolution as the human eye, we were dealing with wavelengths several million times larger than the lightwaves, wavelengths of nearly three or four metres—and you would have to build an aperture several million times bigger than the eye and this came

out to three kilometres diameter. We did this by building a ring of ninety-six dishes, each of them over forty feet in diameter, and did it as cheaply as possible. And I devised a method of converting this ring and making it act as though it were a solid aperture, and that was the key to the radio heliograph.

Through Pawsey's help we got the whole thing funded. I think it was \$630 000, and it was all funded by the Ford Foundation. We found a site, four square miles of flat land, at Culgoora, near Narrabri. It stayed in operation for seventeen years, providing a tremendous amount of data and insight into the way the solar corona works and the relationship between solar and terrestrial phenomena.

AM: This was a time when the Menzies government was giving very large sums of money to physics at the Australian National University. What was the political reaction to such tremendous results which were so modestly costed?

PW: I think we were all carried away with the excitement of it all, I don't think we stopped to study that kind of question. I've always had some good supporters in the ANU. Mark Oliphant was a tremendous supporter. I had some wonderful colleagues. And if I mention one in particular, it's Kevin Sheridan who was the chief electronics engineer, and I've said publicly several times that without him the thing would never have worked.

I remember an incident on the opening day in ... when was that? ... 1967, I think. And it was opened by the Minister for Science, John Gorton. I first went round the circle with him in a car (that's quite a long ride) and explained things to him, and he took them in remarkably well. He didn't seem to be listening, but when he gave his speech he reproduced quite a lot of what I had told him. And then at the end of the ceremony I took him and showed him the instrument and I explained the principle of the heliograph, how the ring acted really as one huge mirror. I said, 'What we would really like is a paraboloid three kilometres in diameter,' and he said (this was just after his second whisky), 'If you need one of those, you shall have one!' And so we got great moral, if not practical, support.

AM: What determined your choice of solar research when you came to CSIR, instead of other parts of radiophysics?

PW: It was really very arbitrary. When I joined the solar group I was given the option of joining a colleague, John Bolton, to do work on radio sources (and he'd done some very important pioneering work) or else joining another colleague, Lindsay McCready, on building a solar spectrograph. And I knew perfectly well that if I joined John Bolton I would be very much a second-in-

command, and I knew if I joined McCready I would be able to do my own thing. And that's why I became a solar man. [*laughs*]

AM: At that particular time, how would you describe the culture of CSIR? It was largely the Rivett philosophy of getting the money to good men and giving them their head, was it not?

PW: Yes, I think the famous Rivett philosophy was to determine the field of study that you want to do, find the best man in the world you can get to lead the group, and then give him his head. So it was very much the 'divine right of chiefs' in those days.

AM: And this brings you to your period as a Chief of Division from 1971 to '77. What were the special research thrusts and the areas of important work that your Division concentrated on at that point? It did have a very high profile and reputation.

PW: Well, the retiring Chief was Taffy Bowen, who had been Chief for a very long time, ever since the war finished in fact. And he had built a number of programs, but they had finished with just really two, which was radio astronomy and cloud physics (that's including the rain-making business). And with his departure there was no point in coupling the two together and so cloud physics became an independent division. Then I found myself being chief of a division that was doing nothing but pure research and I felt very exposed. And I thought it was very important, as an insurance policy really, to protect radio astronomy, to get involved in some applied project which was easily seen to be useful to the community, yet using the techniques of radio astronomy.

And in my looking round I found a very good source of cooperation in the Department of Civil Aviation, in particular with Mr Egon Stern, who was the head of their research and development, who was a delightful person and very cooperative and loved the idea of getting CSIRO involved. So I started a dialogue—well, when I say 'I', I and a number of colleagues—with the department over a period of six months or so. Every three weeks we would meet alternately in Sydney and in Melbourne, and go over all their problems. It turned out that the biggest and most worthwhile problem facing civil aviation at that time was the development of a new landing system, a microwave landing system to replace the present ILS (Instrument Landing System).

AM: So this is the prelude to your great contribution to Australian science and technology, of Interscan.

PW: That's right. And it just so happened that these deliberations coincided with the fact that ICAO, the International Civil Aviation Organisation, was calling for member states to propose new systems. So there was, in other words, tremendous competition just starting. So far four countries—America, Britain, France and Germany—had put in submissions, and we decided to put in our own. The reason was that I had cooked up a system which looked very simple and very attractive, and this was the Interscan System. It's a method of allowing an aircraft to tell its angular position by sweeping a beam across from left to right and then from right to left, and the aircraft picks up two pips, and by the spacing of those two pips it can tell just where it is. Of course the scanning can't be mechanical scanning, it has to be electronic, and the whole scan cycle is over in a hundredth of a second or so.

AM: The background to this—which you've briefly alluded to—is the need for standardisation and the need for a standard landing system throughout the world, and the need to take account of different climatic environmental conditions of areas where planes are landing, and the fact that the existing system was very diverse and unsatisfactory in some places?

PW: Yes, that is true. The environment is important. The present system used a longer wavelength and gets much more prone to reflections which can produce erroneous results unless great care is taken. So one thing was to get the wavelength down from about 50 down to 6 centimetres. The other thing was to allow aircraft to land in bad weather from all angles. At the moment they had to go back about ten or fifteen miles and come down towards the runway along a straight path. Well, the idea of curved approaches, which is done a great deal in good weather, couldn't be done in bad weather; but they wanted us to do this in bad weather too. And, oh, there were a whole list of things where a whole lot of improvements could be made.

Anyway, they started this huge international competition, which got very fierce at times but it was also accompanied by a fair amount of humour. The other countries had their supporters. Australia had no supporter whatsoever, although the Americans were very kind to us. The way we got in is quite remarkable. I think it was December '74, the Americans had been developing two systems in parallel—one was called the conventional scanning beam, and the other was called a Doppler system—and they decided to have a grand evaluation to choose between these two systems. And they did this in a typical American style. They got two hundred people (both from within their transportation department, industry and aviation), two hundred of them, more or less shut up in a building for four months in Washington, to evaluate, examine, test and flight-test results, and look at every possible aspect. And they invited Australia to take part in this, and in fact they made

me one of the seventeen on the steering committee which was going to make the final decision.

After two months of this, one of the two systems showed up so poorly—that's the conventional scanning beam—that it obviously had to drop out. So the Americans, just to keep the competition going, allowed our system to come in vis-à-vis the Doppler system—much to the fury of the British, who were the main supporters of the Doppler system. Well, finally, the final vote came (very close to Christmas, I remember), and the impossible happened and our system won the day. We won because of the simplicity of the Interscan concept and the superb flight-test results. The Americans from then on adopted our system.

Well, there was a long way to go yet, because the world hadn't adopted the system, only America and Australia had. Our first new ally was the Soviet Union, and so it was the three of us more or less against the rest of the world. And then there continued an international evaluation under the umbrella of ICAO which went on for a number of years. Eventually the expert committee decided in favour of our system (of course the Americans called it their system then, 'Time Reference Scanning Beam', as they call it). However, the British would not accept the decision and they worked hard to overturn it. They employed a lobbyist in Washington to do the dirty work, and they caused such a stir that there was a congressional inquiry over the whole matter; and the result of the congressional inquiry was, 'Both sides have been naughty boys. Can't you get down and work it all out and produce the right result?'

And the final outcome was that ICAO decided to hold a full-scale vote for ICAO members, in Montreal. I think that was '77 or '78, it may have been '78. This was preceded by two weeks of technical sessions. And people came from all over the world. There were seventy-seven countries represented, and it was an extraordinarily tense business. It finished up (I think, from memory) 37 to 24, a very clear majority, and the rest were abstentions. This was what the President of ICAO described, after all this was over, as 'the greatest day in the history of ICAO'.

AM: It was a tremendous triumph for Australia. It became one of Australia's great technological exports. Local development of that sort of project apparently was not something that could be done in Australia?

PW: Nothing was 'patent-able', partly because it was a concept and partly because anyone who did try patenting was given the cold shoulder by ICAO because they wanted it to be 'make-able' anywhere in the world. In the end the Department of Productivity decided to form a company, Interscan Australia Pty Ltd, with the backing of the government.

Interscan is still very much alive.³⁶ It's had a number of small contracts for landing systems—you know, half a dozen here and there—but hasn't got the big one yet. The whole thing is coming along very slowly, but we're still in the game, and we're still hoping.

AM: In 1978, you became Chairman of CSIRO. You took up this task after the first Independent Inquiry into CSIRO in 1977. It pointed the organisation towards what was described as 'filling a gap in national research with strategic mission-oriented work'. Did your appointment as Chairman rest to an extent on the fact that you had put the division, and Australia, on the international map and you had this capacity for applying very fundamental work?

PW: Yes, I think it certainly must have done. I don't know that I really wanted the job, but it was a duty to take the job when they offered it to you. I'm sure it had an influence!

AM: How did you see your particular ethos and style of management of CSIRO?

PW: Well, I suppose I always wanted to extrapolate from my own experience, small-scale experience, and some personal opinions. I had a strong desire always to see pure and applied research undertaken side by side in the same laboratory, as we did in the Interscan thing. And I was still very much aware that the recognition that many—most, I should say—of the really important and fundamental discoveries in science which subsequently led to a massive technological development, have come through curiosity-led research, and often through serendipity which is terribly important. I think the Birch report was very helpful, and I agreed with his report that the whole core of the policy-making in the science that we were going to carry out was a matter of defining the right proportion of, shall we say, pure research, strategic mission-oriented, and technical research, and getting that proportion right. And it's a question that can be debated for a long time. I think there were two sides to the question. On the one hand, I felt that fundamental research in the end was going to reap the real benefits in the long run. I mean, discovering something like electricity, you know, which was absolutely fundamental research and had an enormous impact on the world. On the other hand, there was the awareness that you're in charge of an organisation that's spending a million dollars of taxpayers' money every day, and that's a very sobering thought, so you had to keep the reins on as well.

AM: What were the worst aspects of being Chairman at that time?

PW: The frustration of finding it so difficult to bring in new young blood because of the descending budget in *real* terms. I tended to try and offset this by going for one or two specific projects which I knew were very very important, and one of them (which was on the top of everybody's list at one stage) was to upgrade our whole effort in oceanography, including the need for an oceanographic research vessel. And another thing was we got through the Australia Telescope, which was I think really needed to restore Australia's position in radio astronomy. And that was with the aid of [the Minister]. The Prime Minister got the message and insisted that it go forward. And the third thing that Fraser was very keen on was the [Australian] Animal Health Laboratory at Geelong, which of course caused me a certain amount of bother.

But all those things came in Fraser's time, and I can only say that since then—I know that Barry Jones is a very electrifying character—but CSIRO hasn't prospered as well since the change of government. And of course I had terrible trouble over the Animal Health Laboratory when he wanted to close it all down, just when it was nearly finished being built.

AM: This was an area in which social scientists came out with some pretty sharp criticism of CSIRO. They'd also come out in other respects I think on some divisions. One was the Division of Entomology. And there was quite a strong reaction from CSIRO—and indeed from yourself—as to the role of science policy critics of CSIRO. Did you feel that they had no right to intrude into that area?

PW: I suppose both sides were debating a question. Before that time (and maybe the turning point was when the Labor government came in) I suppose CSIRO was a sacred cow, beyond criticism; and I think we had to get used to criticism and get used to defending. I can remember *we* had a little battle.

AM: We did have a little battle over attitudes to policy critics. It's clearly a very attractive area of science policy for policy people to look at, because CSIRO has, as you say, been a sacred cow and it's been the science colossus in Australia. And little research has been done on aspects of its more recent development.

The project that's occupied you for several years is the Very Fast Train, and you are currently Chairman of the Very Fast Train Project. Would you tell us about the origins of the concept?

PW: I'm not too good on dates here, but I think it might have been October '73.³⁷ I had an afternoon appointment in Sydney and, instead of going by air as I usually do from Canberra, I took the XPT which was meant to be the super-train; and I took down a great amount of detail, the speeds and the times and everything. It was meant to take four and a quarter hours and it

took four and three-quarters hours. And I was absolutely appalled by the whole thing. And next day I wrote a letter to David Hill, who was Chief Executive of State Rail. I described my XPT experience and I said, 'Can we, CSIRO, help?'

So it started from there. It takes thirteen hours to get from Sydney to Melbourne. If you could do it in three hours, you'd be totally competitive with air, with Canberra a very nice stop on the way. And so it started.

On weekends I surrounded myself with maps on the sitting-room floor, and charted out a route. And then I got some other colleagues involved, there was no shortage of interest. And we put together a report in very quick time. I think it was May '74.³⁸ It got knocked back rudely by Peter Morris, who was the Minister for Transport. And I got myself in some trouble saying that this knock-back was characteristic of the malaise which the country is suffering, which got into a headline. So I sometimes put my foot in it.

But soon Peter Abeles rang me up, not long after the knock-back, and said, 'I think I can help you with a commercial solution to your problem.' And then bit by bit, with his support, I got a joint venture together. And that's when we did the main part of the work.

AM: The idea to revitalise rail travel, was it an Australia-wide concept, or highly focused on the Sydney–Canberra–Melbourne area?

PW: To begin with it was Sydney to Melbourne. Then it looked very feasible to extend it to Brisbane, and by the turn of the century to Adelaide. But no further, because if you go to very long distances, like Perth, you can't compete with air.

AM: Several critics actually—with headlines like 'VFT the wrong train of thought' and 'Technocratic dreaming' and 'A forbidding concept'—pointed out that Australia is perhaps the *least* appropriate country for the VFT because of the scattering of population and the long distances, unlike Japan and France where the concept was well under way. Would you agree with that at all?

PW: No, you see, our second largest city, which is Melbourne, is bigger than the second largest city of either France or Britain, and to have two large cities which can be joined by a three-hour journey, with the national capital in the middle, is just about ideal. And the French, with whom we've had many dealings, are envious of our situation, because their journeys are always single-ended, it's only Paris and nothing much else.

AM: I suppose the strongest resistance has come from the environmental sector, and the whole question of acquiring the land along the route and

questions of impact. How does a scientist and technological developer as yourself, see his role in terms of these large social questions?

PW: I think they have to be handled very sympathetically. I think you have to see the other's point of view. And very often you can, if you sit down with a landowner, you can do something for them which they never dreamt could be done. I think it has to be done on a very individual basis with landowners. Landowners are one thing, general environmentalists are another thing. But nearly everyone admits that the VFT has so many environmental pluses.

AM: Does the shadow of Isambard Kingdom Brunel linger in your planning of this?

PW: Oh yes! Yes, undoubtedly. He built the railway from London to Bristol. And in fact he was given the task to survey the line, and he was single-handed (apart from two assistants which were both described by him as incompetent), and what he did, he not only planned the line but he fixed everything up with the landowners. By day he went around the countryside on his horse, and by night—he was a total insomniac—he designed the railways and the bridges and so on. The whole thing done in three months.

AM: You've had this most unusual career of being pre-eminent as a pure researcher, with a capacity for seeing and going into great enterprises, and for the whole process of change. You've won many honours and prizes. Looking back over your varied and distinguished career, have you ever felt any sense that you did or did not do the right thing in coming to Australia and making your career here?

PW: I have absolutely no doubt whatsoever that I would never have had the opportunity that I did, in Britain.



Dr Helen Newton Turner,

AO, FTS

Interviewed by Ann Moyal
Sydney, January 1993

National Library of Australia
Tape no. TRC-2902

HNT: My name is Helen Newton Turner. I was born in Lindfield, Sydney, on 15th May 1908. I consider that I was very fortunate indeed in the parents that I had. There were two brothers and myself and I can't remember a time when it wasn't taken for granted that I, as well as my brothers, would go to university. My mother was a university graduate. She graduated in 1901; she was a university medallist in Philosophy and French.

My father had a very different story. He had to leave school when he was fourteen in order to support his family, but he joined what was called the Child Welfare Department at that time and he ended up as second-in-charge of the department, after periods as officer-in-charge of various homes.

I owe a tremendous amount to my parents. They were very self-sacrificing. My father instilled in us a tremendous honesty and integrity. And both of them were keen on us acquiring knowledge. There were lots of reference books about the house, and if somebody didn't know anything we were told to go and look it up. Father's salary was never very high, but we used to go on camping trips in the Christmas holidays up and down the coast of New South Wales. We were taught the names of trees and flowers and birds and fish and so on, and mother used to play games with us to make us remember them. So it was a very happy childhood, and I'm grateful for it.

I went to Mittagong Public School and then to Bowral High School while we were at Mittagong. I left in what was the middle of the Leaving Certificate year when Dad moved to Parramatta, and I went to Parramatta High School for the second half of that. I started high school at the age of ten, so I repeated fifth year and got the Leaving Certificate from Parramatta High in my repeat of the year. Then I went to Sydney University and did Architecture because I couldn't see anything ahead of science except teaching and I didn't want to teach. It wasn't till I got to the university that I realised there were other avenues, we didn't have any careers advisers in those days. We had to do twelve months' practical experience to get our architect's degree, so during all the long vacations I worked with the firm of Kent & Massie, and then I was with them in 1930 when I graduated. I finished the exams in 1929 and took my degree in 1930, and two days after the degree was conferred I went to a business college because Kent & Massie said they couldn't afford to keep me as an architect but I could stay on and do the secretarial work.

AM: Would you have been one of the very few women architecture students at the time?

HNT: There were four women in our year, and one in the year ahead and one in the year behind. Our year was special in having four.

AM: So that in fact though you had the architecture qualification, you didn't practise as that and you became a secretary. So how did your life switch from architecture into statistics and science?

HNT: Well, Kent & Massie closed at the end of 1930, and I then went into a state department to work for the Board of Optometrical Registration and I worked as a clerk there. Then in September of 1931 the McMaster Lab [at the Council for Scientific and Industrial Research (CSIR)] was being established, and they were advertising for a secretary. Much to my surprise I got the job—always intending to go back to architecture when things came good. But it was the mathematical side of architecture that I was always better at than the design side. At the McMaster I did the typing, I answered the phone, I did all the ordering. There was a statistician on the staff, and in typing his papers I got interested in the new discipline of statistics applied to agricultural experiments. And I got more and more interested. I did all the courses that I could around the university (which wasn't much, in those days), I did some extra mathematics. I wanted to go overseas to study more. And I was very fortunate that the man in charge of the McMaster at that time was Ian Clunies Ross, who was very enthusiastic about helping anybody who had any sort of ability or skills that he could see, and he arranged for me to have a year's study leave to go overseas to study statistics. He was my mentor, and I owe a great deal to him in that he encouraged us, and he saw to it that I got the year overseas.

I landed in Britain to work with R.A. Fisher, the 'initiator' of agricultural statistics, the design and analysis of experiments. He was at the Galton Lab at University College London, but he was becoming more interested in genetics then (strangely enough, isn't it, when I ended up as a geneticist later, but I wasn't so interested in what he was doing then). His idea of a project for me was to drop a Debrett on my desk and ask me to work out the inbreeding of the noble families of Britain [*laughing*] in which I was not in the least interested. So I finally worked part-time with him and went part-time out to Rothamsted Experiment Station in Harpenden where they really were doing design and analysis of agricultural experiments. I worked with Frank Yates at Rothamsted. Fisher was very much an eccentric but he was very very good to his students.

I landed in London, I think it was a week before Chamberlain came back from Munich waving his umbrella saying 'Peace in our time', and I wondered if I was going to get my year. And I left, I sailed from Southampton, the day after Hitler walked into Poland.

But when I came back [to McMaster] I didn't have to do any secretarial work any more. I was made a technical officer and I was a consulting statistician to the Lab. For war work I took the opportunity to go to the Research and Experiment Section of the Department of Home Security in

Canberra. But after the Battle of the Coral Sea [I joined Clunies Ross who was] Director of Scientific Manpower in the Manpower Department to help him with a register of scientific personnel and I did that for a couple of years. Then gradually the war was moving further and further away from Australia and I went back part-time to the McMaster.

AM: So then began your long and very productive career in the Division of Animal Health and Production and your work on breeding of Merino sheep and the whole wool research program.

HNT: Well, it was all luck, all the way. And again [Clunies] Ross played a large part in it. I was Consulting Statistician to the Division of Animal Health and Production, and a wild Irishman, called R.B. Kelley, soon after the war was appointed to take charge of the genetic work.

Clunies Ross, I may say, was always keen on genetics. He became Chairman of CSIRO (he was with CSIR, a member of the Executive first of all, and then it became CSIRO, and he was the first CSIRO Chairman) and he was always keen on genetics. He initiated CSIRO funding and finding the people to start schools of genetics in at least three of Australia's universities. And he brought Kelley in to do breeding work. And Kelley started large-scale breeding work; with poultry at Werribee, with beef cattle at Rockhampton and he started sheep at Cunnamulla in south-west Queensland.

Now, data began to accumulate on the sheep, and therefore the statisticians were called in to help handle the data. And gradually I got more and more interested in that. And I insisted on going up and seeing how the data were collected on the sheep, partly because I was interested in being out in the field, but partly because I felt that you ought to know how everything is done all the way along.

Then Kelley retired. The experiments had been going since 1947 at Cunnamulla, and it was 1956 when Kelley retired. And in addition to the breeding experiments at Cunnamulla, Kelley had started a comparison of the various strains. The Merino is not an entity, there are various strains of Merino from fine wool through to strong wool, and Kelley had started an experiment to compare these strains in three environments—Cunnamulla, which is in south-west Queensland; Armidale, which is on the New England tablelands; and Deniliquin, which is in the Riverina. And Ross suggested that all the breeding experiments at Cunnamulla and the Strain Trial should be brought together into an Animal Breeding Section and asked if I would like to lead it. That was 1956.

AM: This was a great achievement in that period, when science was strongly male-dominated. Were you in a singular position?

HNT: It comes as a great surprise to everybody to learn that the first three agricultural statisticians in CSIRO were women. There was Betty Allan at Plant Industry in Canberra, who was sent by CSIR over to work with Fisher; there was Mildred Barnard (who was the daughter of a professor of mathematics from Melbourne), she took herself over to work with Fisher and Yates and when she came back she was employed by CSIR at Forest Products in Melbourne; and then there was myself. During the bicentennial year the *Australian Journal of Statistics* had the three of us written up.

AM: So you were obviously a front-runner in the sense that you were moving into a wider field of research and data collection with the breeding. Could you tell us about this?

HNT: Well, it was a question of how to improve wool production in Australia. There had been a lot of work done overseas about introducing quantitative genetics into breeding of farm animals. And the first step in introducing quantitative genetics is to *measure* things instead of judging them by eye. We had traditional stud breeding of sheep built up, particularly in eastern Australia; we had traditional studs who had increased fleece weight. They had about doubled it from what it was in the middle of last century, till about the 1930s. The graph went up like that, and then it flattened off. And we reckoned that if they could be persuaded to introduce measurement they could come back, perhaps not to that steep rise but at least to increase the rate of progress.

But the important thing is that, if you're trying to improve something by selection, you first of all have to know the heritability. In other words, if you select superior parents, are their offspring going to be superior or not? And that depends on the heritability level, whether the thing is inherited or not. The next thing is what is the intensity of selection. Now, this is why rams are more important than ewes, not because they're any better at passing their superiority on but because you use fewer of them and therefore they can be more superior. So there's superiority of the animals you're using above the average of the group. The heritability and the superiority are the important things. Now, in both of those accuracy is important; and obviously if you weigh something it is more accurate than trying to guess it by eye.

But to try and convince traditional stud breeders of this has been a difficult task. I mean, it isn't yet won. But this was our aim. First of all we had to estimate the heritability in our Australian Merino. Now, I wasn't the only one doing this; there were others in the New South Wales Department of Agriculture, particularly Fred Morley, he was a pioneer and Bob Dun who followed him.³⁹ But we had to work out the heritability, which meant we had to measure everything. We were measuring greasy fleece weight as it came off the sheep. Then a sample was taken from the

mid-side to measure the fleece characteristics, a small sample, and a larger sample to measure the per cent clean yield so you knew how much clean wool you had as distinct from the greasy wool. We also scored the animals for the amount of wrinkling (the Merino of course is the main animal for having a lot of skin wrinkle). And we scored them for the amount of wool cover on the face.

We had to develop all the scoring techniques. We had to decide where the sample was to be taken from that would give us the average fibre diameter, staple length, and the number of fibres per square inch of skin which we were measuring too, and the number of crimps along the staple, little waves along the staple.

At that time—I'm talking about the 1950s—the wool was sold on eye appraisal, and one of the most important things for grading it was this number of crimps. That was thought to be an indicator of fibre diameter. Now, a man called Lang down at the Gordon Institute in Victoria for a long time had been saying that crimp was not a good guide to diameter, and Dunlop and Roberts of CSIRO did some analysis of the data from the Strain Trial and showed indeed that Lang was right and the relationship between crimp and diameter varied with the strain of Merino; it was weaker in the fine wools than it was in the strong wools. So the breeding researchers started talking about the possibility of measuring diameter for selling wool instead of relying on crimp. It wasn't only the breeding researchers advocating this. In the 1960s the Federal government voted a large sum of money to establish the Objective Measurement Policy Committee, involving people from all sections of the sheep and wool industry. This Committee sponsored a tremendous amount of research on how measurement might be incorporated into the selling system. Now, of course, wool is sold on measurement, and average fibre diameter itself has replaced number of crimps as the indicator of fineness.

I used to have arguments with my colleagues about the importance of diameter. Because crimp was so much ingrained in the selling of wool—I used to have *great* arguments about the importance of measuring diameter because it was an expensive thing to do. And I insisted that we in our work should do everything on the measured diameter instead of on the crimp.

AM: It seems that there was a great deal of conservatism and pessimism among geneticists about this.

HNT: The conservatism was in the stud breeders and the pessimism was in the geneticists. It's been very difficult to persuade them [the breeders]. Of course, I think we might have gone about it the wrong way. I think we might have been so sure that we were right that we rammed that home a

bit. I mean, this is speaking from old age probably, but I think if I had to do it again I would do it a little bit more gently. I had to speak at a lot of graziers' meetings round Australia and there was one in South Australia that I remember very clearly. It was mainly a group of stud breeders, and I was talking about wrinkle not being a desirable thing. You see, a lot of stud breeders like a lot of wrinkle because they reckon with more skin they get more wool, but what they don't realise is that it's more greasy wool, not more clean wool, because a lot of grease comes out of the wrinkly fellows. And somebody from the back of the hall stood up and shouted at me, 'You're taking the bread out of the mouths of the stud breeders!' So I wasn't always popular. And there was pessimism among geneticists because it took so long.

AM: From the perspective of getting these messages across, as it were, and pioneering messages, in your judgement was it an advantage your being a woman?

HNT: Well, I was much more accustomed to talking to a group of two or three hundred men than to women. It terrifies me to talk to an equivalent group of women I may say, I'd much rather talk to the men. But there was a very good extension officer in Queensland called George Moule, and he used to reckon that I had an advantage, that the minute I stood up to speak everybody sort of blinked and looked at me. Everybody came awake. And I do remember with joy two country ladies saying to me, 'We were so proud of you, standing up there answering the men's questions!'

AM: Did this work develop hand-in-hand with the work you did on twinning in sheep?

HNT: Well, the work on twinning is what I'm most excited about actually. We had Deniliquin Station—we had sheep down there, the Strain Trial was down there. Now, at Cunnamulla we had something like 15 per cent twins each year on the average, over the years. And those sheep that were sent down to Deniliquin (which is a very much better sheep environment), produced many more twins down at Deniliquin than they did up at Cunnamulla.

Now, that experiment ended. And the officer-in-charge at Deniliquin said, 'For goodness sake, you can't just send these ewes to the abattoir, they've been producing a lot of twins. Can't you do something with them?' So we said, 'Alright.' The accepted wisdom at that time was that twinning was more due to environment than it was to genetics, but from those sheep we picked out some which had had twins twice in the two years they'd been at Deniliquin and some which had had singles in those same two years. We sent

down rams that had been born as twins or singles from Cunnamulla to mate to them. And we went on from there. And those ewes continued—one lot to produce more twins and the others to produce more singles, and not only that, their daughters did the same.

So I got quite excited about this. There was the [CSIRO] extension journal, *Rural Research*, in which these results were published and we had a letter from the Seears family near Cooma who said they'd been selecting for multiple births in their sheep for some time, and 'would we care for a Merino ram which had been born alive in a set of five?' So I went hot-foot down there thinking they probably weren't Merinos, but they were Merinos all right. So we gratefully accepted that ram, and I bought a dozen ewes which had been born as trips or quads, and we took them back to Deniliquin. And the next year they gave us another quin ram, and the following year they gave us a sextuplet ewe.

Now, they had only been selecting on the ewe side, not on the ram side, they'd been just buying their rams in regardless of the twinning history. They'd had a lambing percentage of about a hundred and seventy-something if I remember rightly. And by selecting on both sides we got that up to well over two hundred, which for Merinos is very good indeed. And we named that flock the Booroola flock, because Booroola was the name of the property that we got them from in the first place. Now that flock is famous all round the world. Dr Piper, who took over the flock when I retired, has established that its prolificacy is due to a single gene.

AM: Your own part in the national diffusion process was considerable. I gather that you were very highly regarded as a broadcaster.

HNT: Yes, I was quite surprised when, having to move my room at the laboratory recently and turning out my files and sending a lot of it off to CSIRO archives, I found I had copies of all the broadcasts that I gave. At one stage we had to write the broadcast before we gave it and circulate it to Departments of Agriculture so that extension officers would not get caught out by a breeder in the field saying 'CSIRO says so-and-so' and they hadn't heard about it. So I had these. And I was surprised at how much I'd done all through the fifties and sixties. Dick Thompson was in charge of the [ABC] Rural Department then and he used to ask me if whenever I went to a meeting or did something overseas I would give him a talk on it.

AM: What about Departments of Agriculture? Was the relation a very close one between the research groups?

HNT: Oh, there was a very close collaboration milieu. The Department of Agriculture in New South Wales in particular were doing work on

similar lines to our own, and we collaborated a lot and exchanged views a lot. But also Jim Gill, who was Chief of Animal Production, instituted a scheme whereby extension officers came for a training period round the research laboratories. This meant that we knew all the extension officers, and there was a close cooperation.

AM: There's a temptation, looking back on a very productive period, to see the really excellent science of it. Looking back in another sense, what do you see as the major problems of that time?

HNT: I don't think there were many problems in the research. They were halcyon days in the fifties and sixties. We could start an experiment and there never seemed to be any lack of support for it from CSIRO. It wasn't like today when, if somebody leaves the lab, there's no replacement. It was a golden age.

AM: And I imagine that, having Sir Ian Clunies Ross as Chairman of CSIRO and a man who was deeply interested in the work you were doing, would probably have been a great bonus.

HNT: It was a tremendous bonus. I don't think we would have got anywhere without Ian. He was always a tremendous support.

AM: Do you see him as perhaps the greatest of the CSIRO Chairmen?

HNT: Yes, I think he was. But Fred White was pretty good too. It was Fred White who started what were called the wool labs, dealing with the processing of the wool. That was Fred's baby, and he was very good.

AM: White was a physicist by training whereas Clunies Ross was a veterinary scientist. But I think one of the things historians are finding, is that Fred White comes very strongly out of the documents as a Chairman; and Ian Clunies Ross (though he had great charisma if you knew him, and many who knew him thought that he was perhaps one of the most exciting and the greatest Chairman), he comes less well out of the documents.

HNT: I think this could very well be so, because as you say, part of his success was his charisma. He could speak, he could wind an audience around his little finger with the greatest of ease.

He was a tremendous help. I don't think I would have got anything like the career I've had without his help. But successive Chiefs and Assistant Chiefs also gave support.

AM: Do you think that you were very singular as a woman in science? I don't think many women would have had such a successful experience as you did at that time. It was more of a struggle, for women.

HNT: Well, I don't really feel I ever had a struggle. In fact I don't know how to say this, but it's always astonished me that people think I've done anything. I just did what I wanted. I loved doing it. I loved being out in the field. I wouldn't ever ask anybody to do anything in the field that I hadn't already done or was prepared to do myself. I have never felt that I was discriminated against in any way. I've had that question asked by reporters many a time, I may say. And I can remember I was at some meeting up in Canberra and they asked the question as usual. And I went back—I think it was a group of people from the various Departments of Agriculture—and I said, 'Do you know what question I had to answer for the umpteenth time?' and they laughed and said, 'We just regard you as one of us.' And I don't believe in this business of insisting that a woman get a job just because you have to have a woman there, I think you must succeed on merit. And I just enjoyed what I was doing. I think I had enthusiasm about it, which probably appealed to people. I have never ever felt discriminated against.

AM: You've had a great deal of work with the developing countries. In fact in a sense you have spread your knowledge and expertise across countries as wide apart as Argentina, Peru, Africa, Norway, India, Malaysia, Pakistan, Fiji, Bulgaria and the Soviet Union, and China—it's the most extraordinary roll-call of countries where you've attended and led delegations, held seminars and international meetings in connection with your work—and for AIDAB (Australian International Development Assistance Bureau) and FAO (Food and Agriculture Organisation). What would you say were some of the influential meetings?

HNT: There was an expert panel on conservation of animal genetic resources formed by FAO a few years ago, and this has just been stepped up. I've forgotten what they call it, but anyway they're hoping to put more force behind it because a lot of the breeds are becoming extinct. You see, Australia has been guilty of this. We have sent our wool sheep into humid tropical areas and we don't even try to grow them in such areas in Australia itself, and we've either sent them as aid or we sold them and made money out of it, and they've failed every time. And this is a soapbox of mine. On one occasion I went and spoke to the people who are responsible for authorising the export of animals from Australia, and begged them not to allow these things—because they'd died all the time, and it's hard on the sheep as well as being hard on the people who've bought them or been given them. The aim in importing breeds is to replace the existing ones, which are better adapted usually.

I was absolutely astounded when I was asked to chair the first big meeting of FAO on animal genetic resources. And I don't think it was me, I think it was because Australia was never a colonial power. And you have to be very careful with the developing countries. And in my association with FAO over the years now I have become very conscious of how careful you have to be, that they are getting very sick of being told what to do by us. What worried me about AIDAB was, you'd be working for AIDAB and you'd write a very technical report and you'd feel there wasn't a soul in AIDAB that was capable of understanding it. But I think that changed when Bob Dun went there.

AM: How would the transfer of the sort of data that you were putting in the report be made to interested countries in a way that was going to be useful to them?

HNT: There was one big project in India which was a joint project between Australia and the Indian government, and a great deal of money was spent on both sides. On paper, it looked splendid. There was a stud established along Australian lines on the Gangetic Plain, a hot humid environment. It was established back in the late sixties or early seventies when we couldn't export any Merinos at all, the Indians wanted Merinos and we couldn't let them have them, so we sent Corriedales. And the Corriedales just wouldn't breed. The Corriedale rams were to be distributed to state farms for crossing with the local breeds of sheep and the cross-bred rams were to be distributed to the villages.

Now, on paper that looked splendid, that's the way you should get genes in. It resulted in *not one single valuable lamb*, because there was no training of the villagers. That wasn't built into it. There was no point in distributing a better ram unless you trained villagers in how to look after the better ram. We just assumed that doing things the Australian way was the thing to do. Now, I think that's a mistake we wouldn't make again, probably.

AM: Yours is a tremendous and formidable career. One thinks of you in an ambassadorial role, going forth with this expertise which is tremendously important in different countries, and communicating and reaching the right organisations and clearly having a vast impact.

HNT: Having a vastly good time, too!

AM: In 1970 you were awarded a Doctor of Science on your collected publications. The citation is significant. It says:

The scope of the work is immense. It represents over thirty years of research directed towards the single objective of improving the economic value of Merino sheep. The amount of work is prodigious, the quality is outstanding.

After this remarkable career, how would you advise young women who may be contemplating a life in science?

HNT: My advice to young people is to be prepared to take anything. There's a tendency amongst the young today to say they won't take a job except the one they've been trained for, and I think they ought to be prepared to give anything a go and to give it all that they've got.

AM: On the education of women scientists, your feeling is that, if you are prepared to take things on and go ahead as if you expect to be received as an equal, that this will happen?

HNT: I think so. Well, that has been my experience.

AM: Perhaps it was easier for an earlier generation to take that view rather than in this more specialist period when people opt early.

HNT: Yes, this is so. But what you've got to do is to stop the people, who are graduating now into a high level of unemployment, to stop them feeling that they must have what they've been trained for.

I have a tremendous loyalty to CSIRO. And I had a tremendous amount of personal loyalty to people who have helped me along the way. And I'm not sure that the young nowadays expect to have that.

AM: Finally, having spent a lot of your life gazing at them and manipulating them, you must have a very particular view of sheep?

HNT: I certainly have. It's a fascinating animal. These animals will grow everywhere from seashore up to the roof of the world in Tibet. They grow, with different breeds and different characteristics, from the coldest parts—I went to the mountains in Norway one time when they were just bringing down the sheep from the mountains to keep them in barns for the winter—to the hot humid plains of India where you have to have sheep without wool or they don't survive. They provide you with food, with clothing, with meat, with milk, with skins. They're even used for transport in the high Himalayas where the tracks are too narrow for ordinary draught animals to operate. They're fantastic animals.



Sir Gustav Nossal,
AC, KTCR, CBE, FRS, FAA, FTS

Interviewed by Ann Moyal
Canberra, April 1993

National Library of Australia
Tape no. TRC-2932

GN: I was actually born in Bad Ischl, Austria. Now, why Bad Ischl, which is a sort of watering spa? Well, for the simple reason that I chose to come about four weeks early and my mum was on summer holidays. My parents were Viennese. But that's why I was born in the country. I grew up for the first seven years of my life in Vienna, the third of three boys. And it was in some ways an unusual marriage because my mother was from a very well-connected Catholic family, a family mainly of military people and public servants, and she was actually a Baroness in the sort of pre-World War I era; but my dad came from the Jewish community of Vienna, which was quite a substantial community, and it was distinctly unusual when they married in 1923 for a Jewish person to marry a Catholic person. He came from a merchant banking background. And both had been very well-to-do, but both families had lost a lot of money during the First World War and they were only moderately well-to-do at that time. But interestingly enough, my father wasn't Jewish, he was himself baptised as a little boy because his parents really wanted to assimilate into the general community. And I was brought up as a Catholic, and the Jewish side of things really didn't enter my consciousness until the Hitler time.

AM: At that stage did your parents prepare to leave the country?

GN: Yes. Well, they left it pretty late, I might add. January 1939. But when we did reach Australia in March of 1939, and I was not quite eight years old, we were very warmly welcomed by all kinds of people in the Australian society.

AM: Why did they decide on Australia particularly?

GN: Well, there were two reasons for that. First of all my dad said, 'Let's get as far away from this madness as we can.' And he also was a little bit anti-American, and America would have perhaps been the other place. And the second reason was a coincidence. My mother's family, being very well-connected, had a great friendship with the Papal Nuncio here, Archbishop Panico, who represented the Holy See in Sydney; and they thought that particular connection would be quite useful—as indeed it turned out to be, settling the boys into the Jesuit schools and St Aloysius College, and in various other ways.

AM: So that in a sense you were brought up through your youth as a Catholic boy?

GN: Yes, indeed.

AM: And, though the Jewish side was obviously important, that didn't enter into any of your philosophical ...

GN: No, not at all. I have to tell you something that you might find curious. But, in a curious sort of a way, my father could have been described as somewhat anti-semitic. He was not at all proud of his Jewish heritage, or at least not obviously so. He chose to highlight elements of the Jewish character—like the alleged too great interest in money, and so forth—and didn't draw our attention, except very occasionally, to the great traditions in music and literature and so forth. And the funny thing is that I was not genuinely proud of being Jewish until I was about twenty-eight years old; at that age I moved to Stanford University as a young Assistant Professor, and if someone's name wasn't Berg then it ended in -berg and he was either called Lederberg or Kornberg. There wasn't a single person there who was prominent who *wasn't* Jewish. And I'm of course over-dramatising this. But really it was a very interesting experience to find yourself in the midst of a situation where everybody was Jewish, and that was the most buzzing, most fantastic place that I could possibly have gone for my post-doctoral experience.

AM: Yes. It's very interesting, and not I think entirely uncommon. But to come back now to your schooling. Would you have learnt English before you came to Australia?

GN: I came to Australia knowing no English. And I can remember the very first day at the little public school we went to before starting at St Aloysius College what I describe as 'a noisy wall of silence'. All of the other seven-year-old boys were running around the schoolyard being seven-year-old boys, and they were saying things but I couldn't hear them because I couldn't understand them. And so I went to Alo's (as we called it), I was in Grade 3, and the Jesuits at that time were very keen on numeric scores. Everything had to be assessed. We had term exams, and we actually had a little exam every Friday, all of those nine years. When the report comes back at the end of the first term: 26th out of 26. [*a laugh*] Well, of course, I didn't understand even the words, so I couldn't do anything. But there was something in this stubborn little boy that said, 'I will show them!' And at the end of Term 2, I was first out of 26. I must have said from the beginning, 'You have to succeed.'

Of course it had a very very profound influence on me, the nine years with the Jesuits. There were some very positive factors. The strong emphasis on discipline and self-discipline, the rather scholarly traditions of the Jesuits going back hundreds of years, that was very positive. Emphasis on subjects like Latin, for example; and towards the last two years of my schooling, a heavy emphasis on debating; I found here something that I was really really good at, and I remember being the key debater for the school team, even in my second-last year.

I'd wanted to be a doctor ever since I can remember. I entered Medical School at Sydney Uni in 1948, and there were two things about that time. First of all, the servicemen were returning and, quite correctly, were being given a lot of help to get into uni, and that swelled the numbers. But secondly there was no quota system, and anyone who got their matriculation, their Leaving Certificate, and had an adequate pass and could afford the fairly modest fees could enter Medicine. So we were six hundred in first year.

Now, this over-crowding and really quite modest resources available to the Medical School had a very important influence on my life, because it meant that those of us who saw ourselves as potential high-flyers began seriously in third year to realise that if we were going to learn anything we'd have to teach ourselves. So we formed study groups, and really read up small fragments of the particular subject and then gave each other lectures. We took an interesting area of biochemistry or physiology, studied that up in depth, and then gave a lecture to a circle of friends. And it was through that reading, as much as anything else, that I had to do to prepare when it was my turn to give one of these little talks, that I developed a fascination for the cutting edge of science. The poor teachers who had to lecture to these huge classes, they just couldn't do it.

This particular group was basically based around those who were near the top of the year and, as I recollect, I do not think there were any ex-servicemen in it. But we didn't exclude the ex-servicemen, and they didn't exclude us. They were a very good leavening influence on the class, because we were young stupid kids and they were mature men and women.

AM: You did your residency at the Royal Prince Alfred Hospital. Then your life very quickly moves towards what becomes the habitat of your whole research career, The Walter and Eliza Hall Institute of Medical Research. And I see that you become a Research Fellow there in 1957 to 1959, and you get your PhD at Melbourne in 1960.

GN: Yes, well it was a very interesting year I spent as a senior medical student in 1952. At that time the universities had quite recently introduced (I think I was in the third year of this scheme) what they called the Bachelor of Science (Medical). And this was simply one year taken out of your normal course when you went into one of the professors' or lecturers' laboratories and actually did research as a mini-apprentice. It's not too different from the Honours year that BSc kids do, except it was a bit elitist, out of a class like ours which was 220 or 250, there'd be six or eight kids who would do it. And I had the very good fortune of being advised by my Dean to go with a chap called Pat de Burgh. He was about thirty-eight, and was soon afterwards appointed Professor of Microbiology. At that time he was a senior lecturer in Bacteriology, and the head of that Department was a very wise man called

Hugh Ward, who was a fine fine academic. So I worked for de Burgh for a year, and he imbued me with a *real* love of science. I can honestly say that that was where I got my bleeding in science.

And that year also exposed me to my first major teacher, Macfarlane Burnet,⁴⁰ because he [de Burgh] took me and one other student down to Melbourne to spend a week there in what was thought of as 'the Big Smoke'. And so I met Burnet. I met Burnet twice in that year, and had quite good talks with him. And I think, even though I kidded myself that maybe I'd want to become a cardiologist and I'd better wait for the clinical years and I'd better go and see what it was like in hospitals, I think in reality I was hooked at that stage. So when I went down to Burnet [to The Walter and Eliza Hall Institute] in 1957, it was after completing the medical course and the two years in hospital. I only did two years in hospital; everybody thought I was mad, because if I'd done four I would have been a properly qualified specialist physician, you see. But I didn't finish that, I just went down to do the research.

And then I had the great good fortune, during that time, of meeting my second major teacher, a chap called Josh Lederberg, who also went on to win a Nobel Prize. He came for a three-month mini-sabbatical [at the Hall Institute]. And we formed this great friendship and this great collaboration. I'm still pretty young at this stage, I'm still only twenty-six, very impressionable. And here this man had been heralded for months prior to his coming as one of the world's great geniuses, which indeed he was, and he befriends little me.

AM: Could you tell us about the research you were doing for The Walter and Eliza Hall Institute and indeed perhaps a little about the character of Burnet and his influence.

GN: Burnet and Lederberg were very very different, and I'd perhaps like to contrast these two different influences on me. But perhaps if I could deal with the science first, and then go back to the personalities.

Well, my whole life has been devoted to the subject of immunity, and in particular to the subject of antibody formation. Now, immunity is a subject which the Ancient Greeks knew all about, they began to classify the diseases. And it was well known that if you caught smallpox you didn't catch it again—if you survived. And we knew therefore that immunity was specific; smallpox would not render you immune to diphtheria, but an attack of diphtheria would render you immune to diphtheria. And of course they only knew about diseases through their clinical manifestations. But it wasn't till 1891 (so you're a couple of thousand years later!) that the antibody molecules that brought this immunity into your bloodstream were discovered by Emil von Behring in Germany. And my whole life—would you believe, it seems a funny thing to say, doesn't it—has been around the question of how the cells of your body and

of your blood actually make those antibodies. It's been rather fundamental science, it's not science that is working on a particular disease, but it's science that is discovering knowledge that is very relevant to diseases.

And just before I tell you a tiny bit about what scientific experiments actually got me started, and what I've been able to do in life, I want to mention the diseases to which this fundamental work is relevant. Very relevant to the creation of vaccines. And vaccines are history's most cost-effective public health tool. I mean, vaccines are really brilliant, because they can eradicate diseases from the world. It's relevant to the field of transplantation, because transplantation has as its biggest barrier the transplant barrier, which is an immune barrier—I reject your kidneys, I don't reject my own, but I will reject yours very fiercely unless the immune system is controlled. It's relevant to cancer, because the white cells unfortunately not infrequently overgrow [the red] and cause leukaemias and lymphomas. And finally it's relevant to what we call auto-immune diseases, where the white cells turn traitorous and, instead of making antibodies against typhoid or cholera or influenza virus, they will start making antibodies against components of the body itself, really almost like a police force that has turned traitorous and is beginning to attack the citizenry of its own country.

So that's a big branch of medicine. And immunology has been a very successful discipline, way beyond the confines of its origins in medical microbiology, its origins as a way of making vaccines. That's where immunology got a start; now it's broadened out.

So my task was to figure out how cells made antibodies. And early on I was able to prove a very peculiar thing that was quite unexpected; and that is, that if I immunise you simultaneously with two or three vaccines you will start making two or three antibodies (or, more precisely, two or three more families of antibodies) against the different components, say, of the diphtheria, the whooping cough and the tetanus (to mention the DPT which we give all our kiddies). But when I got down to the single cell level, and I worked out ways (with Lederberg's help, I might add, very important help from Lederberg) to micro-manipulate these single cells in tiny droplets and study the antibody formation at the single cell level, I got the surprising result that each cell always only formed one kind of antibody. There was a most peculiar specialisation in the system. And then later on, over research that spanned maybe the best part of fifteen years, I worked out that this was due to the fact that each cell had a particular antenna, or receptor, on it that allowed it uniquely to see certain, what we call antigens, certain molecules that stimulate the immune response. So, much of my life was devoted to working out how these cells made antibodies, what the precursor cells looked like, what the receptors were, and what the activator mechanisms of the cells were.

But there was a flip-side to the coin. Because of course in normal health we don't make these auto-antibodies, we don't develop auto-immunities—maybe 2 per cent of the population develop auto-immune disease, so 98 per cent of the population don't. So how does the body distinguish self from not-self? That was again a question posed, intelligently and brilliantly posed, by Burnet, but not answered by Burnet. And along (as is always the case) with about twenty different scientists in many parts of the world, I've made a small contribution to the solution of that puzzle.

But in point of fact you never solve these things completely, so you go on, deeper and deeper and deeper, antibody formation, the biochemistry of the cells, the signalling mechanisms that will activate the cells when disease strikes, the signalling mechanisms of an opposite nature that mandate this phenomenon of immunological tolerance. And I suppose you could say my life has been in a sense, in theoretical immunology, but not as a theoretician in the way Burnet was, I'm essentially an experimentalist. I'm not clever enough, I don't think, to spin the theories and the webs that he did, but I get my hands dirty and figure out interesting experiments, and sometimes (maybe dare I say so) even elegant experiments, that attempt to solve some of these problems.

AM: On the experimental side, are you using technology which has become increasingly sophisticated over the period?

GN: No question. No question. The multi-skilling in research is now amazing. When I started off, I had to devise all my own equipment and apparatus, but I have to tell you it was with very pertinent help from Lederberg—Burnet didn't know anything about bits of equipment and that, he knew more about theories. But as time went on, one becomes more and more and more reliant on technologies invented by others. And indeed, might I go on to say, you become more reliant on collaborators, who have different skills. My collaboration with Gordon Ada was a very very fertile five-year period of my life. And I'm a cellular type of person, I look down microscopes, I micromanipulate cells, I study antibody molecules by various kinds of cellular techniques. But he's a biochemist, so he gets right down there to, as it were, one level deeper than mine. The cell is full of millions and millions and millions of molecules, and he understands those. So that turned into a very fertile five-year collaboration. And since then there've been of course many other collaborations.

AM: One thing that strikes me as a historian is the prominent part the research assistant has played at the Hall. For example, Burnet trained many women, and one gathers that 88 per cent of the research assistants at the Hall

Institute now are women. There is a current wide interest in women in science and the question arises, do these women have career paths, the research assistants who work at this coal face doing experimental work and who often may come up with very important research findings?

GN: Burnet did have a lot of very gifted and highly esteemed technical assistants, and indeed what we call research assistants, namely people who'd had a university training rather than a technical college training. And I'm afraid to say he tended rather to use these women, he did not hold them in as high an esteem as I hold their latter-day equivalents. And I think he probably didn't have time to think about it very much.

AM: Well, it was early days of women in science.

GN: It was early days. But there was one person who is very very much on my mind, because in 1943 Burnet was asked to investigate this horrible disease, scrub typhus, that was affecting the troops. And he was doing a lot of work on it and the key assistant was a woman called Dora Lush. And one fine day Dora Lush was injecting a mouse or a rabbit with a preparation of semi-purified scrub typhus material, and her hand slipped and she injected herself. And just three weeks later she was dead. Well now, this interview is being recorded in 1993, exactly fifty years later, and early in May, we will be having a lovely function at the Hall Institute to commemorate that fifty years. We'll be launching the Dora Lush Fellowships and it is a sort of a celebration of women in science and you know, to more or less say that Dora Lush didn't die for nothing.

But I was going to say a little bit about Burnet and Lederberg. Burnet was quintessentially ... I think what he loved most was theories. He loved to work out with his mind how things worked. And he was a good experimentalist, but the experiment always folded into some theory. And, you know, 90 per cent of the theories were bunkum, maybe even 95 per cent; but the 5 per cent that were left were so good that they gave fodder for others to think about. And the way he developed his theories was very solitary. He was no good in the thrust and parry of a vigorous scientific discussion, he just went to water, he couldn't brook being contradicted, he just wasn't good in that framework. But you give him a result, and you say, 'Look, I've got this result, Sir Mac. I think you'll be very interested in it. It maybe doesn't completely fit in with your theory.' You always had to be very tactful as to how you said that to him. And you'd give him the results. And he would go away, and that night he would tinker with it, write a few lines, draw a funny little diagram, and the next morning he would come back and he would say, 'Look, I think I've figured out what that

experiment of yours means.'—And, again, he wasn't always right but he was always thinking about it

And Lederberg was *completely* different. Burnet had a contemplative, rather slow, brain. Lederberg had a lightning fast brain. You would be explaining your experiment, and before you had finished the sentence he'd say, 'Yes, go on. Go on, I've got that. Don't delay, tell me more about it!' You know, he'd completed the sentence for you, because he knew what you were going to say. And then, in the thrust and parry (to use the words again) of that vigorous discussion, he would have these blindingly rapid thoughts. And so what happened with us was that we struck these rapid sparks off one another; because, as I said before, I'm not nearly as deep a thinker as Burnet, I'm not in the same league, but I am a fast thinker, and I also love this business of getting to grips with a problem, thrashing it to death, hearing what he's saying and maybe springboarding over the idea he had to go on to another idea. And that was just such an interesting contrast for me. I learnt so much from both of those two men, who were so totally different in style.

I've mentioned that Burnet was not interested in technology. I think he was rather frightened of it, to be frank. I don't think he liked things that he couldn't control with his senses as it were. And of course, the instruments have become very sophisticated, the output is electronic, and the abstraction that is derived from those instruments is a long way from the little cell that you can see down the microscope. So he didn't like that.

I went off to Stanford to have my time with Lederberg, at a most exciting time of that university's history. The President of the University there had gathered this luminary group of Nobel Laureates and near-Nobel Laureates, a fantastically exciting atmosphere. And of course also, as well as being ideas and things, it was very technology intensive. So, after my two years there, Burnet drifted by on one of his periodic world trips which all scientists take and he said, 'Nossal, I can see you're doing pretty well here. I think you probably should stay here another five years, and just get the most out of this experience.' (He knew that Lederberg wanted to promote me, keep me and so forth.) But he knew me too well. Because then he dangled a little bait. He said, 'Of course, if you were to think of coming back to Australia, well, we'll make you Deputy Director. And by the way, I'm retiring in three or four years' time.' [*laughing*] And you see, the reason I say that he knew me too well is that that little boy of seven who wanted to be first in his class hadn't changed very much, you know; he was still far too imbued with the glitter of fame and being the head of Australia's most prominent medical research institute and so forth. And Burnet knew perfectly well that I was hooked and booked. What I really regret is that Burnet wasn't ten years younger, and that I could have had those ten more years and become Director at forty-four rather than thirty-four. I do regret

that. But, you know, life deals you a hand of cards and you've got to play with those cards.

So, when I did get the job—at the age of thirty-four, a ridiculously young age—I did two things differently. I'd thought that this technology business was pretty important; and that meant growth, it meant mundane things like money, it meant bringing in people with these skills because none had existed within the Institute before, and it meant sort of doing the normal things I'm afraid to say that directors of institutes do. There's nothing very clever or fancy about it. But Burnet had let the technology run down quite badly, and I think had it gone on another five years we mightn't have had an Institute worth picking up, you know. Rich in ideas, very poor in the technical capacity to exploit those ideas.

Now, the second thing that I did differently related to Burnet's scientific style. He was very egocentric and possessive. I mean, that was fine actually, it worked brilliantly well for him. I would not criticise the way he did his science for one second. And he also brought up a great contingent of students, like [Frank] Fenner,⁴¹ and Gordon Ada, Don Metcalf, Ian Mackay, maybe myself. You know, it worked for him. But the degree to which he imposed his ideas on the staff, I thought, went too far. It was not really encouraging of the kind of challenging thrust to ideas—the idea that you fight back, that 'the idea may not be so, Sir Macfarlane'—that I'd come to expect from these top guns at Stanford. So I didn't want to impose my ideas on all of those other people, I wanted them to flourish and to flower and to develop their own line of thinking and their own ideas, and that naturally meant some diversification of the research thrust.

So, instead of having just cellular immunology, which it was under Burnet, we really have four main thrusts now, having the Institute broken into eight units. The Burnetian tradition lives on, we still do a lot of fundamental immunology. But we've got a very big involvement in cancer; we've got a big involvement in auto-immune diseases, including a clinic devoted to auto-immune diseases in the actual hospital; and finally we've got an interest in vaccines, particularly the development of a malaria vaccine. And those four areas of endeavour intersect with one another and fertilise one another, but really each has a great degree of independence.

Part of my work is keeping all of these talented people speaking to one another and learning from one another. [Also] our Institute is the child of two great institutions; it's the child of the Royal Melbourne Hospital, and it's the child of the University of Melbourne. And that creates those links which makes a multi-skilling approach, a critical mass approach to problems, very easy. When we invent a drug or a treatment, we have wonderful colleagues in the Royal Melbourne Hospital ready and willing to put those into clinical trials; and when we have some finding in immunology that really needs to be

exploited by biochemical skills that perhaps we don't possess or through some sophisticated anatomical approach, all we have to do is to shout across Royal Parade and there's the University and there are wonderful colleagues who will be willing to joint-venture with us on that project.

AM: I was very struck by the fact that Macfarlane Burnet once said that the only adventures of his life had been 'inside his own skull'. Now, your life became, for various motivations and institutional reasons, something of a public life. Can we look at some aspects of this?

GN: Yes. In a funny sort of way I think the Institute is in good shape now; it needed both of these Directors. It needed Burnet to vault it to world fame because of the brilliance of his ideas. And then maybe it needed me to bring it kicking and screaming into the latter third of the twentieth century and on into the twenty-first where, *faut de mieux*, we can't avoid the technology. And I've always had that second side to my persona, going right back to the debating days at Alo's (that's at St Aloysius College) of actually loving interacting with people. I sometimes use a stupid little analogy, I say, 'people are like an acid to me,' they stimulate me. I think the fundamental science strand is the important one, that's what gives me international visibility. But this other area of interacting with people, thinking about science politics, thinking in particular about world health, that's been a second important element of my life.

AM: You've been a driving force behind the World Health Organization, and in the application of findings to developing countries.

GN: In approximately 1970, I was asked to join the Advisory Committee on Medical Research of the World Health Organization. Now, the World Health Organization was not unknown to me, because I'd worked on some of their immunology panels, but really mainly on fairly esoteric things. But in 1970 I was brought into this framework of their key advisory body. And I actually stayed on that till about 1978. Now, there two things happened to me. First of all, I met up once again with some of the luminary figures—Lederberg was one; Jacques Monod (whom I didn't know as well but immensely admired) was another; and on the second year that I was there a person called Christian de Duve had joined, who was also a Nobel Laureate (and forgive me for dropping these names, but it's important to the story). And we got to talking at the tea breaks, and we said, 'What are we doing here? You know, we're spending a full week, we're hearing boring presentation after boring presentation. There really ought to be something done, and we ought to use the opportunity of being here in Geneva to try and get the Organization to do something.'

And what grew out of that discussion was a program of research into what we might call 'the great neglected diseases of mankind', malaria, schistosomiasis, or leprosy, diseases of this sort, river blindness, horrible horrible diseases caused by parasites, and in fact we finally said to ourselves, 'We can't do everything. Let's mount a research program on six of the worst tropical diseases.' We, at that time at the Hall Institute, started a parasitology unit under Graham Mitchell, and Lederberg started one at the Rockefeller University, Christian de Duve had what he called his 'Trop Unit'. He'd in fact started a little bit earlier with his Trop Unit but got a tremendous boost through this WHO association. And I actually took 1976 off and spent a full year working with WHO to put flesh on the bones, and at the end of the year we had a donor meeting of the big aid agencies of the world (it's always of course the aid agencies that have to pay for these things), and before we knew it we had a \$30 million a year program—in this previously quite neglected area.

And now, nearly twenty years on from that 1976 meeting, we have some brilliant new drugs that have come out of it. We've got new methods of vector control, and hopefully (keep your fingers crossed) we may be on the threshold of a malaria vaccine, because the malaria vaccine developed from the Hall Institute with partners is now in clinical trials.

AM: What about the other great plagues as it were, that this organisation has helped control?

GN: Well, of course the brilliant one has got another Australian connection, and that's the smallpox eradication campaign. Frank Fenner was very closely tied up with that. He's been an expert on the pox viruses for the whole of his life. And now there is an expanded program of immunisation; this is technology perhaps rather than science, but it's bringing the seven most important vaccines to the hundred million or more children who are born into the Third World each year, just through good logistics and painstaking involvement of the health authorities. That's been a brilliant triumph.

But on the continuing work, I've now left this tropical diseases research group. I'm the chairman of something that they pretentiously call SAGE (the acronym means Scientific Advisory Group of Experts), and the SAGE is trying to get them [the World Health Organization] into new and improved vaccines. You see, the cholera vaccine that we have isn't nearly good enough—about 50 per cent protection. The typhoid vaccine is 60 to 70 per cent effective. Tuberculosis vaccine, the BCG—not good enough. No vaccine yet for malaria, no vaccine yet for leprosy.

AM: In connection with this international link, people are streaming to the Hall Institute from overseas for periods of collaborative work.

GN: Yes. We have at any one time somewhere between twenty and thirty overseas people amongst our three hundred or so. But really the most important pool for us, of internationals, are the 'post-docs'. It's really what you do immediately after you get your PhD, it's the apprenticeship's spit-and-polish phase of a young academic's training. And so you're getting these Americans and Germans and Brits and Canadians and Frenchmen (and women), at the peak of their creativity.

AM: Questions of funding from government have become much more difficult and intrusive in science. Has this affected the Hall's development? And have the collaborative undertakings with foreign commercial firms assumed more importance latterly?

GN: We've always had the approach that we don't want to become utterly dependent on government funding. We believe we win a certain independence from government intrusiveness through not forcing the government to pay too much. And in fact we also get a certain amount of help from the Victorian State government. So, with that and with government fellowships, you could say Federal and State [government provides] about 50 per cent, which means all of the rest of the 50 per cent has to be our own entrepreneurship and that involves many international foundations. We go in there and we compete!—we've had help from the Rockefeller, the MacArthur Foundation, from the smaller Australian bodies like the Anti-Cancer Council of Victoria which is wonderfully good to us. We do have some industrial contacts, and the most important is with a wonderful Australian start-up firm called AMRAD, who are our main commercialising partners.

AM: Some of your books are very much geared towards bringing information about medical science developments to the public.

GN: It's the sort of thing one can do late at night, on weekends. I've got it all in my head. I like communicating with the public. And that little book, *Antibodies and Immunity*, actually went on to win the famous Phi Beta Kappa Science Prize for the best popular science book of that particular year in the United States and has been translated into any number of languages. And I did the *Medical Science and Human Goals*, which is more a philosophical style book. *Reshaping Life* is the latest of these adventures. There's a total of five books altogether. It's not an outrageous amount of extra work to collect your thoughts and put them in writing. And that book, which I do not consider a highly intellectual work, has been an unbelievable success. That's gone into about fourteen languages.

AM: This book has now opened up a whole set of questions relating to genetic engineering. We now have the establishment of the Human Gene Project where, with an international input, researchers plan to catalogue the whole genome. It adds wonderfully to knowledge, and is very promising for people who suffer various diseases who may find some cure through genetic intervention. But it does open such enormous questions—who will have access to DNA sequences? And how will we ever avoid the misuse of the knowledge?

GN: Absolutely. You're so right. I mean, these are deep and pressing, and indeed quite complex issues. And I'd like to take them in three bits. First of all, there's the basic knowledge side. And I think that there's very little doubt that research on the nature of genes and the way that genes control protein synthesis has become the top intellectual underpinning for virtually all the medical sciences. That's often not stated, that's often swept under the carpet. But in terms of what we started with, right at the very beginning of the interview—how cells make antibodies, how your brain works, how your endocrine system works, even how a disease like atheroma begins inside your arteries, which is arteriosclerosis, which is what causes heart attacks and strokes. Even that is being attacked with this genetic engineering technology, to try and understand the proteins that have somehow gone wrong in that situation. So I think at the basic science end one would really have to say that recombinant DNA technology (which is the technical word for genetic engineering) has been an unalloyed good, a wonderful boon to those seeking to learn more about animals/plants/biology generally.

The second level that we have to talk about is actually making pharmaceutical products out of the knowledge that you have gained. In other words, using genetic engineering to mass produce say a hormone, say an antiviral substance, an anti-clotting substance that you'll use to treat heart attacks and so forth. And it's that area that brings in all of the commercial constraints, where there are quite legitimate fears about scientists holding back information from the public information pool and thereby delaying the progress of the march of science. And those fears are honest, they're legitimate, they are sometimes true in that they are realised. But in the main, fortunately, it hasn't happened. To the extent that the products we invent will turn into drugs and do have to be commercialised by pharmaceutical firms who work for a profit, to that degree we have a problem, we have a new factor in the equation.

However, there's yet a third area, and it's the most difficult. And that's the area of gene therapy, actually using the DNA to go and try and fix up some genetic disorder in a human being. And I've got a very clear-cut view about that. I believe gene therapy to fix up one individual in their sick cells (your

muscles have gone wrong because you've got muscular dystrophy, or your blood clotting mechanisms have gone wrong because you've got haemophilia, or your red cells have gone wrong because you've got thalassaemia), provided that the treatment that I am delivering by way of gene therapy is to you and not your offspring, I feel on very strong moral and ethical grounds that if you, as an informed patient, are making the decision to be part of that clinical trial and to have the treatment, I think it's good. Where I have a difficulty, and most other scientists do too, is on the basis of germline gene therapy, e.g., seeking to expunge the haemophilia gene or the thalassaemia gene from a recently fertilised embryo, and seeking to ensure that that embryo's eggs or sperms when it becomes an adult are themselves purged. And the reason that I'm currently against that kind of research in humans (I'm not against it in mice, and there's a lot of that work going on in mice) is essentially a Murphy's Law argument: we don't yet know enough about the human genome to ensure that while we're sticking in the good haemoglobin gene for the bad haemoglobin gene and thereby curing thalassaemia that we mightn't be perturbing the genome in some entirely unpredicted way, and therefore possibly giving the child cancer or spina bifida or something or other.

AM: Well, how does that come out at a national level? What controls exist to keep this fairly open?

GN: Well, we do have a national body that is charged with the supervision of *all* recombinant DNA technology. And that's a very good representative body that has the capacity to stop you doing experiments if anybody thinks that there's a danger. However, there are also softer-edged watchdogs. Every institution now has to have an ethics committee. And I have to tell you that our ethics committee at the Hall Institute properly and rightly is very tough, it's possibly the toughest in the nation. And we really have to justify any experiment that involves human subjects very closely, very carefully. So that's a second level of control. And then, thirdly, the funding bodies exercise a certain degree of control. All our funding comes from peer group review, and if you're now going to embark on a line of experimentation that goes close to the edge of what's ethically feasible then you won't get funding.

AM: This is great for one's own country, if the controls seem exact. But is there any inter-relationship internationally in these controls that would obviate the problem which so many people raise, of the history of genetic excellence and looking for master races and ethnic cleansing and so on?

GN: There are certainly international bodies such as the European Molecular Biology Organization, or the Americans taking the lead from the National

Institutes of Health to try to influence the World Health Organization towards adopting a view of these things in general. But you are right that, fundamentally, these international consortia don't have much in the way of teeth, the teeth are mainly at the national level.

AM: This raises a very central question which has become public since the development of the hydrogen bomb through Robert Oppenheimer's famous remark that the bomb was 'technologically sweet, and therefore scientists had to do it'. So the question of technological and scientific sweetness, the fact that you're impelled on because the knowledge is there and you want it, is of course a very strong stimulant in this area, isn't it?

GN: It is. But you see you've also got, as a society, a certain protection by the nature of genetics. What we are good at is manipulating single genes. So I've mentioned things like thalassaemia, haemophilia, for single gene traits. Were we to understand the genetic basis of heart disease or diabetes, we might be tempted to intervene; but at the moment they are clearly both of them very complex. And diabetes, which we do a lot of work on, we know that there are at least four genes involved in the actual causation of diabetes. And we furthermore know that those genes only account for about 30 to 50 per cent of the probability of a person becoming diabetic. We know that from identical twin studies, because identical twins have identical genes and yet if the first twin has diabetes the risk of the second twin getting diabetes is only about 35 per cent. So there's 65 per cent which is environmental influence, but whether that's toxins, viruses or so forth we don't know. But for the multigenic diseases, doctors and scientists are not yet in a position to even think about going in there and tampering with the genes because the situation is so complex.

Now, let's move on from there to the things which quite rightly give humanity the total heeby-jeebies, 'the master race'—positive eugenics, breeding a whole heap of intelligent people, breeding a whole heap of talented musicians, breeding a whole heap of beautiful women, breeding a race to be submissive. Right? The fact of the matter is that of course all of those characteristics have a strong genetic component; but the genetics of it are so horrendously complex that it will take us not ten years or a hundred years, but it'll take us a thousand years to understand all of that. So, if a latter-day Hitler wanted to breed a master race, even (let's say) of Einsteins, which you might say would be a good thing to do, we wouldn't have the first clue of where to begin. Now, that doesn't mean that we won't know more about it one day, the genetic basis of intelligence; but there's a very big technical lot of things we don't know.

AM: Distance can lend contentment, but the answer leads me to believe that you do trust the scientists. Is that so? Can one trust the scientists?

GN: I'm a person that prefers soft-edged rather than hard-edged control weapons. Do I trust the scientists? I certainly trust many scientists. But I also believe that the scientists need to be controlled. Of course. We're here to serve society. We're not society's masters. And we have all the norms of the democratic process to exercise those controls, at many levels. If you started to pass a law that says, 'Okay, we will now forbid a certain type of genetic research. Let's say we will forbid research on curing thalassaemia.' Now, two things will happen. First of all, the relevant scientists will move to a country where there is no such law, and barrel straight on. And secondly, they'll find a different way of skinning the cat. So there'll be horrendous borderline demarcation disputes as to what any legislative web brings into orbit. And that's why ... I use this little phrase that sort of resonates round my mind, 'the soft-edged weapons of a free and decent society', rather than laws and statutes and regulations.

AM: You've become increasingly concerned across your career with the relationship, not only of medical science and health care and the funding of medical science, but of scientific research to the general future of Australia. This has brought you to questions of the interface between science and society, as a member of ASTEC (the Australian Science and Technology Council) at a time when it was the key science policy advisory arm of government and under the Chairmanship of Sir Geoffrey Badger and more recently as an individual member of the Prime Minister's Science and Engineering Advisory Council. What are your impressions of this machinery?

GN: The Prime Minister's Science Council as it was, the Prime Minister's Science and Engineering Council as it now is, was instituted by Bob Hawke. And the nature of that change, of course, was that it robbed ASTEC of the primacy. My personal view is that ASTEC should retain its independence but should effectively function as a sort of a standing committee of the Prime Minister's Science Council, and that the two heads (the head of ASTEC and the head of the Prime Minister's Science Council, who is called the Chief Scientist) should work close together in harmony. Because, you see, there's one thing the Prime Minister's Science Council cannot and does not do, as it only meets twice a year, it cannot go into issues in big and continuing depth, it can't do what Badger did—bring issues back eight times.

AM: In your role as an individual on the Science Council, what sort of an input can you make?

GN: Well, I make a strong input if it's in an area within my technical expertise. I've had a strong role to play in the formulation of the pharmaceutical industry policy. The input into the general deliberations so far has not been particularly strong, other than to speak up in a generic sense very strongly for basic science. You see, one of the interesting things about that meeting is that there are eight or nine of the most senior Ministers there for virtually a whole day. If you can project through your knowledge, your integrity, and above all your sincerity and forcefulness, the fact that much of this industrial progress that they're interested in—the economic side of science—can only work if you maintain the crucible of the pure science—which is a difficult concept for a non-scientist to grasp. I'd say that, other than in my own area, that's really been my main contribution. Admittedly you can only support, in each of these areas, world-class, world-competitive people, and you must be quite tough on the time-servers in universities, research institutes, CSIRO; but you must maintain that excellence of basic science. And I think gradually, gradually, gradually that message does get through a little bit.

AM: Now, in conclusion, along the way you've acquired many honours and many fellowships from all the major institutions around the world. And one wonders if the seven-year-old boy arriving in Australia in 1939 ever had any intimation of the life of contribution and distinction that lay ahead.

GN: You make it all sound much more important than it really is, you know. In some ways I have been incredibly lucky. I was fortunate to come under Pat de Burgh, who imbued me with a zeal for science. I was unbelievably fortunate to come under two of this century's greatest biologists, Burnet and Lederberg, as their beloved student, each of them so different. And I was fortunate to be given a responsibility early, by the very nature of the job drawing a lot of lustre. And I think my answer to that is: yes, scientists do need a bit of stroking. And why do scientists give each other so many prizes? You know, it's because they never make any dough, and the society ... when you go to a cocktail party nobody really has the foggiest clue of what you do. So what is there [*laughing*] left to strive for?—you strive enormously for that peer group approval. And if you ask me what's given me the greatest joy, I can give you a very unequivocal answer to that. The greatest joy is the sort of tri-Academy—you know, that peers in Australia, the United Kingdom and the United States have elected me to the Academies of those three countries—because that's very special.



Dr Elizabeth Truswell,

FAA

Interviewed by Ann Moyal
Canberra, March 1993

National Library of Australia
Tape no. TRC-2917

ET: I was born in Kalgoorlie in 1941. My father was a surveyor on the gold mines. My mother was a schoolteacher. I spent my early years there in Kalgoorlie—well, actually we lived in Boulder, next door to Kalgoorlie—and stayed there until I was seven. My father, while he enjoyed what he did, hated the underground part of it. He was always very sceptical about the value of goldmining, he used to say that it was digging it out of one hole to put it in another. So he was very keen to move to something else although the families had been long-time goldfield families. So when he was offered a position with the Public Works Department in Western Australia, which was involved with doing surveys for country water supplies, he jumped at that and we moved to Perth.

I was the only child. I spent a lot of time out in the bush. When my father was going on field trips associated with his country water supply work, I would always go on school holidays on those trips. We were always very conscious of the bush.

AM: And your schooling was in Perth, principally?

ET: Yes, it was. I went to King Street High School.

AM: That's one of the major high schools of Perth. Considering that you eventually took up a distinguished career in science, what sort of influences at school induced you to go into fields like geology and palynology?

ET: I had a very enthusiastic biology teacher who was a much loved figure at the school. I initially felt I wanted to go into biology and went to university with that intention. I did biology units in my first year and I did geology as a fill-in kind of unit. But I think it was something to do with dissecting the same dogfish week after week that ... [*laughing*] well, perhaps I'd always been more interested in the botanical side of things.

And then I had a lecturer in geology who was one of the pioneers in palynology in Australia, and who pointed out to me that there was a career here that could combine both geology and botany and, furthermore, there was a likelihood of well paid employment in it. Introduced in the 1940s, the derivation of palynology is from 'palunos', from the Greek, meaning 'dust'. It's really had quite a long history, but it wasn't called that. Aspects of it that were to do with looking at the details of vegetation history in Europe began in the early part of the century, looking at the pollen records of the way the vegetation had changed after glaciations. That sort of work. Pollen counts from peat bogs started in the early part of the century. But it wasn't until the 1940s that it was recognised that it could be used as a tool in petroleum exploration. And that was when it broadened its whole scope, and the major

oil companies began to employ people, pollen analysts, who then adopted the word 'palynology' and called themselves 'palynologists'.

AM: So in that sense you really were lucky that you fell into the tuition of someone who was a pioneer.

ET: Yes, indeed. This was Basil Balme. And he'd worked in the late 1940s, early 1950s, first in CSIRO in Sydney in their Coal Division (and they had adopted palynology as a way of looking at coal seams and trying to distinguish one coal seam from another), then he also worked with a British coal organisation, before taking up a lectureship in the University of Western Australia. I worked with material from north Western Australia for my Honours.

AM: 'Carboniferous microfloras from the Canning Basin of Western Australia'.

ET: Yes. It was a very descriptive sort of Honours thesis. It was a matter of sitting down at the microscope and describing these spores. They are really of earlier plants. I got a Commonwealth scholarship and went to Cambridge. I was very lucky in the project that I was pointed towards when I went to Cambridge, in that it was looking at the early Cretaceous period in southern England and looking at the palynology of it, and that's the time span about a hundred million years ago. And a lot of that was routine. It was involved with setting up time frameworks against which you could look at the deposition of the rock sequences. But, within that span of geological time, the flowering plants first appear, and there are the first records of flowering plant pollen in those rocks in southern England. So I was lucky enough to work on those. And it was a wonderful experience, walking around the coastline in Dorset, and much of my time was spent on the Isle of Wight, collecting sections there, and working on the pollen. They were marvellous areas to collect in, apart from their historical interest. And they are very picturesque and beautiful parts of England. I would work with cliff sections that had been described by Gideon Algernon Mantell in the 1840s, and I found that I could still follow Mantell's descriptions, foot by foot up the cliff sections, unless there had been landslides or something like that. He described them when he was collecting faunal specimens. And his descriptions were still the best available.

AM: Did you have other mentors in your studies in England?

ET: Yes, I did. My PhD supervisor was Norman Hughes who again had pioneered a lot of work in palynology, and he was very very supportive.

AM: And you of course were obviously even then in the early sixties a rather unusual woman, in the sense that you were entering a new field, or a sub-discipline of a major field. And so in 1968 you begin your career as a working palynologist in one of these oil companies, Western Australian Petroleum in Perth at a time of expanding petroleum exploration. Could you tell us a little about the sort of work you were doing?

ET: WAPET was a pioneering company, and still is, in Western Australia. They were involved in the first discoveries of oil in Western Australia at Rough Range in 1953. WAPET was a consortium of American and Australian companies, and they employed a number of people as palynologists. It was fairly routine sort of work. I think I found it pretty flat after a PhD, just to be in a situation where we were dating rocks under direction. And that was I guess why I looked for something else, I looked for something where there was more of a research component, where you had more freedom.

AM: Were you doing extensive field surveys, or only narrow ones?

ET: No, well, I've never been involved in extensive field work, except in a collecting kind of sense. Most of my work is laboratory work, particularly in the Australian scene. Because of the very intense weathering and because of the climates here, pollen isn't preserved in surface rocks, it tends to get weathered out. So it's only from deep borehole material that you can retrieve good pollen assemblages. That really restricts the palaeontologists, the palynologists, to a laboratory role. So when the chance of a post-doc in America came up, and particularly the chance of becoming involved in Antarctic research, I certainly jumped at that.

AM: This was in 1973 at the Florida State University, as a research associate?

ET: Well, I had an offer to go there because there was a research team there looking at the very earliest, or amongst the earliest, glaciations that had affected the Southern Hemisphere. There was a major glaciation in Gondwanaland at around about 280 million years ago. And there was a team of researchers that had been looking at the way this glaciation had affected the various southern continents. And a palynologist was very useful to them, somebody who could put ages on the rocks associated with the glacial sequences. So I was offered the post-doc at Florida State. And while I was there the opportunity to go south and look at some modern glaciers and glaciations came along.

It was part of a much larger organisation, part of what's now called the Ocean Drilling Program. It was then called the Deep Sea Drilling Program,

and at that stage it was a program that was run entirely by the American government. And it was a program that used a ship in a number of roles, primarily drilling holes in the sea bottom at various sites all around the world looking at major geological problems. The program has since gone international and is supported by governments around the world, and the Australian government makes a contribution to that program.

AM: I seem to remember it was set up at the time to attract funding as a sort of rival program to the Moon and space research; that, in other words, the ocean's bottom (it was put forward) is just as important as the Moon. And then they started this great drilling from very elaborate technological platforms.

ET: Yes, I think that was a component in setting it up. I went on—each of the cruises is given a 'leg' number—I went on Leg 28, to the Antarctic. I can vividly remember leaving Fremantle, going out in the evening, and going up the harbour and turning left and just keeping on going. And we went right down to the Wilkes Land coast. We drilled a number of holes, close to Antarctica, or really we started at the mid-ocean ridge that lies about halfway between Australia and Antarctica. And it's that ridge where sea floor is being generated that is part of the conveyor belt that's pushing Australia northward. I was the only palynologist on board. But there were a number of people looking at other kinds of microfossils, foraminifera and other algal microfossils. We were at sea without touching port for seventy days. Weather-wise we were very fortunate. We had a marvellous trip. We didn't encounter any major storms in the Roaring Forties, or the Furious Fifties. In fact, when we were in the Ross Sea the weather was warm enough for us to have barbecues out on deck. It was quite wonderful.

AM: And how many women were there in the team?

ET: I think there were four women on board. There were two technicians, two geological technicians; and then there was what the Americans called a yeoman, who was a secretary who undertook a great variety of tasks. It was very interesting to see the way teams come together on these, because at that stage there were twelve scientists and they were selected from different institutions and they all had very different roles. And we had, I guess, ten days steaming before we actually stopped to do any drilling, so it was in that time that people established their roles and worked out how they were going to get along with each other.

I didn't really work as a palynologist for much of that. I worked on the sediments. People had to do a variety of tasks, so I worked as a sedimentologist more, on that. My motive for going along was to try and get

a look at the Antarctic vegetation before the ice cap grew; that was successful but only in a minor sort of way, there were all sorts of problems in getting that kind of information.

But just working as a sedimentologist was exciting too. We were very interested in the age of the ice cap. At that stage it was imagined that the Antarctic ice sheet was roughly the same age as the ice sheet that covered the Northern Hemisphere in the last glaciation. But what we did establish on that trip was that the ice sheet had been there as long as twenty million years, whereas the Northern Hemisphere glaciation had started about two million years ago. So we did establish that, which was very important.

The other thing that we did, and I was much involved in, was looking at the development of the Antarctic convergence. You know there is now a point in the Southern Ocean where there is a major temperature change. As you steam south the temperature suddenly drops around this boundary. And what is really happening there is that the cold Antarctic waters move north for a while and then they sink beneath warmer waters coming from the north and that's a very rich zone faunally. In the geological record we could see the beginnings of that happening. And I can remember that quite vividly because I had rough logs of the holes that we'd drilled taped up around my cabin and I was colouring in the bits that were siliceous, silica-rich sediments, which marked the diatom-rich Antarctic waters as against the carbonate-rich waters from the north. And then all of a sudden I could see where these things were intersecting and at what points in time this was beginning, that these Antarctic waters were beginning to spread north.

AM: And when were they beginning, when was that?

ET: Well, it certainly started around fifteen or sixteen million years ago. And it was fairly static, and then it took a very sudden leap northwards, probably about five or six million years ago.

AM: You have come out of your Antarctic work with a strong sense of history. In your paper on the Antarctic,⁴² I see the historian in you, in terms of the geological record and of who has contributed to knowledge in the region. It's very interesting to learn that the HMS *Erebus* and *Terror* expedition of 1842, which carried the great botanist Joseph Hooker, was the first to point out that the vegetation of Tasmania and the Falklands and of the territories around the Antarctic and New Zealand suggested the idea of a one-time great southern continent. Then there are big leaps before the next set of discoveries about this around 1902.

ET: Yes, that's right. Well, there was a time of speculation. And to read Hooker's correspondence with Darwin, over that particular issue, is quite

fascinating. They both agreed that there was a 'southern vegetation', there were minor differences about how it moved around and how it was dispersed.

AM: And then on Scott's first expedition to Antarctica they hardly bothered about such points at all. But on the expedition on which Scott and his party died, evidence of collected plant fossils was found in the hut.

ET: Oh yes, that's a remarkable story. It was Edward Wilson who collected those fossils; and to read Wilson's diary, from the time they spent at the Beardmore Glacier collecting these, he describes them in great length, and he describes those particular leaves as very similar to those of British beech. And the mental image that one has of this expedition getting really into a desperate situation, and being confronted here with these evidences of warm climates. But it wasn't British beech—it was *Glossopteris*. That's very old [Carboniferous]. It was the vegetation that was associated with the earlier glaciation, with the 280-million-year-old glaciation. But they carried those with them, I imagine at Wilson's insistence.

AM: And there they were, with the bodies.

ET: Yes. They jettisoned equipment, but they clung on to these fossils. And it's particularly remarkable because there has been a lot of criticism about that particular expedition, the arguments being that there was nothing of scientific value that came out of it. Whereas there was a great deal of scientific value that came out of it.

AM: So that your work has really added quite significantly to the vegetation history and the geology of the Antarctic, hasn't it?

ET: Yes, it has added something. I've kept a great interest in things Antarctic. I've looked at Antarctic pollen and still do whenever I can.

AM: You then, in 1973, joined the Bureau of Mineral Resources (BMR) in Canberra in the Palaeontology Section. What would be a day in your life? What would it be like?

ET: Well, a lot of any kind of science I suppose is routine. As far as palynology was concerned, the sort of thing that had to be done was preparation of the samples. That was done by technicians in the laboratory and there was supervision of that kind of thing, which really means breaking the rocks down and dissolving them away chemically and retrieving the pollen and spores out of them. The excitement comes in the first look at a

microscope slide after a new preparation; wondering whether there is going to be anything in it at all, having a quick look at it, sometimes it's very exciting, sometimes it's poorly preserved and you know you are going to be battling to get anything out of it that's going to tell you anything about the age of it.

A lot of the time is spent looking down a microscope, counting pollen grains (which can be desperately tedious). It's moments of excitement finding something new or something that's particularly well preserved, something particularly beautiful. Then there's the writing phase. Again, some of that is tedious. Some of the descriptive work that palaeontology still involves can be tedious enough. Bringing it together is more exciting.

AM: And when you've published, have you published quite substantially as a lone author, or quite often with collaborators?

ET: I've done both. I've tried, I think, to be as much of a synthesiser as I can. I've always felt that there was a lot of information, that people were holding it in their drawers or their filing cabinets, and there was a great value in bringing it together. So I've generated quite a lot of syntheses with a number of different people. I like working as a team member, I find that's very rewarding. Frustrating at times of course, but it is very rewarding.

AM: Your work on the palaeontological history of rainforests is very interesting on this whole sense of the origin of the ancestral vegetation of the forest of the present day. In your paper called 'Australian rainforests: a hundred-million-year record' you say that 'one of the most exciting aspects of palaeobotany is the increasing number of fossil flowers that have been recovered from sediments with their delicate floral past preserved intact.' So you can build a picture of this development of forest vegetation in Australia. Can you tell us a bit about this work?

ET: Yes, I can. It's not work that I've been involved in myself, but it's happening all over the world. I guess that perhaps people didn't *expect* that flowers were going to be preserved because they are so delicate, but certainly some of the very earliest of flowering plants have been recovered from all over the world now, and there's a much greater understanding of the nature of those early flowering plants. In Australia we don't have the very earliest of the flowering plants preserved. The sort of thing that has been discovered are the remains of some of the species which now grow in our northern rainforests, found in coal seams in southern Australia and in Victoria as fossil flowers. And that has strengthened the view that our rainforests, or the larger part of our rainforests, really are Gondwanic in their origin. I think there was an

older view, perhaps a view that went back to Hooker, that they were late invaders of Australia, that they'd come in from a northern path. There may have been an element of truth in that but certainly the bulk of them were here, and the fossil flowers from the Victorian coal seams have played some part in establishing that.

AM: So that in that huge perspective of time, and a hundred million years of a rainforest, you must get a strange sense of awareness of the insignificance of the human condition. Is that a fact?

ET: Yes, you certainly do get that. I think that, as a geologist, I guess occasionally you're pulled up short. We deal in a day-to-day currency of millions of years, and it's just on occasions, I think, that you come up short and you think, hey, what am I really dealing with here? And I guess, for me, it's always an awareness of looking at things that were before there was any human consciousness, that all these processes were going on, and you have access to them just at the end of your microscope virtually. It's a marvellous window into a time that had no human observers.

AM: How does it affect you when you come to these contemporary questions of the preservation of rainforests?

ET: Well, I think the message that comes, or the impression that one gets, is one of change. I mean, things are changing naturally, rainforests are changing naturally. So there's that impression. But there's also a sense of, I suppose, not exactly a counter-impression, but a feeling that they're very much worth preserving because they do represent long lineages which are rooted very far back in time, so that makes them the more precious.

AM: At the Bureau of Mineral Resources, you rose quickly, becoming a Principal Research Scientist in 1981, and a Chief Research Scientist in 1992. Given the fact that the BMR is a mission-oriented scientific organisation, how does this rather basic research fit into its purview?

ET: Well, it's never been a problem in the sense that I've always felt that we had a mission and, in my case, that mission translated into being a biostratigrapher. I had the job of using fossils to erect a time frame against which you could look at the development of sedimentary basins. So there was a very clear role there. But in doing so there were always these marvellous spin-offs, which were really the biological problems, or the areas of interest. And most of the things that I've become involved in, like the history of rainforest or the Antarctic vegetation story, have really been spin-offs from my main day-to-day working role.

Most of the scientific staff are in that sort of situation, where there's a task to be performed—whether it's mapping the continent, or looking at [the] development of mineral resources—but always there are geological problems that may not be so mainstream, not part of our mainstream mission if you like, and there has been the opportunity to work on these.

AM: In your current role, you're called Chief Research Scientist, Environmental Geoscience and Groundwater, and you are involved with major land use studies, is this a more strategic-oriented thrust?

ET: Yes, it is a much more strategic-oriented thrust. I'm actually working with a whole lot of areas that I don't have a great deal of expertise with, trying to bring people together on them. But I don't have a problem with it. Particularly with the environmental area, I think that we have (in terms of our population) such a small scientific community and an enormous continent with enormous problems, that we need all the expertise that we can direct at it to solve a lot of those problems.

AM: One important area is the Cape York Land Use Strategy.

ET: Well, that developed a number of years ago because there were concerns from environmentalists about the future of Cape York. It was seen as one of the last great wilderness areas and there was a thrust by some groups to have it set aside and isolated completely as an environmental reserve. At the same time, it supports Aboriginal communities, it supports pastoral communities, stations. So the Queensland and Commonwealth governments combined to undertake a land use study of the area, to look first of all at the resources within it, the natural resources in it, the human resources; and once that information was accumulated and accessible, then to use that as a basis of developing some kind of strategy into the future for the best usage of Cape York.

The geology is only one aspect of it. It's a Commonwealth–State project and it involves a number of departments at both state and federal level. And I think there are something like eighteen different projects under way. Five of those are geological. We are looking at the bedrock geology, we're looking at the geology of the coastal zone, the surface geology that helps understand the distribution of soils, and the groundwater. They're all separate projects. But there are other projects being undertaken on the distribution of the faunas, the floras, the insects. Then there will be a whole further range of projects undertaken to look at the human side of it, to look at the social organisation within it, to look at the tourism potential and problems, and at the primary industries.

This is the first major land use study of its kind that has been undertaken, and it certainly points out the kind of problems that are

involved in setting such a program in place. One of the issues relates to the interaction between scientists and the rest of the community. There's a parallel program to the scientific one which is called the Public Participation Program which is involving all the communities on the Cape (the Aboriginal communities, the European communities, the pastoralists), and making them aware of the program, trying to ensure that they have a lot of input into it, trying to make them own it. But what is really interesting is the feeling on the part of these communities that they are somewhat alienated from the science. And they rather resent this; they would like to feel that the scientific information was theirs, that it was theirs to use as they chose. There is a feeling of uncertainty as to what that scientific information is going to lead to.

AM: There is also a kindred development—the question of ownership of data in our electronic age. What do you think about the data situation and the constraints on data, on remote sensing from satellites and that sort of material? Is it becoming very difficult to share this with the public?

ET: I think in some ways it is. I think it should be making it a whole lot easier, information becoming more accessible. But I think the information revolution perhaps came at the same time when governments were being put into the situation where they were asked to recover their costs, so we had a cost-recovery kind of attitude imposed at around about this time, an awareness that all of this information had some sort of monetary value attached to it. So there was that aspect of it, which made people very jealous of guarding that information. And that, as you say, has developed at the same time as there is a public perception, perhaps an enhanced perception, of the value or of the way that data could empower people to act in particular directions, and a concern on the part of the public whether they be urban or rural or Aboriginal communities.

AM: This question of intellectual property and ownership of information is occupying information scholars, lawyers, etc., a great deal now. And governments themselves are interested in establishing who is the custodian of knowledge. Do you find in the Cape York Peninsula study, that this is becoming a problem?

ET: It's certainly something that the organisation of that project has had to address. And they were very keen that the information flow should be as open as possible. And I think that in setting up the framework within which this could happen, I think people were a bit surprised at the demand for information that came from the communities involved.

AM: Do you find in your role as the administrator for the Australian Geological Survey Organisation part of the project, that a woman has something to bring to this role in her communication skills.

ET: It's interesting that the overall Director of the Cape York Project is a woman, Kath Shurcliff, who was a biologist with the Great Barrier Reef Marine Park Authority, and she's the overall Director of the program now, based in Cairns. She has the job of working particularly closely with people who are involved in developing the public participation, the public input to that program.

AM: One of your strengths has certainly been as a synthesiser and someone who sees the value of communication across a spectrum of knowledge. This leads me to questions about the broad sociology of science, and more specifically about women in science. As a woman who has a distinguished career in science, do you see that there are still considerable obstacles facing girls who might contemplate a scientific career, whether you experienced discrimination (either knowing or unknowing), whether you think this is changing?

ET: The issues are obviously complex. And I don't think that I've experienced an enormous amount of discrimination, apart from student days, and a part of it I guess I wasn't exposed to because I've never in my career wanted to spend large amounts of time in the field. I know that some of my contemporaries did have problems, in the BMR for instance. It was, at one stage, considered very difficult to take women to the field, except as wives and field hands. The sort of things that were thrown up there were—'But she may have male field hands, and they mightn't like working for a woman!' [*laughter*—I think we're certainly well past that now, and it's accepted that field work is a part of scientific life.

Operating within the networks, I don't find difficult, in most cases; but there are instances where I think it is, and I think it's the social framework that makes for those difficulties. So many of the problems arise because of the expectations that it is the women who are going to maintain home and family, and male networks are set up around that. Work still continues to be structured as if there is a 'little woman' at home.

AM: One wonders if the whole concept of science and the scientific norms of objectivity, disinterestedness, universality are, as the feminist scholars suggest, male concepts and patriarchal. How do you feel about the norms of science? How do women scientists view the norms of science?

ET: Certainly what was called 'feminist science', which was strong a few years ago, did challenge a lot of those norms. I mean, it pointed out that

objectivity was rarely something that could be achieved, that so much science depended on the human factor, that it was a human construct, and that there were human constraints, human activities, conditions, and motivations that determined the path of science. And the sense of not being emotionally involved, being disinterested, I think that was challenged quite heavily too. And a sense that one had to be in some way or other emotionally involved in it to achieve anything had to be accepted. I don't know whether one could identify if the scientific establishment has accepted these challenges, and softened at the edges in that sense.

AM: It's suggested that deep cultural forces still continue to alienate women from science and careers in science. Is it your perception that young women are not really flocking to science? Or do you get a steady flow of applicants into fields of your interest?

ET: No, and I find this quite alarming. We have been aware of the low numbers of women in the research scientist classification in BMR for many years. But in most areas the applicants aren't there. Since I've been in environmental geology there have been more women applicants in that area, in the soils area, in landscapes history.

AM: Yes, it's generally suggested that women have specialisations and have skills which are going to show in particular areas of science, and environmental and ecological science certainly seems to be one of them.

ET: Yes. I think they're appealed to by the practicalities, the obvious usefulness of it. I think women want to feel that they're doing something that is useful—the human orientation.

AM: One of the norms which Robert Merton, the American sociologist, added recently to the norms of science is humility. Is this more likely to have resonance with women scientists than with men? And I ask you because I notice that your own CV is really very modestly set out, that it does not sing your own praises in the way that contemporary CVs are beginning to do in an exaggerated way. And I think this is a new and interesting part of the sociology of science, what's happening to curricula vitae. I wonder if you've noticed this trend?

ET: Oh, very much so. I see a lot of CVs, and I recoil in horror at many of them. I feel that scientists have to be humble and they can't confront the world and not be, and I think geologists particularly need to be or *have* to be. I'm very concerned at the style of CV that is actively encouraged now. It's

certainly encouraged within Public Service circles and, as far as I'm aware, within academic circles, the idea of selling oneself in a competitive market. Some women are doing it. I don't think it sits so comfortably with women as it does with men. This humility is interesting. I've sat on selection committees quite often, and I've actually suggested that humility should be a selection criterion and this has been regarded with shock and horror by male members of the committee.

AM: On the major question of defining priorities in science, within your organisation you would be engaged in defining priorities as a project leader. Then there is the second level of *national* priorities. How does your thinking run on the question of the growing national emphasis on defining the priorities in science?

ET: I think it has to be looked at in a national context. There has to be a certain amount of freedom in science for people to develop and to innovate; but on the other hand I see Australia as a very large continent with a very small population and with enormous problems, problems in the way we use our land, and I think that we don't have too many choices but to try and channel our best scientific expertise into solving some of those problems. We're in a time of constraint, but I think that we have to find the mechanisms of the best ways to spend the limited resources, and reach agreement on what should be the key areas that we focus on as a country.

The way we've used the land and the consequences of it I think we're only just beginning to understand—from geology, from a whole range of other disciplines. And we've got to come up with some strategies to mitigate the damage.

AM: The BMR has been through a crisis recently that was obviously connected with national priorities, and also the very survival of the organisation.

ET: It was a very difficult period. We felt very challenged as an organisation, the more so because we had been extensively reviewed just three years ago particularly with respect to what our role was with industry, so the current challenge rather came out of the blue. I don't argue that governments have to challenge the role of organisations; but I think that the suddenness with which this happened really did create despondency. I think that the response to it has strengthened the organisation, it has made people evaluate yet more intensely what their role is; to question the very basis of whether a country needed a national geological survey, and to examine the way these similar functions were carried out in other parts of

the world. It was very heartening in that we did draw support from our clients in industry, and we realised the extent of the support that we had.

AM: We've moved away from the question of women. But, in your own life, what have been the constraints in having a really distinguished career, as you have, and a very enterprising career, and being a mother and so forth? I guess that's really been quite a challenge.

ET: Yes, it's a challenge. I guess it's a challenge that most women face. There's always been the pull of work, but at the same time a sense of retaining something of oneself for other areas, for family, and for other aspects of oneself. But it's a case of juggling. I had a great deal of help from my mother while she was alive, she looked after my daughter much of the time I was at work in my daughter's very early years. But it's a struggle that continues. I think there aren't any clear formulae for it. I suppose one muddles through.

AM: Yes, it comes back to this question, as you say, of the male-constructed ambience of work, decision-making, discussion of what's going on in the organisation, all the things which are really vital to your role, from which you have to edge away because you have this other commitment.

ET: Yes, that's right. And I mean in a sense now it still constrains me somewhat about travelling, I have to think twice about travelling, going to meetings out of the state.

AM: Dr Truswell, for your distinguished research career, you have been elected a Fellow of the Australian Academy of Science—the scientific elite which is still a rare distinction for a woman. What message do you have to girls who would be contemplating a career in science?

ET: If there is any message at all, I think the message would be to be themselves as much as possible, to feel that there *is* that freedom there. They may have to push against any barriers that they come up against, but I think they have to have the confidence in themselves and not to be too daunted by what seem to be the rather rigid scientific structures.

I think it has taken me a long time to learn that lesson. I think I conformed greatly, and it's only been in the last few years that I've felt that I had the confidence and the freedom to push against things, to speak out and say, 'Well, I don't think that's the way we are going to do it, I think that we should be doing something totally different.' And mostly it's worked.



Professor Harry Messel,
CBE

Interviewed by Hazel de Berg
Sydney, February 1972

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Tape no. deB577

HM: I was born at Levine Siding, Manitoba, on 3rd March 1922. I was one of a family of six and I came from a family which was very poor. My father was a railroad foreman. The town where I was born, Levine Siding, was a very large place indeed. The total population was the Messel population—one single house and that's all. About five or six years later, when it was time for me to go to school, we moved to a metropolis known as Rivers, Manitoba, which had about seven hundred people in it, and there I spent my next ten years until such time as I'd finished my secondary schooling, at which time I went to the Royal Military College in Kingston, Ontario.

As a child I spent practically all of my time out of doors, I'd been a great lover of out of doors and that's why, for instance, at the present time I'm concerned with a big program of research on crocodiles. I was always very interested in school, tremendously interested in learning. I took a great delight in probing into the unknown. I never associated very much with the other boys and girls around town. I spent practically all my time with Sioux Indians who had a reservation about twenty or thirty miles away, and I learnt to speak the Sioux language quite fluently. It was with them I learnt how to hunt and to fish and to trap for which I earned a substantial sum of pocket money with which I could buy books and clothing and ordinary things. I used to get up at 4 o'clock in the morning and go over my trap lines in twenty degrees or thirty degrees below zero weather, and be back in time for school at eight, and during that time I would cover probably twenty miles. I never used to walk, always used to run or go on snowshoes or on skis, and I'd get back from school in the afternoon and I'd do another twenty miles late in the evening, come back home maybe ten at night and it would be very dark, very cold, and then work on my studies and be back up again at 4 o'clock in the morning. I still only sleep about four hours a day even now.

I had two absolutely magnificent parents whose whole life seemed to revolve around the children getting better and better educated. They insisted that we should do better in life than what they did, although both my parents are very capable people. They're Ukrainians who migrated from the Ukraine in 1890 to Canada, when there was political strife in Europe. They always were deeply interested in education and at all times encouraged their children to do well. In fact it used to be almost the sole topic of conversation in our household. I was encouraged from a very early age to do well at school, even though I didn't require much encouragement, I loved school. To me examinations were a challenge and I used to wait for them with sheer delight because it proved to myself that I understood what it was all about. I then went to the Royal Military College in Kingston, Ontario, Canada.

I got there in a rather odd fashion. I was Ukrainian. No Ukrainians had ever been allowed into the Royal Military College in Kingston up until the time I went there and it seemed to me a personal challenge that a Ukrainian

should go. It so happened that in my early schooling I was never beaten. I was head of the class from the day I went to school to the day I finished, and when I sat for the national examinations, I topped the State, and I could pretty well obtain any scholarship I wanted. It became a challenge for me to see what would happen if I applied to enrol at the Royal Military College at Kingston, they couldn't very well turn down the top student even if he was Ukrainian. And of course I was accepted and I was treated wonderfully well, there was no racial prejudice of any sort. At the college I did well, I got the Governor-General's medal there plus almost every other award that one could obtain in the academic sphere, and I graduated from the Royal Military College in 1942. The war was in full swing so I immediately volunteered for the armed forces. Eventually I went overseas in the infantry, then went into the artillery, and eventually I volunteered for the paratroopers, and when the war finished in Europe, I then volunteered for the Canadian Pacific Forces which was to operate in Japan. [But] because of the dropping of the atomic bomb on Hiroshima and Nagasaki, that task force, Canadian and American, did not have to attack Japan and I was spared.

I entered Queen's University in Kingston, Ontario, in 1946, and there enrolled in two degree courses. At that time many people in the armed forces were being demobbed and [the university] was running courses during the day—the normal university courses—and also courses at night, a sort of a double shift. The university passed a special by-law and I was the first, and I think the only, student ever allowed to take two degree courses simultaneously, one during the day and one during the night. By 1948, I obtained First Class Honours in engineering physics and a First Class Honours degree in mathematics, a Bachelor of Arts with honours in mathematics and a Bachelor of Science with honours in physics, and of course I headed up both shows, so it was a very exciting time for me at the university. Of course, I never worked harder in my life but at RMC I really learned what discipline was all about and I am a person who rebels against discipline from anybody else, although I discipline myself very severely.

When I finished at Queen's University, I had a whole host of scholarships offered to me. I had one from Princeton, I had another one from Brown, I had another one from MIT, the world was pretty well open to me as a student. I also had a scholarship which was known then as the St Andrew's Travelling Scholarship, an exchange scholarship between St Andrew's University in Scotland and Queen's in Kingston. And this university appealed to me a great deal because there was a very famous mathematics professor there, Professor Turnbull, who worked in modern algebra and group theory. I therefore chose the Fellowship to St Andrew's in Scotland. [But] I felt by the time I'd finished at St Andrew's that I had sufficient mathematics in order to be able to use it for some practical purpose, and of course it seemed to me

that the field I could best use it in was one I was fairly well acquainted with and that was physics. So I then decided to go and do my doctor's degree at the Institute for Advanced Studies in Dublin where at that time there were a whole host of very famous men. Prime Minister de Valera had set up during the war [this] very famous institute, the Institute for Advanced Studies at 64 Merrion Square, where he had given positions to some famous scientists in Europe who had fled from the rule of Hitler. Professor E. Schrödinger the Nobel Laureate was there, Professor Heitler was there, Hamilton was there, John L. Synge⁴³ was there—a whole galaxy of famous intellectuals gathered there, and I felt it was a golden opportunity to mix with these people. I started to work with Professor Lajos Jánossy, who is now one of the senior scientists in Hungary. After he left, I then went and worked under Professor Schrödinger and became a very close friend of his.

During this period of time, I published a very large number of scientific papers, twenty or thirty before I even got my doctorate, and I also became very friendly with H.S. Green, who is now [in 1972] the Professor of Theoretical Physics at the University of Adelaide. Green was coming out to Australia to take over the chair of theoretical physics at Adelaide. I wasn't sure what I was going to do but I met a young nurse who was doing nursing at the Rotunda Hospital in Dublin, a young English girl who had come there to do her midwifery course. She had wanted to come to Australia and we thought we'd better come together and of course, that was the beginning of the Messel family. Patricia Iona Pegram was her name, and we came to Australia on the first day of September 1951. And when I arrived in Australia I had taken on a position as Senior Lecturer [in Mathematical Physics] with Professor Green at Adelaide and I had a whole host of other positions offered to me at that time.

In Adelaide I was amazed by the number of young people who were very capable and who were leaving Australia because of the lack of facilities and opportunities in Australia. The more I looked at Australia, the more I was convinced that this country had great opportunities for its youth. And I said, you know if Australians really wanted it there's no reason on earth why they shouldn't be able to train and retain their own young people in this country and eventually get some of the best people from overseas as well. That became a catchcry of mine. I proposed this to a number of people. At the same time I was interested in building up an institute in South Australia which would train and retain young Australians here and attract people from overseas.

The Premier of South Australia, Sir Thomas Playford, had heard about this and it captured his imagination. It was so arranged that I would be a guest at a luncheon to be given at the Town Hall in Adelaide at which I would be the speaker and Sir Thomas would be the replier. The famous luncheon took place and I proposed that we set up an Institute for Advanced Studies as part of the University of Adelaide, which would carry out some top

flight research, which would get some of the best people in the world back here, in which we would be able to train and retain young Australians, and so forth. Next morning, to my amazement, the *Adelaide Advertiser*, on page 2 there was a signed article by the Vice-Chancellor attacking me and Sir Thomas and saying we had enough of science and technology and what the University of Adelaide needed was more of the arts. I got on to the phone to the Premier, and the Premier said of course, the State government was happy to support the university, but it was not in any position to dictate to the university what fields it should follow, and would not do so.

I got in a terrible huff and went in to see the Vice-Chancellor that day and told him I was leaving that night. I wasn't going to wait at that stage—what age was I, twenty-nine or so, I had so many offers, and I was accused of being a traitor by the Vice-Chancellor for leaving the university, and so forth. I said as far as I could see there would be no future. So within two days, I left my wife and one child and also pregnant with the second one, I was on my way overseas. I wasn't quite sure—there was opportunity at MIT, there was opportunity in Canada, there were opportunities everywhere. I wasn't at all worried, I had everything to gain.

But just the morning I was flying from Adelaide to Sydney, I received a phone call from the Vice-Chancellor of Sydney, then Sir Stephen Roberts, asking me whether or not I'd be interested in becoming the head of the School of Physics at the University of Sydney. I said not particularly, because I'd heard what a dungeon this place was back in 1951 and 1952, so he said if I was passing through Sydney, would I mind stopping off and having a discussion with him about it. So I went over to Sir Stephen's office and he was very wonderful and in due course issued me an invitation on behalf of the Senate to take over the Chair of Physics at the University. Of course, I realised this was the senior physics post in Australia. On the other hand, as I walked down the corridors of this old building, there was no light, it looked like an old museum, part of the floor hadn't been finished, and there was sand at the other end. I walked down to the basement and had to put on rubber boots, there was no light and there was about eight inches of water and old boards floating around the bottom. I thought, my dear God, this is really in a bad way but things could be done here. One could make Sydney a world centre for research in physics just as well as one could in Adelaide, in fact even more easily, if one had the staff and the money.

So I had discussions with Sir Stephen, and I told him that I would be making preposterous demands if I were to accept. He said, 'Well, what are they?' I said I wanted the opportunity of making fourteen new permanent academic staff appointments at one blow in the school, and certain minimum research budgets and so forth. I also put down the basis, that we'd be able to get our journals from overseas by air, that we'd be able to go overseas once

every two years instead of six in order to keep up with our field. I put this all in writing, then I put in my letter, 'If you accept these conditions, then I'll be happy to accept your invitation for the Chair of Physics', and away I went. I was in Europe at the time and I never thought about it again, I thought the university couldn't possibly accept such preposterous demands. [But] when I was in Milan in Italy, I received a cable from Mr Maze who was then Registrar of the University of Sydney, which said, in effect, a meeting of Senate has accepted all your points and you are hereby appointed head of the School of Physics from 1st September 1952. That's twenty years ago this year. I well remember a very famous scientist [at Imperial College] in Great Britain told me, 'Messel, you are wasting your time. Things can be done far better in Great Britain than they can in Australia.'

When I look back now, it was a very wise thing indeed not to have taken that advice. The amusing thing was that the individual who gave me the advice would learn much to his consternation about a month later, that everybody, except one of the senior technicians on the high energy physics staff of the Imperial College had joined my staff and was coming out to Sydney. I was also able to bring back some other very well known Australians and some famous people from the United States and from Europe, and in very short order I was able to build up a famous team of young individuals—all very dynamic, all very enthusiastic about the opportunities which Australia had to offer.

However, after getting back to Sydney I soon realised that we weren't going to get very far unless we had sufficient money to do what we wanted to do, even though the university was going to give us what, in those days, was a fairly substantial sum of money. I soon learnt that we needed larger sums. One of the things which struck me early on in the piece here was the fact that, in 1952, the relationships between the university and the community were very bad. There was the community sitting on the side lines and there was the university in its ivory tower, and they were throwing stones at one another and there had been very little interchange between the two. Because of this, when the University of Sydney in 1951 ran a centenary appeal, it only got about 10 per cent of what it set out to obtain, because the people and the big business community didn't seem to be interested in providing funds for the university to do those tasks which it felt it ought to do.

I felt there was much to be gained in Australia in an endeavour to bring together industry, private individuals, governmental and semi-governmental bodies together with the university for the mutual benefit of both. These bodies could offer advice and provide funds, and the university could provide expertise between the two. One of the major problems was the fact that practically all our best students in this country were leaving for overseas to

work for their doctorates and post-graduate degrees and the overseas countries would keep the very best ones for themselves. I felt that Australia was never going to develop as long as we were going to continue losing the best young brains. We had to train and retain these people, and I felt that one could achieve this by setting up a foundation. And that is how the first foundation of its kind came into being in the British Commonwealth. The Science Foundation for Physics, which I started in 1952, was inaugurated in 1954, and established just in that fashion.

When it started everybody said that it could never be done. The first person I ever interviewed for membership of that Science Foundation was Sir Frank Packer, the head and owner of Consolidated Press. I requested an interview to see him, and I went and saw him and he said, 'Right, you've got fifteen minutes.' In fifteen minutes I explained to him that for Australia to go ahead, we'd have to train and retain our own young people and give them adequate facilities here and adequate funds, and there was great merit to be gathered from a combination of forces of the university and industry and semi-governmental bodies. Then he said, 'Right Messel, what do you want?' I said, 'Your money, Sir Frank.' He said, 'How much?' and I said, '£2 000 a year.' He said, 'What will I get for it?' I said, 'Nothing, absolutely nothing.' He said, 'You can guarantee that, can you?' and I said, 'Yes.' He said, 'Right, the cheque will be in the mail tomorrow morning.' That was how my Science Foundation started. Shortly thereafter G.B.S. Falkiner, the owner of famous 'Haddon Rig', became a member of the Science Foundation and he gave £50 000 and then, Sir Adolph Basser, the racehorse owner and jeweller gave £50 000 to start off the computer and that's how computing started up in Australia. We had the first university computer in Australia, known as SILLIAC. And then he [Basser] gave another £50 000 and then W.D. & H.O. Wills gave £50 000 and Stan Chatterton gave £50 000 and so it went, and we've built up this wonderful group of people known as the Science Foundation for Physics within the University of Sydney. It's helped make all those things which have happened here in this school and which have allowed it to become a world centre for research.

The combination of the forces of people in the outside community, semi-governmental bodies—for instance the Australian Workers Union is a life governor of the Science Foundation—all of these bodies coming in together and providing advice and funds, and general leadership with the university, has allowed this school to end up with seven departments and nine professors, one of the largest schools in the world of its kind and some of the most famous research facilities of its kind. [One] research department is in Astronomy with Professor [Bernard] Mills and the best research radio instrument of its kind [the Mills Cross]. Another department is under Professor Hanbury Brown, and the world's first Stellar Intensity

Interferometer at Narrabri [Observatory] is under his control. Then we have the world's largest cosmic ray experiments going on under Professor [C.B.A.] McCusker. The Plasma Physics team here under Professor [Charles] Watson-Munro, the Theoretical [Physics] Department under Professor [Stuart] Butler, the Basser Computing Department under Professor [John] Bennett—these are all things which have grown. Last year we had 180 students from overseas wanting to join our graduate school, and we are tremendously selective. Out of 180, we took one.

In addition to our work at the university here, we took a very active role, going back fifteen years, in the secondary school field. I felt that in order to get the best students to come to the university, we should also work in the secondary schools as well, and one of the first things I did back in 1956 or 1958, I started a Summer Science School for the science teachers and I ran four of these in four consecutive years at which we paid high school teachers to attend. It was highly stimulating. Then we switched to our Science School for High School Students which has been running ever since and has become the International Science School [for High School Students] where we choose the most brilliant students in Australia, in each of the States, and in New Zealand. And we get the top scholars from the United States known as the President's Australian Science Scholars, the five top scholars from the United Kingdom known as the Royal Institution Australian Science Scholars, and the five top scholars from Japan each year known as the Sako Sato [Japanese Prime Minister's] Australian Science Scholars, and each year these kids are received at the White House and at Buckingham Palace and at the Prime Minister's Residence in Japan, and they come here. This has become world famous and we have some of the best people in the world lecturing here year after year. [By 1972] we've passed something like fifteen hundred students through these Science Schools. Two of the former science scholars are now permanent staff members in my school, many of these people have become famous throughout the world in the intervening time. For instance, Bruce McKellar took over the Chair of Theoretical Physics at Melbourne. Last year another student of mine, Chris Wallace, took over the Chair [of Physics] at Monash. A couple of years previously another student, Max Brennan, took over the Chair of Physics at Flinders University. If you go throughout the Australian universities, you will see in the physics and mathematics departments, a big layer from the top to the bottom staffed with former students of this school.

This is what we promised we would do for Australia and this is what we have done for Australia. I consider myself exceedingly fortunate to have been born when I was. In 1951 and 1952 when I came to Sydney, radio astronomy was just in its infancy, computing was just something a few people were talking about, there were a couple of electronic computers in the United

States. But within a decade we trained over three thousand people in their use and in 1954 when we built SILLIAC here at the University of Sydney with the help of Standard Telephone and Cables, this was the fastest computer in the world. We were able to build it because the Americans had given us a blueprint of their upgraded first one, ILLIAC. The field of cosmic rays, that was in its infancy. In the field of nuclear reactors, that was in its infancy. In the field of thermo-nuclear physics, that was in its infancy. In the intervening twenty years, I've seen each one of these fields reach great maturity. Radio astronomy has made tremendous advances and some of the major radio-astronomical instruments in the world are in our organisation.

One of the things I haven't mentioned so far was books. When I came to Sydney, it was an amazing thing that there wasn't a single girls' school in Sydney which offered physics and this meant that any girl who came to the University of Sydney and had to take a course concerning first-year physics was doomed to almost certain failure. This seemed an incredible thing to me, that science wasn't compulsory in the schools. Consequently, when secondary school reform was being discussed in this State [New South Wales] in the late 1950s, myself and the Science Foundation exerted great public pressure—much to the displeasure of a lot of the people—and great private pressure, to try and make science a compulsory subject in the schools of this State. It is well known that the Wyndham Scheme eventually came into force and it is well known that science was made a compulsory subject for every boy and girl for the first four years of their schooling. [But] the question then arose, which science?—physics, chemistry, geology, biology or all of them? And the answer was, all of them. And this brought about a real first, worldwide, in that four years of science would be compulsory but the science that was given would be an integrated course, integrating the subjects of physics, chemistry, geology and biology into a single unity.

We were thrilled by it, but the government implemented the Wyndham Scheme with undue haste and poor preparation, and the schools were suddenly expected to teach the new science course without any adequate preparation in advance or having the necessary equipment. The government invited me to write the necessary textbooks. In order to do this, I assembled the now famous team of science textbook writers (consisting of university academics, practising science teachers, inspectors from the Department of Education, etc.) which wrote the science textbooks covering the first four years' integrated science course.⁴⁴ This became the first book in the world which contained a whole integrated four years' science course, and since that time both the United Nations and UNESCO have taken up the cry and integrated science is now going throughout the schools, throughout the world. The British have adapted these books and so have many other countries. I think one of the most significant things we've ever done is those

science textbooks and bringing about compulsory science in schools for every single individual in the State of New South Wales. In future any politician or any other individual who comes from this State will have gone through the system, and I think the long-term ramifications and impact of this are beyond imagination.

From there, of course, we went on and wrote the books for the fifth and sixth years and we then came up with the first package in the world whereby we had a coordinated and integrated course of science covering the whole six years of secondary schooling. I don't wish to boast about them, but we feel they have made a tremendous contribution in the field of science teaching the world over.

We've brought out many other books, too. Each year we bring out a book containing the lectures given at our International Science School and each year these have been republished worldwide internationally and gained a famous reputation. I've lost count of the number of books I've put out, maybe thirty or forty and probably something like seventy or eighty scientific papers.

Perhaps one of the best ways for me to sum up my whole feeling in relation to science now and in the future is given by the quotation printed in each issue of our annual review, *The Nucleus*, which is the annual review of the School of Physics and the Science Foundation for Physics. It's a quotation by Alfred North Whitehead given in 1916 and it states:

In the conditions of modern life the rule is absolute: the race which does not value trained intelligence is doomed. Not all your heroism, not all your social charm, not all your wit, not all your victories on land or at sea, can move back the finger of fate. Today we maintain ourselves. Tomorrow science will have moved forward yet one more step, and there will be no appeal from the judgment which will then be pronounced on the uneducated.



Professor Messel retired as Professor of Physics and Director of the Foundation of Physics in 1987. His long interest in crocodiles was reflected in the Foundation's acquisition of the *Harry Messel*, an ocean-going vessel with biological laboratory, and a field station in Arnhem Land to study the *Crocodylus porosus* population of northern Australia.

He is now in his second era, as the Chancellor of Bond University, the first private university in Australia. He says 'era two' is even more challenging and exciting than 'era one'.



Professor Peter Bishop,

AO, FRS, FAA

Interviewed by Ann Moyal
Canberra, April 1993

National Library of Australia
Tape no. TRC-2931

PB: I was born in Tamworth, New South Wales, on 14th June 1917, the second son of a family which was subsequently a family of five. My father at that time was a surveyor. It wasn't until we moved to Newcastle that I first went to school, then when I was seven we moved to Armidale and my father became the District Surveyor. I went to the demonstration school in Armidale, and sat for the entrance examination to go to the high school. The high school in Armidale was founded about 1928. I was one of the very early students in the Armidale High School and I passed the Intermediate from there.

AM: Did you show an early interest in science and natural history as a boy?

PB: My family was not at all bookish. My mother was always very keen that I should have a successful career and we bought the twelve volumes of *Children's Encyclopedia* and the *Encyclopedia of Words*, an encyclopedic dictionary. I read the *Children's Encyclopedia*, I found them fascinating; but I got my first enthusiasm to read from the School of Arts in Armidale. The School of Arts was the sort of library of country towns in those days. And the library used to get the books up, from the State Library I suppose once a month. I became very friendly with the librarian in the School of Arts and she let me unpack the books. And science was a big thing; how to make and do things; there were whole journals called *Work* which I used to read, you see; how to make or how do this or that. That was the sort of reading I did.

AM: So you were at the Armidale High School and from there went on to Sydney University?

PB: I went to Barker College at Hornsby. And I remember very clearly, I got myself down to Barker, my parents didn't come with me. At the age of fourteen I travelled down on the train, carried my suitcase up to Barker and presented myself and said, 'Here am I.' So I've always been terribly independent, I refused to be pushed and shoved by anyone. Barker College was at a very low ebb at that time. It was 1932, the middle of the Depression. The school had seventy-eight students, total. And there were five who sat for the Leaving Certificate the year I did it. So the schooling wasn't very good at all. Well, I passed the Leaving Certificate in 1933 but it wasn't good enough to get a University Exhibition. I repeated the Leaving Certificate in 1934, got the Exhibition, and went to the university.

I wanted to do engineering, but my mother imagined me being a Macquarie Street medical specialist and that would be the pinnacle of success for her. So it was medicine that I did. My best subjects were physics and mathematics and I have a sort of engineering cast of mind and I would have liked to have done engineering, but I'm glad I did medicine. In retrospect it

was a very poor course, all medical courses are poor. They have to be, because they're all introductory. There are so many different subjects, paediatrics, psychiatry, you name them. In the introduction you learn the right words to use, so you really don't get trained till after the course. But a very important thing happened in my medical course. There are two things I should mention. One was going to St Paul's College. That was terribly important as far as I'm concerned, because my best friend there was Gough Whitlam, and he and I were very friendly. He published *Hermes*, the literary journal for the university, and I was a co-editor. I wrote poetry in those days, and I published *The Pauline* which is the college magazine; he was the editor and I was the co-editor, so his influence was very important.

AM: It's such a rich life at college, isn't it.

PB: It opened my eyes for the first time. Here was a country boy suddenly going to St Paul's College and meeting all these people. It was a tremendous eye-opener for me. That was the first thing. The second thing was, during my medical course, I did the sections of the brain, and I can remember very clearly holding a brain in my hand and I was absolutely fascinated to think this brain once belonged to a person like myself, had thoughts and ideas and ambitions the same as I had. And from then on there was no doubt at all that I would study the brain. I dissected the brain several times, and even when I was in the Navy, I kept a brain under my bunk. Of course, so much of the war years were just sheer drudgery, convoys and so on, that sort of thing, so when nothing happened I used to dissect the brain.

In 1939 the war started, and many of the Residents at the hospitals joined up after their first year and went off to the war, which meant of course the hospitals had no staff. And so we were effectively, in our final year, given honorary status. We had, of course, not yet graduated, but I can remember giving open-ether anaesthetics day in and day out in the hospital. So we were effectively Residents, and our course was shortened. We sat for our final year exams in 1940—normally the graduation ceremony is the year after your final exams—but we graduated in 1940. Because of that the opportunity came up of being Neurological Registrar. I'd written articles on the brain as an undergraduate. The opportunity came when I was actually graduated to become Resident in Neurosurgery and Psychiatry. I took the job immediately.

AM: You were a Registrar, a Neurological Registrar for the year 1941 to '42. But then you join up, and you went to the war.

PB: I became a Neurological Registrar and assisted in neurosurgical operations and so on. Well, I should mention too, that my mentors were Sir

Harold Dew, one of the persons who started neurosurgery in Australia; Gilbert Phillips, the leading neurosurgeon; and Arthur Neville St George Handcock Burkitt, Professor of Anatomy. Those three were tremendously supportive of me all through my career. After the war Gilbert Phillips and Harold Dew arranged for me to go and work with the neurosurgeon Sir Hugh Cairns in Oxford, to train in neurosurgery.

AM: Let us just briefly come back to your war service as a surgeon-lieutenant serving in the Atlantic and the Indian and Pacific oceans and on through in New Guinea. You were saying earlier that this was a time when there was a lot of routine work and you kept a brain under your bunk. Did you not have a chance to do any neurosurgery?

PB: No, not at all. Having little surgical training I was of no use as a neurosurgeon, so I remained the lowest of the low, which was understandable. I was a surgeon-lieutenant so consequently I served on destroyers (a destroyer was the smallest ship that takes a medical officer), and my job was to make sure that all the people were healthy.

While still in the Navy I applied for the Postgraduate Fellowship in Medicine at the University of Sydney to work on people who had been injured cerebrally with gun-shot wounds so that you can relate the part of the brain that's damaged with the function. Quite a lot of relationships like that had come out of the first war, and I wanted to go on with studies concerned with brain injuries sustained in the Second World War. I was demobilised in June 1946, and almost immediately I went to Oxford to train under the neurosurgeon Sir Hugh Cairns. He arranged for me to do clinical neurology for nine months at the National Hospital, Queen Square in London. While at Queen Square I realised that what I really wanted to do was basic neurological research.

So I went and saw Lovatt Evans, the Professor of Physiology at University College, who was very sympathetic and helpful. J.Z. Young had just been appointed from Oxford to the Chair of Anatomy and he took me on. University College was bombed, quite drastically damaged, and people were only just gradually coming back. But he took me on immediately, gave me an empty room—it was up in the Biophysics Research Unit area—and a project. The project was to see if I could repeat what the Russian authors had said that they could get changes in the electroencephalogram with training in rabbits. That sounds a stupid thing, but it's very important; if you could train rabbits and then show that the electroencephalogram was different, it would be a very important finding. So that was my project. I had no research training, an empty room, and a project.

AM: [*a little laugh*] And three years.

PB: And three years to do it in. Oh well, I had my whole career to do it in. I was determined to do it in my whole career.

AM: This is of course the Postgraduate Committee in Medicine Fellowship from Sydney?

PB: Yes. And I may never have come back, as far as they were concerned. So I had to set to work. I had to build my equipment. I knew nothing about electronics, so I went to the Northampton Polytechnic for two years to learn electronics. Electronics of course got a tremendous boost because of the Second World War, so that most of the engineers who had been trained before the Second World War knew practically nothing about electronics. There was a special course for engineers who had had no experience in electronics. I went there three nights a week for two years to learn electronics. The first year I did the physics of the electron microscope, which wasn't very helpful for me; but all this was very good grist to my mill.

Then I set to work building the equipment. Now, in those days of course it was all valves (transistors had never been thought of) and so I set to work to build amplifiers and all the associated equipment from scratch. I used to go down to Leicester Square. Leicester Square used to have the war surplus stuff out there on bazaar-type stalls. You could buy valves and so on. I used to go down there twice a week and buy electronic items from stalls to build the equipment.

It took me three years to build the equipment. And I was definitely determined to build it, I really slaved at building this damned equipment. I thought a DC amplifier would be essential, and I set to work to build a DC amplifier. It took me three years to build it, and it was a long battle. It was the best amplifier at the time when the circuit details were published. My first seven papers were on electronics, nothing to do with physiology, they were on electronics. And the paper on the direct coupled amplifier was published in the *Review of Scientific Instruments*, and in its time it was the best in the world, a direct coupled amplifier. But useless. I didn't need it.

AM: In those days, of course, multi-disciplinary discussion was not available to you, was it? You could have had help, perhaps.

PB: Well, you see, this is my story. I never had a boss. I was completely on my own. But people should have told me a bit more. But I don't regret it. I was absolutely determined to go back to Australia; and I knew that when I

went back I had to be completely self-sufficient. So I did the Bachelor of Science course in histological methods, doing staining, the three-months course in histology; because when I got back to Sydney I knew I would have to be familiar with these methods and I knew I'd be completely self-sufficient, I wouldn't have to rely on anyone. However I did publish a paper on the brain, which was published in the *Proceedings of the Royal Society*.

Anyhow, I decided to study the visual system. Why did I start with the visual system? Simply because the people in America had shown that there were potentials between the different layers in the frog's tectum, the frog's colliculus,⁴⁵ effectively, and that different cell layers had different potentials across them. And I thought well, I'll try and repeat this. So I started on that project. That was the reason for the direct coupler amplifier, you see, because there were supposed to be steady potential differences between the different layers. That was a rather silly project too. I started inserting the electrode into the tectum. Well, as soon as you start pushing electrodes into the brain you cause damage, and damage causes potentials. So these potentials I was recording, big potentials, were of course nothing to do with the layers in the colliculus but were due to damage.

AM: You were gathering to build a research career which was going to also offer teaching from a research position?

PB: That's right. When I came back to Australia in 1950, Harold Dew was in the new Medical School, which is now called the Blackburn Building—he gave me four empty rooms. So I had to start again.

I think I ought to mention at this stage the important thing that happened in 1950. At Sydney University a Bachelor of Science (Medical) course had been started in 1949. I'd heard about this, and immediately realised its potential. I was not then a member of the Faculty of Medicine. The four empty rooms I called 'Brain Research Unit', I had it painted on the doors. No one said, 'You can't do that!', so I called it 'Brain Research Unit', and immediately applied to the Faculty to have as many students who would be interested to do a Bachelor of Science (Medical) course.

It was a very busy year for me, because I had four empty rooms, four students, and virtually no equipment. I had to get projects for them. It was very good of them to even come and work with me. There was Richard Gye (who subsequently became Dean of the Faculty of Medicine in Sydney), David Bell (who became a psychiatrist at St Vincent's Hospital), David Glenn (who became a senior surgeon at Royal Prince Alfred Hospital) and James Scougal (who became an orthopaedic surgeon at the Royal Alexandra Hospital).

I think one of the interesting bits of work I did was with David Bell. We worked on human bodies, because neurosurgery depended very much on

what are called stereotaxic co-ordinates of the brain, that is, being able to pinpoint a part of the brain from outside the skull. And I was one of the first persons to start doing stereotaxic co-ordinates of the thalamus. Of course, it was a project beyond my powers.

But anyhow, this is what David Bell and I did. As soon as the bodies came into the Medical School the caretaker would inform me immediately, and frequently it might be Sunday morning, or the middle of the night. We would immediately get the bodies and insert long straight wires into the brain, and in addition fill their ventricles with radio opaque substance and take x-rays. So we were able to see where the wires were in relation to the ventricles. Then we'd subsequently take the brain out and we would cut it up and work out the relationship of the thalamus to the wires we'd put in, and so do it in three-dimensional co-ordinates.

We used to have lots of troubles with the electricity. There were the blackouts. We'd be in the mortuary with the bodies and the lights would go out and we'd have to sit amongst all the bodies till the lights came on. Oh dear, oh dear, that was eerie.

At University College I had begun work on frogs but I realised that mammals would be more acceptable to the Medical School in Sydney so I switched to the cat as my experimental animal. Cats are very good for my work because their brains are next to the primates, they have very advanced brains. They have very furrowed brains; not like a rabbit which has a smooth brain. So they're well advanced. And they have binocular vision—two eyes in front. And that was terribly important for my work on binocular vision. And they are small, they survive anaesthetics extremely well, and so on. They're ideal animals. And they are very similar—when you do work on a cat in Australia, the cat in Alaska is virtually the same. It is with very great difficulty that animal breeders have been able to change the characteristics of cats. They are very stable genetically, so that you know that whatever work you do here can be repeated somewhere else. Dogs are two-a-penny, oh, thousands of breeds; they can be changed by animal breeders very readily. But cats not so. Cats only differ really in the fur, the coats, or the length of their tail. They don't differ in any essential as far as their brain is concerned, they are all the same. That's terribly important.

AM: As you were in charge of your Brain Research Unit, and not yet a member of the Faculty, you were still, as it were, your own boss and rather cast off on your own.

PB: Well, that was the first year. In 1952 I became Senior Lecturer in Physiology in the Department of Physiology, and Frank Cotton was the Professor and he really gave me *carte blanche*. I gave some lectures, but not very many.

AM: And what was the state of that School, or Department, at that stage? It had been not exactly a research area when you were an undergraduate.

PB: No, I virtually started basic research work in the Medical School. Eccles of course pre-dated me. Eccles was at the Kanematsu Institute from 1937 to 1943 and of course he did tremendous work there.⁴⁶ He was of course well trained in England, he'd worked with [Sir Charles] Sherrington and so on, so he'd had good training. But he wasn't well thought of by the medical establishment in Sydney.

AM: But then they were probably not in a position to know very much.

PB: They didn't know much about him at all, no. So it was only after the war that I started basic medical research in Sydney. I don't know about the other universities.

AM: Did you begin to build up a team of colleagues?

PB: Well, not really. For every year after 1950 I used to have two or three or four medical students, who worked with me. Jim McLeod, now Professor of Medicine at Sydney University, was one of my students, and Bill Levick who is a Professor at the Australian National University.

AM: Were you still building your own equipment? Or did you have some assistance then in that?

PB: Well, I had a technician, but I had to tell him all about it, he was not trained in electronics. There were other people in the Department of Surgery (I was in the Department of Surgery effectively from '50 to '56 or thereabouts) and they had a workshop, a good workshop, so that a lot of the straight workshop things—like building routine equipment—they could do, so that they were a great help there.

AM: I note that in 1962–63 you take a sabbatical leave at MIT in Cambridge, Massachusetts in the US, in the Department of Biology and Research Laboratory of Electronics. So I presume, there again, you were equipping yourself for the hard toil.

PB: Yes and no. That was really not a very productive year. Well, I worked on the spinal cord. But it was an interesting year for me, because the people I was associated with were all the weirdest people you could possibly imagine. Here's me the straightest guy, and there were people like Norbert Wiener⁴⁷

there. They were the sort of people with whom I was out of my depth as far as mathematics was concerned. The Research Laboratory of Electronics where I was, was very important during the war, because tremendous advances in electronics were done there. The Laboratory subsequently published thirty volumes of what had been accomplished during the war on the various aspects of electronics. They led the world in this research.

If I can make another point. Until 1958 or thereabouts, the work that I did was on electrical stimulation of the optic nerve, stimulating the visual system electrically. And the reason why I did that was because all the emphasis up till that time in the world was on synaptic transmission (which is what Eccles did), synaptic transmission as well as how nerve impulses are conducted in different diameter fibres, nerve fibres of different diameter. A lot of effort was put into trying to work out why the visual system has nerves of different fibre diameters, and what their physiological significance is. So that electrical stimulation of the visual system was important from a basic neuroanatomical point of view. Why are there different sized fibres, and where do they go and what do they do? Now, the big change came about as far as I was concerned in 1958–59, when I realised for the first time the importance of psychophysics.

Psychophysics is what the brain can *do*—it's as simple as that. When you go to have your eyes tested, that's psychophysics. You are testing the smallest letter you can see. That's psychophysics. How many different colours can you see? Psychophysics. I went to Johns Hopkins in 1958 to see Steve Kuffler who had worked with Eccles. He was at Johns Hopkins University in the Wilmer Institute. And in the course of that visit I met for the first time David Hubel and Torsten Wiesel. They won the Nobel Prize for Physiology in 1981, for work on vision. A very remarkable couple, Hubel and Wiesel. I went and watched them doing an experiment. There is such a thing as a multi-beam ophthalmoscope that had been invented some time earlier in the Wilmer Institute for shining spots of light into the eye to study how one spot of light can interact with another spot of light. And I watched them doing the experiment. This would be just when they started together in their joint work for which they were awarded the Nobel Prize. And I was fascinated by it. So when I came back to Sydney I was determined to build a bigger and better multi-beam ophthalmoscope.

Hubel and Wiesel were much brighter than I was—what they did was to chuck that multi-beam ophthalmoscope out and simply put a tangent screen in front of the cat and just wave things in front of it. Because what is the visual system there for but to see objects in the external world? You wave things in front of the cat (of course the cat's anaesthetised) and see what the brain does. So that was when the realisation of the importance of psychophysics struck me. And I worked on binocular vision because

binocular vision (this is cooperation of the two eyes) is a fairly advanced field in psychophysics. There is perhaps only one other field of research that can rival it and that is colour vision, the understanding of colour vision.

AM: Apart from the two researchers that you visited, was it a field of wide interest or were there only a few people working in it?

PB: Oh no, only a few people.

AM: So, with your enthusiasm—and as you say, you made these mistakes, which I think is a very important and charming piece of your history because it's interesting to learn that by making these mistakes you get there in a big way.

PB: [*laughing*] Oh, I made lots of mistakes. Well, of course the multi-beam ophthalmoscope was ... I chucked that out. But you see, Hubel and Wiesel ... already had the equipment to work with it and knew its shortcomings. It wasn't till I'd built the dashed thing and worked with it that I realised how limiting it was research-wise.

AM: It's interesting through the interview the presence of the Nobel Laureate, Sir John Eccles, is there, in Sydney before the war. But he comes to Australia in 1952, to the John Curtin School of Medical Research at the ANU.

PB: Yes, of course we knew one another. But he really didn't have much effect on me, because he worked on the spinal cord (he subsequently did work on the cerebellum but his main work was on the spinal cord), and of course my main interest was the visual system. Furthermore, he worked on the motor side and I worked on the sensory side, and there's a very important difference. The motor cells, particularly in the spinal cord, are big neurones and they stand up well to being pierced. I can remember very clearly getting the news in England at University College that Eccles had recorded intracellularly from motor neurones in the spinal cord. People couldn't believe it. The world was agog, to hear that Eccles in New Zealand had done this. The motor neurones in the spinal cord are very big and they can stand electrodes being inserted into them.

What I did originally was to do work like Eccles but on the visual system, on the sensory side. Of course I was able to electrically stimulate the different fibres in the optic nerve and work out a lot about their conduction velocities and all that sort of thing; that was no problem. But when it came to synaptic transmission we didn't succeed, simply because it was just too tough.

AM: Would you clarify synaptic transmission for the layman.

PB: Each neurone is a separate identity, and impulses are conducted from one cell to the next through a junction, and that junction is called a synapse. And that's what Eccles got the Nobel Prize for, synaptic transmission. We tried to do that same thing on the sensory side, but we didn't succeed. And it was only subsequently of course that I realised that the best way to study the visual system was the way I've just been telling you about.

AM: Though your work didn't exactly overlap, you have both been described as the two great father figures in Australian neurophysiology. And, certainly, Australia has now found a very significant place in this field, hasn't it?

PB: In neuroscience we do extraordinarily well, as good as anywhere in the world. The volume of course is not comparable in any way to America, but in terms of the quality of what we do here, is quite up to any standard anywhere in the world. I give first place to Eccles.

AM: Now, you have this long and very productive period, at Sydney. And then in 1967 you're appointed Professor and Head of the Department of Physiology at the John Curtin School of Medical Research at the ANU and you hold that post until 1982.

PB: One point I'd like to make about the John Curtin School is that Eccles of course realised he wouldn't get many students from Australian universities (David Curtis⁴⁸ was one of the very few students from Australian universities). And that's quite understandable. Two reasons for that. First of all, the PhD degree was introduced for the first time in Australia in 1949 or thereabouts and that meant of course that students were obviously being kept in their State universities, they wouldn't want to send them to the ANU to do their PhDs because they wanted to keep all the bright students themselves.

The second thing about it is that the sort of work that I did and Eccles did was very specialised. And I didn't really want PhD students, because it was rather too specialised for them. I wanted to take on people who knew what they wanted to do and that this was the field they wanted to work in. Both Eccles and I attracted people from all over the world. And that was tremendous, because first of all they were already trained, they had worked in neurophysiology before, and secondly they'd come thirteen thousand miles and they hadn't come here to loaf, so they were terribly keen. So for those two reasons I had a whole succession of very able people. And Eccles' experience was the same, he had very few students from Australian universities.

John Curtin School then had a wonderful workshop, and I got on famously with them because that's the sort of work that they loved doing. I always feel people in the main workshop ought to get much more credit than they do, fitters and turners. I had inherited a very wonderful main workshop.

AM: So how would you describe—I think this is very revealing to hear—how would you describe a day in your life as a research professor in your field?

PB: I was always in the lab by nine o'clock, rarely much earlier than that. The experiments I did used to go on for five days and almost invariably finished at midnight. So I worked for four or five days a week from nine o'clock in the morning till midnight. I didn't very often go on after midnight, for the simple reason that I found out that, if I worked through, right through the night, I was so tired that I couldn't do much the next day. As long as I had a little bit of sleep, four hours was all I needed. Eccles is an extraordinary man, physically extraordinarily vigorous, and he could stand up to that sort of thing.

AM: This brings us to this important crowning time in your life and that's the award of the Australia Prize in 1993. And just to give the background to the prize, it was set up by the Hawke Labor government, spurred by its Minister for Science, Barry Jones, in 1990; this is the third year; and it's to reward excellence in scientific research and to open competition to Australian and international scholars. And, as it carries \$250 000, it's one of the largest money prizes I think in the world for science. It's awarded each year for a selected area of research—which makes it distinctive from the Nobel Prize.

PB: Yes. What I think the Selection Committee does is pick an area in which we're very good. There are areas in which we are very good such as immunology, and neuroscience particularly, those two, and radio astronomy, all branches of astronomy. Now, once you've picked those areas, you can be fairly sure that there will be an Australian who could be given the prize. And that's important.

The second thing is that the Committee couples the Australian with one or two scientists from overseas who they think could be said to be of equal or better standard. If they didn't do that it would mean that the Australia Prize would always be given to say an American, or a Swede, and that would obviously not help Australia much. So that's what I imagine the Selection Committee does. It has to be that way, because the person selected has to be equated with the best that there is around the place. And that's I imagine why it's done, and it would be silly not to do something like that.

AM: It was awarded jointly to three, to yourself ...

PB: Vernon Mountcastle of Johns Hopkins, and Horace Barlow from Cambridge.

AM: How do you see it?

PB: In my acceptance speech, I stressed three important influences or events in my life. The first thing was the importance of the people who worked with me, particularly Bachelor of Science (Medical) students, and there were quite a number of those. And the second influence was of course my wife, Hilare, to whom I am tremendously grateful for her help and support throughout my career. And the third thing was the importance of the Australia Prize, and I congratulated the Labor government on setting it up because it's time that science was emphasised a little bit more than sport.

You say it was about \$250 000. Greg Norman, the golfer, would get \$250 000 in an afternoon. I'm not sneezing at \$250 000, but it's nothing really to what people in the sporting world get.

AM: Do you think science is very much undervalued in Australia?

PB: It is, I think. And our future is very much involved in science, absolutely. I suppose we have to get the Olympics in 2000 but I think science may be of more importance than the Olympics.

AM: How does one make science attractive to young people?

PB: Unfortunately, unless you've got it in you (put it that way), I don't know what you can do about it.

AM: You were excited by holding a brain, and you felt that ...

PB: That focused me. I think I would have done something in science irrespective; but it wouldn't necessarily have been in medicine; it might have been in something else. Whether I would have done equally as well in some other field I don't know, but I would have done science—because that's me.

The brain is what makes us different from everything. The brain is the last unknown subject. I always get annoyed, you know, when people like Paul Davies talk about the mind of God, or Stephen Hawking with his 'we now know the mind of God because we know all the four particles'—you know, the strong force, the weak force, the electromagnetic, and gravity—

'once we know how to unify the field we know everything.' That's absolute and utter nonsense! We still don't know anything about the nature of consciousness. One of the reasons why I worked on binocular vision—and because of psychophysics—I knew that I could work on that without having to involve memory and consciousness. As soon as you have pattern recognition, that's an enormously tough problem, because to have pattern recognition you must have seen it before, otherwise you wouldn't recognise it. The term 'recognise' means you've seen it before, and that means memory. And of course memory and consciousness, they are enormously tough problems; and that's what I would have liked to work on, memory and consciousness, but I knew that they are too tough at this stage. Binocular vision is much easier to work on, and so I got quite a long way with it because it doesn't involve consciousness, doesn't involve memory.

AM: And would you say that this whole question of the uniqueness of man among his fellow creatures is really one of the major challenges to understanding?

PB: That's right. Absolutely, absolutely. Yes. But even, you see, understanding the uniqueness of man requires an understanding of the *whole* brain. You see, synaptic transmission and all those sorts of activities are some of the building blocks that have to be put together to describe the whole man. But the whole man is a mighty tough problem. That's my interest, of course, the nature of consciousness and the brain–mind problem. And I think a lot of the neuroscientists like Kuffler and Eccles too (his first book was *The Neurophysiological Basis of Mind*), they were interested in that problem. I don't agree with Eccles on his dualism. But that's what fascinates him too, the nature of mind. It's the sixty-four-dollar question.



Professor Alfred Edward
(Ted) Ringwood,

FRS, FAA

Interviewed by Ann Moyal
Canberra, March 1993

National Library of Australia
Tape no. TRC-2923

TR: I was born on 19th April 1930 and that was just at the beginning of the Depression. My mother and father were both born in Australia. My mother's parents came from Ulster; Presbyterians from Scotland before that. My father was Australian-born. His father was born in New Zealand and his mother was born in India, but before that my great-grandfather was Australian-born too. I was born in Melbourne.

AM: What did your father do? What influence did your family have in your reading and your early development?

TR: Well, it's a complex story, because my father went through the First World War as a very young man and then had a reasonably good time I think during the twenties. But he had no particular training, he'd left school at twelve or fourteen (whatever was the age) and then he was basically, I think, a clerk in a shoe shop until the war, or just did odd jobs. Then at eighteen he joined up, and had a reasonably difficult time, he was in the trenches in France and got gassed and trench feet and all that. So he didn't have any scientific background.

But my grandfather, on his side, did. He was entirely self-educated. He went to the working man's college as an adult and became very interested in chemistry and radio. And I suppose my strongest and most enduring influences in science I would have got from frequent contact with my grandfather. He built radio sets from the very early days, he followed the whole history and built his own sets. So that when, as a youngster, I started to get interested in radio (when I was, say, ten to fourteen), he was able to help me and sustained my interest. He also had a very good knowledge of inorganic chemistry, and he had a whole set of volumes, ten of them, on inorganic chemistry that I used to browse through, mainly looking at formulas for explosives when I was a kid. So he was, in retrospect, a very important influence in getting me interested in science at a very early stage.

On my mother's side, her father had a small foundry in Fitzroy, so he was a very practical man and good with his hands, and he had a small business with which he kept himself (and one of his daughters, who kept house for him) in a modest standard of living.

AM: So your interest was shaped very early in the hard sciences, in a sense, in chemistry and technological applications rather than kicking around among the rocks and natural history lines?

TR: Oh, that came very much later. Yes, you're quite right. I was very interested in radio and chemistry. They were the two things that I was

interested in, until at the age of about fourteen I suppose, when I went to boarding school.

AM: So first of all you went to a public school and then you got a scholarship to Geelong Grammar School.

TR: I went to Hawthorn Central School. So, as I say, I grew up during the Depression. My father was out of work for most of the Depression and away from home for a good part of that time. My mother had book-keeping skills, and so she basically kept the family going, she had clerical jobs for part of the Depression. And at other times she had a whole range of assistance, they had a closely-knit family, and so I was boarded out with them for quite substantial periods. My recollections of childhood, even though that was a bad time in retrospect, I don't think I ever felt it, I enjoyed life very much. I found myself quite good at school work and I was usually, whilst I was at the State school, dux of the class and all that kind of thing, and began to enjoy things like cricket and football as well. So my memories of that period are actually very very good, although when I look back on the economic circumstances, they were pretty difficult.

AM: Your mother was obviously one of those strong women of which Australia I think is rather famous.

TR: Well, it was she that had the ambition for me to get a tertiary education all along. She always wanted to see me attain a good education.

AM: What about Geelong Grammar School? We always hear about the famous people who have come out of it in history and the humanities, in law, public administration. One hears much less of key figures who have been educated there in science. Was it a school that was good at science?

TR: That's curious; because, I was encouraged to go for a scholarship there in 1943, and I got it, so I started at Geelong Grammar in 1944 when I was about thirteen; and I would have said that the quality of science teaching was just excellent, absolutely excellent. I remember those teachers very well and they were rigorous and they really had high standards.

AM: You specialised in certain subjects and got a scholarship to university?

TR: There was a substantial amount of specialisation, I was taking the science/maths courses as I was interested in that strand. I don't think I had any clear idea at Geelong Grammar where I wanted to go at that time.

I ought to go back to the times at Hawthorn West. I started to get interested in chemistry and radio. And I had a friend who was convinced that metallurgy was an area to go into. So I was starting to get interested in materials science, I guess. And then at Geelong Grammar there was no formal geology teaching, and I'm not quite sure how I got interested in it, I think more or less by accident, but in the end (by the age of about fifteen or sixteen) I looked at what the courses were at Melbourne High School in geology and got the books, and started to work through them myself, and because I was a year ahead, I got through Matric early and the last year was very light, it was just a repeat. So I think at that stage of my career I had no idea or intent of being a professional scientist. I became interested in geology and earth sciences as a means of two things: one, when I was somewhere around between fourteen and sixteen I was still idealistic and thought that Australia with its natural resources could be made a better place by the utilisation of these things, and so I read Ion Idriess's books and all those kinds of nationalistic books very intensely (I read very widely in the school library); and then I think that by the time that I'd got ready to leave I'd got more self-centred and I saw earth sciences as a way of getting rich.

And so I took geology as my major at university. And I actually took university work very lightly. I just did enough to get through, and enough to keep my scholarship at Trinity by working hard at geology.

AM: Well, you got First Class Honours, I see.

TR: In geology, yes. But at that stage my intent then was to become a very successful exploration geologist and find new resources and become rich myself, and I also had ideals of doing some good things with all that money I was going to earn. And so my early undergraduate course was really spent with the objective of becoming an economic geologist.

AM: Who were the good teachers at that point?

TR: Edwin Hills was Professor of Geology at Melbourne and he was an excellent lecturer. Particularly he got the first year kids very enthusiastic about geology, and in fact we saw most of him in first year. Another one in palaeontology was a good lecturer and a very able scientist, but I never had any intention of going in that direction. A man called Arthur Gascoyne, who became a very close friend,—at that stage geochemistry was a very new subject—and he had got interested in that himself and he gave the lectures there, and that got me interested in geochemistry. Later on, I made a complete career switch, I dropped the economic geology and the idea of becoming very rich by discovering new Broken Hills, and got into fundamental research.

AM: You did a Master's degree at the University of Melbourne and then subsequently a PhD degree. Of course, they were quite early days for the PhD in Australia, weren't they?

TR: Yes, they were. First of all, you had to do a Master's degree in those days, and that was done in basically a field mapping and petrology field, at least mapping Eastern Gippsland which was very enjoyable, so I mapped the area around the Snowy River. And there my economic geology became fairly practical, because I found a deposit of lead and I used to mine this lead whilst I was doing the thesis, and I sold it. I used to sell it in Melbourne. The guy who had the shot tower—in fact it has now been enclosed I believe in that big structure, the Japanese-built shopping centre [the Diamaru Complex]—he used to make lead shot. And he used to buy my galena, lead sulphide, at an excellent price. And I made a lot of money (for a student!) something like £250 a week. I used to have to take a packhorse. It was very rough, rugged country, very isolated, north of Orbost towards the New South Wales border. It's what is now just south of the Kosciusko National Park. There used to be one very difficult road in there, and very few people lived there. I mapped all that area as part of my Master's thesis. And I used to spend half the time mapping and half the time carting lead and selling it. So that kept my finance going, for my Master's thesis, quite nicely.

AM: What did you study for your PhD degree?

TR: This is where I made the main change. Initially I started off to do a PhD in economic geology—research on how ore deposits, mineral deposits, were made. And first of all the equipment took a long time as it was going to be an experimental study, and then, after designing the equipment, getting it into the workshops there, and everything took a long long time to get going. Then I started to read other things. And under the influence of Arthur Gascoyne, I started to read more about fundamental geochemistry. And that was probably the most decisive change that I made in my life, because I got interested then in crystal chemistry and inorganic chemistry, coming back to the role of chemistry in the earth sciences, particularly how we could use it to understand the Earth's deep interior. And I kind of just got seduced by that set of interests; and, after about a year on my PhD, I changed it over, and made my thesis one really on the geochemistry of the Earth's mantle using some quite new concepts, crystal chemical concepts based on the work of V.M. Goldschmidt,⁴⁹ to elucidate the structure of this region, which was almost unknown at that stage—what the structure of the Earth's mantle was. It was just, you know, a great unknown territory. So I then spent another two years over finishing my PhD in that area, and that set my course.

AM: It was really very pioneering, wasn't it, the work you were doing? As you say, even geochemistry was in its infancy.

TR: At that stage, the deep interior of the Earth was totally inaccessible and nobody really knew what it was. It was speculated about what it was, but it was not really considered a legitimate or an accessible scientific territory. And, based on some of Goldschmidt's concepts of crystal chemistry, I exploited those and developed them and was able to show that the crystal chemistry of materials, in which germanium replaced silicon in the structures, caused phases to be accessible at relatively low pressures in the laboratory.⁵⁰ And I then used those insights to get an understanding of how very high pressures would act on the silicate material of which the Earth's mantle was made. And, in particular, that work led to the first thermodynamic estimate of the pressure at which the main mineral of the upper mantle would transform to a denser structure—the spinel structure—which was purely hypothetical and speculative at that stage. It had been speculated on by a couple of people in the thirties, but was kind of just on the edge. And a lot of my work was aimed at understanding the properties, using germanate and the phase relationships between germanates and silicates to understand whether the main mineral of the upper mantle would transform to a dense state at high pressures and that this would then explain the seismic properties of the Earth's mantle. So that was the main part of my PhD thesis.

AM: How in those rather early days did you get the materials for this mineralogical study of the upper mantle?

TR: There are rare parts of the Earth where volcanoes bring up samples. Volcanic matter has come up from depths of about a hundred kilometres or so and dragged up samples of the uppermost mantle. So that's one of the reasons that we believed that olivine was the main mineral, Mg_2SiO_4 , mineral of the upper mantle. The question is, what happens to this at much higher pressures and the regime going down to the core at twenty-nine hundred kilometres. This was all totally unknown, and at that stage, there were no techniques to study directly the behaviour of materials both at high pressures and high temperatures. So what I was really doing was developing indirect analogue methods and thermodynamic methods based on crystal chemistry and phase relationships between germanates and silicates, basically to give quantitative information on what should happen at much higher pressures. And that led to certain predictions which turned out were verified, we verified here [at the ANU] about ten years later.

AM: Your thesis was obviously a very important one, and took you then to Harvard for a year.

TR: At Harvard there was a man called Francis Birch who had written a very influential paper which impressed me very deeply when I was doing my PhD. Birch had used physical arguments, the arguments of physics, to suggest that phase transformations changes and structures of minerals at high pressure might occur deep in the Earth's mantle; but he didn't have any way of testing those—basically predictions, based on elastic properties of materials and of the Earth's mantle.

So he was very encouraging, and I would say that in terms of scientists who have been influential in my career, he was by far the most significant. He was a very gracious and kind New Englander, and an excellent experimenter, and he encouraged you to do what you wanted but was always ready with advice and help, and was of course a first-rate scientist. So Birch was certainly the most influential professional scientific input that I had in my career, and I had fifteen months in his laboratory which was great.

Birch was one of the pioneers in actually developing high pressure equipment. So, whereas in Melbourne I hadn't had any high pressure equipment and I'd been using crystal chemical relations between germanates and silicates and thermodynamics to study the Earth's material, in Birch's lab I had the chance to test some of these predictions experimentally. And one of the very important ones I found quite soon after arriving at his laboratory, was again the transition of olivine to a spinel structure, in the iron-rich mineral of the olivine series.

AM: What exactly is a spinel structure?

TR: Well, it's the lattice, the crystalline lattice, the arrangement of atoms in the lattice and in olivine the oxygen atoms are arranged in a particular symmetry, in a particular pattern, in which the symmetry is largely hexagonal. Now, because high pressure tends to make atoms take up more closely-packed structures—denser structures—the oxygen atoms can rearrange themselves into a cubic structure in which they're more closely spaced, and can take a cubic structure which the spinel structure happens to have. So it's basically just the arrangement of the atoms in the crystal that governs the density, and basically the higher the pressure, the greater is the tendency for minerals to adapt denser and more closely-packed structures. Once you get a change in crystal structure, this will then affect the velocities of seismic waves as they pass through. So seismologists can then find evidence, from velocities of seismic waves, that something funny is happening at a particular depth in the Earth's mantle. And here we're using both the indirect and the direct methods to show that given change in seismic

properties in the Earth's mantle has been caused by a change of crystal structure, the effect of high pressure acting on the upper mantle minerals to transform them into more dense structures.

AM: So you at that stage decided to come and make your career at the Australian National University and this I think was about 1959. Did it offer you the opportunities for building these devices and the high pressure apparatus that you would need to carry on your research? Or why did you choose to come to the ANU?

TR: A great accident. I was up at the International Union of Geologists and Geophysics Conference in Toronto around June or July of 1957, and I met John Jaeger.⁵¹ And we got talking, and he learnt of my interests and that I was working with Birch whom he already knew. He was expanding the Department of Geophysics at that stage, and the next thing was that I had an offer from Jaeger for a job in Canberra. I'd always intended to come back to Australia and there weren't too many opportunities in fundamental earth science at that stage, and I was interested in research and not in teaching, so I was immediately attracted to the offer. Jaeger offered me a Senior Fellowship, but then he couldn't get it through the Physics Faculty Board, so it was re-offered as a Senior Research Fellow, which didn't bother me at all.

AM: Having stayed at the ANU, you moved very rapidly up the ladder, to Professor.

TR: Yes. I mentioned that Birch was a very great influence on me. That was more directly in the area of science that I worked in. But likewise John Jaeger was a very impressive person to work for, sometimes quite difficult and cantankerous but also with great vision. I mostly had a good relationship with him, sometimes we had some spats, but basically we respected each other, and I eventually became his second-in-command in that school, in that department. And so it was really a very good time to be in Canberra. It was small, but I liked the environment very much, and I have never had any inclination to leave the ANU ever since. I've had plenty of offers, but I've never really bothered to take them seriously.

AM: Over that time I imagine many students have passed through your hands.

TR: I've always had, in my field, great difficulty getting students, because I'm working in fields where there is very little teaching, undergraduate teaching. Most of the undergraduate teaching in earth sciences deals with the crust (and more recently the upper hundred kilometres of the mantle),

and there is very little in the earth sciences syllabus that gets students interested in the very deep interior of the Earth, the deep mantle, the core, and then other areas like the relationship between the Earth and the Moon, and the Earth and planets. So that's been my main areas of research. I've had a few good ones. But not as many as I would have liked.

Later on it became clear that backgrounds in these areas would have been important for anybody wanting to do something in materials science. But that wasn't clear at that time. So I've really worked more with colleagues, with post-doctoral Fellows and with Research Fellows, than with PhD students. Alan Major, for instance, joined in 1965 and he's been here ever since and we've had a very close relationship on experimental geoscience approaches.

AM: It was with him that you developed this very high-pressure apparatus, was it?

TR: Yes. I was always quite good at designing experimental apparatus, at analysing its performance and improving the systems. But I was not a good experimenter, being much too impatient. Alan is very skilled with his hands and patient, and proved to be a much better experimenter than me. So, on the experimental side, I designed the apparatus, and Alan was responsible for the very delicate task of assembling the pressure cell and carrying out the experiments, and we've continued to work in this mode to the present day.

AM: Could we just for clarification, get to the bottom (if I may put it that way) of the crust, the upper mantle, the lower mantle and the core. What sort of area are we traversing with these descriptions?

TR: The crust is the upper forty kilometres, which is really the province of geologists, and most geoscience has to do with the crust. And at the time when I started science there was very little work going on on the mantle from a petrological or geochemical aspect. The geophysicists had the mantle to themselves because they could measure velocities of seismic waves as a function of depth and so get the variation of density with depth or the variation of elastic properties such as seismic wave velocities with depth. So that told us a bit about the physics of the interior. And likewise the seismologists had the core to themselves. So the interior of the Earth was largely in the hands of geophysicists, and I guess my main contribution was to take the interior of the Earth into the hands of the geochemists. And I suppose I would be seen as a pioneer in that whole area of understanding the chemistry of the Earth's deep interior. That's the region below the crust.

AM: At what depth does the core begin?

TR: Well, at the depth of twenty-nine hundred kilometres in the Earth's mantle there is a major change of seismic velocities and density, and that's where you enter the Earth's core, which is mainly made of an iron alloy but with some lighter elements in solution. And the core is where the Earth's magnetic field is generated.

AM: The transformation of minerals, that you referred to briefly earlier, this becomes the key to your geochemical studies?

TR: Well, there are a number of strands, but that was certainly an early major work. Without doubt, that's the most important research that I was doing in the first fifteen years or so, or twenty years. But that was one area. The other area that proved to be very successful was done in collaboration with David Green (who became a Research Fellow here [at ANU] and worked with me) at somewhat lower pressures but in the upper part of the Earth's mantle. And we had some very successful collaborative work on the origin of basaltic magmas and what we can call the petrology of the upper mantle, the upper couple of hundred kilometres or so. And he stayed here until the mid-1970s and then became Professor of Geology at the University of Tasmania.⁵²

AM: All these major breakthroughs in this field, I presume, are included in your general book *Composition and Petrology of the Earth's Mantle*?⁵³

TR: That's a very good overview, a very complete overview of my research on the Earth, up until 1975. But I of course also had interests in the Solar system and the Earth and the Moon, and that's not really covered in that book at all, nor is there anything on the Earth's core. I hadn't done much on the Earth's core at that stage, I became more interested in the Earth's core after 1975.

AM: What about the dynamics of the mantle?

TR: As a result, very substantially I suppose, of my work, we obtained a fairly detailed understanding of the mineralogical structure of the mantle as a function of depth and the roles of phase transformations. And it's an understanding of those roles which, later on, have been seen—and are being seen even right up to date, it's a very exciting area—to be very important in regulating the nature of convection processes and flow in the Earth's mantle and basically regulating the nature of mantle dynamics. It's also recognised that they probably cause deep focus earthquakes. So I bear a significant responsibility for demonstrating the important role of phase transitions in the

Earth. Birch predicted them on the basis of the elastic properties. Basically, I and my colleagues here (all at ANU) discovered virtually all of the important phase transitions which occur in the mantle, and were found here at the ANU in the sixties.

AM: A very formidable achievement, isn't it?

TR: It will be seen as such. There are three very major transitions that occur around depths of four hundred kilometres involving different mineral phases and which are now well established. And one has been called Major-ite (it's a very important one), one is called Wadsley-ite (after Dave Wadsley at CSIRO who worked with me on the crystal structure of that particular phase which we found here), and the other is Ringwood-ite (which is the spinel form, olivine). So that was all worked out at ANU.

AM: It's really quite something. It's like naming the areas of the Moon, which have usually been given to famous figures. It's very nice to know you're tucked away down there in the mantle.

TR: And in the second set of major phase transitions occurring around 650 kilometres, I predicted that there would be a transition to a structure called the perovskite structure at the lower mantle, and Major and I found the first perovskite in calcium silicate in the late sixties and early seventies. And John Liu, whom I hired as a Research Fellow, using the new diamond anvil techniques found the predicted transition in magnesium silicate perovskite. That was probably the most important earth science discovery of the decade, I think. So at ANU we actually did it all in the sixties and early seventies.

AM: It must have put the ANU on the map in a significant way.

TR: Well, it did. And there was a lot of other excellent work going on. So, yes, the earth sciences at the ANU became recognised as, I suppose, one of the four leading institutes in the world at that stage, and it still has a good reputation.

AM: And many honours were beginning to be heaped upon you at that stage. You then go on to a study of the Earth's core—but of course all this is happening in a circular way, isn't it, all your findings?

TR: Well, I usually spend two or three years working on one thing, taking it as far as you seem be able to take it at that stage, and then move off and study something else. That's been my pattern most of my life. Certainly between

1956 and 1966 phase transitions were a main area; but in '63 I started collaborating with David Green and we started working on another, the origin of magmas at high pressure. And then, in parallel, I would also take time off, even from '59 onwards, to work on planets and the Moon.

AM: And do you come back and circle round?

TR: Oh yes, basically you follow a line of enquiry and you mine that out, and then you move on to something else totally different for a while. There may be some developments overseas, or you see some new potential ways to attack that previous problem and you come back to it. And that's really been the way that I've tended to go about my science.

AM: And therefore the work on the Earth's core, in which you were looking at the principal light elements of the core, when was that work occurring?

TR: I had an early interest in that. Again, that's something that I've come back to repeatedly since 1959 but I really hit that very hard from 1977, and come back to that repeatedly as new experimental techniques [evolve]. That was very largely a question of solving new experimental problems and developing new experimental techniques to attack it. It was quite a difficult enterprise, and we just approached it very gently and gradually. And now I think there's been a large degree of confirmation, by other people now, of the work that was done then.



This interview was never completed. Professor Ted Ringwood died on 12 November 1993 in the midst of his far-ranging research and planning. He was sixty-three.

The following excerpt, touching his innovative research style, is taken from his speech of acceptance of the V.M. Goldschmidt Medal of the Geochemical Society awarded him in 1991 in recognition of major achievement in geochemistry and cosmochemistry.

I have to concede that [the] comparison with the British astronomer—'often in error but never in doubt'—[is] aptly directed. It really is so easy to be seduced by one's own hypotheses. However, if there's one lesson I've learned the hard way, it's that serious criticisms should always be faced. Usually, at first you believe your responses have been successful. The model survives! But later, it often happens that you notice one or two nagging problems

which refuse to disappear. When that stage is reached there is only one thing to do—change the model or throw it out. Otherwise, everybody's time is wasted. So I've never been embarrassed about changing my mind on scientific issues and being quite explicit about it. Indeed, this has happened on many occasions ...

There is, of course, another side to this coin. Sometimes, on reflection, you remain convinced that the criticisms can be answered. You must then stand your ground. My early work on mantle phase transitions received a torrid time from referees. Yet today, it's quite respectable. Our recent work on oxygen as the principal light element in the core and its implications for other global geochemical processes also encountered heavy weather, initially. Now I sense that the climate may be changing. It will be interesting to see how the current controversy on the source of protolunar material and the origin of the Moon turns out. It would be nice if that could be resolved in the next few years ...

Our understanding of the Earth in all her aspects has improved dramatically over the last twenty-five years. In my own field, we now possess the capacity to investigate the chemical and physical properties of materials which may occur throughout the upper and lower mantle and in the Earth's core under controlled conditions in the laboratory. These advances have provided a greatly improved understanding of the composition and constitution of the Earth's interior. They are now setting the stage for a fundamental enquiry into the dynamical behaviour of the mantle and into the nature of the engine that drives the system and ultimately, I hope, into the origin of the Earth itself.

This has been an exhilarating period to have been an earth scientist. I feel very fortunate and fulfilled to have been able to participate in some of these developments.



Professor Ralph Slatyer,

AC, FRS, FAA, FTS

Interviewed by Ann Moyal
Canberra, December 1992

National Library of Australia
Tape no. TRC-2891

RS: I was born in Melbourne and grew up in Perth. I was the fourth of five children. We were spread out over fifteen to twenty years so I think I had a good deal of time from my mother and, to a lesser extent, from my father. My dad was a banker, and in those days bankers used to get transferred around the countryside every two or three years, so they were transferred to Perth when I was about one I think. My mother was extremely influential on me. She had a love of learning and inculcated that into the family. Not that my dad didn't have a love of learning, but I think the main thing he contributed to us was that he enjoyed his work so much and that was an important influence also.

My mother inculcated a love of learning particularly of the natural world. She had a special way of arousing one's curiosity, you know, 'Why do you think there are so many colours in flowers?' or 'Why do you think this rock is grey and this one is blue?' or whatever it might have been. And she had enough background from her own reading that, whatever response we as children might have made, she was usually able to provide an informed response. And it was very much a continuing process, that sort of love of learning.

AM: Agricultural science, which you did your first degree in, and then your Masters and then your PhD—what was the impulse towards that study?

RS: Well, both my parents had come from a somewhat rural background. I say 'somewhat' in the sense that my mother's father was also a banker; my dad's father was the manager of a cattle station in Queensland. So we always had a strong feeling for the countryside and the bush. They were basically Queenslanders. I guess one would say now that my mother would have been very much involved with ecologically sustainable development. That was really what it was all about, farming was seen very much as a continuing process from generation to generation.

I'd finished school in 1946, we did five years of secondary school then, and I went to university while I was still seventeen, the year I turned eighteen. I suppose I was very impressed with what I thought was an enormous global problem—that is, to feed the world—and agricultural science was very appealing to me from that point of view. During my course we had to spend summer vacations on field work, either on farms or on research stations and I spent two of my vacations at CSIRO field stations in northern Australia. The first, at the Kimberley Research Station [WA], which was a fledgling station right there in the bush, that was in the '48-'49 vacation; and the second at Katherine [NT], the '49-'50 vacation—and that was tremendously exciting to a young, budding agricultural scientist. And also to realise that it wasn't just a matter of doing things that might help to feed the world, but indeed to discover whether sustainable agriculture was

possible in those environments. That added another dimension to the whole idea of utilising northern Australia.

In those days it was just assumed there was an enormous agricultural potential in northern Australia. People would say, 'You have to just go up there in the wet season and you see the grass grows two metres high', something like that. And of course we very quickly found it might grow two metres high but it was really just straw. Indeed, animals grazing it lost weight; it lacked nutritive status to such a degree. But it was that question, 'Can sustainable agricultural systems, or pastoral systems, be developed in northern Australia?'—that underlay the work of those research stations.

I didn't do a PhD, I did a Masters degree and then got a DSc. Perhaps I can just digress a moment and say that one of my colleagues of long standing, John Philip, had a somewhat similar background to mine in the sense that he did not do a PhD after an engineering degree. Both of us were elected to the Academy of Science rather earlier than our peers and we often posed the question, in talking to one another, as to whether our careers actually benefited through *not* doing PhDs. The situation in the early 1950s was that graduate programs were just getting established in Australia and it was quite unusual for people to do PhDs. Also, CSIRO was hiring people without PhDs as research scientists who they thought had research potential. So that is an explanation of why I didn't do a PhD.

AM: So from that start in the field stations of CSIRO, you joined CSIRO in 1951, and became Associate Chief of the Division of Land Research in 1966.

RS: That was a marvellous period of my life. I found the CSIRO environment tremendously stimulating, as was my boss Chris Christian, who had a flair for putting together teams of scientists, interdisciplinary teams, and challenging them with an objective, supporting them well and letting them get on with it. For a person who had enough self-confidence to drive ahead with that sort of encouragement, it was a really marvellous environment.

After the Second World War Chris Christian had, in fact, prior to appointing people like me, begun what were called the North Australia Regional Surveys. Again, the background to that was this assumed potential of northern Australia for much more intensive agricultural and pastoral activities. He devised a rapid survey method so that, in the space of a few years, the survey teams in Christian's group were able to survey much of northern Australia. It was a sort of reconnaissance survey based on aerial photographs and, instead of mapping soils or vegetation communities or other specific features of the environment, they mapped the whole landscape. It was essentially a geomorphologically-based survey technique.

That was the basic framework on which the work of the agricultural research stations was based.

I was actually appointed to be a climatologist, and the challenge I was given was to develop a methodology for estimating the length of the growing season in northern Australia from climatic and soil data with the idea of predicting what crops might be grown where. The idea was that I would devise this methodology, the people in the research stations would validate it from what actually happened with respect to particular crops and other experiments, and we'd put those two things together.

I was sent over to sit at the feet of James Prescott in Adelaide at the Waite Institute and learn what he could teach me about different sorts of climatological assessments, something that Prescott had largely pioneered in Australia together with Hugh Trumble.

So I got started on this work, initially working with the survey teams to do climatological studies of the areas being surveyed, but then the more challenging task was to try for any one site (and of course Katherine Research Station was an excellent site) to see to what degree one could combine climate, soils and plant responses, to produce predictions of growing-season attributes that agreed with field experience. After a couple of years Christian appointed more people to work with me and I was able to build up a whole group in what then became the Division of Land Research in CSIRO. We gradually built these models, probably the first real models of growing-season attributes, using the very simple and unsophisticated computers that existed in those days.

AM: The community doesn't always know what CSIRO has really achieved in any detailed way, and this is an example of it. So was the methodology then adopted by countries overseas?

RS: Yes, it was. In fact a whole lot of that land research methodology was adopted. UNESCO took a very active role in spreading that methodology world wide; and indeed that started one of my first connections with UNESCO that developed later in my career. The climatological models were subsequently developed much further by Henry Nix and Eugene Fitzpatrick and have also been used extensively internationally.

What came out of that work was a much heightened awareness of the unpredictability of the growing season in northern Australia, particularly at the start. That was the first thing that came out very strongly. And the second was the variability within the season. So those two factors together meant that rain-fed agriculture was a very precarious proposition in northern Australia. The other important factor that emerged was that most of the soils suited for agriculture by reason of their topography and their

general arability had very poor water-holding capability. So that, on both counts, both the unpredictability of the climate and the poor water storage capacity of the soils, when you put those two things together there was essentially no future for rain-fed agriculture.

AM: This was a very practical outcome from this research. But I'd be interested in your comments on the general culture of CSIRO at that time in the sense that it was still very much as David Rivett had set it up to get good men, and then get the money to the good men and give them their head. And was this true while you were Associate Chief of Division up to 1967?

RS: Well, I think the main feature to me was that each CSIRO division had a mission, and within that division each program had a mission. It was a very strongly mission-oriented organisation at that time. So all of us knew what the division was on about, what was the area in which we were expected to work, and made a real attempt to resolve the problems. But once that mission was taken on board, how we went about it was completely our affair. And, of course, that was again why it was a marvellous research environment.

In my own case for example, the work moved from these broad climatological surveys and the general area of environmental physics, into plant physiology where I then spent most of my research career while I was still actively involved in science. I moved later more into ecology. But I suppose, to the extent that I developed a reputation, it was in the area of plant physiology that was perhaps most prominent; and yet when my work started it was climatology I was working in. That was an example of how much flexibility you were given in CSIRO, at least in Chris Christian's division.

AM: And then you leave and go to the Australian National University in 1967, and till 1978 you're Professor of Biology and Head of the Department of Environmental Biology in the Research School of Biological Sciences. So what is the thrust of your work then?

RS: Christian sent me to Duke University on what was essentially a post-doc. I went there to sit at the feet of Paul Kramer, one of the doyens of American plant physiologists and, again, a marvellous person. He was also a very important influence on me.

Both these people, I might say, were also extremely good at fostering young scientists and at keeping an eye out for opportunities for them in making sure that if there was some particular award, or medal, or prize or something, that they had an opportunity to be in the competition. It wasn't patronage in the sense that those people had to earn those awards on their

own merits, but at least they were made aware that there was a competition, which a lot of young people wouldn't have known about otherwise. They were very generous, in other words, in their science. And I must say I've tried to do the same thing, given the experience that I had myself.

So I went to sit at Paul Kramer's feet, for only about six months. That period gave me confidence that I was right about a number of features of plant-soil-water interactions, ranging from the control of internal plant water status by atmospheric and soil conditions, to the availability of soil water for plant growth, and to the effect of plant water deficits on photosynthesis, plant growth and biological productivity. That work was fascinating in itself and also many other things followed from it, in agronomy and irrigation practice and things like that. So it was really quite fundamental to a lot of rural practice you might say. I then developed that work fairly strongly through the late 1950s and right through the 1960s. In the process I was able to build a group of about ten scientists in CSIRO. Most of the people there, I'm pleased to say, went on to Chairs and various other senior appointments in CSIRO and Australian and foreign universities.

Then the ANU started their Research School of Biological Sciences at the end of 1966 [and] they wanted to establish one of the Chairs in Environmental Biology, actually Environmental and Population Biology, and I was offered the job. Although I was reluctant in many respects to leave CSIRO, in a sense the timing was perfect for me, because I'd developed profound interests in eco-system dynamics, which were easier to pursue at the ANU. I had all sorts of ideas about how I could use relatively simply structured ecological communities (like arid communities and those at the alpine tree-line for example) to test some of my thoughts on how ecological systems were structured and how they functioned, particularly how they responded to perturbations like a fire, or grazing, or something like that.

Several of my colleagues from that CSIRO group came over with me to the ANU. Not that we 'mined' that resource; both teams prospered. But Ian Cowan came over with me. Barry Osmond (who is now Director of the School), I'd hired as a post-doc or a Research Fellow to the CSIRO group. He came too and, together [we] built an exciting research group at the ANU in Environmental Biology. First we concentrated on photosynthesis and water relations and that work has continued to the present day, bringing credit to all those involved and establishing the group as a leading centre for the study of plant-environment relations. As that work became established and for the rest of the period when I was an active researcher (which was really until about 1978, when I went to UNESCO) I worked very much in the ecological area.

One of the most stimulating subjects was to work on ecological succession, the general response of ecological systems to perturbation. My main colleague in that area of work was Ian Noble who had joined the group

from the University of Adelaide, another was Joe Connell from the University of California at Santa Barbara. We worked together on trying to simplify, but not over-simplify, the mechanisms involved in ecological succession. You can appreciate that, given the complexity of any ecological system, its response to perturbation is difficult to comprehend. If you look at a particular area you'll see the result of a whole lot of interactions over a period of decades or even hundreds of years; if you look at another site there'll be another set of interactions because, let's say, one of them may have missed a particular fire, another may have been grazed. In other respects they would have been identical, but there were very powerful differences.

Anyway, Joe Connell and I decided that in fact, rather than a myriad of mechanisms operating, there were just three main mechanisms which could explain most of what one observed in nature. And Ian and I looked in more detail at the functional attributes of organisms which enabled them to persist through a disturbance or to arrive at a site after a disturbance. That work was great fun and attracted a good deal of attention.

If I can make an aside, one of the things that has been so enjoyable about my scientific career has been the stimulation, the pleasure, the friendships from working with other people. I feel sorry for scientists who prefer to beaver away on their own because, for me anyway, it really enriched my whole life in all sorts of ways to work in a cooperative way with other people.

AM: In 1978 you're appointed Australian Ambassador to UNESCO in Paris, which was the first appointment of a scientist to such a role, and it was obviously a very important and seminal time in Australia's environmental relations. Could you tell us something of that experience?

RS: When I was first approached about that job, I knocked it back, straight off—because my attitude was I'd never really conceived of doing anything like that before and didn't conceive of it then. It just seemed almost inimical to most of the things I wanted to do! So I went home for lunch that day and told my wife that I'd just knocked back the job as Ambassador to UNESCO. She was sort of horrified, I guess you might say! She said, 'Well, if they offer it to you again, let's just think about it, shall we?' A week later they did ring back and said would I like to reconsider it, which I did, and we went.

It was a fascinating experience. Through the 1970s, when we had this tremendous intellectual ferment of the Whitlam years, there was a sense in Australia of a much greater feeling of national self-confidence. I'd become very conscious by the beginning of the seventies that science had an important role to play in Australia's future. But it was just one element in Australia's future, and there were many other factors that I thought someone like me should try and explore in some way to make a contribution to Australia.

So I started doing other things in the 1970s, on the Australian Research Grants Committee and on the UNESCO National Commission. In fact I'd first become involved with UNESCO in 1956 when there was a [UNESCO] climatological symposium in Australia. In the early 1970s I joined the Australian National Commission for UNESCO which provided Australian input to the UNESCO programs, and in about '76 I was appointed Chairman by the then Minister for Education. Afterwards, looking back, it seemed quite logical given the length of connection I'd had with UNESCO and that I'd been involved in their educational programs, their cultural programs as well as their scientific programs that I should have been appointed Ambassador. Nevertheless it was something of a surprise. But having developed that rather broader view of how I thought I could make a contribution to Australia, then the ambassadorship was a marvellous vehicle for that.

AM: You moved very rapidly into a very striking role there, with the 'Man and the Biosphere', and you were Chairman of its Council. What was the major work there?

RS: UNESCO started its 'Man and the Biosphere' program, which was really an ecologically sustainable development program, in 1970. I'd been quite active as an Australian delegate to the early meetings of the 'Man and the Biosphere' program; together with John Philip who also played a very important role in that. We—Australia—were quite influential in determining the character of that program. I became Chairman of what was called The International Co-ordinating Council for the 'Man and the Biosphere' program in 1977, so I was already Chairman of that Council when I was appointed Ambassador. But being there in Paris meant one could be much more effective as Chairman.

I worked very closely with the secretariat for the 'MAB' program (as it was called), and indeed, while I was Ambassador, tried to build up Australian content in everything that UNESCO was doing, in the science programs in particular, but also educational and cultural programs. In the science programs over that period we succeeded in having an Australian as Chairman of three of the four major inter-governmental programs. Not at the same time, but over a period of four or five years an Australian chaired the programs in 'Man and the Biosphere', the International Geological Correlation Program, and the International Oceanographic Commission.

At the same time the World Heritage Committee was just getting under way, and had its first meeting in '78. I became very actively involved in that program, serving continuously on the Executive Committee for five or six years and as Chairman for three terms. Again, Australia had a very influential role in the character of the World Heritage Convention, in the operational

guidelines for the program and the rules of procedure for the Committee. It's important to get those things straight of course, right at the start.

AM: I see that the Great Barrier Reef was the first Australian listing and Kakadu National Park, in three stages at different dates, and the wonderful Daintree rainforest and Fraser Island, the tenth. But in your role as Chairman of the World Heritage Committee, did you encounter the politics of World Heritage listing in terms of Australian governments being very supportive of it and some States not being supportive?

RS: Very much so. Each of those nominations, where there has been a contest between the Federal and State governments, has been very difficult. And at the World Heritage Committee level, other countries just couldn't understand why there was this degree of confrontation, because most countries want to get their properties on the World Heritage List. The idea that there's disputation about it is really quite strange to the Committee in general. It would only occur I think in countries with federal systems. But, to the best of my knowledge it has not occurred in other such countries.

At most of those contested nominations, the State governments, and sometimes other bodies like industry groups have actually turned up at the World Heritage Committee meeting seeking to intervene and make their case as though the World Heritage Committee was some sort of a jury! That caused enormous problems. The World Heritage Committee is not a jury, it has well laid down rules of procedure, and can only deal with State parties to the Convention—and the State party for Australia is the Commonwealth government.

AM: In general, during your time on the Committee, the Australian nominations to the Committee were fairly straightforward, were they?

RS: The first few were all agreed nominations between the particular State and the Commonwealth government—that is, the Great Barrier Reef, Willandra Lakes, Lord Howe Island, for example. I think the first contested one was south-west Tasmania. You'll recall the Tasmanian government wanted to build a dam which would have seriously weakened the claims of the area to be listed on the World Heritage List; and IUCN [International Union for the Conservation of Nature and Natural Resources]—the body charged with the expert evaluation of the nomination—recommended to the bureau of the World Heritage Committee that the property be listed, but added a caveat to say that, before it could make a final recommendation to the full Committee, it would want the Commonwealth government to provide assurances about the dam construction. As the final meeting of the

Committee approached, no such response had been prepared by the Federal government. At the final Cabinet meeting [in Canberra], when I was actually about to leave on a plane for Paris to the meeting of the Committee, at that very last minute they finally decided that they would provide a document to the World Heritage Committee. But it was a fairly evasive document [*laughing*], I would have to say. It fell short of saying there would not be a dam, but provided some assurance to the World Heritage Committee.

The fact of the matter was that Mr Fraser was ill at the time. And he was of course a very dominating figure in his Cabinet, and when he wasn't there, despite the fact he could talk to the acting Prime Minister or the Deputy Prime Minister and his Cabinet colleagues on the phone, he was rather less persuasive than when he'd actually been in the Cabinet Room. So that's another reason why the response was rather less fulsome than it might otherwise have been.

But, anyway, that was how that nomination was proceeded with. The Tasmanian government sent a delegation to oppose the nomination in Paris. As I said earlier, they didn't get standing, but nevertheless they were there and they got some pretty good media coverage. In the event, the World Heritage Committee listed the property, and that required quite a bit of difficult presentation in front of the Committee. And then, of course, several months afterwards, the Fraser government fell, the Hawke government came in, and the Tasmanian government challenged the validity of the Commonwealth legislation which gave the Commonwealth the authority to put the property on the list. The High Court upheld the Hawke government's decision. But it was a long story and very difficult.

AM: Another field which you were closely identified with in your role at UNESCO was with SCOPE, the international Scientific Committee on Problems with the Environment, and you became President of that [1982–85] after, I think, you left Paris, and you're still a member of the Executive Committee.

RS: Yes, SCOPE is a non-governmental body. It's one of the special programs of the International Council of Scientific Unions (ICSU). SCOPE, I think, has been an extremely influential body in global environmental issues.

While I was President SCOPE took on the task of looking at the environmental consequences of nuclear war, a highly controversial, highly political subject and one which such a body would normally shy away from. But there was a great deal of confusion in the mind of the public as to whether 'nuclear winter' was anything more than a doomsday sort of prediction without much scientific depth. And it was clear that no other

body was game to touch it, particularly with the East–West antagonisms that were embedded in this sort of issue.

Anyway, at one of our first meetings I had the job of deciding if and how we would address this issue. In the event, we set up a special sub-program, it was so major in scale and so significant in content. It took us about two years to produce a report with this very effective group headed by Sir Frederick Warner from the UK.

I think that the final publication⁵⁴—which concluded that there was a very real risk of nuclear winter—had the standing of being prepared by that independent working group which had tapped a large number of leading environmental scientists from around the world, including both the US and the Soviet Union. (I was told that it was the first program that had brought American and Soviet scientists together on an issue related to nuclear war.) As a result, the report held up against scrutiny from all the sorts of people who wanted to dismiss it for one reason or another. I like to think it played a significant role in easing Cold War tensions and in promoting disarmament.

AM: In working in these complicated international arrangements on the environment, do you get a strong sense of collaborative attitudes? Do you get certain conflicts and tensions?

RS: Well, at the scientific level there's been excellent cooperation, I think without exception. At the political level though, that's not necessarily the case. There are always countries who wish to defend whatever it is they're doing and for whatever reason. Indeed, a number of developing countries, which may have, say, very severe population/environment problems, would resist the notion of a MAB project that might look at, let's say, land degradation, because they would be conscious they'd come out of it in a rather bad way. And some of the larger countries would have their various geopolitical agendas to run. So, while I think MAB has been one of the most successful UNESCO programs (it's still running after almost twenty years, and stronger than ever as far as I can gather), not every country has embraced it equally effectively.

AM: You have the reputation of being a very skilled conciliator and facilitator, and as you have been chairman of these important committees, that must be a very valuable skill to have.

RS: [*laughing*] Well, I don't know if I have that skill, but I must say I've enjoyed the chairmanship role. I suppose I have a very simple approach to these sorts of matters; that at the end of the day one has to try to devise a solution that gives each person something to take away. I suppose my

strategy has just been to talk and listen, to try to understand where each of the parties is coming from, and to talk through with them their own concerns and issues so that one can gradually devise such a solution.

AM: So though you came out of a research field and, as you say, slightly reluctantly, to take this job in the international arena, how do you look back on that decision now?

RS: Well, it sort of wrecked my scientific career in the sense that it was obviously going to be very difficult to get back into real science after those three years. Indeed, when I was asked to stay on for another term, I decided I didn't want to because I wanted to get back to my scientific career. Having got back though, and having got my scientific career restarted, I found that I'd changed considerably from that experience at UNESCO; and I was inclined, more than before, to see science as just one factor—a significant factor, but just one factor—in what happens in a particular country, or globally, and therefore more inclined to try to support science, as it were, from behind, rather than be a practitioner myself.

AM: That's a very different view from that of the scientist who remains a researcher and who often has a slightly tunnelled vision. Yours has expanded right out, and in a sense perhaps chimed in with what you thought of as a young man when you began to feel there were large resonances in science.

RS: Yes, I've always had those other tendencies. Mind you, I think while I was very much involved in my scientific career, I was as involved in it (as obsessed by it) as any of my colleagues. I do think if you are going to be a successful researcher it has to be almost an obsession, it has to be something that just drives you all the time. And, you know, as people say, most of your good ideas happen under the shower or at some other time, which means you're really thinking about the scientific work you're doing all the time.

So, yes, it was in that sense a bit of a return to where, I suppose, I'd started off as a younger man.

AM: Well, you came back to the Research School for a while; but, even before you went to Paris, you had been involved in some advisory role to the Australian government with the interim ASTEC—the Australian Science and Technology Council—appointed by Prime Minister Fraser in 1975. When you came back you became the part-time Chairman of a newly-formed ASTEC in 1982 to '87. So you had early interest in a policy for science, before you went abroad to UNESCO?

RS: My interest has always ranged from an effort to try to create an environment within which good science could be done, through to promoting the role of science and technology in the economic and social life of Australia, and seeing Australia become a prosperous country which draws on science and technology in what I suppose you might call ecologically sustainable ways.

AM: The early ASTEC was part of an evolutionary process in science policy in Australia. Did you see it as a useful advisory body in those early days when it was groping for shape and purpose?

RS: Yes, I did, and very influential, in fact particularly with Mr Fraser. I think it was a creation of the time. In the 1970s there was an attitude, not just here but around the world, that government 'think tanks' (if one can use that expression) were a very effective way of providing detached high quality advice to government to supplement, to complement, what they were getting through their normal advisory mechanisms. And I think ASTEC was created in that context. Nowadays of course one wouldn't necessarily do that, but at the time that seemed to be the right way to proceed. Of course it meant that the emerging science policy role of the Academy of Science was somewhat truncated.

AM: When you became part-time Chairman of ASTEC in 1982 to '87, did you experience some tensions with the Academy of Science?

RS: No, I didn't, but I was conscious that the Academy, if it was going to fulfil an effective science policy role, had to be adequately supported for that role. By establishing ASTEC, and funding ASTEC, clearly there was no case to fund another body to do the same thing.

AM: ASTEC has drawn on a fairly strong contingent of scientific members, and under your chairmanship I think it was widened somewhat to bring in more industrial people and an economist or two. One of the criticisms of ASTEC has been that, though it has a long record of reportage, and it has brought out many reports on scientific institutions and scientific issues, they are not always very probing or incisive?

RS: I think those comments are justified to some degree. Could I say first that, if you were to ask me what had been the main science-related initiatives taken by government over the past decade say, I would have said: 150 per cent tax deduction for research and development; the setting of new directions for CSIRO along industry sector lines rather than disciplinary lines, the establishment of the Australian Research Council for university

research, and the Cooperative Research Centres Program. The first three of those were all ASTEC initiatives. So, to that extent, there are runs on the board. And they all occurred, I'm pleased to say, while I was Chairman.

Now, as to the content of that advice though, the degree to which it was insightful and well balanced, one of the realities of a part-time body like ASTEC (even though ASTEC's members are paid to be a working council), the reality is that they are very busy people and the non-academic members in particular have a great deal of trouble making the time available to serve on working groups and activities like that. So, on the one hand there is an expectation that you will receive high quality input from those people—the reality is otherwise. That means that a great deal devolves on the Chairman and the secretariat, and, depending on the amount of time the Chairman is able to put into it, it devolves more or less on the secretariat. Any good secretariat of course tries to largely develop its own agenda, and there is therefore a risk that you get a report which, while it is said to be a report of the Council, is to a degree a report of the secretariat. And I think ASTEC to some extent is vulnerable to that sort of situation developing.

AM: Another criticism of ASTEC has been that in Australia across the last two decades the discipline of science and technology policy studies has grown up quite substantially in the universities, and that it is very rare to see a member of this discipline on ASTEC or its various committees. The criticism is sometimes expressed, that here is expertise which the government does not choose to call on, and in fact there has been a tradition in Australia and in Britain to have science policy development very much in the hands of working scientists. Do you think this tradition is ideal, given the fact that, scientists sometimes have a particular vision rather than the opportunity to read widely in an intellectual field?

RS: Frankly I think you can argue it from either direction. I think you can say—and probably this would be an Academy of Science view, for example—that it's better to have a body of working scientists that collectively will generate a fair bit of science policy wisdom even though they would be coming from their own specific disciplinary areas and so on. Or you can argue that ASTEC as a body would be much better with science policy people on it. Now, there are other elements to that. One can also say that if you have a science policy person who has their own research group in a university or wherever, that in a sense they will be having two bites at the cherry if they also have a role in ASTEC, and perhaps they should be more independent and generate their advice from their own centres rather than through ASTEC.

AM: Your role has always been very influential as science and science policy were perceived to be more important. The developments of 1989 in which

Prime Minister Hawke decided to appoint a Prime Minister's Science Council and to appoint you as Australia's first Chief Scientist, was obviously a major step along the policy route. I would be very interested to hear your perceptions of this development and how in fact it has moved along since 1989.

RS: I finished my ASTEC chairmanship late in '87; there were significant funding cuts in '88 in the Federal Budget and there were widespread demonstrations by members of the scientific community later that year. One of those Mr Hawke was exposed to in November or December '88, and was greatly exercised by it. He hadn't realised that there was that degree of concern in the scientific community about funding. And he immediately set up two committees to report to him on what should be done—one of them a committee of officials to talk about the nuts and bolts, but also to identify the problems, and the other a group of Ministers under Senator Button's chairmanship to talk about new mechanisms that might be introduced to enable the government to be more aware of what was going on.

In about April 1989, as these bodies began to report, Mike Codd, who was Head of the Department of Prime Minister and Cabinet, and I discussed the possibility of my taking on a job like the Chief Scientist job. He envisaged an arrangement involving the post of Chief Scientist (that is, an adviser to the Prime Minister on science and technology), a Prime Ministerial Council, and a committee of officials, to develop links between the Council and the Committee, and to ensure that within departments there was an awareness both of matters that the science and technology community took seriously and also the opportunities to look at them broadly throughout government rather just on a portfolio-by-portfolio basis.

We talked this through, and Mr Hawke was keen that we should set up this machinery and that I should take the job. My first response was 'Well, I've been there and done that, having been Chairman of ASTEC for five years.' Mike said, 'But this job is quite different. When you're inside the Department of Prime Minister and Cabinet, it's quite different to being on the outside offering somewhat gratuitous advice' (which was essentially what ASTEC was doing; sometimes it hit the target, sometimes it didn't, but you didn't even know at the time you provided that advice whether it was timely or not). But obviously inside the Department of Prime Minister and Cabinet you know when the opportunities are going to arise for a certain piece of information or a certain piece of advice to be taken seriously. So I said, 'Okay, I'll do it for three years.' It has been a most fascinating period to be the first Chief Scientist and to work with two prime ministers in the job and to set up the Prime Minister's Science Council and the Coordination Committee on Science and Technology.

When I took it on, I thought that the formal things; that is the work of the Council and Committee, weren't going to be terribly time consuming, and that the job was going to be very interesting in an avuncular role; that I could go around to universities, CSIRO and government groups, and to industry leaders and talk up the importance of science-based activities, and technology and innovation generally, in Australia. And, of course I did that to the best of my ability, but I found very quickly that to make the Council and Committee work well took a great deal of my time, even though I had an excellent secretariat. I also found it absolutely essential to keep the network of senior officials, heads of departments, other senior officers and Ministers well informed as to what I was thinking, and try and talk through issues with them sufficiently so I knew what they were thinking.

The first time that the Coordination Committee met I think there were actually nineteen of us around the table. Knowing the territorial imperatives that each of those departments would exhibit, I guess I took the view right from the start that the best way to get the Committee to develop its own strengths and capabilities was to only have on the agenda matters that crossed departmental boundaries, so that none of them felt they were being, as it were, attacked and had to defend turf on a particular item. I recognised that collectively around the table we had the departments that either provided or spent 95 per cent of all the federal money flowing into science and technology bodies in Australia. So it was a committee with tremendous potential influence.

AM: And the evidence of the working of the Committee perhaps suggests already that there's a much greater understanding being achieved across departmental boundaries on science and technology resource allocation. Is that the case?

RS: Yes, I believe so. Actually one of the most impressive developments has been the role of the Department of Finance on the Coordination Committee. They have always in principle opposed increases in real terms in funding for any area, not just science and technology, and can argue quite effectively from their own point of view of how funding should be arranged in the Federal government. But successive Deputy Secretaries of Finance on the Committee played a major role in every discussion, very constructive. They said that they have gained a considerable amount from the discussions, and all of us I think have gained from their input.

AM: The Prime Minister's Science [and Engineering] Council, has both the Ministers attending and a certain number of distinguished scientists who are added for their wide vision. How has this worked in practice in terms of

really educating Ministers across many portfolios in a sense of national and scientific and technological goals?

RS: I think it's worked very well—somewhat to my surprise, incidentally. We bring the Ministers fully up to speed with what is in a working paper that's being prepared. But there is some chemistry in that room when those meetings take place which defies logic in the sense that it's as though they're being exposed to ideas for the first time. And both Mr Hawke and Mr Keating themselves have been very active in asking questions and making comments on the material. So, to a surprising degree, Ministers have picked up the issues that have been raised at the meetings and developed them within their portfolio basis. Now, I don't know whether that will continue, whether we're just in a honeymoon period (although it's been now for almost three years); but so far that approach seems to be working pretty well. The Prime Minister's involvement in these affairs is absolutely crucial. So if the Prime Minister [isn't] interested, it is pointless to have a Prime Minister's Science Council, it is pointless to have a Chief Scientist. You can't force this sort of advice on a Prime Minister. Fortunately both Mr Hawke and Mr Keating have been extremely supportive.

AM: The role of ASTEC must have been diminished by the establishment of the Science Council?

RS: Well, I think it was inevitable that ASTEC's role would be diminished to some degree. When the Chief Scientist's office was established, Ray [Martin, Chairman of ASTEC] and I talked together about our roles and agreed that it was pointless for ASTEC to continue to offer briefing material to the Prime Minister since we were doing it, in-house, as a matter of course. On significant matters, like comments on major reports to government, we made a point of seeking ASTEC's views and tried to incorporate them, either as a joint view or reporting the ASTEC view separately to our own view. I suppose my own feeling as to the role of the two bodies is that if government wants to have an external advisory body, then a body that looks rather like ASTEC is going to be maintained or created.

AM: I wonder across the breadth of this prominent role as a key science adviser, how do you evaluate the understanding of politicians about science? Historically from the fifties onwards, in some of the technical portfolios like Energy which touched on atomic energy, the Ministers were often very nervous. And that's why pre-eminent men like Sir Philip Baxter, who was a very strong-minded man, governed the policy making. And in my studies it has come out very clearly that politicians have been nervous in scientific and

technological fields. I wondered if indeed in your much wider acquaintance with them and in many different areas, the policy makers in science are more confident than they might have been ten or twenty years ago?

RS: Well, I think the Labor government Ministers, that is, those with science in their portfolios, have tended to be economists and have really seen their policy-making role very much in an economic context. In a political context first—I mean, that is *always* there—but then in an economic, and often social context. And economists in general underrate the importance of technology in economic growth, even though that's fortunately starting to change, and may well change quite rapidly, and hopefully will be seen as more important. I think, in general, they had two attitudes to science, which were often well-rehearsed; first, that it has to be related to something important in terms of economic and social development, something practical, and [they] often have a totally unrealistic idea as to how that can be achieved; and secondly, they want it to be good, in other words they want the science to reflect well on Australia. Those two things are sometimes in conflict.

AM: That's true. But would they see some areas of science, it may well be astronomy or it may be perhaps an area in palaeontology, that they felt would have no practical outcome. Would they see it as very important in terms of cultural and social good and excellence? Or would they be likely to give that a lower place on their register than science which yields applications?

RS: I think, with few exceptions, they all get captured by the cleverness of science and therefore see it as a very significant cultural element in Australian society. But, you know, if you took the Science Ministers that Labor has had, Barry Jones, then Simon Crean, then Ross Free; Barry, of course, was ready-made to be totally captivated by it and indeed he's played a marvellous role in talking up the importance of science in the community. Simon Crean came I think very much with the attitudes that I just described for Ministers in general and became captivated, and also an extraordinarily effective Science Minister; he was undoubtedly greatly impressed by the cleverness of science. Ross Free has also been a very enthusiastic supporter of science.

AM: We've mentioned several times the question of the Cooperative Research Centres which I understand was your concept. And so would you tell us, then, about the concept and the evolution of the Cooperative Centres.

RS: Well, I suppose I've always been by nature a cooperator. And people work in different ways, scientists work in different ways, some are very monkish in the way they work, others are much more gregarious. It's a personality thing

largely, I think. But in my career I've got enormous benefit out of collaboration with other people, and enjoyment of course as well, and feel as though people who have not benefited from that really have been deprived in some way. So, carrying that around with me for twenty or thirty years, I've always been keen on trying to build cooperative linkages, but I think realistic enough to recognise that they only work when people really want them to work; there has to be a perception of mutual benefit.

I've also discovered over the years that, while you have some born collaborators and cooperators, and you have some born non-collaborators and non-cooperators (and say there's 10 per cent in each of those two categories), there's about 80 per cent in the middle of people who will cooperate as long as they perceive that mutual benefit. Also I believe an essential ingredient for effective cooperation is that a substantial part of the funds for that collaboration must come from a common source. If these conditions exist, I believe you can entrench collaboration. And so, when the opportunity came to put a proposition to Mr Hawke for a major initiative, my thoughts naturally turned to a cooperative research program.

The program was driven also by the first meeting of the Science Council, when Mr Hawke sought views on problems with the scientific enterprise in the country. He got a very clear message that, in areas which really benefited from or required a team approach, even in areas where Australian scientists had been right at the front line internationally, we were tending to fall behind; the level of support just wasn't sufficient to ensure that we stayed in the front line. So the climate was right for an initiative that really addressed this reality and provided sufficient funds to ensure that groups could be built that would have critical mass, recognising that not every research area needed them, and recognising of course that in any team approach the individual's research still is a very important element.

I think Mr Hawke was very impressed by the 'falling-behind' aspect [and] by the need to address the fragmentation of our research effort in Australia. He was also impressed with the notion that the best way to build links with research users was to have the users involved in the work of a Centre, not just on the sidelines but part of the enterprise. For government agencies, and corporations of one form or another, to be involved in the planning and guidance of the research and, when appropriate, as research partners. So that was the soup that we stirred together.



Professor Susan Serjeantson

Interviewed by Ann Moyal
Canberra, February 1993

National Library of Australia
Tape no. TRC-2907

SS: Let me start by saying I was born Susan Wyber. I was born in a small town close to Sydney, a small town called Riverstone which is now a suburb of Sydney although it's some thirty miles from the centre, and I was born I suppose in that baby boom cohort. My father was demobbed from the Royal Australian Navy where he'd been working on radar, on the *Colac* in particular but on other ships also, and three days after he returned to Sydney he married, started university at Sydney University under the Repatriation Scheme (an opportunity that frankly would not have been available to him otherwise if he'd not been helped with the Repatriation assistance), and took up residence in Riverstone simply because it was the cheapest rent that he could find. I was born in the middle of his first year exams, on 14th November 1946, and my brother was born the following year. And the oral history in our family is such that when I was born the family stopped taking in papers because they were tuppence each, and that my father, Robert Wyber, walked to Sydney Uni from Redfern Railway Station in order to save yet another tuppence.

Despite the difficulties of raising two young children in penniless surroundings, he managed to top his year in mechanical engineering and electrical engineering. And, I suppose from that time, he encouraged both myself and my sibs in at least pursuing education. He held education in very very high regard. He'd always made it clear that we should at least try to pursue as much education as we could.

AM: And he felt this as much for a daughter as for a son?

SS: No, I don't think he did. In principle he did, but in practice in those days it was always made very clear to me that unless I actually got a scholarship I would not be able to go to university. My brother was smart, and if fees were going to have to be paid for anybody, well, they would certainly have to be for my brother. And I understood that.

I started school in England, actually. I was there for my first two years when my father went to British Air Corporation. Then he moved to Adelaide for five years with the Weapons Research Establishment, and so my primary schooling was in Adelaide. And when I started high school we returned to Sydney, and I started then at Caringbah High School which was a brand new high school for the baby boomers.

AM: Were you influenced by good teachers into an interest in the world around you and science, or did you go along other tracks?

SS: It's hard, isn't it, in retrospect to see how one has been influenced. I have a very strong memory of the Deputy Headmaster taking me aside on a

number of occasions and telling me that I really should plan to study medicine at university because in that way I would find a doctor to marry. And of course I was irrevocably foresworn against medicine ever after, because I thought if I took up medicine everybody would think I was only going to marry a doctor. [*laughing*]

We had some wonderful teachers at that co-educational high school, mostly men (I recall very few women, I think the women teachers were only in languages), but excellent men who encouraged us to read very widely and particularly take up Australian literature—I think that was somehow left out of the syllabus in those days, it very much focused on Shakespearian plays, but we had teachers who introduced us to Patrick White, and ... So I think in retrospect they were young and enthusiastic and quite imaginative.

My father believed that girls' schools had neither the facilities or the competition to foster an education in science, and so insisted on my attending the local co-educational school. I was good at science at school and so I felt that all my options would be open (because remember that at this stage I still knew that I had to get a scholarship to go to university), and so I concentrated on pure science, pure maths, English and French. But I took a very traditional, safe set of subjects that would almost be certain to ensure that I could get a scholarship to go to university.

AM: You didn't do natural sciences, in the sense of biology and anything in that line? Just pure maths?

SS: Yes. Mind you, when I was exposed to biology when I went to university, I loved it. I was at home and it was one of the first times I was reading science purely for pleasure and for understanding. But perhaps I was well prepared to read biology and zoology because of the background in pure science.

AM: So you obviously got a scholarship.

SS: I went to the University of New South Wales, in response to an offer by the Wool Board for a bond-free scholarship. In the mid-sixties it was common for scholarships to be accompanied by a five-year bond, and even those scholarships in the Diploma of Education were accompanied by a five-year bond. So it was very attractive to have available, when I was not sure what I wanted to do in the future, an open-ended scholarship with no bond so that if it turned out that I did want to pursue my studies that I would not be bonded to go into employment.

AM: Among your school friends were there many other girls who chose science?

SS: I can't recall any girls from my cohort at high school who actually went to university, whether it be in Arts or in Science. You see, most talented young women in those days aspired to teaching as a career and so a majority might have gone to Teachers College. And for many country people it was totally unacceptable to send a young girl off to the city to fend for herself and go to this university that was full of long-haired rarbags and people who protested against Anzac Day. I mean, it was unacceptable. Certainly in my particular year, when I graduated, in my group I was the only woman of, I think, some dozen or so graduands.

AM: In the actual laboratory situation and in the learning situation in the university, did you find that being rather a lone woman had any effect on your general attitude to the work? Was it a problem at all?

SS: I don't recall it being a problem, no. I became very interested in genetics once I had been exposed to the laws of Mendel, and it was a beautiful intermeshing of biology and mathematics because all of genetics is based on the laws of probability. And I had an excellent mentor in Dr John James who supervised my Honours project when I was at the University of New South Wales. We were always telling John James that his idea of Honours projects, which included counting the bristles on the abdomen of fruit flies, was mind-numbing and anti-intellectual, but he always persisted with this, because at the end of the counting there was of course, the statistical manipulation and the analysis of laws of nature. And he was right.

AM: And were there any women mentors? In a sense I suppose there was a mentor in this field in the shape of a distinguished woman, and that was Helen Newton Turner. Did you know of her existence when you were at the university?

SS: Yes, I knew of her existence. I read her papers, and I recall her papers listed her Christian name, and it occurred to me then, 'I wonder if some of the other people whose papers I'm reading are actually women, even though they're identified only by their initials?' I had no idea, of course. But I did hear Helen give one seminar. She was working in sheep genetics so it was highly relevant. I was very much aware of her contribution in sheep genetics.

AM: So, having done this Honours thesis on some aspect of the fruit fly, where did you take your work from there? That probably had some effects, counter or otherwise, on your new direction.

SS: Yes, I decided that the *Drosophila* or fruit fly was really not where I wanted to spend the rest of my life. And my reading had taken me by then into human

genetics, and there was a fledgling Department of Human Genetics starting at New South Wales Uni and I was welcomed in there to start my PhD. But in the meantime I had applied to the University of Hawaii where there was an excellent program in human population genetics, and I'd applied for a US-funded scholarship under the East–West Center to pursue post-graduate studies at the University of Hawaii, and I was fortunate enough to get that scholarship and that's where I went. It had a great reputation. And Newton Morton, who was the Director of the Population Genetics Laboratory there, is no longer in Hawaii but he is still a major leader in the field of human population genetics.

AM: Do you think that the United States had a leadership role in that field of human genetics?

SS: My own feeling is that science is as intimately interwoven with politics as every other part of our life. And Lysenko, who was extremely popular in Russia, and in fact whose mentors would brook no dissent, felt that genetic characters could be acquired and passed on to the next generation, that ... for example, if wheat was fertilised and grew twice as tall as the unfertilised wheat, then the offspring of that particular wheat would also be tall, irrespective of whether it was fertilised or not. And the Americans reacted very strongly, politically, to this idea, and were very keen I think to encourage the development of genetics. One of their nationals, Morgan, was particularly outspoken in the thirties against Lysenko, and was a quite powerful person, but I think motivated funding within the US of genetics departments just to show the Russians how wrong and silly they were. It doesn't surprise me that the Americans did put their funding into this area. I think it surprises me more that the Australians did not. We really were quite poor in genetics, but particularly in human genetics. Perhaps we concentrated more on the agricultural research side.

Well, when I went to the University of Hawaii I was very keen to work with human populations. I'd had a long interest in Papua New Guinea, and I was keen to undertake field work in New Guinea and to look at very simple genetic markers that we had in those days, that included blood groups for example, markers that are found in plasma that we call electrophoretic markers. And it seemed rather exciting to me, to pack up my equipment and trek off to New Guinea. Under the terms of the scholarship I was able to do that, the scholarship also funded my field work.

By then it was 1968, and there was a great deal of interest in the biological area—as there is now, in fact, in preserving biodiversity—but a great interest at that time in understanding the small groups of mankind, as they were then. I mean, it was already realised that mankind for most of his existence lived in small bands, mostly hunting and gathering; and it seemed to us by the late sixties that, unless the information was collected then, the

opportunity could be lost for ever. And this was particularly applicable to South American Indians, as well as various parts of islands in South-East Asia and New Guinea.

In fact, in 1970 the International Biological Program was set up, whereby there was enormous cooperation not only between countries but also between disciplines, so that physiologists and geneticists and medics and so on would band together in teams to descend on these few existing bands of what was seen then as primitive man, to document everything that they could.

AM: So that the work you were doing, and the information you were bringing in from New Guinea, was a special part of this?

SS: Yes. Particularly after I graduated and moved to Papua New Guinea to take up residence.

AM: How then did you conduct that research and collect the data?

SS: I was pretty cheeky; the first time I went to New Guinea I didn't even know how to do a venepuncture, I must admit. I had a pretty good idea of the area I wanted to study. It was the remotest I could find, and it was in the Western Province, in a place called Kiunga. It's about five hundred miles up the Fly River and very close to the border with what was then West Irian but now Irian Jaya. And I was interested there because there are a couple of language groups which were, as far as I could tell, of a size to be an appropriate model for how men may have existed and interacted for most of our history, and I was interested in finding how genes flow between villages and what the impact of inbreeding might be because of particular marriage practices and so on. So, although some aspects were practical, at that stage the main thrust of it was still theoretical.

AM: It also required anthropological understanding.

SS: Yes, it did. And it was then I think that I became aware of the implications of the sort of work that we were doing for anthropology, in the interests that some of the physical anthropologists had in some of our findings.

When I was in Kiunga I became engaged to the agricultural officer there. He looked pretty good, he was the only man around who owned a pair of shorts, and the only one who spoke English, I think. [*laughing*] And when I graduated from the University of Hawaii I was fortunate enough to have a position offered at the Institute of Medical Research in Papua New Guinea. And the institute itself was based in the highlands, at Goroka. But there was a small vacated laboratory in Madang, on the north coast. And it seemed ideal to

me, because the sorts of things I wanted to study were tropical diseases, which were more prevalent on the coast than in the highlands where Goroka was. So I set up a one-man laboratory in Madang.

It was then that the comparative work came about. There was a great amount of data that had already been collected by then, in 1970 under the International Biological Program, that essentially I analysed, just statistical analysis. But I was particularly keen on looking at the genetics of malaria (that still remains an interest to some extent) and the genetics of leprosy.

My early work in New Guinea also showed some insights into different diseases. For example, there is a common skin disease in people living on the north coast of New Guinea; about 10 per cent have a fungal infection called *tinea imbricata*. And there were some social implications about this. I mean children going to the high school with *tinea imbricata* are often ostracised and they're considered unclean and so on. It's a skin fungal disease that's not dissimilar from the sort of tinea that we know, but it covers the entire body with unmistakable twirling flaking skin. And as a geneticist I was interested to see how it tended to cluster in families and that there would be some family members with beautifully clean skin although other sibs would be covered in *tinea imbricata*. Until I started to look at the distribution in families, there was literature suggesting that Fijians, in Fiji, got this fungal disease and Indians did not because the personal hygiene of the Fijians was poor in comparison with the Indian residents. And we showed this was absolutely bunkum, the whole thing is genetically determined, and that there's a single gene that's recessively inherited. That means the individual needs to get a copy of the gene from both mother and father, and he's then susceptible to infection with *tinea imbricata*.

And this very simple piece of work, that resulted just from being in a village and noting how the skin condition tended to cluster in families, became of interest because it was the first clear-cut demonstration of inherited susceptibility to an infectious disease.

AM: What about your work on leprosy? Was there quite an advance that you could make?

SS: Ah, leprosy. Yes, it actually took an enormous amount of effort for the results that it generated; but I still think it was probably one of the most worthwhile things that I did in New Guinea. I undertook to visit every village in the Bogia sub-province, and there are 183 if I recall. And I based myself at Hatzfeldhafen, a Seventh Day Adventist mission, for part of the work. I usually had one assistant, a Papua New Guinean from the mission, a former leprosy patient, who accompanied me. And we would look at the old patients, see how their disease was progressing, make sure that they had access to treatment, inspect all the adolescents and children for any signs of leprosy.

and generally get all the records in order and ensure that care was being delivered (which in general it was not). You see, leprosy is not contagious if people maintain their drug regime, but if there's difficulty in delivering those drugs then the person, especially those with lepromatous leprosy, can be infectious and infect people in the group.

So that involved a lot of walking, and patrolling and travelling through the bush. I can remember walking around Manam Island, which took about ten days. I was drawing blood also. And also concerned with the epidemiology of the disease.

AM: How did you control the blood samples, lifting them around the country, literally on your back?

SS: On Manam, for example, I would employ a runner. There was one central mission with one fridge, and they would be run back to the mission. Then when I got there I would process them—which was a very simple procedure of separating them in a centrifuge.

AM: With the leprosy, was the evidence there that there was a genetic predisposition?

SS: Yes, once again. And it turned out to be a little more complicated in terms of the theory and the analysis than I'd anticipated. Because what was happening in the community was that if a woman got leprosy her husband would almost certainly divorce her, and then if she was resident for a short time at the mission hospital then she would tend to marry another leprosy patient. And then their children may well be born in the institution also. So then, is that child susceptible because he is born in the institution, or because both parents have leprosy?

The challenge to me was in the modelling, to try and put human behavioural aspects, like choice of marriage partner, fitting that into a model of susceptibility for disease. So there was the practical side. But, again, I think the main contributions came from the theory and the application of probability theory, because this is then a universality which can be extended to other diseases.

AM: So this work, which is obviously both scientifically diverse and anthropological is leading you into the broader field of microevolution.

SS: In evolutionary biology, yes. Yes, I suppose the microevolution was just another word for the sorts of things I had been doing and that we'd been calling population genetics. Evolution was a part of it, whether it was because genes got lost in small roving bands or genes were favoured by selection. And the *tinea imbricata* story that I told you about may well in some way have been

providing some advantage, in terms of survival, to the early Melanesians. I mean, perhaps they were less subject to malaria, for example. So all of this can be gathered together under the general umbrella of evolution.

AM: What interests me from what you're saying is, do you think your particular scientific route is something that's more likely to be taken by a woman? There's a distinctive style in your research, you go from very specific pure scientific research with a mathematical base, into populations, and then disease. And it's people-oriented, isn't it, or it becomes people-oriented. Do you think that's a particular style of a scientific woman? Or does that exaggerate the point?

SS: It's hard to say. I think perhaps that it's true, that women are happier working with problems with which they can identify, rather than working on the mouse model or the fruit fly model, and somewhere at the back of the mind is this extrapolation that it's going to do the world good in the long run. I think that perhaps women are more pragmatic. That's the only way I can explain how many enquiries I get from women, to come and work with me.

AM: You mentioned these lonely treks you took in New Guinea, taking blood samples from people who were not at all accustomed to that. How did you find that?

SS: Oh, I was quite successful. [*laughter*] I spoke the lingua franca, quite well in the end. It's interesting that some of the men actually were happy to get in the queue. You know that in New Guinea there is in some societies an underlying fear of women menstruating, and there's the feeling that this gives women strength of some sort. And even at the local Teachers College, where the students there were comparatively sophisticated, the boys would not go to a class on the ground floor if there were girls on the first floor. So this is quite a widespread fear. And the men in some areas actually felt that the blood-letting was something of a catharsis. And they were far less reluctant to give a blood sample than, for example, to give faeces or saliva. Faeces and saliva are traditionally used in sorcery, whereas blood is not so much. And so it was not a big deal. [But] there are some restrictions, yes. In most areas where I worked, the man is not able to give information about his wife's family. I think the first field trip I did, I was trying to really identify people, and I was asking the man what his wife's father's name was, to get the wife's maiden name. And at the end of the first half-hour I'd found out that every woman had as her father 'Tabu'. I said, 'How can this be?' And what they were telling me was that it was taboo to tell me. So I'm writing down 'Tabu', 'Tabu', 'Tabu', as the father's name for the wife. [*laughter*]

Once you understood the restrictions it was okay, because you knew whom you should ask for any information and then no one was embarrassed.

AM: You then come in 1976 and begin your career at the John Curtin School of Medical Research at the Australian National University, as a Research Fellow. What work do you then concentrate on in that setting?

SS: We returned to Australia in 1976. And my husband, having been in the tropics for many years, refused to come any further south than Brisbane. But by then I'd established a warm collaboration with the Human Genetics group in the John Curtin School, and everything felt so right there, I just knew that was home. So, rather reluctantly, John came south of Brisbane.

I then set about trying to set up a laboratory that would test for transplantation antigens. These were only just being defined in international laboratories and the methods were exceptionally crude, I suppose. We were only just starting to understand how variable the transplantation antigens were, from one individual to another. Whereas for a blood transfusion, for example, it's quite a simple matter to match a blood group O donor with a blood group O recipient, when it came to the matter of transplantation (which was just starting to be a surgical procedure) it was known that when one tried to match the white cell it was a very different story from the red cells and that there were numerous white cell antigens. I think in the mid-seventies we had no idea what we were letting ourselves in for. We knew that there were more antigens than A, B and O. But there are literally hundreds and hundreds of these white cells, as we know today.

So our first techniques were based on serology, and we needed serum that would recognise the antigens on the outside of the white cells. And the only way we could get those sera was by appealing to the generosity of the women in Canberra. And we screened blood from almost every pregnant woman, for maybe half-a-dozen years; and also from donors at the blood bank who had had a number of children, because women will (not always, but sometimes) raise an antibody against the foetus who is carrying the white cell antigen that has come from the father. And in this way we were able to build up quite a collection of anti-sera, and also establish links with other international laboratories that were trying to achieve the same goals, and we exchanged sera on a regular basis. And many good friendships resulted from this mutual generosity.

Ultimately we ended up, before too long, with our repertoire of 180 anti-sera which we needed to test for the antigens. And nothing was known at all about the antigens expressed in some of the minority populations. And that's when, I think in 1980–81, we took the laboratory—all young and enthusiastic ladies [working as technicians]—to Fiji for six weeks and set up in the Colonial War Memorial Hospital.

We went to New Caledonia on two occasions, and Nauru on a number of occasions. And ultimately we built up, using these anti-sera, a profile of the populations in the Pacific in terms of how their transplantation antigens compared with those elsewhere. And it was exciting, we had no idea of what we were going to find, logistically, this was an extremely difficult exercise, because the white cells that we were testing had to be viable. At the end of our test we count up how many cells are alive and how many dead. So our cells had to be alive in the first place, which meant that we had maybe 24 hours between drawing the blood and reaching the end of the assay, and that pretty much confined us to major centres such as Suva and Noumea and Nauru. And yet there were many populations that really should have been tested, we felt, and not the least of those populations were those in Australia, but logistically [it was] extremely difficult to get a representative sample of Aboriginal Australians.

So when in the early eighties DNA techniques started to emerge, we were very quick to try and adapt any new technology to our transplantation analysis, because we had that extra motivation, not only of doing it quickly and accurately (and it turned out to be much more accurate than the old serology), but also in being able to look inside the cells so that we no longer needed a living viable white cell. What we could do was to have a small sample of blood snap-frozen anywhere in the most remote location, kept on ice, and shipped to a centre and put in the fridge. So all of a sudden the logistics were totally different and we could access populations that hitherto we could not look at.

AM: In the business of networking with this information you were gathering, were you publishing results rather quickly, or was it more informal exchange of data and findings?

SS: No, we got smart. We publish first before we tell anybody. [*laughter*] In the mid-eighties we were developing methods that were to revolutionise this area, and we weren't running about, telling everybody about them until we actually put them into practice. This new method was based on what we call 'southern blotting' or on RFLPs. The RFLPs or the southern blotting [when tested in other populations in parallel with the serology] showed us that serology had been missing many of the antigens which were present, and could potentially account for some of the transplantation rejection that had seemed unaccountable before.

But that was only one step in the development of the DNA-based protocols. And not too long after we had these established there was yet a new technology developed, mainly through Cetus Corporation in California, where a piece of DNA can be multiplied indefinitely, a million times—one can take a small blood stain and multiply the DNA indefinitely, thus providing enough material for analysis. So we then went on from our techniques of the mid-

eighties to apply this polymerase chain-reaction technique which multiplies DNA. Now, this has meant that we can look directly inside the cell, rather than at the antigen on the outside of the cell, directly at the gene that's responsible for encoding the transplantation antigen. We cannot get it more accurate than looking directly at the gene, and so the potential for improving the matching of donors and recipients, which is so critical particularly in bone marrow transplantation, has made a monumental leap forward in terms of the sensitivity.

AM: Your work then had a revolutionising effect?

SS: Yes, I think we've contributed a lot in terms of the protocols and the methods. All our methods have been published internationally. But we've had the added interest of taking it further, to look at populations that other groups simply don't have access to.

AM: You mentioned work with Aboriginal people. Would you have had a large sample?

SS: It's been a fairly sensitive area and we've tried to collaborate with medical officers and others who have a close relationship with particular Aboriginal groups, so that everybody is certain that the consent when it's given is informed and that appropriate feedback is given. And so in some cases it's been a slow process, but a very rewarding process when groups truly understand what's involved. And one of my most precious possessions, I think, is a letter from the Council of Groote Eylandt, saying that they understand the need for transplantation (they've seen cases of renal transplantation in their community), and that they understand fully what we're trying to do and they would like to help us in any way they can. And it is really gratifying when you know that the people you're working with not only understand what you're doing but that they actually appreciate what you're doing.

AM: Transplantation within, say, a Groote Eylandt group would be interiorised, wouldn't it, you wouldn't go too far outside a group for them.

SS: That's what we're finding. But we didn't know that until we started to look, we didn't appreciate just how great the diversity was within Australia. Even though I've worked in transplantation genetics for many years, I've been shocked to find the extent of the diversity and the practical problems which that will introduce in terms of finding appropriate matches for Aboriginal patients.

AM: In contributing this major advance in your field, what do you see as your most significant publication?

SS: There's no one single paper that dropped a bombshell and there it was. It's a long history of slogging. We try to publish as we go, so that our contribution is on the record. There are papers where I've tried to bring together a lot of the various information, and I suppose they would be the most remarkable. But I think I'm most proud of the long history, the track record if you like, and the persistence of pursuing this to the end, from the old crude serology days when we had no anti-sera and tried to make anti-sera in rabbits (not very successfully), and following that through for more than fifteen years. I think it's little drops of water, you know, dripping away at a stone. Looking back now, it's pleasing to see the progress that we've made and how little we knew when we started.

AM: Scientists approach the question of attribution very differently in their publications. You invariably list a number of co-authors. I presume you are the prime author, and you give credit to those who have assisted you. And would some of them be medical technicians?

SS: In medical research there seems to be a point in one's career where one shifts from being the first author to the last author. I suspect I might have done it a little younger than others. But I prefer not to have any hassles about who is an author, and my inclination is to include whom I think has made a contribution. There's a lot of debate about this approach. Some people believe that co-authorship is only earned if one could intellectually defend the paper in a debate. My attitude is a little different; I feel if someone has made a contribution, whether it's a clinician who has referred patients and I've relied on his clinical judgement (and many of our co-authors are clinicians), or whether it's someone in the laboratory in a technical capacity who has gone beyond the call of duty. And my philosophy with students is that they should be the first author if there's a paper arising from their general project. So it's true that most of my papers are multi-authored, and I think that's encouraged the involvement of the staff, and there's nothing like a first paper for a PhD student to make him want another. I see no advantage in excluding people, I'm happy to have the whole football team on if they want to be on it. *[laughing]*

AM: In my perception this approach would be more likely to come from a woman scientist than a man. I'm not saying that some men don't do it. It's a nourishing approach.

SS: Yes, some of it's nourishing. And there's no doubt that one can reap the rewards of the nourishing, particularly with loyalty.

AM: You also publish in a very wide range of journals, medical, human biology, human evolution and even Pacific history. It's a fairly wide field. Would this be usual?

SS: I think perhaps it's unusual in that we've worked in an area that has implications beyond the original and immediate objective, which is improving transplantation outcome. Because the transplantation antigens are so diverse, and the genes that encode them are so diverse, and because we've concentrated our studies in populations in the region of Asia and Oceania, it's been possible to exploit those data and make some comment on matters of interest to physical anthropologists. So, although I'm not an expert in that area at all, some of our data are of interest to the anthropologist.

[For example], as a consequence of the large numbers of individuals we've studied genetically, we've been able to make some analyses of the inter-relationships between people in the Pacific, and make suggestions about the mode of colonisation of the Pacific. In some cases our results have been at variance with the conclusions in physical anthropology, and that's where the interest arises. For example, Howells found Polynesians and Micronesians very similar to each other on anthropomorphic characteristics, that is, when he measured skulls and various features of the anatomy; and yet our own studies in genetics show that there is barely any relationship between the two groups, and when we did a very formal analysis of looking at the ancestry of Micronesians it would seem that the Polynesian genes made up only about 5 per cent of the Micronesians. So this was in stark contrast to the physical anthropology results. This has been one area of fruitful development, that the linguists actually agree with the genetic analyses, that the Micronesians and Polynesians are quite separate and did colonise the Pacific through different routes. In other words, that the Polynesians didn't island-hop through Micronesia, they more than likely came out of South-East Asia and moved along the north of New Guinea. So this is one example that I can give of how something that started off as an analysis of transplantation antigens actually put us in a position where we could tell a story about the Pacific.⁵⁵

AM: In 1988 you became a Professor and succeeded Bob Kirk as head of the Human Genetics group at the ANU—that's a distinction for a woman in the Institute of Advanced Studies at the ANU where you must be the only woman science professor. It's encouraging for young women to see your track record, how you have been such a contributor and then that some of the rewards, in the reward system of science, have appropriately begun to flow. It would be interesting to learn how a day in the life of a research person like yourself goes in the laboratory?

SS: I don't know whether I dare tell you that I start my day every day at 4 am. This might be most off-putting to any of those young aspiring medical researchers. But I do. And I find that is such a productive quiet time to do my writing. And I read. I find that once I actually come to the laboratory I don't have time to read, and it's very difficult to write, given some of the interruptions. So that's literally how I start my day.

I think you were asking me about the sort of techniques that we may be using in the day-to-day routine analysis of these transplantation antigens, or of any genes for that matter. And our procedure is to take a blood sample, whether this is from a patient or a healthy donor. And these days we're far more careful with the blood samples we take than we may have been ten years ago, and they're handled with due bio-hazard precautions. And from that blood we extract the DNA. Now, one of the most wonderful things I think in science, in the laboratory, is to experience the first extraction of DNA from blood, because you're shaking up all this protein and digesting it with proteinase and precipitating it with ethanol, and all of a sudden out pops this beautiful pure DNA in long swirling strands that one can actually visualise with the naked eye. And that's the starting material for almost all of our work now.

More and more we're becoming dependent on this polymerase chain-reaction technique which will take a single strand of DNA and multiply it and make two strands and then take those two strands and make them into four, and so on, and exponentially amplify up millions of these strands. So that no longer is material a limiting factor in our assays. Having done that, as geneticists, what we are interested in is looking at variation between individuals. So we've now amplified up a piece of DNA, and we can look to see how it varies from one person to another. And there are a number of ways in which we can do that. It can be a sequence, to determine the code of the DNA. And that's the most accurate way of doing it; but sometimes if we want to look at large numbers of individuals or large chunks of DNA, that's not the most effective way of going about things and we use other short cuts that will look for differences between individuals. So that's the aim of our work. We have many different short cuts for trying to look at differences between individuals. And then we're looking at differences between disease groups; we're comparing gene patterns in multiple sclerosis patients, for example, with those in healthy people, and trying to zero in on those genes that predispose to particular diseases.

AM: I hesitate to ask what time you finish your work. You began at four.

SS: I usually leave about five-thirty. [*laughing*] I have a young son, in after-school care. For many years I believed that motherhood was out of the question, it was not even a matter that I considered. I was a researcher, and that

was it. And it wasn't until I was in my late thirties that I actually stopped and thought well, perhaps this is my last chance. And I must say that I'd never ever thought that I would have a child, but having done so I think it's my greatest achievement. And what a joy.

But I'm particularly lucky, I think, in my circumstances. My mother is a wonderful traveller. And it's so important, I think, in amongst the competition, the international competition, to be travelling overseas and to be part of the important meetings and to accept invitations if they're prestigious, and my mother has been a wonderful travelling nanny when it's been necessary.

AM: In 1992 you were awarded a National Clunies Ross Award for Science and Technology, an award initiated in Australia in 1991. What is the citation for that?

SS: The citation is actually rather specific. It's for transferring science from the laboratory to a technological environment, and it's also for persistence. So it is for our persistence in trying to improve the outcome of transplantation, and also for trying to extend from the theoretical, from our laboratory, extending those protocols so that they're in a suitable form for use in laboratories throughout the world.

AM: It's evident that you represent a very positive strand in the history of women in science in Australia. Many remain cooped at the level of lecturer, sometimes tutor, and some laboratory technician. But you have emerged as a pre-eminent leader. Do you think that feminists commenting on the role of women in science exaggerate the difficulties?

SS: I think women in science need to remember that some doors will close but other doors will open. And even the fact that you've invited me today suggests to me that the door is open because I'm a woman. I mean, doors will shut, of course they will shut. But they'll also open, hopefully. And I think the idea is to try and exploit the open doors.

There's one point I'd like to make, though, about the advancement of women in science. And it comes from observing, from my unenviable position of sitting on promotion committees and on selection committees and electoral committees, that very often women who are qualified are hesitant to put themselves forward. For some reason (and I think I've probably been amongst them) they sit back and think, 'well, if anyone thought I was any good they'd invite me'. Someone would tell me to apply for a promotion. Or someone would tell me to apply for this job if anyone thought I was very good. And of course the call never comes. Men sometimes may over-estimate their qualifications, but at least they're prepared to throw their hat in the ring. And

women, it seems to me (in general; one can only generalise), are more reluctant to expose themselves to public scrutiny, and this is possibly reflecting a lack of confidence. I think that women can make positive progress in advancing other women in some of these positions, not by any discriminatory or equal-opportunity type of approach, but simply where we see excellence encouraging those women to apply for what's available, and in fact what is their due.

AM: The building of confidence among women is a very deep problem, isn't it, because, almost from the earliest times, they are streamed off from the boys who then become more assertive. And women also have a sense of needing to be liked, which is quite a barrier for them in some ways.

SS: Yes.

AM: I also notice that a woman's Curriculum Vitae is very much more 'sotto', more underplayed than a man's of some equivalence or even less equivalence. So that you may well think, simply from the presentation, that the man is a better qualified person. And women have difficulties, don't they, in presenting themselves and maintaining that presentation without revealing any sense of uncertainty?

SS: Yes, that's right. But I think things are improving greatly though it's going to take a while for women to be represented as well as they should be. But that's already happening in the John Curtin School. More than half of our PhD students are women who are taken in on a competitive basis.

AM: Reading today about women in science in Japan, I liked the fact that one of the distinguished women scientists from an earlier period had called her story 'I Want to Know More!' Do you perhaps see that as a cry for your own life?

SS: Oh, I think it's a wonderful statement. And I think it's a cry from any scientist, in any field. And that's what drives people, isn't it. And that's why there's no question of not being able to look after family life and a scientific life; it's not a question of ever sitting down and thinking there was any option to stop doing research.



Robyn Williams,

AM, FAA

Interviewed by Ann Moyal
Sydney, January 1993

National Library of Australia
Tape no. TRC-2901

RW: I was actually born just outside London, of rather strange parents in that it's very easy listening to me, to say 'Oh, he's English', and having grown up there to put that sort of label on me, but funnily enough my mother had not a drop of English blood in her, nor did my father. My mother came from one of those Middle European mixtures which is sort of Polish, Jewish, Russian, on it went. And my father was about fifteen-thousandth-generation Welsh. We were evacuated in January 1944 to a place called High Wycombe known for its hell-fire cave and various people grouping together on the hillside for orgies. But they didn't do that quite during the war.

AM: Can I just ask you when you were born?

RW: January 1944, and then taken to various places around Britain, including Wales where lots of ancestors were. I went to seven junior schools before I was eight years of age, and that was partly a vehicle of the war, and partly, of course, due to the fact that my parents kept getting different jobs. They were kind of working class, but the Welsh and the Jewish working class are not quite the same. My father got a degree in mining engineering from the University of Cardiff when he was seventeen, at the same time as working down the pit from five in the morning till God knows when, and he was always a very severe, old-Welsh Baptist, and disciplinarian type, but highly intellectual, very well read, somewhat Left of centre. My mother on the other hand was a brilliant linguist, she could speak umpteen languages, and was a great traveller as well, she used to zot round Europe, often hitch-hiking, because if you remember back in those days people often used to don walking shoes and rucksacks and the sky was the limit virtually, up the mountain, down the valley, and away they went.

So what I inherited very quickly was a feeling for words, a feeling to some extent for science because that was the way of the future everyone said, and also for languages and travel. So at the age of seven, having had a fairly lonely upbringing because I kept moving and I was feeling as if I was on the outer, then I became the ultimate new boy when we moved to Vienna. There we were for the next five years.

I was eventually popped into what's called a Volksschule, a primary school. They all spoke German, and they had fairly severe attitudes to the way youngsters should behave in school—the teacher comes in and you stand up and you bow. Now I, even at the age of seven, knew that we'd won the war and I wasn't bowing for anybody. Being the 'Englander' who'd won the war was my only recourse, and so I became leader of the gang, learnt German in what seemed to be like six weeks, and from then on it went quite well.

I went to the Gymnasium in Vienna (which is like a grammar school), and then left and went to London where I got into another grammar school. And one just simply had one's head down, and nothing remarkable happened

except my meeting of Australians. There was one fellow there who seemed to be unbelievably articulate, who was the best runner, came first in everything. His father had just been on television the night before, a bloke called Reg Goldacre who'd been at Sydney University at the age of fifteen and he was the guy who had worked out that cells have polarity. So I had this vision, you see, of Australians being all absolutely brilliant academics who were also master athletes; because Reg Goldacre would have been selected for the Australian Olympic team except that the war intervened, and this house in Balham had the most extraordinary range of famous people turning up. And the people from Australia came from universities, were literary types, and I had this vision of Australia as a kind of Shangri-La in the sun.

I decided to go to this Shangri-La in the sun, and paid £10 and came to Australia in 1964. And that was for a two-year stint. I quickly discovered, of course, that all Australians were not simply straightforwardly scientists who were athletes and wrote novels all at the same time, but that things were slightly different.

I had two years in Australia, and met a person to whom I'm now married, who worked for the ABC in concerts, Pamela. And my intention had always been to use Australia as a launch-pad from which to hitch-hike back to Europe, which we then did, in 1966, from North Sydney all the way to Piccadilly, which took six months. And then I went to university. My mother had applied for me and I ended up in one of these rather drab colleges in North London where I did a very old-fashioned BSc degree that they'd designed. T.H. Huxley and various other people had put this degree together and it was virtually everything that you could mention to do with science. Some of the questions for exams were about three pages long—you know, name this part, name the other part, and what's this funny thing on the end? So there was all that kind of taxonomy and anatomy. And then of course, as the century had changed, they discovered genetics, physiology, biochemistry, and all this stuff got added on. So I was doing a degree at the University of London that we all knew was bonkers.

AM: But surely this was a great background for a potential budding science communicator?

RW: You're absolutely right! I'd heard of every concept. I had no idea what it meant, and if you're in the job of finding out what things mean and taking a plausible stance of curious interest, then you are very well primed. So I was terribly lucky. God knows what happened to the others who took this degree, because they immediately abolished it after we'd graduated. We were the last ones ever to take this immense old gargantuan truck of a course. It was an Honours Degree in the London University. So, having graduated, I was again feeling the bleakness of London, despite the fact that it was the end of the

sixties and the beginning of the seventies. At the same time as doing this degree I'd been doing various bits of telly and my wife had been working for the BBC and Peter West's (known for rugby commentaries) daughter said, 'If you're going to Australia, and as you've done a science degree, why don't you look up a chap called Humphrey Fisher?' I said, 'Who the hell is Humphrey Fisher?' And she said, 'Well, his dad crowned the Queen. Geoffrey Fisher was the Archbishop of Canterbury. And Humphrey invented a program called 'Your Life in Their Hands', and he's now running something called TV Features in the ABC.'

So I wrote to Humphrey Fisher. And he wrote back saying, 'Quite amusing. Jobs don't grow on trees. Come to see me when you arrive in Australia.' So, after having got on a boat—we then had a son aged a half, and there I was, I had a degree, a BSc Honours degree, a wife who'd just left broadcasting and a sprog, and no visible means of support—I arrived in Australia, got something like a flea-pit of a flat in Neutral Bay, and went along to see Humphrey Fisher, who told me again that jobs don't grow on trees. And he was being quite laconic, and a very nice man. But I got angry and told him that this was absolutely ridiculous. Here I had this degree, I'd been working very hard, and to be told, apart from anything else, that I had to come back in ten years' time when I'd got ten years' experience and had written two books, well that wouldn't actually pay for the groceries next week. He was obviously quite amused with my insouciance and wrote a little note to his opposite number in radio, Peter Pockley. Peter Pockley, was the founding director of the ABC Science Unit, which had been set up in 1964. And Peter Pockley had lost two producers the week before, one of them Max Bourke and someone else whom I never did find out much about. And so Peter Pockley said, 'Come and have a job!' I ended up in the ABC Science Unit.

AM: It's very heartening to me as a historian of science to hear that T.H. Huxley, who applied unsuccessfully for a job as Professor of Natural History at Sydney University in 1849 or 1850, but didn't get it and, that his long hand stretched out through his course and brought you, a distinguished science communicator, to Australia.

With these early days of science under Peter Pockley, who was a pioneer, it seemed to grow very rapidly when you joined the organisation. Can you tell us a little about your early contribution?

RW: When I joined the Science Unit in 1972, we had a program called 'Innovations' which was fifteen minutes long and presented by Glen Menzies (a freelance person, better known for doing a music program), we also had 'The World Tomorrow' presented by Michael Daley (who is now deceased, he died of leukemia, aged forty-two), and we also had a program called 'Insight', fifteen

minutes long, which was produced alternately by Peter Pockley and John Challis, who was then the executive producer. 'Insight' was one of these programs, a bit like something else that was going on then called 'Guest of Honour', where you had a distinguished person go on either in scripted form or in interview about what they did, their line of research. It was an ideas program. So here you had one on innovations, one a general half-hour science magazine program (which Michael Daley did very well; somewhat flamboyantly shall we say, and haphazardly occasionally), and then 'Innovations' about technology and so forth, basically material that you get from overseas or wherever.

Now, there'd been something of a stand-off between the radio science people and TV people about whether the group should be integrated. And without going into any of the politics, lots of this meant that Peter Pockley was having a difficult time. He'd also represented, to some extent a sort of establishment view of science. Now, he had been brought in, I understand, as a result of a conversation between 'the Knights of Science', I'm sure that the once Chief of CSIRO, was in on the conversations, Sir Frederick White. Sir Rutherford Robertson was there and maybe Lloyd Rees, also of CSIRO, a distinguished person. Dr Darling, who was then Chairman of the ABC and had also been Headmaster of Geelong Grammar, remembered a senior boy at Geelong Grammar, a talented chemist called Peter Pockley. But this view of science being that which distinguished old men produced ...

AM: The Establishment science.

RW: Yes; and that one had simply to sit here and say, 'Oh well, fine, Professor. Now, if you'd like to go on in any way you like and just tell us about the terrific things that you've achieved.' And that seemed to be the flavour of things, except that Michael Daley was always quite contrary in the sense that he was a born journalist, never had a degree, came from New Zealand, often felt in some ways on the outer, was a great big chunk of a man. So you had this tension in the Unit about, on the one hand, being journalistic and confrontational which meant that Michael tended to rock the boat a bit, and Peter Pockley, who despite his absolutely worthy and splendid aims, would take the view that one had to be rather more austere and responsible about these matters.

So when I joined it was obviously either going to be closed down because of these tensions (and, in fact, Michael Daley went across to television) or something else would happen. Now, the something else that happened was that at an ANZAAS Congress Peter Pockley spoke about how he was being restricted from doing television, and two things happened immediately afterwards, one of them being that Sir Talbot Duckmanton, the General Manager of the ABC, called Pockley to his office, and Peter Pockley was offered

a job at the University of New South Wales. So that the Science Unit was then, if you like, open for different things. And I was there.

I'd always taken an attitude that broadcasting is a difficult thing and my characteristic is not simply to let things emerge but to work bloody hard. So I tended to look for ways that we could use the medium in an adventuresome way. This was very much encouraged by John Challis. He also hauled in Robin Hughes. She's since become the first managing director of Film Australia, and a person of amazing intellectual force. She started a program called 'Investigations', which was two or even three hours long, live, and using all the kinds of techniques and technologies then available to radio. We did the first 'talk-back'. We had international satellite links with anyone you could wish to mention who was famous in the world of science. I mean, we had them all together and we would go to epic lengths, and this was just terribly exciting stuff. So since then we have explored both the range of radio, using the kinds of developments that my friends and colleagues are so good at, and also refined some of the intellectual ways of interpreting science.

AM: It's one thing to look at this development, but what must strike any person who listens to you is the enormous range of subjects and diverse interests in science, including the history of science and the society and culture of science as well as every discipline of science that you've covered. It's one thing to have had the 'Huxley course', but this must take enormously hard work on your part to put yourself into a situation where you can have these discerning, penetrating and very stimulating talks with people of all kinds and disciplines.

RW: Yes, the work has got more and more demanding, there's no question; because the circumstances of the ABC, in parallel to those of sister organisations like the BBC and CBC in Canada, have meant a kind of austerity whereby you don't have these great teams of people handing you research stuff—you know, 'Here is your interview for the morning, Mr Star; here are your notes, and this will be all lined up, made convenient for you, and we will then take away the product and make a program out of it.' That does happen with television. Now, there's no question: to understand science stuff takes an immense amount of experience, and research, and indeed the persuasion of people in science itself that they should bother to come and put themselves on the line, because they don't need to, they are persuaded that it's in their interests these days because otherwise they won't get funded.

AM: But this has been one of your crucial contributions, surely, that you have with the diversifying programs—various television interviews—that you have brought an enormous range of scientists and social scientists to the public, and that probably now many of them are panting to come.

RW: Well, yes, I can't think of many, apart from perhaps Dr William McBride, who'd be reluctant to come into the studios. But, you see, the way it's done is frankly old-fashioned leg work. You don't sit in here waiting for people to come, you go out there. Now, that's the difference between what a public service broadcaster does and everyone else. We have to reflect the society and its complexities, and therefore you go to the Flinders Ranges, you go (where I was last week) to Launceston, listen to the speleologists, the cavers. You spend time, not only in Australia, but try to go to other countries, because the whole point about science is that it's international. We produce 2 per cent of the research papers but everyone else produces 98 per cent. And the one thing about travel overseas is that it tells you how very good Australian science happens to be. And, having conversations with overseas scientists, often you refer to the Australian research as a matter of course, and this is terrific.

Now, if you do that kind of leg work (and, as I said, with Peter Pockley, 'Dear professor, what are you up to?'), then if you're on the spot, you have a much more informal way of presenting the material. You know, you've got bird sounds and you've got people standing there in shorts, and you say so, because what you're trying to do is show that science is done by people in shorts.

AM: This must surely have influenced the image of the scientist, which is one of the key questions I think that worries people today. There is a recent report that suggests that the scientist is regarded as—the term is 'a nerd and a loser', that he is either a mad, wild-haired looking person (which has been a view that had been quite traditional), and that he is not really related to the community. Now, your programs, and the audiences they've attracted, 'The Science Show' particularly, must surely challenge this survey research. Would you agree that the scientist is viewed as a person of no interest, after all this formidable work that's been done?

RW: Well, it's very kind of you to say 'formidable work'; but there was a piece in the *Financial Review* saying that in a survey at a summer school, the CRA Summer School of young students, they were asked who Robyn Williams is, and, of the 10 per cent who responded, most thought it was 'that American actor'. And that is not surprising. I mean, I've got plenty of stuff on ABC television, but it tends to be on at times when youngsters don't see it, and therefore the average Australian youngster need *never* hear of me or our science programs in all their variety. And if you were to ask yourself, as I've written on a number of occasions, to name one major person from drama who is a scientist who is not loopy (you know, he's not sort of a freak or a time traveller like Dr Who or Spock, or someone slightly unlikely like Indiana Jones), who is there from recent drama on television or in the feature films? I mean, they had Sean Connery playing a doctor in the rainforest the other day, but you are

scratching; whereas if you want to mention any number of crims, lawyers, nurses, police, or what-have-you, I mean there are zillions of them. And therefore you have no models for youngsters to look at. You're mentioning someone who rings a bell in terms of a science course, an obscure profession, or someone who makes bangs and stinks and problems.

However, in a recent survey (of the British Association for the Advancement of Science), the latest Gallup Poll which they do to coincide with the conference every year has shown a *doubling* of the respect for science, very much also amongst young people. David Attenborough, who is the President, was amazed. I happened to be the President here for the [ANZAAS Congress] meeting that started three weeks later; and I don't know what surveys might have shown in this country, but I do know that it's been shown that the high school graduates have opted much much more for science this time than was the case last year.

AM: I understand that the survey which produced this negative finding on scientists was presented to the Prime Minister's Science Council; and presumably, because it ties in with education and attracting young people into science, there was concern at the Prime Minister's Science Council. Were you called to give evidence?

RW: Yes, a number of us were asked to go—Cathy Johnson, who was then at the *Sydney Morning Herald*, Ben Selinger, with his own personal Chair of Chemistry at the ANU; somebody from BHP; and also I do believe somebody from the Science Teachers' Association—were all called in to make quick suggestions as to what might be done, and we said our bit. I suggested that we put some of our science programs on the curriculum; and this was welcomed by Kim Beazley, the Minister for Education, and indeed Ross Free, the Minister for Science. And Peter Baldwin was there, then the Minister for Higher Education. So Kim Beazley wrote to the Curriculum Corporation which has the job of making this sort of thing happen. And, as you know, there is a sort of glacial rate of operation, well, for me, a journalist, where you do things within thirty seconds, if not sooner, thirty seconds of air time is absolutely ages, so we have yet to get some sort of formal action on this, despite people saying that they approve.

Now, I think it's a good idea, because we've got, as you've implied, twenty, twenty-five, thirty years of science broadcasting. We have had everyone virtually this century, even Lord Kelvin (would you believe) recorded by a manner of means in 1909 or whenever it was. We have had Lord Rutherford, speaking in 1931. Anybody you can think of in the world of science has been on our programs saying, as a sort of primary source, what they think and how they did their work. This is a goldmine of material. And our continuing coverage of

this stuff is, I would have thought, the essential thing, not simply for carting information into young minds, but giving them, as you implied before, some picture about how they work. You know, they leap off boats wearing very few clothes, and they climb mountains, and they're very very active. They are, in many ways, like Indiana Jones, and they're not boring old farts with white hair. Nothing wrong with boring old farts with white hair, if you happen to be one, but science ain't like that any more, it's very very demanding. So we could give a picture of what science is like.

AM: Let's look at some of the other important things you're engaged in, which are all linked with this process of educating communities about science and looking toward the future. You're the Chairman of the Commission for the Future. What do you think it's achieved, and where do you think it's going?

RW: In the beginning it was set up because of a bright idea from Barry Jones. He wanted people in Australia to be able to debate the rate of change as affected mainly by technology. You know, he kept on saying, 'Well, in the old days when they invented cars and telephones, nobody knew what sort of effect it would have on life. The machines just came, and we changed our lives accordingly. Isn't it a better idea, especially at this time when you clearly have a revolution through micro-electronics and computers and all the rest of it, to say what sort of world we would like to live in, not just in Australia but anywhere, and some sort of debate about it.' This was the terrific idea. And Barry got the Commission created and he set it up. And I was a foundation member, as was Phillip Adams, Peter Mason, Leonie Sandercock and a few others. And I'm still there, I have been Chairman for two years.

The achievements in the beginning were necessarily a lot of (and perhaps too much) running around, and trying to do too many things at once. People did this thing called 'networking', and almost every other person I met seemed to be involved in some way with the Commission for the Future doing something, which I always found quite intriguing because I'd never met them before and never heard of the project. It was quite clear that the Federal government was going to cut us off and so we pre-empted it by saying 'Okay, you don't have to give us more than a limited amount of funds (less than a million dollars, progressively \$700 000 and then \$500 000) until we've been set up to earn money by doing specific research projects.'

AM: Do you think that the community has really registered the existence of this organisation?

RW: The community often expresses something called 'proud ignorance'. We have, for instance, made Suzuki, David Suzuki, famous in Australia; he is

enormously well known (sometimes to the chagrin of lots of conservative persons). Similarly, the Greenhouse Action—we filled up Dallas Brooks Hall in Victoria, and had other arenas chock-a-block. We also won international prizes for our publications; we have had tremendous success with the magazine *21•C*, which the minute it was launched sold nearly 21 000, and we've got state support throughout Australia for bringing that to every school in the country. Yes, there is a degree of success. But you can always find people who haven't heard of something.

AM: One of the striking things about the achievements of the ABC Science Unit has been the plethora of medals and awards, the media peace awards, and the Michael Daley Award for the Promotion of Science. The ABC has itself launched the Eureka Prize for the Promotion of Science. It's a very great achievement. And it would appear that, compared to other countries, Australia must rate very high in science communication. Is this so?

RW: That's what they tell me when I go overseas. When I'm going to the American Association for the Advancement of Science, I shall turn up and be greeted as one of the old hands. You know, the editor of the *New Scientist* will expect to see me there, various people from the *New York Times*, and so forth. But, funnily enough, there will be nobody there from radio, let alone television, in the United States. Extraordinary. And they have asked me to speak at places like MIT (the Massachusetts Institute of Technology) and elsewhere about how we do it, and how they never do. And I can't believe the paradox. I mean, whatever the state of American science at the moment, in sheer numbers and weight it is gigantic. And yet they have absolutely bugger-all in the way of broadcasting.

There are two very big science magazines. One of them is *Omni*, which is run by the people who brought you *Penthouse*. I'm glad they make *Omni*, it's a contribution, but it's a surprising thing that it should be done by, shall we say, the publisher of some of the greatest and most successful naughty magazines in the history of the planet. The other one is *Discover*, which I think is terrific, and is owned by Walt Disney. That nearly folded and that's how precarious it is. And an awful lot of their writing up of science in the papers is sort of back-page stuff, in a kind of ghetto, whereas our writing up of science is often right in the front of the paper, certainly in the middle, and if you're down the back it's because you've got a five-page section like the *Australian* newspaper today which has got about ten different pages devoted to science itself.

AM: So, given your exciting career in science, even though you may suffer from slight disillusion at times, would you encourage boys and girls to go into a career as a science communicator? There are a number of courses now being

offered in universities, and there's a Masters to be started at the ANU in science communication. What do you think the prospects are? Is this the best way to educate yourself to be a science communicator?

RW: I've always felt the best thing to do is to get a degree or some qualification in something, and then do your communication course. So that you have the confidence, as a historian, as a lawyer, as a scientist perhaps, and then you go off and do your communication. When it comes to getting actual jobs in science communication—well, there are very few of us, necessarily, and it's tougher and tougher to get in; but there are zillions of jobs, if you think about it, working for the *New Scientist* which is published in Melbourne, working for universities, doing promotion for CSIRO, for many of the companies which have got a strong scientific base, from BHP to CRA and so forth; and also, going beyond that, thinking as if the barriers don't exist.

It seems to me that even though you obviously will need conventional strong-focus training as a brain surgeon, as a mining engineer, or as a person designing planes, in the rest of society you have a breakdown of barriers such that being without a scientific background will be disastrous for you from now on. Whatever your job—you know, just mention any job, a cook, collecting garbage, you name it, being a clerk—even being a person unemployed sitting at home listening to the radio being told about how the climate is changing, you'll get depressed and almost suicidal unless you've got a background in science and you can think, 'Ah, I know what to make of what's just been said, so I am now free to go out and enjoy myself because I know what's going on.' But in these other jobs, if you're in nutrition, hygiene, you name it, in transport, let alone the necessity of being able to communicate effectively as a senior person perhaps in these jobs, it seems to me that science is absolutely fundamental.

AM: What about the role of museums, like the Australian Museum of which you're Chairman? In the nineteenth century, of course, museums were the great centres where the populace turned out and streamed through. They still stream through. Is this not a great educating force?

RW: Well, if you're a young person walking down the street in one of our big cities, where would you go to see science? You will go to places like botanic gardens and museums and they have been the immense growth areas in that public sense right across the world. Sometimes they're called science centres, and I'm quite pleased that during my time at the Australian Museum what we've tried to do is build a museum-without-walls (even though some of the walls have been there since 1827), and that means that it's got a direct

communication with the community. You've got trucks going round, or museums-in-a-box, going to schools in the remote bush. Those sorts of things have been built on, so that the barriers of this pompous old-fashioned science that belonged only to senior people who knew funny names has now become something where you can, for instance, take your own found object into a museum and have it, not just identified, but described by the person and swapped for something else. You know, you've got an interchange there. That's the concept of a museum-without-walls. And we've had the lecturers coming from all round the world. Some of the most famous people in science have been during my spell there as a part of a major program to bring science to the community.

AM: So, in summing up, in what so far—and for many years obviously ahead—has been a major contributing life in educating the Australian community to science (and in many ways you're a workaholic in this whole endeavour of communication and enlightenment), what do you see then as the positive things in the culture of science in Australia?

RW: Well, the positive things are two centuries of European excellence in education and tradition which could be used or could be squandered. And it [science] can be used effectively (and I hope this is the way we will go) by people realising that it's there and that it's mainstream and not simply something over there in a corner that funny people do. If we do make the most of that, then we can connect to our area, both the physical and environmental part of it (which we've got to look after and that can be done only through a scientific background), and also make connection with our neighbours who are very keen to be with us because their own traditions are not like that, they're Confucian. There is a damn good reason why the Chinese didn't invent the industrial revolution even though they had a technology. One of my great friends, Robert Hanbury Brown, has said in his book *The Wisdom of Science* that we call ourselves a scientific culture but that this is more true of our gadgets than our ideas.

Okay, connect the ideas, bring them mainstream, realise you've got them, don't be ashamed of them but make the mainstream be proud of them and convince the kids it's worth persevering with science. They don't want to be terribly rich, these youngsters, times have changed; but they want to be involved in something real. Now, either we go one way and consolidate the reality of science and our world, or else we become, I suppose, a southern supermarket.



Dr Michael Gore,

AM

Interviewed by Ann Moyal
Canberra, February 1993

National Library of Australia
Tape no. TRC-2906

MG: I was born in a small place in Lancashire called Farnworth, just north of the city of Manchester, in 1934. My father was an electrical engineer, a power-station engineer, which in many respects I suppose was my role model, because I, in later life, decided to be an engineer. My mother had no career, she was the classic housewife. And I was an only child. My mother certainly had an influence in encouraging [me] to go on beyond the normal secondary school education. It was mainly because my father had no formal training whatsoever and found in his later career that he was blocked continually from promotion because he didn't have a degree, and they were determined that that would not happen to me. So they spent a lot of time encouraging me to go on.

At that time in England when I was growing up, during World War II (I was five when World War II started), the way of getting into university in Britain was by going to a grammar school, usually. And the only way to get into a grammar school was by passing the Eleven-Plus Examination, which meant all children just over eleven would take this exam; and the sheep would go to the grammar schools which led into university and the goats who failed the Eleven-Plus would go into what were called the secondary modern schools and in general did not go to university. I was a goat. I failed the Eleven-Plus—not once, I failed twice, in fact—and ended up going to a small secondary modern school on the north side of Manchester, which was probably one of the bleakest parts in my career. It was a rough-and-tough school and it was a huge class of forty-five boys, there was a lot of bullying went on, the teaching was not of very high quality. Two years after I was condemned to this particular school, something happened which I've never been able to explain. I was normally about thirty-eighth in the class, when all of a sudden, at the end of one particular term, I shot to second in the class. Somehow at the age of thirteen I became what people called a 'late developer'.

AM: Why do you think you were thirty-eighth in the class? What were the factors that left you at that level, given that you had encouragement from your parents?

MG: I don't know. I can remember very clearly making a conscious decision once, to write the way it was and not write stilted English. My command of the written English language was not particularly good at that time. But I remember once, instead of writing the stilted English essay, deciding to put some soul and heart into it, and suddenly finding the mark for the essay was miles above where it had ever been before. And I felt something sort of let go within myself.

And it was about that time that I passed an exam to go into a local technical school. And that was one of the greatest markers in my early career, because once I got into that school I was in a class of twenty-five students, all of whom wanted to work, which was considerably different

from the secondary modern school I'd been at. Everybody was keen to succeed, and we had some marvellous teachers. In fact Alfred Whitaker, my mechanics teacher, had an indelible effect on my life. Not only was he a superb teacher and gave me a great love of mathematics and technical things, but he also demonstrated something that was very valuable in my later career, that teaching was a very honourable profession and that you could make a big difference to people if you were a good teacher. So, you know, even today, forty or fifty years on, I still remember Alfred Whitaker with great affection, and the way he taught and the way he made it fun.

AM: One hopes that the Alfred Whitakers of the world are still going into the school systems, because the evidence of a number of scientists seems to be that they've gone forward, often from quite unpromising starts into great careers, because of the influence of a teacher.

MG: When later in my career people used to sort of be disparaging about teachers, and that teaching wasn't all that important, especially when I started to teach at university, I used to take up cudgels with the opposite movement and say that teaching was extraordinarily important and that even at university one should put one's all into making the subject interesting by putting your little bit of theatre into your lectures and not having your students fall asleep on you as so often is the case.

The stimulus for this sort of approach that I gave right from the beginning of my university teaching career came from that one man. It was not so much what he taught me; what he did was give me a lot of the subjects so that I wanted to go out and find it out for myself. And I decided that, if I could do the same with university students, then I would succeed. It's not a matter of teaching them by rote, but giving them the love of the subject, letting them feel the excitement of the subject so that they couldn't wait to get out of the lecture-theatre and go into the library and read for themselves. And that's been my personal philosophy.

AM: So what governed your choice of physics, as well as electronics?

MG: I'd caught up with the grammar school people and I did the School Certificate at the age of fifteen, and passed, with the exception of one subject. And the one subject I failed at the School Certificate in 1950 was physics. And I laid that down to the fact that the person who taught me in that subject was abysmal, and I always hold him as a glorious example of what not to be.

I left Worsley Tech in Lancashire, and went to Bolton Senior Technical College, and there I spent three years doing the Higher School Certificate, which was the way in those days to get into university. And this was very

valuable, because I went at the age of sixteen into an education system, in Bolton Tech which was not very much different from a university.

This unlimited freedom at the age of seventeen proved a bit of a disaster. In fact I flunked out that year (I passed the exams, but it wasn't with any great success), but what I did was save myself the trouble when I went to university because I'd done my failing, so it was an ill wind that blew considerable good. I clawed my way into Leeds University, where the professor said (and I shall never forget his remarks), 'Gore, we're taking you as a bit of an experiment. You've come to university by such a curious route, we were anxious to see how you would go.'

When I first got to Leeds University in 1953 I was doing Electrical Engineering, in the first year all the electrical, mechanical and civil engineers, about three hundred of us were all lumped together, we all did the same lectures. And I remember thinking to myself 'Well, I've got to make a mark here.' Mainly because, when I went to university, the Lancashire County Council Education Authority, which gave the grants in those days, reneged on a verbal promise they'd made to my father that if his son got into university through the route he'd come, they would pay him a scholarship. They said no, I wasn't fit to go to university. I remember my father took the case to the then Minister of Education, who was Florence Horsbrugh, who happened to be the Member for Farnworth where I was born. And she looked at the matter and instructed Lancashire County Council to pay what turned out to be a very niggardly bursary.

This was, again, another ill wind that blew good, because I was determined to overcome this 'not worth going to university'. And it was at this point that some of my early organisational skills started to appear, because [*laughing*] I went into the Brotherton Library at Leeds, and pulled out all the examination papers in Engineering I for about twenty years, and found that all these papers bore a marked similarity. So I steadily worked my way through and it was a matter of plugging in different numbers from different angles. The result was that when I took that exam at the end of the year, out of 270 engineers I came third. But it was as much as anything by reading the examiner's mind and knowing where to look.

From then on I didn't look back. I remember talking to my professor about how I was going to go in finals, and he said, 'You know, it's tremendous that you've got this far! Even if you only get a Third, ...' And I said, 'I don't want a Third. I'm going to get a Second!' I knew I wasn't First-class Honours material, but I *knew* I was Second. I always remember that conversation. Anyway, I got a Second-class Honours degree. And all the time I was at university I worked in power stations up and down Lancashire, learning how to be a power engineer.

When I got my degree I decided to go on and do a PhD, and I moved over into the Textiles Physics Department at Leeds University. Leeds has a very big Textiles Department, which runs a whole gamut of subjects from how to

design cloth right through to textile physics and textile chemistry. My PhD topic was to look at some of the electrical properties of keratin. Keratin is your fingernails or your hair or wool, these are all materials called keratin. And the whole part of the thesis was in fact to build the electronic apparatus necessary to make these measurements. My supervisor who took me on, H.J. Woods, of Asprey & Woods fame, people who did tremendous work on x-ray spectroscopy at Leeds in the thirties and into the forties, out of the early work that the Braggs had done also at Leeds on x-ray spectroscopy,⁵⁶ H.J. Woods took me on because I was an electrical engineer who was interested in electronics. Well, I was going to have my first big let-down, and found that the electronics I'd learnt at university was of no practical value whatsoever, I couldn't build in the first instance. I had to start all over again and learn electronics from the ground up.

And that was another ill wind that blew some good, because three-quarters the way through my PhD I said, 'Well, looking back, I could really teach an electronics course. I know exactly how you would do it, so people would understand.' My number two supervisor was an Australian. And he suggested that I apply to some of the Australian universities. This was right at the beginning of the sixties, when Australian universities were starting to blossom. And one day he walked into the lab and said, 'Here's an advertisement from the Australian National University. They're looking for a lecturer in Physics, with responsibility to start and develop electronics courses.' So I applied. I also applied at many other places around the world in 1961. So I applied to a couple of American universities, and to my amazement was accepted in the Electrical Engineering School at Brown University in Providence, Rhode Island, to go and do some research work and some post-doctoral studies and some teaching at Brown. And the following day the ANU offered me a post. So I accepted that and flew to America, stayed for nine months, and then came to Australia.

AM: Were you then beginning to get a feeling that there was a need for the promotion of science? How did you see the culture of science in those days?

MG: I wasn't looking out beyond the university in those days, what I was doing was developing my lecturing skills. And I suppose there's always been a touch of the performer behind what I've done and I decided that, you know, without hamming it up, one could really make lectures entertaining. It would take me some considerable time to recover from my early physics teaching, so when I started to take on board other parts of physics other than electronics, I was learning it for the first time. So I was going into my students with the enthusiasm of a recent convert. Now, that made me considerably different from a lot of my peers, who had done this many years before and it was old

hat. I mean, I was learning atomic physics right then and there in the evening and going in the following day and saying, 'Hey, have I got a story!'—you know, and tell the story like that. Well, it went over big. I enjoyed myself, they enjoyed themselves, and we learnt a lot. And when I didn't know the answer, I'd say, 'Well, I don't know the answer, I'll find out', and I'd chase off. And that was tremendous fun. I'm still having a whale of a time because you never stop learning of course. My enthusiasm in my lectures was pretty well defined, and I'd got something of a reputation for being an interesting and entertaining lecturer. And I use the word 'entertaining' advisedly.

The turning-point for it all came, and I didn't see it coming at the time, when the prep room, the preparation room in the Physics Department [ANU Faculties], got into such a mess. We had all these cupboards where we kept all the apparatus we used for lecture demonstrations, and you'd frequently come down at quarter-to-nine for a nine o'clock lecture and find the bit of apparatus you wanted was either broken or not there or part of it wasn't there. And we all used to commiserate and complain to each other over tea in the Physics Department, and everybody agreed that one of us really ought to take time off (like six months!) to sort it all out. Well, that situation went on for several years. And then I blew my stack one day and said, 'To hell with it!' And I went downstairs and I pulled stuff out, and I set it up and got it to work, fiddled around, got more stuff out. And eventually I had a whole series of tables around the place with about thirty or forty different experiments all set up and running, demonstration experiments.

Well, I never saw it at the time, it was only looking back. When people say 'When did it all start?' (this is the National Science Centre and Qwestacon). It started on a day when a Catholic school in Goulburn rang up and made a request, could they bring their Year-12 girls round the Physics Department to see what was going on, what was being done in research. So, always having had the gift of the gab, the job was tossed to me to take them around. And I took them round the various research laboratories, and explained in everyday language what a shock tube was, for high-temperature gas research, it was like a big bicycle pump going 'kphow!' And I showed them all the various things about lasers, and explained briefly what the research was. And at the end they said, 'Well, what else is there to see?' And I thought, well, short of showing them lecture theatres, there isn't ... Then I thought 'Wait a minute! There is! Yes, there is!' And I said, 'Come with me!' So I took these twelve girls, and the Mother Superior who was with them, into the prep room. And I went up to the first demonstration, I showed it to them, and I said, 'Look, try it for yourself! Here, you take this piece of aluminium, and you try it!' It was an eddy current experiment, with a very large electro-magnet. And these girls said

'Aluminium is not magnetic.' And I said, 'Isn't it? Well, you try it.' So they each tried it in turn, and they were quite taken with this. So I said, 'Come and try this one!' and went to the next experiment. By the time I got to about experiment number ten, I found I'd only got two girls with me and the others were strung out behind me, all having a whale of a time.

And so that day—and I don't know when it was, but it must have been about 1972 I would guess, possibly 1973—was the start of the Questacon.

In late '75 I went on sabbatical leave for the second time to Holland. I'd been to Holland in 1970 and worked with Philips to learn more electronics to bring back to my students in Canberra; and I changed my whole course between 1969 and 1971. And on the way over, myself and my family, we stopped in San Francisco for about two days and it was there that I saw a tiny advert in the newspaper which said:

Palace of Fine Arts
Hands-On Interactive Science Center

and I said, 'Let's go!' And there was a great growl of disapproval from my three children, who were then eight, six and four, and who didn't want to know about this. And my wife didn't look all that impressed. Anyway, I got them into this place, kicking and screaming. And three hours later I had to drag them out kicking and screaming.

Subsequently I was carrying messages backwards and forwards across the Pacific from Oliphant to Oppenheimer and back again. I thought to myself 'Well, it's not surprising that it really appeals to me, this place, because I'm an engineer and a physicist.' But it did appeal to my wife and my three children, vastly different ages, vastly different interests. I thought 'This place has got something.'

So I came back to Australia in '77 and started to tell people what an exciting place I'd seen. And eventually my great friend and colleague Chris Bryant, who is a Professor of Zoology, said, 'Why don't you stop talking about it and do something about it?' and in 1978 I wrote a submission to the Commonwealth Schools Commission, got some funding, and off we went. [Questacon] opened its doors in the Ainslie Infants' School, with about a dozen exhibits in September 1980.

AM: It was obviously an extremely enterprising venture, and many volunteers were involved in getting it going, weren't they?

MG: Oh, very many. The thing that marked the Questacon was the help it got from so many different areas of the Canberra community. The ANU watched it from a distance, kept it at arms length, I think the top brass of the

university certainly were a little bit wary of it. By the middle to the end of the eighties when they saw what it developed into, became very proud of it of course. But there was still this gentle ... I could feel it, above me, you know, and even around me, from other academics, 'This is a bit odd, popularising science, Gore! Oh, a bit difficult.'

The whole of Questacon is based on education, not the formal education as in secondary school and the universities, tertiary institutions, but the broad education of the masses, everybody, from eight to eighty. Now, if I were to give up my teaching—which I loved, my lecturing—then I would not be true to myself. Of course the Physics Department is doing a lot of research, and the workshop is supporting that research. It was also supporting the early Questacon. Now, if I had marched out, all my support would have gone out of the workshop as well.

AM: So where did you recruit the volunteers?

MG: I held a public meeting. I can't remember whether I advertised in the newspaper, saying that I was going to start this interactive science centre and I was looking for interested parties within the Canberra community and I was going to hold a meeting in the Physics Department lecture theatre on a given night. There are people in Canberra at this time who still remember the day and date of that meeting. I can remember the meeting, there were about twenty or thirty people turned up. One of the great supporters in the early days and for a longish time was Harry Taylor Rogers, an ex-petroleum engineer. And Ron Hewitt, who was an ex-naval architect.

I explained to them what the role of the explainer was—just to bring the human face to science, not to actually talk to the visitors and give them 20-minute lectures but just to talk to them generally and to tell them anecdotes about the various things, and to link these rather curious devices which we had (I mean, they weren't exhibits, they were devices, 'props' as I preferred to call them later), to link them to the everyday; so that when you started to whirl this stick around and you found this curious effect, somebody would say, 'You felt that, did you? Now, that is the principle behind the helicopter ...' or whatever it happened to be.

I got about twenty of them to sign on as explainers. And then they suggested, amongst them, that I should advertise to some of the professional organisations. So I wrote out to all the retired engineers in Canberra, all the retired physicists, retired CSIRO-type people, and invited them to come along to some subsequent meetings.

Somewhere about 1983 or '84, I must have hit critical mass, as the nuclear physicists say, because it took off by itself. And my explainers—and this is still happening today, with the new National Science and Technology

Centre (which, incidentally, uses the nickname of 'Questacon', and all its programs are 'Questacon this' and 'Questacon that', we use that as the trademark now, it's too good a word to lose)—but amongst the explainers I find new faces on the floor all the time. And what's happening, they go home, they go to the clubs, and they talk to other people who say, 'That sounds interesting. I'd love to do it!', and they say, 'Well, come along and join us!'

Some people are not scientists at all. You don't *have* to be a scientist to be an explainer. What you need predominantly, to be an explainer at a science centre (and there are now a ring of science centres round Australia, I'm proud to say, which all took the lead from Canberra), is an interest in the world around you. We divide devices which the public can use—with their eyes, their ears, their hands, whatever—so that people get a feeling for a branch of science, and then the explainers in fact really explain how it fits in.

AM: This is the central problem in science communication. You indicate that perhaps the higher echelons of the ANU were a bit wary about popularisation of science, scientists are always a bit wary about colleagues who go out and attract the press and popular attention, yet we're always taking the view that they must be made into better communicators. But, in the end, you find this a problem.

MG: Well, certainly there is a feeling amongst the scientific community (though I think it's starting to change now) that if you try to talk to the general public in terms they can understand that in fact you are demeaning science. This is not the case. And if you look back to the Royal Institution which was founded in Britain, in London, in the late 1700s, one of its charters was to do just this. And of course this is one of the most august bodies in the world, and their charter was (and still is) to purvey science to the general public. And I've looked to the Royal Institution as a role model for what we're doing here in Canberra.

AM: Yes. And to bring children into the interests of science. As a historian, I've been always very interested to discover that of the great colonial exhibitions of the nineteenth century, the one in 1879 in Sydney attracted one million people, and then a later one (which was at the Melbourne Centennial Conference I think in 1888) attracted two million people, with a small population. I've always thought that 'people science' was very much stronger in the nineteenth century than it has been in the twentieth century, until recently with these sort of developments that you've helped to spur.

MG: Back in the nineteenth century you were in the stage of the proton and the neutron and the electron. What's happened is that we've moved up in this

century and we've come out of the Second World War and we've come into the fifties and the sixties, and there's been a fantastic escalation of knowledge in all branches of science and technology. It has taken off with exponential growth, it's phenomenal.

Now, as a result of that, I think that far less attention has been paid to what was being done in the last century by way of purveying this. There aren't by any means the number of people like Robyn Williams, who talks to the public over the radio and has the power of the radio behind him. So that's what has changed a lot.

AM: But there is a problem, and I imagine you've encountered it in Questacon and in the National Science Centre which Questacon has become, and that is that it's very difficult to present all aspects of science. I guess there's a concentration on the physical sciences; whereas chemistry might be a bit dangerous to portray; biological sciences would present another set of problems. Or have you managed to get a good balance between the different disciplines?

MG: Well, the balance is certainly better now than it was when we started, back in 1980. But you've got to bear in mind that when it started there was very little money, and your props do cost money to construct or keep running. So I decided very early in the piece that we should use very simple things which could be repaired—by me, if necessary, and very frequently were—and with a band of very eager helpers we didn't need any skilled technicians for some of the early things. Expediency dictated that we stuck to physics, because it provided so many simple and cheap things to do.

I felt rather embarrassed that some of the very early things in Questacon were so simple. And then a very interesting thing cropped up. The visitors' book we had in the foyer started to record time and time again people saying 'What we love about this place is the simplicity of the things here.' And I thought 'We've hit on the winning formula through financial expediency!' It never occurred to me that this would be what people really like. I thought, naturally, that people would like big glossy things which were shiny or huge and highly technical and sophisticated. One or two people do, but the vast majority prefer these simple things. And we tried to stick with that.

The metamorphosis comes between the Questacon of ANU, and Questacon of the National Science and Technology Centre. In 1982 the Bicentennial Authority set up a task force to look at how we might celebrate our bicentennial as far as science and technology was concerned. After a lot of thinking and looking at all sorts of ideas that had been generated, that we ought to have a *national* science centre, and it 'should be not a museum but

an interactive science centre like the little place the ANU has got called Questacon.' The next thing, they set up a committee. I was pulled onto that committee, the committee was charged by the Bicentennial Authority with looking at how and what could be done. That committee recommended to the government that there should be such a thing set up for 1988, that it should be interactive, and that it should be in Canberra.

Sir Ian McLennan, who has just retired as the Managing Director of BHP, came to the very first meeting of what was called the Green Committee (that was Roy Green, who was Deputy Secretary of the Department of Science, under Barry Jones who was Minister for Science at that time). We held the first meeting in Canberra and I was at that meeting, and it was decided that yes we should have a national science centre, it should be in Canberra. And part of the way through the meeting I always remember Sir Ian McLennan said, 'What exactly is an interactive science centre?' And the chairman Roy Green said, 'Mike, could you take Sir Ian to the airport, on his way back to Melbourne, and show him Questacon as you pass through Ainslie?'

Well, at the next meeting, when Roy Green was reading over the minutes, he came to the three or four decisions we'd taken. And when he got to the bit about 'it should be sited in Canberra'. Sir Ian said, 'I don't recall us taking that decision.' And though he never said anything, it was fairly obvious to myself and Roy Green that what had happened was that he'd seen the Questacon, realised its importance, realised its excitement, and what I hadn't seen at the time was the fact that Sydney looked like it was in the running to get a large amount of money for its National Maritime Museum from the United States, and Melbourne had its nose out of joint at that time and wanted something big in Melbourne. And what they were seeing was to build a national science centre in Melbourne.

That actually was another ill wind that blew some good; because I then went out on a limb to make sure as early as possible that this was a national science centre for the nation, not just Canberra. Barry Jones took the Cabinet submission up in 1985, and it went through and in 1986 they started construction.

But, following on Sir Ian McLennan's comments about 'it should be truly national', I had already started training student-explainers at Questacon to give science demonstration shows that I'd pioneered. And by early 1985 I had a team of I think six boys and six girls (six undergraduate women and six men) who were all giving science demonstration shows and getting better and better at it. And I was absolutely amazed at how they were growing in stature at communicating with the public. We got the push we needed when 2GN, the radio station in Goulburn, invited us to take the Questacon, or as much as we could put in a furniture van, to Goulburn in

April–May of 1985. One weekend we transformed one of the primary schools in Goulburn into a miniature version of Questacon. That was the Science Circus and that was the first move to making it something that went over the nation, all of the nation. Right now, in 1993, we've probably got three-quarters of our effort throughout Australia.

AM: It's a fantastic venture. As a Churchill Fellow, while you were at the Questacon, you inspected science centres abroad.

MG: A lot of the input came from my comments on what I'd been seeing overseas.

AM: Which was the most inspiring of the science centres, from your point of view?

MG: The Exploratorium [San Francisco]—it always was. The Ontario Science Center also was a tremendous place, and still is, one of the biggest and brightest of the stars in the firmament of international interactive science centres.

What happened was that the Bicentennial Authority—I believe that what they did was to talk to a lot of other nations about what they might give us as birthday presents. They talked to the Americans and the French and well, they talked to everybody. And in fact it was a two-way dialogue. Once it was made known that Australia was going to celebrate its 200th anniversary of white settlement, then other nations said, 'Well, what would you like as a birthday present? What's appropriate?' Somebody in the Bicentennial Authority (well, I assume it was somebody in the Bicentennial Authority) suggested that the Japanese should be the people who should sponsor the National Science Centre. Now, the Government took the line that, if the Japanese came in to sponsor this building in a major way, the Australian government would back it. On the other hand, the Japanese said, 'We will come in, providing the Australian government shows good faith and they back it.' So we had a bit of a Catch 22, a Mexican stand-off. And that was sorted out by Barry Jones.

AM: His role, I presume, was very important in the whole development.

MG: Very. It was crucial. If it hadn't been for Barry persuading Cabinet to do it, it wouldn't have happened. Because once he got Cabinet approval, then the Japanese came in on side and it was all away, everybody was happy then.

What happened with the money was that the Japanese made a commitment of \$10 million—they gave the job to the Keidanren, which is a sort of a club of the top industrial concerns in Japan, and this club was organised to pull \$5 million from industry, certain sections of industry. The

government said, 'If you can provide \$5 million from industry, the Japanese government will match it with another \$5 million.' And that's what happened. They came up with \$10 million. The Bicentennial Authority chipped in with \$5.4 million dollars (and of course that's Australian government funds, because that's where they got the money from), and the rest came in dribs and drabs from all over the place. There was about \$300 000 came from Australian industry.

The main problem was, of course, that there was (and there still is, I suppose, to this day) a great antipathy throughout Australia to Canberra. I mean, everyone sees Canberra as nothing more than a House of Parliament. So the tactic that I've adopted, and that the National Science Centre has adopted, right from the word go, was to go to the big companies and say, 'Now look, if you sponsor something you obviously want it to have as wide a visibility as possible. So what we propose is this. If you give us the money to build this exhibition, we'll run it in Canberra for twelve months, and then we, the National Science Centre, will undertake to move it right round Australia with your name on it and our name, national coverage.' Now, that tactic, until the recession arrived just recently, that proved extraordinarily successful.

AM: Could you name the company which has been the most interested in terms of the funding?

MG: I wouldn't like to say the most interested. I'll name you the major ones who have been in with us, in a major way. The first one that came in was Shell, because Shell looked over our shoulder at the fledgling Questacon Science Circus, and Shell were looking for a bicentennial project. It's worthwhile saying that IBM, OTC and ICI were three other major companies who produced for us with their funding very big exhibitions which are now out and around Australia. We pulled about \$5 million worth of sponsorship in, during the first five years of operation. Which is a lot of money.

AM: It's obviously been a most successful project, and I note that in 1992 you were awarded the Eureka Prize for the Promotion of Science, which is one of the Eureka Prizes which the ABC gives, and that was also given not only for the promotion of science but for 'the impact on education'.

MG: The Eureka Prize was awarded to me and the National Science Centre, and not just simply to me. It was a sort of joint award, for the promotion of science.

AM: Let us get to this central question of the enthusiasm of small children for science. What always interests me is the way very small children ask very pertinent questions about the world around them, in the sense that they are

without knowledge and they ask rational sounding questions. And very often their parents have no answers, or they find the whole thing tiresome, or they're embarrassed, they don't know what that hole is about or why those animals are dying because there are too many of them, or what about the stars in the sky. So what happens along the way (because there's a general argument that there's not a wide interest in science), what happens along the way to this very innate interest in the world around them that children have, to a point where they're no longer the slightest bit interested in science? And how do we pick it up?

MG: Well, firstly not all of them *do* lose interest in science, some are more and more dedicated and get turned on as they get older. Children naturally, like any small animal have an insatiable curiosity, they want to find out about *everything*. However, my answer to this—and I think most schoolteachers would agree with me, especially secondary schoolteachers—is that when they get to a particular age all their interest in investigation sort of funnels down to one particular area—on each other.

AM: Yes. And yet the gender differences show at this point. You're probably less likely, in the process of acculturation, to get little girls going on with a keen interest in science than you are to get little boys.

MG: Well, that of course is because, to use the words of *South Pacific*, Oscar Hammerstein's words, 'you've got to be carefully taught'. But little girls have been carefully taught now, for many many centuries, what their role in life is, and certain areas have been left out, deliberately, by certain people. I mean, let's not lay the blame at anybody's doorstep, but just look around. I mean, little girls were told by just about everybody—their mothers, their schoolteachers—that they would do domestic science, when the boys were doing physics. The girls were dragooned, for a long time, through our education system both here and in the United States and in Western Europe, into doing things which people felt were more 'suitable for girls'.

AM: The nurturing roles, yes.

MG: Yes, that's right. Now, there is a big difference, I've come to the conclusion, between boys and girls as far as science is concerned. And that is that girls in general (in general, and this is another sweeping statement) are far more people-orientated than boys are. This shows up very heavily in the Science Circus team, the Science Communication Scholarship scheme, that was started in collaboration with the ANU. Science graduates from all over Australia come and learn about communicating. It's a post-graduate one-year

graduate diploma course. And this year is a classic example; there are eight women and four men. It appeals far more to women than it does to men.

The fact that girls have not gone into science is for a whole stack of reasons. Firstly, they're almost brought up from the cradle, many of them, to believe that science is not for them, by a whole raft of people and quite often including their parents. I mean, it may well be unwittingly done by some fathers who, because they had a rough time with science themselves at school, have tried to steer their daughters into some other subject which would be safer and more profitable. You know, 'Oh no, you don't want to do science. I had a hell of a time with it, and you wouldn't enjoy it, learning formulae ...'—or some other deprecating remark like that, which is totally untrue but will turn them off onto some other path. Now, that's been going on for a long time.

Quite often the boys, by a certain stage, tend to push everybody, including their own male contemporaries, out of the way, and the biggest bull gets to the front of the herd. And girls are not interested in doing that sort of thing, that's not their scene. And the girls stand back with a slight smirk and say, 'Well, let 'em go! What the heck.' And if you are competing to get into a particular course in science, it may be 'Why bother?'

AM: So that, in the exhibitions, do you find there's much difference in the response of girls and boys?

MG: No, not at all. They are both equally fascinated. Once they get onto common ground like that, they both are equally enthusiastic about what's going on.

AM: Do you think, after the Questacon and the Science Centre has been going now for a number of years, that the level of understanding and feeling and the sense that science is fun has been spread quite deeply and widely in the community?

MG: I know it has made a marked impact on quite a number of people, and for every one letter or person who rings up and tells us there must be another ten or twenty who won't bother. So we're obviously having quite an effect.

I know one story. There was a boy who came to Canberra in a bus trip from Sydney, who didn't want to come, complained all the way, had been very much a disruptive influence at his school for about two years before; having got into the Questacon, he was in about half an hour when his teacher said it was almost as if somebody had flicked an electric light switch, he suddenly became very quiet and decided to check things out. Within two years he was a BHP Science Prize finalist, and he was at the top of his class in just about every subject. His teacher said, 'I saw it happen! It was absolutely amazing!'

I know several people who have changed their careers. I know some of the girls who have been Questacon explainers have changed from deciding to do an Arts degree to doing a Science degree, or gone into engineering or something like that.

AM: Now, as one of the inspired science communicators in this country, and with tremendous enthusiasm and the sense of excitement, what do you think are prime ingredients in these courses that are springing up in science communication? What is the best way to approach training in science communication? You've come at it from your own instinctive methods and your personality, and trial and error, and 'ill winds' and so forth; but if you were doing it in a formalised way, what would be your advice?

MG: Well, what we've been doing for the last five years at the ANU is starting to formalise but in the early days of Questacon the whole thing grew like Topsy, and what we've done now in the course is to look at some of the ways into communication. Obviously the human voice is the main one, and we train the students in a highly practical way in public speaking. So we train them in the art of public speaking, eyeballing your audience, how to speak, how to lend a little light to what you're saying by moving your arms, moving about a little bit. We teach them about radio, television, work interviews, giving interviews, taking interviews. We get people from RMIT [Royal Melbourne Institute of Technology], from the school down there on journalism, to teach them to open their eyes to some of the problems of writing, say, for *New Scientist* or magazines like that, or for newspapers—not writing lab reports. We're talking about writing for the people out there in Australia.

So we're highly practical. It's about the actual mechanics of doing it. When they go out on the road, they go out to Aboriginal settlements and have to change the vocabulary. I mean, when you get out to an Aboriginal settlement in the middle of Cape York Peninsula there's no point in starting to talk about traffic lights, because they don't have any traffic lights and they've probably never seen any traffic lights, it's not relevant to what they're doing. So you've got to take the science you've been talking to the people in Rockhampton about and try and talk about the same science but using different language and different ideas. Great training! You're on your toes again, you're in front of these Aboriginal children, you can't fluff it, you've got to do it.

We teach them about talking over the radio, because we involve them with School of the Air, around the country. And we do all sorts of projects about getting them to describe things.

AM: So that this is one career path for people trained in science communication. Of course there are other career paths.

MG: One of the big areas I see for these people is to come and be the translators for science, to stand between your science researcher and the general public. Your science researcher is not paid (and usually does not have the expertise, nor should he or she) to give public broadcasts and lectures, particularly an interesting and entertaining one about their subjects. They're paid really to do high-class research. Now, if you get a second group of people who can turn to these researchers and say, 'I speak your language, tell me what you're doing', they will assimilate what they're doing and then turn round to the general public and explain it, and take the time and the trouble to put it in the phrases and get at the analogues and draw the mental pictures.

The other role that I see for them is in industry. You've frequently got people in the various industrial concerns, like BHP and ICI or whatever, who interface with the public. For instance, whenever anything goes wrong they've always got their spokesperson who is their PR person. Now, wouldn't it be better if the person who goes on television to talk about the problem that they had with something which went wrong etcetera (and I don't just mean the bad things, I mean the good things as well), if you had somebody who'd been trained in communication who could mix it with the television interviewers, and get the message across and say, 'Now, this is what the science is all about.' So I'd like to see all the topline public-interface people of all the topline engineering and chemical and scientific companies around Australia by the year 2010 staffed with these communication people.

AM: Well, in looking back over your very creative career, with all its ill winds and its successes, would you do it again?

MG: Oh, yes, I've been extremely fortunate in my life. I have, if you like, turned a hobby, a great love into in fact a job. I love teaching, I think teaching is one of the greatest of all tasks entrusted to us human beings, teaching other people about the world about us; and to be able to get to a position where I'm actually paid to do it, is wonderful.

I'm a great believer in the conservation laws that, you know, what goes up must come down, what you put in you get out and you don't get something for nothing.

Notes

- 1 See Ann Moyal, *A Bright & Savage Land: Scientists in Colonial Australia* (Sydney: Collins, 1986; Ringwood: Penguin, 1993); R.W. Home (ed.), *Australian Science in the Making* (Melbourne: Cambridge University Press, 1988 and 1990); and Roy MacLeod (ed.), *The Commonwealth of Science. ANZAAS and the Scientific Enterprise in Australasia 1888–1988* (Melbourne: Oxford University Press, 1988).
- 2 Ann Mozley [Moyal] 'Oral History', *Historical Studies of Australia and New Zealand*, 1967, pp. 571–8. This paper with its account of the pioneering Columbia University Oral History Program is, reportedly, the first paper on oral history published in Australia.
- 3 Sir Macfarlane Burnet, *Changing Patterns: An Atypical Autobiography* (Melbourne: Heinemann, 1968) is one rare exception. Gavan McCarthy, *Guide to the Archives of Science in Australia. Records of Individuals* (Melbourne: D.W. Thorpe, 1991) points to a few unpublished ones.
- 4 *The Hazel de Berg Recordings*. With an Introduction by Tim Bowden (Canberra: National Library of Australia, 1989).
- 5 Ronda Jamison, 'Moral History: The Conflict Within', *Oral History Association Journal*, 1991, no. 13, p. 24.
- 6 An exception was Helen Newton Turner's mother who won a University of Sydney Medal in Philosophy and French in 1901.
- 7 Harriet Zuckerman, *Scientific Elite: Nobel Laureates in the United States* (New York: Free Press, 1977) and R.K. Merton, 'The Matthew Effect in Science', *Science*, 5 January 1968, vol. 159, pp. 56–63.
- 8 The Council for Scientific and Industrial Research (CSIR) was established in 1926 and renamed the Commonwealth Scientific and Industrial Research Organisation (CSIRO) in 1949.
- 9 William Bragg at the University of Adelaide (see note 56) and T.H. Laby at the University of Melbourne are two cases. Cf. R.W. Home, 'The Physical Sciences: String, Sealing Wax and Self-Sufficiency', in Roy MacLeod (ed.), *The Commonwealth of Science*, op. cit., pp. 147–65.
- 10 Cf. Ann Mozley Moyal, 'The Australian Atomic Energy Commission: A Case Study in Australian Science and Government', *Search*, 1975, vol. 6, pp. 365–84.
- 11 Cf. Ann Moyal, 'The Australian Academy of Science: The Anatomy of a Scientific Elite', *Search*, 1980, vol. 11, pp. 231–9 and pp. 281–8.
- 12 D.D. Millar (ed.), *The Messel Era: The Story of the School of Physics and its Science Foundation within the University of Sydney, Australia, 1952–1987* (Sydney: Pergamon Press, 1987).
- 13 R.W. Home, 'The Physical Sciences', op. cit., pp. 160–1.
- 14 Stewart Cockburn and David Ellyard in their *Oliphant. The Life and Times of Sir Mark Oliphant* (Adelaide: Axiom Books, 1981) discuss some differences between these two leading physicists.

- 15 Cf. Woodruff T. Sullivan III, 'Early Years of Australian Radio Astronomy', and S.C.B. Gascoigne, 'Australian Astronomy since the Second World War' in R.W. Home, *Australian Science in the Making*, op. cit., pp. 308–73.
- 16 Cf. F.C. Courtice, 'Research in the Medical Sciences: The Road to National Independence', in R.W. Home, *ibid.*, 1988, pp. 277–307 and Ann Moyal, 'Medical Research in Australia: A Historical Perspective', *Search*, 1981, vol. 12, pp. 302–9.
- 17 J.C. Eccles, 'My Scientific Odyssey', *Annual Review of Physiology*, 1977, vol. 39, pp. 1–16.
- 18 J.C. Eccles, 'Under the Spell of the Synapse' in F.G. Worden, J.P. Swazey and G. Adelman (eds), *The Neurosciences: Paths of Discovery* (Cambridge, Mass.: MIT Press, 1975), pp. 158–79. Advanced plans to interview Sir John Eccles for this volume halted with his ill health soon after his ninetieth birthday.
- 19 M. Fraser, Government Approaches to Science. Address to the Australian Academy of Science, Science and Industry Forum, 1 March 1969. Canberra: Australian Academy of Science, 1969. And for a review of science policy developments see Ann Moyal, *Science Policy and Technology Assessment: New Directions in Australia*. CIRCIT Policy Research Paper no. 3 (Melbourne: Centre for International Research on Communication and Information Technologies, 1990).
- 20 Joseph Ben-David, *The Scientist's Role in Society: A Comparative Study* (Englewood Cliffs, NJ: Prentice-Hall, 1971).
- 21 Albrecht W. Hofmann introducing Ringwood for the 1991 V.M. Goldschmidt Award, *Geochemica et Cosmochimica Acta*, 1992, vol. 56, p. 4333.
- 22 Quoted in Alec Bolton, *Interviewing for Oral History at the National Library of Australia: A Short Guide* (Canberra: National Library of Australia, 1994).
- 23 Lord Rutherford (1871–1937), New Zealand physicist, became the most prominent experimental physicist of his time.
- 24 Vannevar Bush, *Endless Horizons* (Washington DC: Public Affairs Press, 1946).
- 25 Oliphant was a member of the British MAUD Committee on atomic research, 1940–41.
- 26 Oliphant was one of the 'four wise men', including fellow Australian expatriates Sir Howard Florey and Sir Keith Hancock and New Zealander Raymond Firth, appointed by the ANU planners to form an interim academic advisory committee in London, in 1946, to advise on the new university.
- 27 Sir Howard Florey (1898–1968), a medical graduate of the University of Adelaide, Professor of Pathology at the Sir William Dunn School of Pathology, Oxford, and joint winner of the Nobel Prize for his work on penicillin, had himself been invited to become Director of the ANU's John Curtin School of Medical Research which he finally declined.
- 28 Cockburn and Ellyard, op. cit.
- 29 John Carver, Professor of Physics and Director of the Research School of Physical Sciences, Australian National University, 1978–92. Cf. Hazel de Berg interview with Professor Carver, National Library of Australia, deB935–6.

- 30 Sir Philip Baxter (1905–89) came to Australia in 1949 as Professor of Chemistry at the NSW University of Technology (later University of NSW), was appointed Vice Chancellor in 1955 and from 1957 to 1972 was jointly the influential Chairman of the Australian Atomic Energy Commission (AAEC).
- 31 Sir Ernest Titterton (1916–90), was a member of the Manhattan Project and took part in the Bikini atomic weapons tests in 1946. He came to the ANU as Professor of Nuclear Physics in 1950 and later became Director of the ANU Research School of Physical Sciences 1968–73. He was one of Australia's most vigorous advocates of nuclear energy.
- 32 Anthony George Michell (1870–1959), an engineer and inventor, devised the revolutionary Michell thrust-bearing in 1905. He was elected a Fellow of the Royal Society of London in 1934.
- 33 Dorothy Hill was elected in 1956 and remained the only woman Fellow until 1969 when Melbourne physiologist Molly Holman and mathematician, Hanna Neumann, were elected.
- 34 Professor David Rivett, Chief Executive Officer, Council for Scientific and Industrial Research (CSIR) 1926–46 and Chairman 1946–49. Sir Ian Clunies Ross, veterinary scientist, joined CSIR in 1931, became a member of CSIR executive in 1946 and was Chairman of CSIRO 1949–59. Sir Frederick White, joined CSIR in 1941, was appointed Chief Executive Officer in 1949 and was Chairman of CSIRO 1959–70.
- 35 J.L. Pawsey (1908–62), Assistant Chief of the Division of Radiophysics from 1951, had contributed to wartime radar development and radio astronomy, and joined the CSIR Division of Radiophysics in 1945.
- 36 Paul Wild was awarded the Royal Medal of the Royal Society of London for his work on Interscan.
- 37 Later in the full version of the interview, Dr Wild recalls that the date was 1983.
- 38 Similarly, this date should be May 1984.
- 39 Bob Dun, veterinary scientist, was Director-General of AIDAB 1983–93.
- 40 Sir Macfarlane Burnet (1899–1985), Director of The Walter and Eliza Hall Institute of Medical Research, Melbourne, was a joint Nobel Prize winner in 1960 for his study of acquired immunological tolerance.
- 41 Professor Frank Fenner, Professor of Microbiology 1949–67, John Curtin School of Medical Research 1967–73; Director, Centre for Resource and Environmental Studies 1973–79.
- 42 Elizabeth Truswell, 'Antarctica: A History of Terrestrial Vegetation' in R.J. Tingey (ed.), *The Geology of Antarctica* (Oxford: Clarendon Press, 1991), pp. 499–537.
- 43 Heitler was a specialist in quantum field theory, Hamilton in nuclear theory, and Sygne, a cousin of the Irish playwright, worked on relativity theory.
- 44 H. Messel, *Science for High Schools: An Integrated Four-year Course* (Sydney: Nuclear Research Foundation, School of Physics, Sydney, 1965).

- 45 The tectum of the frog's brain corresponds to the superior colliculus in the mammalian brain.
- 46 Sir John Eccles (b.1903), a graduate of Melbourne University, was a Rhodes Scholar and later research fellow at Oxford 1925–37, Director of the Kanematsu Institute at Sydney Hospital 1937–43, Professor of Physiology at the University of Otago, New Zealand 1944–51 and subsequently Professor of Physiology at the John Curtin School of Medical Research, ANU until 1965 when he moved to the USA. He was awarded the Nobel Prize for Physiology and Medicine, jointly with A.F. Huxley and A.L. Hodgkin in 1963 while at the ANU, for his work on the ionic mechanisms of synapses.
- 47 Norbert Wiener, the father of cybernetics.
- 48 Professor David Curtis, Director of the John Curtin School of Medical Research 1989–91, and President of the Australian Academy of Science 1986–90.
- 49 Victor Moritz Goldschmidt (1888–1947), leading international figure in geochemistry.
- 50 'Phases' refers to phase transformations representing a convergence of two materials under pressure to form another material.
- 51 John Jaeger, founding Director of the Department of Geophysics, ANU.
- 52 David Green became Director of the Research School of Earth Sciences at the ANU in 1993.
- 53 A.E. Ringwood, *Composition and Petrology of the Earth's Mantle* (New York: McGraw-Hill, 1975).
- 54 *Environmental Consequences of Nuclear War* (Chichester: SCOPE ICSU; New York: J. Wiley, 1985).
- 55 A.V.S. Hill and S.W. Serjeantson (eds), *The Colonization of the Pacific: A Genetic Trail* (Melbourne: Oxford University Press, 1989).
- 56 Sir William Bragg (1862–1942) and his son Lawrence Bragg (1890–1971) shared the Nobel Prize for Physics in 1915. William Bragg began his scientific career in Australia as Professor of Physics and Mathematics at the University of Adelaide from 1886 to 1908; Lawrence was born, and had his early education, in Australia.



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Sir Gustav Nossal, Dr Elizabeth Truswell, Professor Harry Messel,
Professor Peter Bishop, Professor Ted Ringwood,
Professor Ralph Slatyer, Professor Susan Serjeantson,
Robyn Williams and Dr Michael Gore.

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