Bob CROMPTON

I'm speaking to Bob Crompton on the 5th December 2011 at his home.

Bob, I wonder if you could cast your mind back to the far distant past when you were first a student of physics in Adelaide: what was the department like in those days?

Very small. It comprised, essentially, of the professor, Sir Kerr Grant; his reader, Dr Roy Burdon; and Mr George Fuller, who was, I guess, the senior lecturer. Just the three, that's how it began, as far as I remember – oh, Gordon Aitchison was also there, I think, just appointed as a lecturer. So there was the four of them. A little later on there were two others added to it: Michael Iliffe and another fellow. They were recruited – they were older men, but they were recruited a little bit later. But when I went there there was just the four of them. And so a fairly heavy teaching load they had in those days. Kerr Grant used to take the Physics I in the morning and George Fuller used to take Physics I in the evening. People went to the morning session for entertainment and they went to the evening session for education; that's how it was generally thought of, anyway.

I think students did that for many generations.

(laughter) Yes, I think that's true. Anyway, they were riotous times at Physics I, I must admit, especially on the last lecture, and most particularly on the last last lecture; that was a real hoot. That was Kerr Grant's last last lecture, and all sorts of shenanigans occurred. It's a wonder – the old Physics I lecture theatre is now where the workshop is, and of course had a tiered floor to it, which must – I was quite shocked, actually, to find all that gone, because I had quite a lot of difficulty recognising where that large ground floor lab now is.

Yes. It was very steep, wasn't it.

It was a steep lecture theatre, yes. And so the last last lecture was absolutely packed, and I think some of us – certainly I – wondered what would happen if there was a fire, because there weren't all that number of exits and that steep, steep raked floor

would have been quite to get everyone out if anything had gone wrong – and, of course, there were shenanigans as well, so something *might* have gone wrong.

I've often wondered how that business worked. From my memory of doing Physics I a roll was marked, and it was done by observing the numbers of the seats which were vacant, and so recording who wasn't there in that way. I wondered how that worked, when students were attending both Kerr Grant's lectures and George Fuller's.

Gosh. I don't remember that, actually. I do remember in later years, Physics II, we just had a card that was passed round and we had to tick it –

Yes, we had.

– but I don't remember how attendance was registered. Must have been a bit ropy, I would think; in Physics I there were such a large number of people.

How big was Physics I in those days?

I should have thought in the order of 200. And I would have thought sort of 50-odd for the evening class as well, probably, but I'm not sure about that. But George Fuller took the evening class; and later on Gordon Aitchison took it, of course; and I had a few stints at that as well.

Yes. And then you did Physics II and III.

Yes. Well, Physics I was rather formal practical classes: very set experiments. I don't think we learnt very much physics out of the experiments; they were too set to work, really, essentially. Physics II was better, and George Fuller was in charge of the Physics II class in those days. Goodness.

He still was, when I got there.

Was he, really? (laughter) Yes.

In 1959.

And I bet you had the Kater's pendulum, did you not?

Yes, yes.

And a lot of time was spent trying to get Kater's pendulum to work. (laughter) And that wretched wax paper on the recording machine, which Fuller vaguely would interpret.

Looking back on those courses now, what was the curriculum like?

Well, no quantum mechanics at all, of course. I guess Stan was the one who introduced quantum mechanics for the first time.

Yes.

Huxley, when he arrived, he took over Physics I at least for a little while – didn't last long, because it was very rigorous, the way he wanted to teach it, and I think it was a bit of a shock for people coming in from leaving or leaving honours. So I think you could probably still do first year from leaving in those days.

Yes.

Those of us who'd done leaving honours had a head start in my day, because it was very similar – first year was very similar to leaving honours; in fact, if you got a good leaving honours pass, you could get exemption, I think, from Physics I, so they were really parallel courses. I don't think Huxley lasted long, teaching Physics I. He was in his element teaching the higher years. I don't think he ever taught any quantum theory; I think that was left to Stan; but Huxley was teaching at the higher years pretty rigorous electromagnetic theory, which was his forte, really.

And yet Huxley taught me Physics I in '59.

I think it would be the only year, probably.

Electromagnetism.

It was '59, was it?

Yes.

He taught you [electromagnetism]. Righto, I'm mistaken then, because he arrived in '49, didn't he, so that's 10 years after he'd begun.

Yes. And so did you.

What did I teach you? Not electromagnetism .....

Geometrical optics.

Geometric optics. I hated geometric optics. But did I not teach you physical optics, which I thoroughly enjoyed? Did I not teach that to you as well?

Not in Physics I. I remember the textbook. It was Curry.

Oh, Curry, yes. I remember the "sign convention" – do you remember the "sign convention"? (laughter) Horrible! No, it was Physics II where we did physical optics, which I used Jenkins & White –

Yes, yes.

– which is a great textbook, I think, and really felt I was on top of physical optics and really enjoyed teaching it. I thought it was fascinating. That was in Physics II, though.

So were those topics that you were teaching then new to Physics I from the time you'd done it?

No, I don't think so. That, the physical optics I'm talking about, is Physics II, and that might have been relatively new, I think, because I think Jenkins & White was a relatively young textbook. So that might have been a bit of new stuff. I think the Physics I course didn't vary a great deal between as it was taught by old K.G. and George and Gordon. I don't know it did change very much, actually, Alastair, but – not to my knowledge, anyway; but it might have done, but I don't think anything other than classical physics was ever taught in Physics I, was it?

No. No, not for a long time.

No, I think that's right.

How did you rate Roy Burdon?

Very thorough, a very thorough teacher, but not particularly exciting, I don't think. But he knew his stuff very well. And George Fuller was not an exciting teacher, either, but a very good teacher at the first-year level.

Yes. Systematic, Barbara Kidman described him as.

Yes, probably right. Roy, I think, taught well in Physics II and Physics III, but not excitingly, I must admit.

Now, at this time, I gather, there was almost no research happening in the department; is that right?

I would have thought, in the first years I was there, the only research being done, I would have thought, was probably John Symonds, who was working on some stuff which – John loved to be quite important, so his work was something to do with using silicon for detectors for radar work, which he was carrying out in the Physics III lab, very hush-hush. So that was a piece of research work that was going on. And the other piece that was going on was quite serious stuff by John Cowley on the electron camera downstairs. Now, do you have any recollection of ever seeing that Finch electron camera?

No.

Don't know when it went. You know, it's a sort of a precursor, almost, of the electron microscope, so you had a beam of electrons that came down, reflected off a crystal, got diffraction patterns off the crystal and analysed those to get the crystal structure. Now, John Cowley, who went on to really make a name for himself in CSIRO and then overseas, he was the expert on that old Finch camera. It was quite old, even by the time I saw it, and it was put together – it was a professional job, bought from the UK, I guess – put together with soft-solder joints everywhere and pumped by a mercury diffusion pump. The combination is not good, because the mercury immediately goes for the solder joints, which started to leak, so the electron camera was smothered with Apiezon wax and that's smothered with Plasticine and frequently repaired. (laughs) But that's how it worked.

The source of electrons was a wine bottle, a cut-off wine bottle, sitting at the top of the apparatus with the top sawn off and a cathode put in the top where the neck of the bottle had been sawn off. That was sealed to the top of the apparatus with an Apiezon joint and shellac and it was a cold cathode device and it just used to put a

high-potential ..... and it was the source of electrons. Pretty difficult. (laughter) But John Cowley did some serious work with that instrument.

So what was he measuring; do you remember?

No, I don't know – no idea what it was. But all his work was, of course, by reflection off metal surfaces. So he'd get a semi-circular diffraction pattern and, by measuring those rings, he'd get the surface structure of whatever it was he was investigating.

Yes. He got an MSc in 1945.

Yes. See, I would have joined as a cadet, I would have thought, about that same time. But by the time I got up to that level it was sort of '48, '49ish, I suppose.

Yes. Roy Burdon did some famous work on the surface tension of mercury, but that was all finished by then, was it?

All finished, yes. His apparatus is still there, isn't it, Alastair? A beautiful piece of work for the so-called sessile drop method; is that – – –? It should be in one of the cabinets there in the museum. All made of quartz.

Yes. I think that's all in storage now.

All in storage. Yes. I would like to find out whether one of my things is in storage, too, because if it's going to come to that I might go to the department and ask. There are two things I'd like to know about: one was a Wilson cloud chamber which I fabricated for one of the conversaziones, and the other was this mass spectrometer leak detector, which I'd love to know if it still exists.

When you got to third year, how big was the class then; do you remember that?

I would have thought somewhere of the order 10 or 15. No more. Yes, I think that's about right. And I thought about somewhere between 30 and 40, I would have thought, in Physics II.

Then you became a cadet, did you and did honours over two years; is that [right]?

Cadet right from the word 'go'.

Oh, were you?

Yes. And so, yes, I went into the department as a cadet, Roy Burdon's cadet, and I think Graham Elford might have become a cadet a little bit later, but I don't think he – he didn't start off when I did. But I went in as a cadet, so it lengthened my course to beginning of honours from three years to four, and so – – –.

And then did you do honours in one year or two?

Probably one. I don't really remember.

Because Graham, I think, did honours over two years.

Well, I might have done too, I think. Because honours was a funny old course. He would have told you about that, that we had to absorb this awful book, Joos, by reading some of it with the professor. No, that wasn't; no, that was something different, because with a professor sitting on his couch we read scientific German – which was an absolute hoot, of course. But Joos, we had to do our serious physics. Have you ever seen a copy of Joos?

Yes, I have. It's a dense-looking book.

(laughter) Not exactly exciting. It's very rigorous, but oh boy, it's pretty dense. That's all we had to study, actually. That was what we were examined on.

It was all theoretical physics.

Yes. Yes. That's what we were examined on. It was pretty hard. So I didn't enjoy honours, really. I thought that was pretty tough. I enjoyed what I did experimentally, but I didn't – – –.

So who lectured that?

No-one.

So there were no lectures in honours?

No.

You just studied the book.

Just studied the book.

With guidance of some sort?

I don't think there was much guidance. I'd be very interested to ask Graham that question, but I don't think there was much guidance, because scientific German, sitting on old K.G.'s couch, was just – (laughter) just more or less amusement, you know, trying to understand scientific German with a dear old boy whose cleft palate was quite difficult – – –. (laughter) I don't think we – must ask Graham. I should have asked Graham a few things before we had this, but you can ask him as well. Have you had your interview with Graham?

Yes, I have.

I wonder – did he say that we had any guidance with Joos?

No. We didn't talk about that.

No. Well – – –.

There are things I need to go back and [talk about].

I was going to say a memorable omission, I would think. (laughter) Yes, that's what we had to do for honours. It was tough, it really was.

But that was an unusually large class, wasn't it, when you did honours?

Oh, it was. Yes, it was nine. Usually – it was usually, I think, somewhere between two and four, and this was a spike because I think it went back to the more moderate numbers. It was just a coincidence it happened that year.

Between none and four, actually.

Was it?

1941 there were none. 1945 there were none.

Is that so? What year are we talking about now?

We're talking about '48.

'48.

A distinguished group, I would say.

Tell me who were in it.

Well, I'll go through them. Canny?

Yes, Nick Canny. Now, I'd love to know: what did Nick do eventually, do you know?

I don't know.

No, I don't know.

Then there was you. Deland?

Yes, Ray was a bright boy. Suffered with a bit of problems with personality, he was a difficult boy to get on with.

Graham Elford?

Yes.

Robert Fry.

Yes, Bob Fry. He went into the Atomic Energy Commission.

Colin Gum?

Yes. As you know, became a very distinguished astronomer.

Barbara Kidman.

Yes.

Robert Spurr.

Bob Spurr ended up in England – at Imperial College, I think – but doing what, I don't know. But I think he did very well, too.

And David Sutton.

And Dave, yes. Dave was probably – I wouldn't be surprised if Dave was the brightest of the lot. He was a very shrewd cookie. It's a tragedy he died when he did die – – –. He and I worked closely together when we began doing our honours work,

and so we probably did honours together, I think, and actually did our experiments – built our equipment together, even. But Dave – I think, of that bunch, I think Dave was probably the brightest. He was a very shrewd boy.

Yes. We're jumping ahead a bit. You finally submitted your PhD theses in the same month.

Who?

You and David.

Yes, that would be right, yes.

But in that honours class, out of the nine students there were six firsts.

Yes, and I think they'd be well-merited, too, because – – –. Who were they, actually? Let's see if we can get them. Barbara would be one.

Yes. I did have a list on which they were marked.

Barbara would be one; David would be another.

Graham was one and you were one.

And Graham, me.

So that's four.

Four. And two others.

Don't remember. Then what did you do after honours? You were eventually appointed a lecturer and began a PhD, but how did you get into that from the end of honours?

Well, we're now talking about – yes, we're talking about Huxley's arrival now.

Yes. He arrived early in '49, did he?

Yes, probably – probably early – – –. Wait a minute. Yes, early '49, because, interestingly enough, Alastair, my father and mother went overseas to see my brother, who was in England at that time, and Huxley rented my father and mother's house. And so I saw a bit more of them then than I might otherwise have done, because Father and Mother's car was garaged in the garage in their house and I was

expected to run this car occasionally to make sure it worked. And so – have you got a date when Huxley arrived?

January 1949.

January '49. Yes, well, Mother and Father – I don't know what they did for the first little while, but I think Father and Mother wouldn't have gone overseas until the European summer, just a little bit later. Huxley had their house. Margaret was five – I remember very clearly; dear little girl – and my family's car was still garaged in the garage at that house, so I had to go to the house every now and again to run the car so it didn't get fouled up over the absence, and so I used to see the Huxleys on those occasions, which was interesting. (laughter)

So Huxley came into a department which was, you would have to say, at a low ebb.

Oh, yes.

Essentially, no research happening.

Yes. I must tell you how it all began, because he walked in – you have to remember he'd come from a readership in Birmingham, and academic salaries in England were appalling at that stage. I don't know when they picked up, but they were awful. So he arrived in – they weren't shabby, but they were decidedly old clothes – tweed sports coat, grey flannel trousers, a felt hat. I remember clearly, as if it was yesterday, him walking into the office and saying, 'My name is Huxley.' (laughter) It was a great moment, actually.

Yes, and so what did he do? The first thing he did was to set two of us, David Sutton and me, to work on doing similar experiments that he'd just been doing with a student in England, an Egyptian called Zazu[?] who I think must have run rings round him because I thought he had a small apparatus, diffusion apparatus, made of stainless steel which couldn't possibly have worked, so I think Zazu probably faked every result that ever came out of it. (laughter) Anyway, so I built the very first one of these things – very simple; only two centimetres in length, similar sort of thing to that – – –.

So that happened very quickly after Huxley arrived.

Instantaneously. We began our PhDs pretty well straight away. And it was on electron diffusion, and so I built this jolly apparatus and we began straight away, and had to do all the glassblowing on the vacuum system, and so – – –.

How did you learn that?

Oh, just taught myself. It was interesting. It's an exercise – that sort of glassblowing is an exercise in defying gravity, because you can't move anything. A proper glassblower, as you know, gets the equipment in his hands and rotates it and gets nice even heating, nice even gravitational pull on the glass. But when you have to put together a vacuum system consisting of pipes and so on, static, you have to learn how to defy gravity, so it's quite a difficult job. So everyone used to have various tricks, like making sure the ends of the pieces of tube you were going to join together were not exactly butted but had a bit of a gap at the bottom so, when you pushed it together, you'd get a collection of glass at the top which you could then swage down to the bottom so eventually the joint ended up with fairly uniform glass thickness everywhere.

But how did you find that out?

Oh, just did. Just learnt how to do it. I don't know how; just did. Anyway.

I gather from what people say that you very quickly had a reputation as a very skilled experimenter and glassblower and – – –.

That's probably right. That's probably right. I used to love making things, actually, Alastair. See the little – this little fellow, for example? That's a bit – was at that time. That's later, I guess, but this is one of the things I made early on, which is still going, as you can see.

Oh!

It's got about – I think it's about an eight-pole rotor in there, it's of course highly synchronous, so it keeps perfect time, a little thing shining through the window just tells you it's still going.

So you made that around 1950, '49.

No – later, I guess, that one probably was. I did all sorts of crazy things. I made a synchronous gramophone motor, of all things. And to make it run at 78 rpm requires a rotor and a stator with 77 teeth. So 77 multiplied by 78 comes to something or other within point one per cent of what it ought to be. (laughter) But the problem with that thing – the rotor would have been this diameter, I guess – problem with that thing is the stator had also to be mounted flexibly, because if you didn't mount it flexibly the thing hunted, so (laughs) 78 would be 78 plus or minus probably about – I don't know what. Used to go (hums in variable pitch), which wasn't good for the reproduction. So eventually I cured that by mounting the whole of the stator on a ball race, highly damped so it could move a bit, so all the hunting was taken out on the stator rather than on the rotor, which had a fairly big mass by the time you have the rotor and the turntable sitting on top. The preference was for the stator to hunt a bit, rather than the rotor, and so if it started to hunt the stator would just jiggle round a bit and then settle down. (laughs) I think it was 78 fairly constantly when finished.

Now, Huxley started work in some ionospheric research and the meteor work.

Oh, yes. Yes.

Did that start very quickly, too?

Yes – I think simultaneously. I think Graham's work, upper-atmosphere work, began at the same time as the laboratory study, so he got both lines of research – – –.

So he really hit the ground running when he first arrived.

Oh, he certainly did – especially when, as you've just commented, the only research work was going on by then – I think Cowley must have gone, I think John Symonds had gone as well – I doubt if there's any research going at all when Huxley landed in. And so, as you say, he hit the ground running and got things going very rapidly.

So what was the social life of the department like – you know, was the department a community?

Yes.

Was there a tearoom where people sat and chatted?

There was indeed. It's on the first floor. I think it's pretty well along that – in one of those rooms facing the Darling Building –

Upstairs.

– upstairs.

It was there for a long time.

Yes, it was there for a long time.

But that was there in Kerr Grant's day?

No, I don't think so – or was it? Well, Helen will remember that, because – yes, I think it was. I think it was there in Kerr Grant's day. But I don't remember old K.G. coming up to that, though. Helen, by the way, was in the department in those years as the photographer.

Oh.

And so she goes right back into those years as well. I'll just ask her – it's an interesting question – just to find out whether – – –. (break in recording) We don't remember Roy Burdon or Kerr Grant ever coming to the tearoom, but George did, certainly. And Helen said it was in a very small room initially and then it expanded out into a bigger sort of common room on that side facing the Darling Building.

Yes.

And you say what about the social life. Well, the Huxleys actually put on parties, which were a great success, in their house. Not quite sure whether they were more than just Christmas parties, but they were – Molly certainly did that, which was great. And I think they were a bit of an innovation, because old Roy never did it, neither did George, and Kerr Grant certainly didn't; he would have been too old. So I think Molly Huxley's parties at her house – not frequent, but they were there – were an innovation, which was much appreciated by everyone.

When I was an honours student, I became aware that there was an annual departmental picnic.

Yes.

Was that an institution from the early days?

I don't think so, not that I remember. I don't remember having them. I remember picnics here, but I don't remember them in Adelaide, I must admit.

Now, working on your PhD project, you clearly did a lot of construction of the equipment yourself. What technical support did you have? Was there a workshop in the department.

Oh, of course – yes, there certainly was a workshop. But I must say that I did all the fabricating the apparatus, a photograph of which is still in my thesis but I don't have one here, and did put together the vacuum system. So I would have done it all, I think, which was – in those days I was rather fussy about how things looked. I guess I never got over that, really. So the vacuum systems had a nice duco black board on which the retort stands, which were chrome-plated, were mounted; and then all of the glass tubes and so on were supported off that. And the only thing I wouldn't have done would have been I wouldn't have glassblown the envelope for the apparatus; that would have been professionally done. But all the glassblowing for the vacuum system I would have done myself, and made the apparatus itself.

Now, Perry Miles referred to a business run by Oliphant's two brothers.

Yes. Michael – not Michael; Michael's the grandson. Harry. Harry Oliphant. I don't remember – I suppose there was a second brother, but Harry was the principal man, and certainly for tough bits of glassblowing – like making little glass diffusion pumps – they were done by Harry Oliphant's company.

Yes, that's what Perry talked about.

That's quite right.

Said he thought they were in Hutt Street.

I think that's about right, yes. But he had some – I don't know how good, by this time, Harry himself was, but he had a young man there working with him; he was very skilled. Of course, little glass diffusion pumps are quite nasty to build. They've got interior jets to them, where the oil vapour comes out; a couple of reservoirs, one receiving, return reservoir, and the main reservoir where the oil is boiled; and so they're quite hard to make. Eventually, though, some of the more complicated things – the original envelope for that, by the way, was made at WRE, of all things.

And what about John Gascoigne? He was in the department from –

Age 17 or so.

– from the year you were doing honours.

Yes. And he and I got together really quite early on, and I would think that he made all that, probably. If I remember correctly, I designed it, he made it. And when I first would have been teeing up with him he would have been about 17, I think. And then I doubt if I was using his services much then, but gradually we worked closely together. I certainly designed that, I can remember. He might still have the original sketch of it, which was pencil on cartridge paper, (laughs) and we made it from that. And I can remember some exciting times we – in those days, we felt that we had to keep everything scrupulously clean – in fact, we did have to keep it scrupulously clean – so it all had to be degreased. How did we degrease it? With ether. And so I can remember John and I starting off for an assembly of one of these things, probably during the day – it might have even been in the morning, I think, because it was a long job. We had to clean all the parts with ether, in a fairly confined space, assemble the apparatus and finish it off in one go, and sometimes that finished up at 2 in the morning, by which time we were as high as kites on ether vapour. (laughter) It's a wonder we didn't kill ourselves. But we really did have to keep all the surfaces spotlessly clean.

So there were joints there that are greased, are there?

Not in this one.

To seal?

Not in this one.

What was the assembly, then?

Assembling all the interior bits, everything, starting with – – –.

But the process of assembly – was that essentially a glassblowing exercise?

No, no. No; mechanical. You see, this is all held together – – –.

So how is it sealed?

Oh, eventually – these are later days, here – but the assembly is, essentially, putting all this structure together with a whole lot of accurately-ground spaces, tie-rods through the middle, accurately aligning it – eventually we used something called a Taylor Hobson microscope which enabled you to peer down through the source hole, which is a one-millimetre hole in that plate at that level, and it had to be accurately aligned with the axis of the apparatus, so you did that optically. But the assembly of those things, getting it scrupulously clean because we wanted to avoid anything that was going to outgas, we started off early in the morning and just proceeded till the whole job was done, however long it took.

But then you've got this assembly of that inside structure.

Yes.

Which has to be placed inside the chamber.

This. Yes.

How was it then sealed to the chamber?

Well, one thing that's – this is later, a bit later on in the piece, this one, but let's describe this one. First of all, the whole of that apparatus hangs – I might have said this a bit earlier – it hangs on a ball-and-socket joint. The female part of that is attached the copper-to-glass seal, which is on a glass tube running down from there. The male part is mounted on this, and to assemble the whole thing you just put it up and turn it through a small angle and it just hangs there. But the fact that it's a ball-

and-socket joint enables you to centre it accurately in the apparatus with these little adjustable screws there. And it has to be very accurate, because that rotor has to spin within that glass tube, so the actual alignment has to be done very carefully. Then, in the early ones, we just used to have here – we used to have a copper ring and we used to seal that onto this and onto this with Apiezon black wax, which has a very low vapour pressure – so that's later on.

Yes.

But eventually we decided, 'Well, we could really – – –.' Well, John Gascoigne, again, developed a wonderful glass lathe, a vertical glass lathe, so you could manipulate the two halves of this very accurately. This one's on the carriage – yes, this one was fixed, I think; this was on a moving carriage, which moved up and down extremely accurately; was controlled by oil, I think – you know, hydraulically controlled – you know, you could position the thing to a thou. So he had a set of ring burners around that join there. When the time came, he'd just put one part to the other, heat them up –

Fuse the glass.

– and then give a little blow and that was it. All done very quickly before any of the stuff could get oxidised. A nice piece of work, actually. Very nice piece of work.

Then sometime during all this you were appointed as a lecturer. When did that happen?

'49, I think. Right at the beginning. Right at the beginning.

So Huxley appointed you and –

Graham –

– Graham.

– and David.

Yes. Now, Graham and – – –.

No PhDs, because there weren't any.

Yes.

(laughter) So we were the first PhDs and we were appointed to our positions immediately to take on some of the lecturing load and to do our PhDs while we were doing it. That's how it was.

So what topic did you lecture first?

Physics I, I think, and probably very early on it would have been Physics II. I don't believe I ever did third year. I might have done, but I can't remember what I taught – might have been Modern Physics I taught in third year. But Physics II I certainly taught, and Physics I. So we were thrown in the deep end, Alastair, because, you know, we were pretty young then. No lecturing experience. As far as I can make out – you never know whether this is true or not – but as far as I can make out the first-year lectures were okay, as far as I know. Never heard anything to the contrary, anyway. (laughter)

That's a good sign.

Yes. I had actually no tomatoes. (laughter)

Or paper darts.

Or paper darts, no.

What was Huxley like as a supervisor?

The only thing I could say would be laidback. He practically didn't do any supervision of our PhDs at all; we had to do it, essentially, all ourselves. He was always interested, enthusiastic, and – rather contrary to the feeling I think that you might have had of him – he had a keen sense of humour and he was usually serious but I really got on with him extremely well. I thought he was a lovely guy. And of course I got to know him extremely well by the time we'd written a book together, because that took place over, I should think, two or three years after he retired from the Vice-Chancellorship here. I used to go out to his house in Hughes every afternoon for day after day, week after week, while we wrote this wretched book; and, as I say, the one thing I remember most strongly is that, during all that time

when we could have – we had quite vigorous discussions over this and that, we never had one cross word, which was pretty good.

That's marvellous. I only ever knew Huxley as a first-year student.

Right.

And my memories from first year are of Huxley as a fairly serious person, I suppose.

Yes, yes.

Presented solid, systematic lectures.

Yes, exactly.

And I remember his eyebrows. (laughter) And then I had your lectures in geometrical optics –

Oh, ...

– and my memory of those is of a very enthusiastic lecturer, and you used to write on the blackboard with one hand and simultaneously erase with the other and there'd be a cloud of dust –

Oh, poor you!

– a cloud of chalk dust.

I went on and did the physical optics, though, didn't I?

Not in first year, as far as I [recall].

Oh, that was first year, was it?

Yes.

Yes. Didn't I take you for physical optics in second?

No, because you left.

Ah. Well, physical optics in second year, I really worked that up. By the end of that course, I felt I really knew my stuff and thoroughly enjoyed it. Jenkins & White was a wonderful textbook, and so I – – –.

Yes. We had it.

Yes. So I had my course strictly based on it. Thoroughly enjoyed that. Geometrical optics I never really enjoyed, but – – –.

So what would you say was Huxley's impact on the teaching curriculum?

I would say probably rigour, as much as anything. First year had the terrible book, which was Kerr Grant's Physics I lecture notes. I don't suppose you've ever seen a copy of that.

No, I haven't.

You're lucky. (laughter) And so I would think his higher-up courses, Physics II and particularly Physics III, were rigorous and by Physics III people would have found them very tough, I think.

Yes.

And so he is very meticulous as a lecturer but also didn't spare anyone, as far as content was concerned.

Other people I've spoken to have said that, in Kerr Grant's day, there was an almost total lack of what you would call 'modern physics' from the curriculum.

Oh, yes.

Now, did that change quickly when Huxley arrived?

I would have thought probably the main influence was Stan, probably, although – just trying to think whether at the more descriptive end the younger ones may have introduced the modern physics, at the descriptive end, into the earlier years as well. But you're asking me a bit there, Alastair, because I can't remember that with any degree of certainty at all.

Yes. What do you recall of Stan's arrival and his impact on things?

Well, he, of course – see, Huxley was still very much in the classical mode, a classical lecturer in electromagnetism. Electricity and electromagnetism was his forte, although he did have a quantum physics course he taught for a little while, I

think probably 'til Stan came. Stan introduced all that. But Stan was a great asset to the department, absolutely no question. I mean, his line of research in biophysics was excellent. He had good students, and I think it's – I just can't actually comment too much on just what his impact was as a lecturer, but being Stan I bet it was extremely well done, very meticulous and thorough. I suppose you – do you remember hearing tales about their young family? They had this very sick child.

Yes, yes.

Very sad, that was.

Yes, Harry told me about that.

Who did?

Harry Medlin.

Harry, yes. Something – I don't actually know what it is, but the disease she had was some lung disease called bronchiectasis, which is a foul thing. It took the little kid off at quite a young age, which I think broke their hearts.

Yes. Then it came time to write your PhD thesis in a university where there was no tradition of what a PhD thesis looks like.

Oh, absolutely none. We were the first that ever versed, as they say, upon that silent sea. (laughter) Yes, that's quite right. Yes, that was quite something. You see, I think Sydney was the only one that had PhD – they weren't courses, either. We didn't have a course.

No. I think Melbourne was the first one.

I thought it was Sydney, but it may be Melbourne. But anyway – when do you think Melbourne began, any idea?

A couple of years before I – – –.

Yes. I think Sydney might have been going a bit more before that. But really the PhDs began to burst on the scene round about the late '40s – yes, late '40s.

Yes. You see, Graham Elford tells the story of during your honours year that he made an appointment to go and see the Vice-Chancellor, A.P. Rowe –

Yes.

– who had not long been appointed, and Graham was thinking that he might need to go to the United Kingdom to do a PhD and the Vice-Chancellor told him to stay around because by the end of the year it would be available.

Was that ’49, do you think, or ’48?

That was ’48, I think.

Yes, I think that would be right.

Yes, that was ’48.

’48 – yes, that would be right. Yes. So he must have – – –.

And furthermore he told him there was a new professor coming, who would be introducing new research.

Yes. Now, Graham must have relayed all that back – – –. Oh, Graham was probably just a little bit ahead of me, because although we started our courses, undergraduate courses, together, I was already, as from the word ‘go’, I was a cadet, and Graham didn’t come to be a cadet until the later years and so he probably got one year ahead of me, so he was probably talking to Rowe about those things before I was at that stage, I think. I think that’s how the timing goes.

So what guidance did you get about writing a thesis? How did you know what the requirements were?

(laughs) Didn’t. (laughter) We didn’t.

Because later generations of PhD students, they look at the theses of earlier students.

Earlier ones, that’s right. Did Graham give the same answer, do you remember?

I don’t recall asking him that question.

(laughter) I think we essentially got none.

That's very interesting, isn't it, that you go into a PhD program where there was no tradition –

Right. Exactly.

– and embark on it with essentially no supervision, and write a thesis – – –.

With no supervision.

You really defined – you defined the Adelaide PhD.

(laughs) I don't think so. I pretty much doubt that. But they were pretty thin, ours, too, as you probably remember seeing. Mine's only about that thick, I suppose. The last one of my PhD thesis students here wrote a volume like that. He was a bit verbose, perhaps, but oh! Enormous. (laughter) And, of course, one of the things which made life tricky in those days was no easy method of reproduction, so all one could do was to punch out the original with five copies with five sheets of carbon paper in between.

Well, it was still the same in nineteen sixty-whatever-it-was when I did mine.

Was it really? Yes, it's hard yacka. (laughter) If you made a mistake, having to correct four copies by hand, as well.

But you were the first – you and Dave Sutton and Dave Robertson were the first Physics PhDs.

That's right, yes. I wouldn't have picked Dave Robertson, but I certainly knew that Dave and I were.

Well, they were all awarded at the same time. He was six months later putting his thesis in.

Who was?

Dave Robertson.

Dave Robertson, yes.

You and David Sutton put your theses in –

Together.

– essentially together.

Yes.

So that was very much a collaborative work that you did.

Well, we must – yes, we were certainly working very closely together. There's no question about that. And so, of course, we had to write our things both independently, but the work – bulk of the experimental work we did together.

Yes. David later on went into seismology. How did that happen?

You've asked me that in the set of questions. I really don't know, Alastair. I don't know how that began. I think that he felt that, by the time we'd come through the end of the PhD, it was time to have separate projects, otherwise we'd be tripping over each other. So I really would like to know the answer to that question.

I've got a feeling that it is something to do with the International Geophysical Year and that some opportunity came up that Huxley got hold of or something.

Yes. There's actually no way of knowing that, because I don't think there's anyone we can ask about that.

Which is a pity, isn't it?

It is, because I noticed that was one of your questions. It would be worth asking Graham, to see if he knows.

It may have been Graham that told me about the International Geophysical Year. I've forgotten now.

Yes. I have a – IGY was mainly – – –.

Which was around about 1957.

Yes. I always thought the IGY was more related to the sorts of things that Graham did. I don't remember what the terrestrial aspect of it was, the IGY, though being geophysics there must have been a very substantial terrestrial component, mustn't there. But I don't know the answer to that.

And so how did your work develop after you finished your PhD?

Well, I suppose the most notable thing was the design and construction of that thing, which was designed for two purposes. Well, first of all I should say that if you were doing work on nice easy gases like hydrogen and nitrogen the divergence of the electron stream isn't too severe, but when you start to do monatomic gases, particularly the lighter ones, the wretched stream goes out everywhere. So the only way of dealing with those, to help get versatility to cover what you're trying to do, is to have a thing of variable length. So that's not only got variable length, that apparatus, but also the receiving electrodes – because all you do is to measure two currents on the receiving electrode – so you've got two choices: you can vary the length, and you can also have an electrode which has various divisions in it. So that thing has a one-centimetre central disk, the next annulus is two centimetres, the next is three and the next is four and so on, all highly insulated from each other, each mounted on copper/glass seals, and each with a very, very fine gap between the two so you don't lose electrons. So that was quite a major achievement, actually, because all the electrode segments are held on these double-ended copper/glass seals, and so the way John did it we mounted them all on that with silver solder, got the whole thing assembled and then ground the surface completely flat.

There were two requirements, actually. First of all, all the surfaces had to be extremely flat and highly-polished. The gaps had to be very narrow between the electrodes. And it was all a nice piece of work on John's part to actually fabricate all that.

Yes. So when did that piece of equipment come into operation?

I would have thought it came with us in a box from Adelaide to Canberra in '61. I wonder if John's got written on the back when it was made. He might have. No. Only the photograph was branded.

But did you use it to collect data in Adelaide?

Yes. And that came over in a large box, spring-mounted, by car, across the Hay Plain. (laughter) Yes. So that's its genesis. So that was finished in the Adelaide days.

Could I go back to Huxley's role in all of this? He, I presume, provided theoretical support to this work.

Yes. He was absolutely not an experimentalist. So, by this time, I think all the theory was done, so all – at least, all the theory behind this kind of experiment was done, not the analysis. So very soon – well, let me go back a bit. He was not an experimentalist, he was not a designer of equipment, so he wouldn't have had any input into that thing at all. His forte was in the basic theory of electron motion in gases. But he was overtaken to some extent, because what really pushed the subject along a huge amount was being able to interpret the results of these things using numerical solutions of the Boltzmann equation. Now, you just couldn't solve the Boltzmann equation numerically when he began all this stuff. It only came along later, when we had computers to do it. And the first person to break into that area was Rodney Jory, one of my students. He came across from Adelaide, so he started in Adelaide and finished here. Halfway or two-thirds of the way through his course here the first IBM computer was introduced to the ANU, the 1620 – a small thing; you know, your pocket calculator can do just about as much now, certainly any of the desktop ones easily just eat it. Anyway, Rod was very good at doing things. He got down, learnt how to program this wretched thing with punch cards, and so he was the first one to do numerical solution to the Boltzmann equation, which was used to –

That's interesting.

– to analyse the results of these experiments. Now, all Huxley had, all he could do, was to use a thing which he pushed just about as far as you could go, and that was called 'free path theory'. So you sort of tracked the electrons by free paths and so on, and compared with the Boltzmann theory it's very crude, but you couldn't do anything with the Boltzmann theory until you had electronic computers to do it. So Rod Jory did a great job, actually. He got onto this immediately – pretty well soon after we got here, the 1620 was introduced, and he used it to compute the – – –.

So that was here in Canberra where you had this 1620.

In Canberra – that was in Canberra, yes; past Adelaide days.

Yes. So what was Huxley's role then, when the numerical methods came in?

None, actually. No. No. His role in analysing these experiments finished with free path theory, actually. And so it moved on to another generation, actually, when we got here, the interpretation of experiments. You see, all we were doing in his day was to calculate what you'd call – well, mean free path: average quantities, that was the average distance an electron travelled before a collision, related to the cross-section but not actually the cross-section; and something called the fractional energy lost per collision. So all his theories were based on those two parameters. Very soon after we got here – Rod was the first to pioneer all this stuff, really – we started numerical solution to the Boltzmann equation which not only gave us a way of getting very accurate cross-sections out of the data, but other things as well. And this work, I think we pretty proudly say, led straight on to I think the most accurate electron/atom cross-section that's ever been measured, and I think we still hold that record, probably still. It's very – you know, only one-off case, and that's for electron helium scattering, but we got that cross-section accurate to about plus or minus two per cent. It's from a swarm experiment, you know, not a single-collision experiment, but for one of these things. And that required the numerical solution of the Boltzmann equation, and it could be done. You get a unique result for an elastic cross-section. We did it. And that result, which was accurate to about plus or minus a couple of per cent from a few millivolts up to about 10 or so electron volts, I think is still maybe one of the most accurate cross-sections that exists, and it agrees with theory. When theory came along and did the same thing, they fit right on top of each other, which is very nice.

That's nice. So who was your first PhD student that you supervised?

John Lowke, jointly with Huxley I guess, but Lowke and Rod Jory were the first two.

So when – – –.

When we came to Canberra, those two came too.

I see, yes. What do you remember of the arrival of Bert Green and his impact?

Not a great deal, actually. I remember that they occupied a room very near my office upstairs. They couldn't have been more disparate characters, of course. Did you know Messel, ever?

Not really, no.

No. Well, he was the extrovert, you know, completely extrovert. Bert was the very reserved scholar. And so those two couldn't have been more different. I remember one occasion (laughs) when poor old Bert, who had a beard, came in and Messel says, with his rather raucous Canadian accent, 'Hey, Bert. You've got egg on your moustache!' So he goes up and rubs it off. (laughter) Poor Bert was flabbergasted.

And yet, evidently, they got on very well.

Oh, they got on well, very well. I think they were a good complement to each other.

Did the arrival or the advent of Mathematical Physics have a rapid impact on the teaching curriculum?

I guess it did, actually, Alastair, but I don't really know the answer to that question. It must have done, I think, but it was at a higher level, I think, than I would have been teaching when they arrived. So I would have been doing first, second year and a bit of third, but I think they were doing Mathematical Physics at a higher level. So I don't know whether they ever reached down into the earlier years; do you?

Well, interestingly enough, Harry Medlin introduced a liberal arts type subject: it was originally called Physics, Man and Society.

Yes.

Bert taught in that.

Did he? That would be later on, of course, quite a bit later. Well, gee whiz – that was – – –. I'm not surprised, actually, because he was a great fellow, Bert. A lovely fellow. I'm sure Harry would confirm that.

The other interesting thing, with your comment about Huxley's essentially analytic theory being overtaken by numerical methods, that when Messel was there with Bert Green they published an extraordinary number of papers on the theory of cosmic rays – air showers, I guess, in the atmosphere – which was a breakthrough

for experimentalists at the time but within a relatively short time was overtaken by Monte Carlo calculations with the coming of computers. And it's a very parallel thing.

Interesting, isn't it. Yes. I'm just trying to remember. I remember seeing one occasion where Messel had some enormous formula which literally filled a blackboard. I've forgotten what it was, but it went on (laughter) and on and on and on and on. They were both very bright, I think, those two.

Yes. And then came the day when you people packed up and moved –

They did, yes.

– moved to Canberra.

Yes.

Was that – must have come as a shock to the group, did it?

Yes Well, you see, Huxley had come over – first of all he left, after about – did he leave after about 10 years, I think? – and went onto the executive of CSIRO. He only lasted there – at his own wish, of course – for about 10 months before he got a call, would he like to be Vice-Chancellor of the ANU, which he accepted. I would have thought it would be more along his line, because he's always academically-inclined. And so he took that job and then, very shortly afterwards, I received this call from him: would I like to come over and bring the group, too. Now, that was an upheaval because it involved Malcolm – he was easy, Malcolm Elford, because he was single, had no commitments at home; for John Gascoigne and me it was very tough, because we each had just built our houses and put a lot of loving thought and sweat into them, so both John's family and our family were firmly-established in newly-built houses. We came across and spied out the scene, and the houses the university were going to give us to rent were horrible, and so that was – I mean, it was pretty good that our wives agreed finally to come, I must admit, because it was a shock from what we'd had, built to our own design, to rather primitive accommodation here. Eventually, we were able to build this house, for example, and we got what we wanted, but first of all it wasn't easy.

Going back, though: it was easy for Malcolm, and I think it was easier for the two students, John Lowke and Rod Jory. Rod had only just – probably a year or two into his PhD. John Lowke was just finishing up, essentially, so he was really virtually at the writing-up stage. So we were able to keep him going pretty well without interruption. Rod probably had an interruption of a year while we established the lab here, because we were told there'd be lab accommodation in the so-called Cockcroft Building, the Research School of Physical Sciences, but it had had a fire, which was the reason we could get into it, and so we were assured there'd be a laboratory there when we moved across; when we moved across, it was exactly as when we first saw it, just a burnt-out shell. (laughter) So that was a rude shock, but in a way a godsend, because John was able to design a lab to our requirements, which were quite different from any the Research School of Physical Sciences ever had before. I mean, we were doing precision work under clean conditions, and we needed that and we needed the shielding room. And so John designed all that and oversaw it going in. But it was a while before we ever had that. We had to make do in temporary accommodation for probably about a year. So Rod Jory's PhD was probably put back about a year by that.

Yes.

John Lowke was well-advanced in his, so he was probably not much put back, because I think most of the experimental results had been taken, or else he was able to set up the equipment he wanted to do it, to finish it off.

So John designed the lab with air-conditioning and so on, which was a great step forward to us because in Adelaide – (laughs) gosh, this seems amazing – we had to have temperature control on our experiments, because if that wandered all over the place the number density wandered all over the place when we were using a constant pressure; all that – so in Adelaide at one stage – well, we had a shielded room in Adelaide, too. We were using very low currents – 10^{-12} amps and so on – which meant that any electrical interference on the electrometers was fatal. So in Adelaide we built a screened room out of Oregon and chicken wire and that was in one of the upstairs rooms and stayed there for quite a while.

Yes.

Here, we did the same thing, a bit more professionally.

So you built that?

Is that still – – –?

No, it's not still there, but I remember it.

Yes, yes, we built that – because we couldn't do the experiments without, Alastair, because any electromagnetic interference on the electrometers used to measure these extremely small currents was absolutely fatal, so we just had to have very quiet electrical conditions to do – – –.

This was upstairs towards the western end on the southern side.

Yep. That's right, I'm sure. Yep. Yes. And it was fairly crude, that. It was chicken wire, and it was probably double-screened.

Double-screened, yes.

Double-screened, yes. Anyway, it served a reasonable purpose. But air-conditioning, no. Initially, we did it with blocks of ice, which wasn't the best.

So there was a period when Huxley went to the CSIRO and you were left in Adelaide. So the group then was you and Malcolm Elford –

Yes, two students.

– John Gascoigne and two students?

Yes, that's right, yes. And so that's what eventually came across, that lot.

Yes. When did Huxley go to the CSIRO; do you remember that?

When what?

When did Huxley go to the CSIRO? Must have been – you see, I did Physics I – oh, that was '59 I had Huxley as a lecturer.

You did, in '59.

Yes. You're talking about '61.

Well, I would have thought he left probably – – –.

Probably at the end of that year.

I would think so.

Yes.

That would be my guess. That would be my guess, yes.

So he'd gone in '60 and then you finally – – –.

We went in '61.

'61.

That's right. Now, something else I was going to say. Oh, yes: temperature stability was one of the things which was absolutely essential for us. So first of all we tried to get reasonable temperature stability with blocks of ice in that upstairs room, the shielded room. Then eventually we put in a temporary air-conditioner and that was a great help. And then, when we came here, we put in as a requirement air-conditioning to get stability to a couple of degrees or so, because otherwise it was hopeless trying to get constant number density in our experiments.

Now, the other aspect of Huxley is to do with funding for research, and I gather that he renewed his contacts with the Radio Research Board from earlier in his career, and that became a source of funds. Was that – – –?

Yes. There were two sources in Adelaide: Radio Research Board and the Electrical Research Board. Both were tapped into by Huxley, and both to only a very small amount. So they bought some of our equipment and so on, which – well, it's hard to imagine, actually, Alastair, but in the early days of my research I remember a reel of hook-up wire was a prize. That seems hard to believe, doesn't it? But there was nothing. And I remember all the retort clamps I had to get cast myself, and then we had to actually machine them all. We had nothing! And so (laughs) then a little bit of money came in from the ERB and RRB.

When we got to this place the transformation was just huge, because the first thing that – Malcolm had just come back from the States and it was just at the dawn of

ultra-high vacuum technology, so he came back with a little bit of knowledge of ultra-high vacuum technology. What do we do? First thing is we had enough money to build four sets of ultra-high vacuum rigs in glass with greased taps. It's not quite the thing you do with UHV, but remember we just wanted to be able to get low levels of impurity and provided the taps didn't outgas that would be good enough. So we built these things, four separate rigs, each with its own vac ion pump, small vac ion pumps. That was absolutely unheard-of, to have money to do that. In Adelaide we had practically nothing. It was very hard going.

Yes. What do you remember of A.P. Rowe as the Vice-Chancellor?

(laughs) Not much, except I think when he came to the University of Adelaide Huxley thought this would be an absolute godsend. And then, of course, somehow or other they got across each other, and I can remember him coming up – he always used us as a bit of a sounding board and a bit of a confidant – I can remember him coming up on some occasions being absolutely livid at something Rowe had done that he disapproved of. (laughs) So I don't remember much about him. I think Harry might have quite a different perspective, because he was quite good with the students, A.P. Rowe, and I think that Harry saw some of that, I think. I just – my view of A.P. Rowe (laughs) came largely from the reaction that Huxley had to him by this stage. Remember he was in charge of TRE in Malvern, and Huxley was one of his staff –

Yes.

– and so they would have known each other well then. I don't think they got across each other, because I think that probably Rowe might have been responsible for getting Huxley out. But it wasn't long before they were at each other's throats in some way. I don't know why.

Yes. I rather gather that, when the chair was advertised, it was Oliphant who suggested to Huxley that he apply.

Yes, I think that's right, yes.

And that when Rowe got the application he promptly accepted it because he'd had high regard for Huxley during the War at TRE.

Yes.

It's very interesting that when it came to the next time the chair was advertised it was Oliphant, who was then here, suggested to John Carver that he apply.

Yes. Interesting, isn't it. Yes. Well, John was – he was 10 years at Adelaide, wasn't he, John was?

No – must have been more than that.

Was it really?

Because he came in – well, that must have been '62. About the same time you left, I suppose. Because there was an interim period when Huxley left to when John Carver arrived, when Stan Tomlin was acting head.

Acting head. It was a short period, though, wasn't it?

But it was a year or more, I think.

Was it?

I've got a feeling that John Carver was in Adelaide for about 17 years – I think it was about '78 or something – – –.

Was it really? Could well be. Could well be.

Well, Bob, my commission really is to talk about Physics in Adelaide.

(laughter) Yes. Well, we've done a lot of that, haven't we?

I think we've got up to Physics in –

Canberra.

– Canberra now, so perhaps it is time for us to stop.

Stop. I think so.

Thank you very much for your time.

It's a great pleasure. Thank you, Alastair. Thank you for thinking of coming. I'm delighted to have had the opportunity to talk to you.

It's been a pleasure.

END OF INTERVIEW