

**Perry MILES**

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**This is an interview with Perry Miles at 90 Mitchell Street, Millswood, on the 20<sup>th</sup> October 2011.**

**Perry, you were telling me that after you left Adelaide you went to Imperial College in London to do a PhD.**

Yes.

**Could you just talk a little bit about that and the career that followed?**

Yes. Well, it gets rather tangled up, but I will. I can just tell you about my arrival in London and how all that transpired. I went to Imperial College specifically to work with Willis Jackson because he was well-known, a friend of Leonard Huxley, who had given me letters of introduction to him and to some of his other associates in industry in London. Now, when I arrived there, which was early May of '51, Jackson was on vacation. Not having anything much in the way of funds to keep me going, I thought, 'Well, what I'll do is I'll go and get a job for six months and then come back and pick up my studies with Jackson.' So I went out and I interviewed with – I think it was called Electrical Industries or something, but again people to whom I had a letter of introduction from Huxley. Well, they offered me a job on the spot. So that was great. I said, 'Well, you understand when Huxley (sic) [Jackson?] comes back I've got to make some rearrangements.' 'Yes, that's okay.' Well, he turned up within two weeks and I went and explained to him what I'd done. He says, 'Oh!' And I said, 'Well, I said I'd work for them for six months; that's what I'll do.' He says, 'No, don't worry about that. I'll fix that. They're all friends of mine.' (laughter) So on the spot he gave me a research assistantship, which I held for the next three years. Went off very well.

I had to think of or develop a thesis subject. He – that is, Willis Jackson – was very much involved in defence science in England. He was half the time a research director at a big engineering firm, Thompson, and the other half the time he seemed to go back into the university, so he had a bifurcated career. A very influential fellow, he was finally given – he got his knighthood and then he got a life peerage

for his work for the government. Anyway, they had a problem at the time having to do with the new magnetic materials, which were ferrites. These were the non-metallic magnets. And he wanted to know if there was anything that I could do to help there. Well, I looked all into it and decided that doing magnetic resonance to establish the character of these materials would be a contribution, so that's what I started to do and for the next three years I did magnetic resonance of ferrites.

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Well, as I say, these were the new magnetic materials which had been developed first of all I think in Philips Eindhoven and they were just making their appearance throughout the world. The work we were doing in London consisted of my characterisation of the ferrites – the materials themselves were being made in the Chemistry Department in Imperial Department by a young lady who was doing her PhD, Elizabeth Carter, so she would make them, I would measure them and away we went. And, as I say, that took about three years to do that.

Now, it involved completely new equipment for this particular department. They didn't have any of the material I needed, equipment to do the work, so I designed and built and cobbled it all up together and it all worked pretty well. It was an interesting place because Jackson had students from all over the world. Now, there weren't very many doing PhDs – there must have been about seven or eight of us, I think, altogether – but it was quite a varied group. There was a man from New Zealand, Alec Kibblewhite, I think he'd come out of the New Zealand navy. He was doing work on barium titanate, which was a new ferroelectric material. All this has a bearing on my future career, as you'll see as this all develops, because ultimately the career depended on people. What I did depended on the people I interacted with more than the physics I interacted with. Anyway, Kibblewhite was there. There was another Australian, who I'm sure you'll know: John Farens[?]. He was doing his PhD. He was working on trying to get long wavelength infrared radiation by exciting little particles, electrically exciting, and the little particles were sort of fractions of a millimetre in size, and the idea was that you'd give them a jolt and they would radiate sub-millimetre radiation. And, unfortunately, that didn't work out as an experimental ..[fact?], but I did work with him a lot. We used to be in there at nights and I stuck a hand in his big equipment trying to get this thing to radiate. Never did, but anyway, the attempt was there and I think he got his doctorate for sheer doggedness. (laughter)

Then there was an Egyptian student, couple of Egyptian students. One in particular was [?? Karadli?] I think he went back to be a professor in Egypt, in Cairo. Never had anything to do with him again. There was an Israeli, Yehudi

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Klinger, who I think went to the United States. He was a very nice guy. He had been struck by polio when he was young so it was very, very difficult for him to move around. He was the most argumentative person I've ever met, but he was a great man. I liked him. Then there was an American – maybe I'd better forget his name, because he really didn't have a clue. He did an experiment and he showed us – oh, I know what he was doing: he was working for a man named John Brown and they were doing ultrasonic resonances. They'd have a great big sphere of fluid and they'd put an ultrasonic pulse in it and look for characteristic losses[?] from this ultrasound, and he developed traces and brought them in for us to look at and John and I would look at them, and you couldn't make head nor tail of them. There was no way it was going to fit his theory. The points were all over the place. So we said to him, 'Well, they don't fit, but what's the level of error, error in measurement?' He looked at us and ..... said, 'Error? There ain't no error.' He'd done the work, he'd made the measurement; there wasn't a question of error. Experimental error didn't occur. Anyway, that was John Karpowicz[?]. But it was all right. He owned some forests back in the United States and he was older than we are, so I'm sure he had a great career but it wasn't going to be in physics.

Okay, so I get through my work and decide that I would like to go to the United States for a period of about two years just to get more experience and background of other institutions, other environments, and then come back to Australia. So Jackson gave me a letter of introduction to Arthur von Hippel, who was a professor at MIT. He was the only one there doing solid-state physics. Well, the upshot is that I worked for him for a number of years, but the structure of that place was interesting, too. You see, it was experimental physics that – I was doing the solid state in the Electrical Engineering Department in London; I ended up doing the same sort of thing in MIT, because at that stage – this was 1954 – MIT did not have any solid-state experimental work (clock chimes) in the Physics Department. I'm not even sure they have now, but they didn't then. They had a lot of theoretical work, theoretical solid-state – they had John Slater, who was the leading theorist at the time – and they did a lot of particle and nuclear physics. No solid-state. The solid-state

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was carried out in the Electrical Engineering Department, and that department was actually run by an Australian, Gordon Brown. Von Hippel was an aggressive fellow when it came to establishing his department, establishing his laboratory. He wasn't thought entirely kindly of by the powers that be in MIT, but he was effective in doing things. He was a wonderful, enthusiastic fellow. He lived to 105, as a matter of fact. And when my wife and I went to visit him many years later he met us and he was the most gracious person – as he always was – but when we left we were quite convinced he hadn't the faintest idea who we were. Now, this was some years later. And when he died, and I realised he was 105, I realised at the time we went he was 100 years old – – –.

**Good heavens!**

But he came out of Göttingen. His wife was the daughter of James (sic) Planck? Yes. Nobel laureate. Von Hippel was this enthusiastic man who carried along everybody with him. So he took me on and I started doing magnetic resonance work there, sort of a continuation of what I'd done in London, because he was interested in the same area. He had produced, during the war years, a large compendium of the dielectric properties of materials which were important to the defence industry at the time – dielectrics and magnetics and whatever – so I was able to contribute to that. And he again had students and associates working for him from all over the world.

So this was '54, and I continued doing that sort of work for a number of years, until I got interested in the maser field, and developed – of course, Charlie Townes – Townes, Basov and Prokhorov had invented the maser and the air was filled with some of the things and I went to conferences, this, that and the other. So I decided what I wanted to do was to take a crystal of ruby and excite it by ultraviolet light and produce an inversion in the lower levels of the ruby by a transition which went up to an optical level and back again, and I was going to invert the lower levels and get microwave emissions by optical pumping. It was a great idea, and I think eventually it was made to work, but it wasn't made to work by my technique, which was to take an intense ultraviolet light and shine it down a long tube down to a helium

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temperature enclosure; it was gotten to work years later by using laser excitation. But that was again water under the dam.

So I was working with these ruby crystals, and I was very familiar with all the levels of ruby – that's chromium, chromium ions in the sapphire crystal and aluminum oxide crystal – when out came a very strange newspaper article – I think it was *The New York Times*, I'm not sure which now – by a man in California working at Hughes Labs, his name was Theodore, Ted, Maiman. He was announcing the first ruby laser. The establishment – by which I mean the physics authority, physics hierarchy, in the United States, and I guess involved in Townes and ..... and others – they didn't accept it as being particularly significant. Now, that's an about-turn from – the latest thing I heard was that Townes – oh, and he published – that is, Maiman had a little letter in *Nature* magazine and fairly recently Townes has said that that article was the most significant short article ever published by *Nature*, so he really changed his attitude. But poor old Ted Maiman, he couldn't get the physics establishment to recognise him, but the moment it went out in print, of course, anybody with a little bit of imagination latched onto it. So I latched onto it. Here I was, working with ruby crystals down at low temperatures; I had it all set up to repeat his experiment, which I did. So mine was the first on the East Coast, as it turned out – not that that was anything significant, because if you're second it doesn't matter a damn. You're either first or you're nowhere.

But I was helped by the fact that another of the professors at MIT was Harold Edgerton. Now, he became quite famous in the development of flash tubes and he contributed significantly to underwater research for the Cousteaus, he worked with them, and he did some fascinating photographs to catch the public attention of a bullet passing through an apple and all those sorts of things –

**Ah, yes.**

– very fast flashes, so you froze the frame – and he again was one of these marvellously dynamic people. I used to follow him up the stairs – you know, he's about 60 at the time – he bounds up the stairs and I can't keep up with him. I had a great association with him.

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Now, I started to repeat Maiman's experiment with the same geometry. Now, the geometry was – and I don't want this to [involve] too much detail for you, but it was a little crystal about two inches long and a big flash tube which was about that big. It was one of Edgerton's flash tubes. I mean, they were commercially available. And then I thought, 'Well, no, I can do better than that. I will have a little linear flash tube, the same size and shape as the crystal, and I'll put them in the two foci of an elliptical reflector,' which I did, but it didn't work very well. And Edgerton looked at me and he says, 'Ah, no, that's crazy,' he said, 'don't do that. Just wrap them up together – that is, you put the crystal in the flash tube and you put some aluminum foil around it,' and I did that and it worked like a charm, you see. (laughter) He was a very practical guy, was Harold. Anyway, so then I was launched into lasers and I worked in solid-state lasers for a number of years, glass lasers and others.

Now, I thought I had a brilliant idea – again, one that didn't work out, but I was not to know why. Nobody had had a glass laser up to that point, but I got into conversation with a man at Corning Glass and together we tried to make the first glass laser, but I chose – I think it was a terbium atom to try, because I looked at the fluorescent spectrum and it was a beautiful, bright, narrow red line and it had an absorption spectrum out into the rest of the visible which was quite broad, and on paper it was a dead cinch, and I thought, 'Here we will have the first glass laser, this brilliant visible thing.' Okay. So we struggled for that for about four or five, six months even, couldn't get the damn thing to work. Ultimately, I found out why: because, when you excite that particular atom, when the things get up into the excited state they not only emit a narrow line coming down but they also absorb in the same wavelength going up. So there was excited-state absorption going on which suppressed –

**That's a coincidence, isn't it.**

– yes – so that suppressed it. So we got pipped on the post. And then, anyway, then I finally decided, 'Look, we'll go back a step,' because the uranium – or, rather, neodymium laser in a crystal had already been developed, and neodymium was just another thing. I thought, 'Well, that would have worked. We could have started

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with that but didn't,' after .....[?] ..... neodymium ..... So I get the neodymium glass made, I get the little reflectors; again, a pattern of people – Harold Edgerton worked in the early days for the man who developed Polaroid. He was a professor up at Harvard at the moment. There were two other men. There was a man, William Hargraves[?], who grew crystals, and there was another one who coated things. So this group of people, I sort of combined their talents, then I could get crystals made or glass made; I could get the optical work – oh, Frank Cook. Frank Cook was another of them. He worked for, again, the Polaroid man. And the coater. So I had all these very skilled people to help me.

Anyway, I got my little neodymium glass laser, brought it back to the lab. Before I'd even turned it on there was a report that – it was the program of the next Physical Society meeting in Chicago, and there was a paper going to be presented there by Eli Snitzer, American Optical Company, he was announcing the first glass laser neodymium. I had this in my hand. I dropped it into my equipment, hit the button, there it was. Again, if you're second it doesn't matter. And I thought, 'Well, will I rush up to Chicago and present post-Eli's paper?' I thought, 'No, I don't want to take that away from Eli. It's his show.' So that one went. Anyway, I did a lot of work on neodymium lasers.

**So when was that?**

Well, we would have to work it out. That must have been – – –.

**Roughly?**

Oh, roughly '61 or something like that.

**So we're into the '60s, yes.**

We're into the '60s, now. I've forgotten exactly the year. So then I did a lot more work with that. Just to finish up that thing, at one stage I visited my friend at Lawrence Livermore Laboratories, where they were – and still are, as a matter of fact – attempting to make a laser fusion device using neodymium glass as the laser source. They built an enormous building with hundreds of big neodymium rods, tonnes of the stuff, and there'll be multiple paths for amplification and bringing it all

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together and all focusing it down in a poor, poor little target which was supposed to go nuclear. As a matter of fact, as things happen, I have a daughter who is still working on the physics of the targets of those machines. But anyway, I walked into this place and there in front of me was a whole – you know, sort of a football field building full of neodymium glass and I knew where that all started from: those tiny little bits. That was sort of fun.

Anyway, I worked on those lasers for a while, and then my one drawback – and it was a drawback which came from impatience more than anything else, both in Adelaide and at MIT – I kept going from one subject to another subject and achieved technical success in something but then my fancy would be caught by something much more interesting and I'd go to it, and I never stopped to publish the stuff well enough. Now, the result is that when you're – it didn't really matter, to my mind. I thought, 'Well, I've done the work. It'll be obvious that it's there.' (clock chimes) But at MIT and the other institutions it's publish or perish. Well, I didn't publish and from their point of view I'd perished, and I became the associate professor. I didn't get my full professorship, and I went into industry. I didn't get tenure, et cetera. But looking back on it, as a matter of fact, it was to my advantage because I've met all the people who did get tenure and they lived an academic career entirely and it was relatively dull, I thought. (laughs)

But I went to industry, and I went to a place called Raytheon Company, which was the leading defence industry in the vicinity, and I worked for the research division for a number of years there. Now, let's see, what did I do there to begin with? Oh, it was some laser work. Yes, and in particular I developed the first and not the only but the first high-power – oh, yes, and I'd been working on gas lasers, I didn't mention that, in MIT. I developed a water-vapour laser, a great monster thing – well, it was far infrared emission, and it was interesting in itself but it really didn't lead to much because nobody was interested in far infrared. But when I got to Raytheon again one of these articles came out about so-called 'gas dynamic laser' and it involved carbon dioxide – CO<sub>2</sub> laser and a mixture of carbon dioxide and helium and whatever, and it was announced by somebody in Avco, and nobody

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would say how it worked, but it worked by – but what they did say was it involved supersonic expansion of the gas. Well, I had a fellow physicist there who was a theorist, Frank Horrigan, and we sat down when we knew it was that and worked out how it worked, because clearly what they were doing was they were having a supersonic expansion. You had a population which was developed at high temperature (phone rings) suddenly cooled and produced an inversion between the populations of the excited ones and the lower ones, and you got emission. So we got interested in CO<sub>2</sub> lasers. Now, I didn't work with the gas dynamic one but I did work with the straight electrical discharge CO<sub>2</sub>, and I designed and built a CO<sub>2</sub> amplifier specifically for a project of Lincoln Laboratory, where they wanted to have a radar, a CO<sub>2</sub> laser radar, and they needed to take their little CO<sub>2</sub> pulses of it, amplify it up to kilowatt level, and I produced – you know, did the engineering of that thing and for many years – I was amazed how long it survived – it was up in a building at Lincoln Laboratories Observatory, and they would get their little laser, they'd run it through my stuff – which took up about the size of that kitchen over there – and then pump it up, out, and bounce it off ..... and bounce it off. So high-power lasers was stuff I worked on.

Now, at that stage, the work going on at the research division at Raytheon was changing. They had a project – for them it was an important one – of trying to make infrared detectors, state-of-the-art infrared detectors, using indium antimonide essentially photodiodes. They couldn't get these damn things to work, and they didn't work because they could make a diode but the reversed resistance or the leakage resistance, that needs to be very high or you lose the power that you generated. So they needed very high-resistance devices. The only people who seemed to be able to make them were the Hughes aircraft laboratories. I think they still may make them, over in Santa Barbara. Anyway, they said, 'Can you help there?' And I searched through the literature and realised that there were two additives that were used to produce the indium antimonide basic material, I think it was n-type, and then you had to do a thermal process which produces a p-type layer. Whatever. But the original doping was either zinc or cadmium. I didn't know what

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Hughes used, but our diode developers used the zinc. So reading the literature it was clear to me that if you used zinc you were much more likely to have crystal defects built into the thing as you tried to get the – you diffused the zinc in, and the cadmium would give a much better physical structural result. So I said, ‘No, you’re going the wrong way. Use cadmium.’ Well, nobody was going to accept that when here’s this clown who doesn’t know anything about the chemistry or anything else tells them they’re doing it wrong. So the people who actually made these diodes, they stuck with zinc, and I said, ‘All right. You stick with zinc. We in the research division will do the cadmium,’ which we did.

But there was something else involved. It was to make a very high-resistance surface coating, and you had to produce some way of getting rid of the wrong sort of carriers in the surface. Hermann Statz, who ran the division at the time, knew that – I think it was ammonia – an ammonia ion could produce sort of a counter to the conductive elements which were in the surface. So I thought, ‘All right – –.’ You see, all these diodes are made – first you make the diode and then you produce an anodised oxide layer on the outside to protect it from the [outside?]. So I thought, ‘Why don’t we do the anodisation in ammonia instead of anything else?’ So I tried that, and again it worked like a charm. We had the highest-resistance diodes anybody produced,  $10^9$  ohm resistance round the outside. They’d matched and exceeded Hughes’. And I can remember going out to an infrared conference in Santa Barbara and presenting all this stuff. Hughes were there. Anyway, that was a big success.

And then I essentially left the research division. Reason was I didn’t think that Raytheon Company was responding well enough to the things which were going on in the research division and the biggest part of Raytheon producing their money, their profits, was the missile system division. I was offered a very high consultancy over there and I took it, because I thought, ‘If I get in there, I may be able to influence their policy of using good things coming out of the research division.’ Big mistake. They didn’t listen to me. (laughter) They didn’t listen to me. Anyway, but they didn’t mind that I should be on their staff, providing I provided my own funding

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for whatever programs I was working on and that's what I did for a number of years at that division. (break in recording)

Okay, so I went to the missile system division and there I worked on infrared detectors, detectors – assemblies – infrared missile guidance, essentially, both the detecting aspects of it and in particular the domes, because there was going on a dome production over at research division for infrared transmittable... Oh, yes. We've almost forgotten: our big success over there and the one that brought me back to Australia for the first time after 25 years, at that stage the only infrared transmitting dome material was hot-pressed zinc sulphide, and you couldn't really see through it, but the infrared got through it. Okay. They wanted something better. In that division, research division, they had a chemical vapour deposition group. They were making all sorts of other things. Anyway, we decided that we'd try this vapour deposition as a way of making a much more transparent sulphide, and again it was one of these things that worked beautifully. The first one – I didn't know anything about it, but Jim Pappas and the guys working for him, they were the industrial chemists, they knew how to do it. And one of them came along one day – I can remember just as though it were yesterday – the corridor. He said, 'Perry, is this any good?' Now, he had a piece about an inch square and it was about, oh, a fraction of a millimetre thick, and it was black as pitch. And I took a look at that and then put it under a microscope and said, 'Well, first of all, we've got to get the carbon off,' because it was laid down on a carbon mandrel. It was crystalline on one side, it was just bright little shiny crystals; on the other side it was dead black. So he took it away again and very carefully took away the black layer. I put it under the microscope again and there's an absolutely wonderful clear material. It was fully-crystallised and no holes at all except of course it wasn't flat, because of all the crystal face. But it was clear, yellow material. Well, we ended up making things as big as this table, this thick, clear, and domes and everything. Anyway, that was a big success.

And that was what brought me back to Adelaide, because the defence department, who were running this program, said, 'Look, we want you to write a paper that's

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going to be presented at – – –.’ What’s the weapons lab people up here? Whoever they were. No, it was the Long-Range Weapons Laboratory.

**Yes, originally the Long-Range Weapons Research Establishment.**

Yes, that’s what it was.

**DSTO now.**

They were going to have a conference which involved the New Zealanders, the Canadians, the Americans, the British. It was a joint – I think there were five members. And I said, ‘Look, I’ll write the paper, but’ – they were going to give it – I said, ‘I’m going to give it.’ Well, that was – – –. They said, ‘Okay, come along.’ So I did, and that was a big success. So we got through all that stuff at Raytheon’s. That was a combination – see, I was always shuttling forward between research division and missile division, so to some extent I did help them change the company attitude.

Then while I was working there I was approached by – again, I think it was DARPA, or whoever it was. They wanted to have a high-power laser test program to develop materials which would counteract high-power laser weapons, and I had the background to do all that stuff. So I became the experimenter for this (clock chimes) rather large program where we tested ablative materials and other things to protect against the weapons that they were sure the Russians would have, and that was a tactical success because – there’s a little bit of politics in this – the high-power weapons project was under control of the air force, and so was the countermeasures under the control of the air force. Well, DARPA realised that wasn’t very smart; you should have somebody who didn’t have a direct proprietary interest in having high-power weapons, if you’re going to protect against them, so DARPA had their program. Mine was the DARPA program. I worked for them. It succeeded beyond anything anybody wanted it to succeed. Well, that wasn’t all *my* work, obviously. I just did the experiments. Well, that was the first completely automated experiment that anybody had dreamed up. I had computers running. I didn’t know anything

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about computers but I found star people who did, and we were able to do a large number of tests.

And, as a matter of fact, beyond that, I went into thinking about protecting things from lasers and decided that what we were doing, which was essentially absorptive techniques, there's another technique altogether and it is essentially loss-free reflection, so I did the theory of high-albedo materials, which simply means you have a highly-transparent material, essentially no loss, and you break it all up so that it scatters rather than transmits directly, and if you run through the mathematics – and it turns out to be very simple – the profile of the radiation pattern through that material is not exponential, as you might expect; it's a linear form, and it goes from whatever you put in the back to something at the back end of it nothing, or the further you go the lower it gets. And I realised that, with the sorts of materials that we were developing, you could make a reflector which was at least three nines reflective and, if you really pushed yourself, four nines reflective. And that would wipe out any chance of anybody getting through, no matter how big they built this bloody great weapon. So, any rate, that's where that bit of it ended.

So what happened next? So I'm now at Raytheon Company. There's an outfit in California called RDA, and they were looking for somebody with my general background to help them advise the government on their own research programs, and I fit the bill and I took a job out there and worked for them essentially for the rest of my career. And I didn't do any more experimental work, but I did do some elemental theory, and certainly a great deal of general intellectual management of their programs. And I found it a very interesting part of my career, because, typically, government laboratories would be approached by some character. Often they would come from Russia, and they would mystify and bamboozle the government scientists and they'd think these guys knew something just because they were Russian. So they'd drag me in and I'd look at what they were doing and I'd just say, 'It's all rubbish,' you see. So it was my job to deflate a fairly large number of these propositions, and I was in many meetings where it was me against the meeting.

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**I wonder, Perry, if we could leave your career there –**

That's essentially the end of it.

**– and go back to the beginning –**

Right.

**– when you were a physics student in Adelaide.**

Right. Well, I entered Adelaide in 1949, I guess ..... .., because it was – no, no, it was in 1946.

**It must have been before then.**

1946.

**Yes.**

From Adelaide High. At that stage, the professor was Kerr Grant, and then there was – I think the reader was Burdon. There was Gordon Aitchison and there may have been one other. Tomlinson came later. And there were a number of helpers who were two or three years ahead of me like Bob Crompton – you probably know the names better than I do.

**And Graham Elford.**

Elford – of course, he became professor later. So I went in there, and I'd already – at Adelaide High we had some excellent teachers there, who were themselves university lecturers, so we got a very fast start. And I took essentially Physics and Mathematics and Chemistry to begin with, first year – oh, and Geology under Mawson.

**That's the combination I did in first year.**

Yes. Well, incidentally, of course then I dropped them off one at a time. First of all, I dropped Geology, although I must admit that I really enjoyed that. I did double work in my Geology class. I don't know how many other people did it, but I went to Mawson's lecture for the personality and the interest, because, as you will remember, he really didn't care much about teaching Geology but he did educate us in the

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beautiful images that he'd brought back of icebergs in the Antarctic. So I'd listen to his lecture. Then I would go back to the same lecture given by the reader to get all the information that I would need to pass an examination. (laughs) Anyway. And then, as I say, I did Mathematics – oh, and Chemistry, of course. I think McKellar – was McKellar Inorganic? – and then Inorganic Chemistry. And then slowly dropped them off. And then I ended up doing an honours degree and a master's degree.

Well, my career there, it all went off fairly successfully, except that I made a mistake in my honours examination. Although I knew the material cold, I was very annoyed by one of the questions. I think it may have been written by Burdon. I'm not sure. It was on quantum mechanics. Now, I want to be careful the things I say about the faculty at the Physics Department because they were all good men, but they were the product of their time and their background, and theirs was not modern physics, as a matter of fact. They knew about elemental quantum theory and they knew about the Special Theory of Relativity. They knew nothing about the later quantum mechanics. I don't know what your experiences were. Anyway, this was a question which was, I thought, so ill-phrased and off the point that I was forced to correct it all and do a decent answer to the proper question. Well, that was absolute stupidity on my part.

**Are you saying that you were aware of quantum theory in a way that the course didn't venture into?**

Not entirely, because I wasn't all that educated in it, but I could recognise when they'd got it wrong, just because of the way it was.

**And this was an honours exam, did you say?**

This is an honours exam.

**Yes. Was that in 1949?**

Yes, it would be '49, because I got – yes, it was '49. So I think there were four or five questions, I've forgotten how many. Anyway, and this was – I think it was the next-to-last, and when I finally got through this thing I looked at the clock and I had about 15 minutes to answer the last one. Well, I didn't get through it. So here they

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were presented with an examination paper where the last one I didn't finish it and the second one they thought was all this nonsense – 'What's he talking about?' – so they didn't give me first-class honours. Well, that really – it set me back temporarily. But again, looking back on my career, that may have been an important setback because it really energised me to do something fast. And that's the other aspect of that faculty: of course, it was their attitude. In Adelaide University and in English universities, the student is essentially left up to his own devices – or was at that stage; I don't know what they do now – to find your subject and do it, and if it succeeds you get your degree, if it fails you don't get your degree. Now, the American university – and I had graduate students over there – is quite different. In the first place, you graduate with your regular degree, you go to an advanced degree and a master's in particular or certainly a doctorate, and you are assigned a professor who makes sure that what you're going to do is going to be significant and that you've got a chance of going about it the right way, so he sort of leads you into it so that you don't waste your time, don't waste his time. That didn't happen in Adelaide, and I don't think it happened necessarily in the British universities, either. You were up to your own devices.

**Yes. You were talking about a time when you started on a master's degree.**

Master's degree, early 1950. Right.

**Yes. Who was your supervisor?**

Well, looking back at it – I was asked that question yesterday and I thought, 'Who the hell – – –?' Because I was never conscious of a supervisor in the way I'd become a supervisor myself, you see. It must – no, I'm pretty sure it was Huxley, because – – –.

**So Huxley actually had arrived by that time?**

Oh, yes. And that's the other thing, you see. I'd done my – – –.

**In fact, Huxley had arrived the year you did honours.**

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Exactly. That was possibly part of the problem, my problem. No, the previous year, of course, it was Kerr Grant. Huxley had arrived. Whole new attitude to life, I suppose. And the new Chancellor was Rowe, and of course these were both out of, I guess, the weapons industry in Britain, so they were a new breed of cat. Huxley didn't know me; I didn't know him. I didn't like Burdon very much. In any case, Huxley – well, of course, he'd done all this very nice work on low-energy electrons in gases and I took my cue from that, as a matter of fact, and I developed an idea that I was going to have some fairly low-energy – well, I was going to create an electron beam, run it into a gas container, and then when the ..[stuff? 52:00] came out through the other end I was going to do an analysis and show there would be characteristic losses. So I designed this thing and it was the clunkiest, worst design I have ever made, the only time I made a bad one because I learn from my experiences. I had a triple vacuum system; I had a square box with a flat plate on top where I generated the electron beam; I had a short section of a tubular part, in which I was going to let in low-pressure gas and then it was going to come out through a hole in the end; and then it had another, symmetrical square little box. In it I used an electrostatic energy analyser – it had just appeared in the literature, so I used that – and you have a radial field and that produces a bending and so you get a spectral distribution at the exit plate and you can analyse that by changing the voltage of the plates. So that was my design. I don't think anybody ever looked at it. They certainly didn't critique it. So I tried to have it built, and I ran into a problem with the staff of the workshop there, who really didn't understand – well, they knew what I was trying to do, but they gave me the wrong information about how difficult it was to do anything. But I finally got this thing built, but before I used it I'd already made myself some calculations and realised that the scattering cross-section of this gas, because I had to have it at low pressure or it wouldn't work, the scattering cross-section was so low I was never going to get any result. Now, this took six months of my year, and nobody had ever pointed out to me the scattering cross-section or anything else – again, left to your own devices: 'Go over in the corner, do all your stuff, and it works or it doesn't work.'

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Well, now I had to start again. (clock chimes) Six months in. And I again quickly read the literature and I found a German paper where somebody had measured the reflectivity of alkali halide crystals to low-energy electrons and they'd shown that there was a characteristic loss if the electron energy matched one of the levels in the alkali halide molecule, essentially, or atom, but ....[?we used a] solid. So I thought, 'Well, I can do that, and I will extend it and do half a dozen of them,' so that's what I did. But in order to do it, starting from scratch, I needed a new design, new equipment, new everything, and, 'Well, I'd better get someone to help me.' Now, Bob Crompton and Elford knew a man who was a railway clerk or a clerk in the Adelaide railways. He had his office at ground level there just opposite the government buildings. His name was Fred Crook, I'm pretty sure – you can check with Elford. Elford will know, because they were in great cahoots. Elford, as a matter of fact, in that whole era, they had equipment made for them by three people. Fred was one of them. The other two were Oliphant's two brothers, who had a glass manufacturing vacuum system. They would make oil diffusion pumps, and they made me my oil diffusion pumps.

**Glass – from glass.**

Out of glass, yes. There'd be a little conical flask here, conical flask there, tube down there; there'd be a venturi thing. You'd boil the stuff up and there'd be oil and there'd be a two-stage pump and they'd pump the stuff out there, condense it, then pump it out again, back it would go. And I used to go to their – I think they had a place in either Hutt Street or someplace in Adelaide, and they'd be sitting there. They were nice fellows. Anyway – – –.

**This was an oil pump?**

Oil. Yes.

**Not a mercury – – –?**

Oil diffusion pump. (telephone rings)

**Yes.**

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So they made my pump – they made everybody’s pumps in the department. Fred Crook made for the department little hydrogen mass spectrometer leak detectors, and they were a little device about that long and you’d put them in a magnet and the protons, if they appeared, would be brought around an arc – they used old magnetron magnets – and would end up to detect. So in order to detect a leak you attached this thing in part of the vacuum system and you sprayed it with hydrogen and if the hydrogen went in, if you used ions, you’d get a hydrogen ion and the leak detector would pick it up. Well, these very nice little detectors were made of brass and they fitted in with a conical tapered joint into the glass system. They were made by Fred Crook. So they said, ‘He’s the man. Go and he’ll make your equipment for you.’ So I went to talk to Fred Crook. He said, ‘Yes, I will help you.’ But he says, ‘But I won’t make your equipment. You’ll make it.’ That guy, I think – I get a little bit emotional about this guy – he helped me more than anybody else at Adelaide for the rest of my career, because his great interest in life was to make model engines and trains to absolute perfection. Everything worked. The little engine was this big. Everything was there, every nut, bolt, whatever, and they worked like a charm. So he had high-precision equipment. He had a flatbed Southbend lathe. He said that’s the only good lathe there was, flatbed Southbend. And he taught me how to use the lathe, how to actually – how to make the bits, because you have to make a bit a very particular way, depending on what job you wanted to do.

**This is in the Physics workshop?**

No, no, no, no. It’s down at his workshop down at home.

**An outside business.**

I would go down there in the evenings. When he went home, I would go with him and used his equipment, his workshop, night after night. And we would start out – for example, the equipment I designed, now, with my experience of all this clunk stuff, I designed what I thought was a very elegant design, which was linear. It had a first section where I generated the electrons and controlled their energies over a distance of about like that, multiple grids. And the beam came along, then it went

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into a [centre] section into which I could introduce, through a conical taper port, I could introduce the target. I could turn it in one direction and evaporate onto it the alkali halide I was interested in, then turn it around and reflect electrons – electrons would be reflected at an angle, and then I'd take what came out and in the last section I would analyse it. And I would analyse it – well, actually, no, no, it's not quite like that. The analysis was essentially built into the overall design. The energy analysis was handled by me essentially slowing down the electrons until they stopped – yes, okay, I've got the geometry right. I've got the geometry right but the function I've sort of ..... ..

Incidentally, that equipment may still be stuffed someplace in the Physics Department. If it is, I'd very like to get my hands on it or give it to somebody else. Anyway, it was this long cylinder. It was pumped from the centre. From the top of the centre was the piece where I put in – and what I did was I had the – the electron beam was formed this way; the source of the – ..... [?]. .... was in this back end. I would turn it, turn on the source, evaporate my layer, had to be very thin or you wouldn't get the electrons through it and they'd be all get charged up stuff. So then you'd turn it back again and then the beam would come down. I slowed the electrons down and then measured the current which was detected coming through – – –.

**Yes, so it's a retarding field.**

It was a retarding field. That gave me my energy analysis.

**Yes. Then you have to differentiate that.**

Exactly. There were problems because the spectrum coming out of the hot filament was fairly broad [brought]. I mean, it's up to what, 700 degree, 1000 degrees. Too broad. So I had to introduce plates which essentially chopped off all but the high-energy part, the sort of a tail of the distribution which came off the filament, so I could get some sort of resolution. But that worked. Again, you know, I'm keeping my fingers crossed. This was a complete design. It either works or it doesn't. Anyway, to cut a long story short, I got all this and then I had to learn to cut tapers. We would start with a block of brass like that and I would learn the saws to cut it off,

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and then a drill press to cut through, cut tapers in it, cut a female taper, cut a male taper to fit, everything had to be just right or you wouldn't hold a vacuum. And I made all this stuff, and by the time I'd finished I could use all his equipment. Not only that, I knew for a fact, beyond that, and I knew whatever the technicians told me in Adelaide, Imperial College, Raytheon Company, anyplace else, I knew what they could do if they wanted to and what they couldn't, and that really set me in good stead.

**Yes. You're describing the beginning of a tradition which continued for quite a long time in Adelaide –**

It did.

**– where students of experimental physics made things.**

Yes.

**And then, in their careers, knew how things were made and what could be made.**

Yes. Well, I'm glad I wasn't the only one, because, as I say, that man helped me more than any of my academic professors did. He really did, because it gave me the confidence that I could do it, and when the whole thing succeeded that boosted my confidence – – –.

**What's your memory of Huxley at that time?**

Well, he was a gentleman. I think he again was a man of his time. He was a very courteous man. He probably knew his field but it sort of wasn't mine. And he certainly helped me, in the sense he furthered my career by introducing me to the next level of professors that I encountered. And the same thing – now, you see, interestingly enough, they were all a combination of – they were engineers rather than physicists. They were all involved in engineering enterprises; that is, they ....[? 65:20] employed by them. So I went from Huxley to his industrial friends in London and to Willis Jackson, who was really the arbiter of what went on in physics and engineering development in the British Government for a long time. Now, he was instrumental and he deliberately sent me – I think it was unnecessary – to visit people

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like – I think it was Cockroft. The reason that he sent me was because Cockroft – I think he was up in Birmingham or someplace – he had a magnet. I had to buy a magnet for my magnetic resonance work in nonferrites in London and they didn't have anything which would serve the purpose, but instead of relying on local talent he sent me off to talk to, I think, Cockroft, who had a magnet, and they had – he had [...Wilson.... cloud chambers?] and I saw ....{?stuff}. But I think it was more for me to meet them and sort of get that experience.

**Now, it must have been during your work on your master's project that Stan Tomlin arrived.**

He did.

**Did he make an impact on you at all?**

No. He knew something. His was a more modern background. Well, he did have an impact in this way: I think he was the one who had a student who was a returned serviceman who started to do magnetic resonance – was it nuclear? I remember he set up an equipment which had a magnet and used microwaves. I've forgotten what his experiment was, but it didn't involve – it was a magnetic resonance experiment, essentially. And I was fascinated by that experiment. You would probably be able to find out who it was, but he was a returned serviceman.

Now, of course, that was the other aspect of my experience at Adelaide University, was in our year half the students were, as I was, immediate high school graduates used to doing paper exams with no great problem. The other half were all returned servicemen, and a lot of my friends became and the people I played lacrosse with were returned servicemen.

**Harry Medlin was one of them.**

He certainly was, he was one of them, and of course he was going through. And Harry I liked very much – still do, of course – and then there were others. But that was an interesting year because their attitude to study was different and we young students were actually able to help a number of them because they ran into trouble

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sometimes in handling all sorts of questions which were posed to them, and for us it was easy. (clock chimes)

**How did the department change during that period after Huxley arrived? It seemed to me he began the modern era of establishing a set of research groups.**

Yes, he did.

**Did you see that happening?**

Only the first part of it. I think it must have taken longer than ---. See, he came in what, the - let's see.

**He arrived at the beginning of 1949, I think -**

Yes, that's right.

**- which was when you were doing honours.**

Yes. Well, you see, I was only there for two years, and by the time I'd left he certainly had Crompton I think was running the - was at the electron stuff, gas.

**Yes. The slow electron ---.**

I don't know what Elford was running, if anything, but it was only in its formative stages, so it really wasn't established by the time I left. I wasn't conscious very much of that. I was just conscious of them as individuals.

**Did Stan Tomlin bring a more sophisticated approach to quantum mechanics, do you think?**

He may have. But, you see, it didn't impact me because by that - now, this is the other aspect of education there which was again different in the American university. In Adelaide, if you went for an honours degree, you didn't do very much in other physics. You did physics courses. When you did your master's, you weren't expected to do anything else other than your thesis work.

**It was just a research project, no coursework.**

It was just research. I'm not sure. I don't think I did anything more than that. No, in my honours year I decided to take Economics, first-year Economics, to get a little bit

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of breadth. For me that was a disaster. I couldn't make head nor tail of economics. That was the only course I ever got a C on was Economics I. (laughter) These stupid, subjective questions. It wasn't objective at all, wasn't a science at all. And of course we still have these same problems with economics. I mean, even at very high levels of gurus. Anyway, no, I didn't do any coursework. So what Tomlin did or didn't do I was not conscious of. He may very well have increased the sophistication because, after all, he would have learnt a sort of generation later, pretty well.

**Yes. What were the library resources like at that time? You talked about starting your master's project that you read articles and got ideas.**

Oh, yes. I spent a lot of my time in the Barr Smith and I found it a very good library for the sorts of things I wanted, and I've spent the rest of my life in libraries and I've been very lucky, one way or the other. MIT had an excellent library. MIT and Harvard are only separated by two miles in Cambridge and I would go up there. The thing that attracted me when I got to Massachusetts, into Cambridge Mass., was that here, first of all, there are 29 universities in the Boston area and the two dominant ones were MIT on the river and Harvard two miles away, and I would go to seminars up from there and we had a whole raft of eminent professors up there, including Van Vlack and Nic Bloembergen, who I got to know very well, and each week they would have a seminar and I would always be up there. Oh, and another ..... – Nobel laureates right, left and centre. In fact, the godchildren of my first child, they had come to – they stayed; they were both Harvard graduates – they said they never wanted to leave Cambridge because everybody came to Cambridge. There was no point in going anyplace else. (laughter) Anyway, that was interesting life.

**Now, when I talked to Graham Elford he talked about the early days when Huxley came that Huxley established a literature reading group that met regularly.**

Yes, I think he did.

**Were you ever part of something like that?**

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I don't remember – I vaguely remember that there was a reading group. I don't think I was part of it. That must have been probably while I was doing my master's degree. If it had started before I would have known about it. But – well, as I say, I vaguely remember it but I don't – – –.

**Yes. The other practice that still continues, I guess, is higher-degree students working as demonstrators, laboratory demonstrators, to undergraduate students during their project.**

I wasn't part of that.

**You didn't do anything like that?**

But, of course, I thought that – I'm not sure whether Elford and Crompton were doing that. I had a sense that they were, but again – – –.

**Yes. The other thing that occurred was that the department would appoint cadets.**

Yes. They would, yes.

**I think Graham Elford became a cadet and did honours over two years.**

Yes. That's right. And there was Ziegler, he was a cadet. I don't know whether Harry Medlin – was Harry Medlin a cadet? I thought he took an extra year.

**Harry had a complex history at that time, I think.**

Yes. Well, whatever. But yes, yes, I think it was Murray Ziegler.

**Ziesing?**

Ziesing, was it?

**Ziesing.**

Was it Z-I-E something or other?

**Yes.**

He was a cadet.

**He finished honours in 1950.**

Yes. Well, that would be right, you see.

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**Same year as Harry Medlin.**

Yes. Right, exactly. Yes, well, they established that. Again, I was not part of that.

**So what was the department like as a community at that time? You knew all the other ---?**

Oh, I knew all of them. Definitely.

**Knew them – did you go to morning tea with all these people?**

Yes. Yes, exactly. Sure. And each week we had what they called a ‘conversazione’ – we’re very Italian, you see – (laughter) and that would be, I think, mid-week and we’d all have tea together and then somebody would stand up and give a talk at the conversazione. The only thing I really remember was that all the professors would be lined up in the front seats. About the only person who really couldn’t stand it, couldn’t stay awake, was Gordon Aitchison. He invariably fell asleep. (laughter) But he maybe had a lot of things to do. But yes, we had those. No, we were a good social group. I knew them all; they all knew me. We used to play ping-pong together. I learnt to play ping-pong with Graham Elford.

This is a brand new thought as a result of our discussion: that, hearing what you suggested that Huxley developed in his tenure, it becomes a little more obvious that I must have been fairly in what we would describe as a ‘cleft’ between the traditions of the past with Kerr Grant and the traditions of the future with Huxley. So I was never part of Huxley’s structure. That developed the year after I left, apparently. So I was essentially left to my own devices. Now, again, looking back ---.

**What year did you leave? [78:00]**

Now, see, that’s another thing. I have a history of finishing my work and leaving before anybody gave me a degree. (laughter) So I left probably February of 1951, where I’d already got, you know, a ‘Yeah, it’s good.’ Off I went. So I was not there for the ’51 academic year at all; I was already in London, already employed as a matter of fact, in I guess April or May, and all off onto my new academic work. And, incidentally, following that line of thought, I finished my work in London, I got my thesis done, I got it accepted, and off I went to America and I never collected a

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degree in London, either. (laughter) No. I wasn't sticking around. So anyway, I left Adelaide early '51.

**Yes. Could I just go back to when you were making things –**

Yes.

**– and you were telling the story of – I think you must have been saying you went to the home of a person who had his own workshop.**

I went to ---.

**What facilities were there in the Physics Department at that time? Was there a workshop?**

Oh, there was a big workshop, had all the equipment. What it didn't have was skilled machinists. What it had were guys who were quite willing to tell me that something was impossible and would only take a month to do when in my later experience I knew what [was] first of all possible and it could have been done in a week. So they were a real hindrance to anybody who relied on them. Now, there may have been other, better ones, but the ones I interacted with were more a drag on anybody they worked for than ever they should have been.

**Yes. How many technical people would there have been in the workshop then?**

I can't really remember. I have the vague suspicion there were about three machinists. And maybe there was probably somebody who ran it. But again Crompton would be able to fill you in on that.

**Yes. Graham Elford tells the story of those people running essentially an illicit business building Variacs – you know, the variable transformers.**

Yes, of course, I know them. I didn't know anything about that. What I did know was they, in retrospect – well, even then I realised – they were hindering more than they were helping. And it was my great good fortune for both Graham and Crompton to introduce me to Fred Crook, who was a superb machinist and somebody who was willing to educate me.

**Yes. You built this equipment.**

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Yes.

**Where was your laboratory?**

Ah! Now, that's another story. It was on the second floor of the Physics Building, right in the middle. It was a very large room. It was the only one with a balcony in the centre of the building that overlooked the road beneath, and I got to know everybody who wanted to come out and peer and see what was going on outside. I only took up about one-third of that large room. I was on one side.

**Yes. Now, who else was working in that room?**

Nobody.

**Nobody. You see, that's one of the rooms where, for example, Harry Medlin established his work –**

Did he?

**– and there was an electron microscope in there.**

Not when I was there. Because, you see –

**Later on.**

– he was a year – essentially, a year behind me.

**Yes.**

Because he took two years to get his honours degree. No, I had the room to myself, except for my frequent visitors. And there was a story there which I told, somewhat to the embarrassment of Crompton and his wife, Helen. I didn't realise it should embarrass them, because it shouldn't have, because it was a very innocuous thing. As I described in my experiment I had to form a beam of electrons. Now, you can't do that at low velocity, so I had a battery pack at 800 volts – at one stage it was 1200 – 800 volts DC to get this fast beam well-defined and heading for my target area (clock chimes) before I slowed it down. So I got 800 volts just sitting there, out in the open. One day, Bob's future wife, Helen Gibson, walks across the room – very comely girl; I was distracted; I got across this 800 volts. Wham! All I could think of

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was, ‘Why did she hit me?’ (laughter) I told this 10 years ago and they seemed to think that was a little bit strange. But anyway, that’s what happened. That’s where my lab was. Now, I was up there, working away on the 11<sup>th</sup> day of the 11<sup>th</sup> month of 1950, when I got my first positive result. I was following the current as a function of voltage or a function of retardation, and it was nowhere near zero, and it started to come down and went through a minimum and went up again. That’s ..... ..[?]. So from the 11<sup>th</sup> November I quickly went through about half a dozen more – I think it was about half a dozen – different alkali halides and plotted them all out, and that was the essential basis of my thesis.

**Then you wrote up and left in the following February.**

Exactly. ‘To hell with them!’ Off I went. (laughter) But you asked about the library. Very important place. The library had all I found that I needed. They were largely modern periodicals that I was after, and I found everything I wanted there. I was again fortunate at MIT: they had an excellent library and I spent a lot of time down there. And then I’ll tell you a quite remarkable coincidence. When I was working at Raytheon and developed this beautiful zinc selenide, I wrote a survey article, ‘Infrared materials’ – it’s in the literature – and when you look at the things I wrote I always took great pains to accurately define the history of where it was, where we went through and where we got to, and in this case I went right back to the beginning of infrared materials, and it goes back certainly hundreds of years. They didn’t know they were infrared, but they were heat: heat would get transmitted through these things like rock salt. And there was a development of the science and it took place – I think it was – let me think – it must have been in about 1790 or something or other. I’d have to go back now, I’ve forgotten what it is. I came across three articles that had been published in that year – one in the Franklin Institute of Philadelphia, one in an Italian magazine and the third one was in the British – the leading – maybe it was Phil. Mag. or something or other – way, way, way back. So I got the references, and there was a library in the air force Cambridge research. Now, there’s an air force base outside of Boston that’s called AFCRL – Air Force Cambridge Research Laboratory.

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**Yes.**

They had a little library there. It was a miniscule library. You wouldn't believe it, but those three articles from sort of 150 years ago, they were all there and I thought it was wonderful and I read them all. I couldn't read Italian well – well, I could read it well enough to understand what was going on.

**Yes. Now, when you were a student here in Adelaide and going to the library –**

Yes, sure.

**– and you'd read a journal article, did you make notes? These are the days before photocopiers.**

All notes.

**Did you write off to the authors for a copy?**

No, no, no, no, no. No, no. There wouldn't be time for it anyway, the way I worked. No; I simply wrote notes. And I don't now whether there were photocopies there or not. I suspect not. I remember doing – no, there weren't. I did photocopying – well, there may have been. My memory might be at fault. I know there were photocopies in the library at Imperial College, because I used to get these things and they would photograph them on 35 millimetre and then they'd print me up the things, and you'd have a curled-up piece of paper, hard copy, really hard, and you'd have to sort of spread it out to see what was there. But I can't remember anything of that at Adelaide. I think I simply took notes. It would have been the sensible thing to do. I'm pretty sure that I certainly didn't. There weren't photocopiers, and I don't remember ever asking anybody to copy anything, so I think I simply wrote the notes and went on from there. Not that there were very many articles. All I needed was one.

**Well, Perry, I think probably we should stop there.**

Well, okay, if that's ---.

**Thank you very much for your time.**

Not at all.

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**It's been a very interesting story.**

Okay. Well, there we were.

END OF INTERVIEW