Malcolm ELFORD

This is a conversation with Malcolm Elford at his home in Canberra on the 7th December 2011.

Malcolm, thank you for making the time to have this chat.

Pleasure.

I gather you first enrolled as a Physics student in Adelaide in 1949, which is straight after the Kerr Grant era.

Yes.

Huxley had just arrived. What was it like then, what was the Physics Department like?

Well, my experience was a bit strange in one way, in that, for some particular reason, I got half of all my units in my first year. Why anyone allowed me to do this seems to me a bit strange, but my schedule was so crowded – – –.

Half of the units for the whole degree?

Half of the units for the entire degree I acquired in my first year, including Geology I and all sorts of other things, and in order to fit all this in I had to do Physics I at night. My memory of doing that is all the girls would take the front sort of couple of rows – one of whom had a blind dog, I remember, who used to sit docilely through all these lectures; crowded lecture halls – it was a period where the buildings and facilities hadn't caught up with the influx of all the ex-servicemen after the [Second World] War, and most things were crowded.

So how many Physics I students would there have been?

I can't answer that question.

But a large number?

A large – I would have thought a couple of hundred or something. You know, it was significant. Let's see, what else comes to mind?

Did Huxley lecture?

As I said before we started recording, I never heard Huxley lecture until we took lectures from Huxley – until I was an honours student, when he gave a course in the use of functions in solving differential equations. So no; I never had him as a lecturer in my undergraduate years – and I don't think he was very interested in doing undergraduate lecturing – – –.

Yes – but he did a bit, didn't he?

I understand that he did, he followed on from the Kerr Grant tradition of doing first-year lectures for one year, and I understand also – from Bob, I think it was – that he never repeated the experience.

Ah.

He was a meticulous lecturer. I mean, if you could get it all down you would have a magnificently logical, structured –

Rigorous.

– rigorous – he was also incredibly boring. I found him very boring as a lecturer.

Yes.

Was very different to my relationship to Huxley, later on.

Yes. You see, I did have Huxley as a lecturer in Physics I in 1959.

Oh, really? So he went back to doing Physics I?

He must have; I don't know when he did that. That was his last year. I wouldn't have described him as 'boring'. He was challenging. Serious, some serious stuff coming – – –.

Oh, yes.

I remember the bushy eyebrows.

Oh, the bushy eyebrows, absolutely – curling up at the ends. They were his trademark. I remember him as a very shy – well, in those years, they were not exactly 'god professors', but I mean they were sort of pretty remote from the average

student, and he appeared to be a bit more aloof than most. And it wasn't until later on I realised that he was ....[in fact] a very shy person and he was at his best, really, if he was with a very small group rather than large bodies of people.

Yes. Were you aware of changes happening in the department at that time?

As a student, not much. I remember most of the equipment was incredibly – either a bit out of date or required work or – I mean, I remember George Fuller and Kater's Pendulum and these things that he – – –.

So do I.

I was going to say how George used to rig it so that it didn't give you the right value of the gravitational acceleration, so anybody who got the right values of it had been cheating. (laughter) I loved that!

What do you recall of the curriculum, and was there any modern physics in it as you went through?

Very, very little. It was all pretty classical stuff. You got a bit in – yes, but I'm getting slightly confused with what I had in honours year, where there was quite a lot of modern physics. I mean, Graham ran a reading course on atomic energy; Harry Medlin took us for Spectroscopy; Doc Burdon gave us a pretty boring set of lectures on thermodynamics, which is actually not a subject which lends itself to great excitement, I think, but pretty important.

Yes, unless you're excited by the elegance and rigour of it all, I guess.

Yes. Well, later on, of course, it was – I think he did Statistical Mechanics as well, as part of that. And of course later on that's where I was heavily-involved from a research point of view – Boltzmann equation solutions and things of this sort. Pure statistical mechanics, of course, is the basis of electrons in gases stuff.

What about Stan Tomlin; did you have lectures with him?

Yes. Stan Tomlin taught me wave mechanics, and that was I think probably an honours course, because I remember having to reproduce the hydrogen atom in an exam. I thought he was good.

How would you describe him?

A bit hard to get to know. He had a sort of puckish sense of humour. He was fairly reserved, I thought. The person I warmed to mostly, actually, was Doc Burdon, who was a sort of genuine human being and very interested in what people were doing, particularly students. And, in fact, later on, he lent me some of his precious mercury. He did some research work with a guy called Murray Ziesing – and I think, from memory, that Mark Oliphant built his mercury still; I'm not sure about that.

No; I think it was Mark Oliphant's brothers ran a business.

Harry.

I don't know what their names were.

He was called Harry Oliphant, because Harry Oliphant did some of the construction of experimental glass tubes which involved quite a lot of tricky little glassblowing, down in a little shop he had in Grote Street, somewhere in the city.

Grote Street?

Yes, somewhere like that.

I have two votes for Hutt Street.

Oh, okay.

Memories are dim.

No, I don't – well, that's pretty hard to answer. Can't answer that one – actually, pretty hard. But I'm trying to think of the other lecturers.

But I think that Mark Oliphant did some work on mercury with Roy Burdon earlier on.

The only one thing that I was associated with – well, I've read about – is the sessile drop experiment, you know, looked at the shape, contact angles, mercury. But Burdon was very useful to me when I started doing a PhD because I had to build an apparatus to calibrate a pressure gauge built by Bob and John Gascoigne, and so I used mercury manometers to do it, and of course the question was how to actually

know what the level of a mercury column is, because it reflects so perfectly as a mirror any background. So the background and the top of the drop, top of the surface, just vanish, merge. So he suggested I use a Lloyd's mirror interferometer technique in which you shine light very obliquely across the surface and you get a series of interference fringes, and you can then put your crosshairs of a travelling microscope on these interference fringes and move – as the surface moves, these interference fringes move with it, as long as you've got the light shining across it, parallel. And that was an enormous help and he supplied all the mercury for that and told me – – –.

So you can measure changes in the level –

That's right.

– without ever being sure exactly where the level is.

Well, two arms: one was evacuated and the other one I was doing the measurements on. So he showed me how to pour mercury from under the surface, things like that.

Graham, you see, described Roy Burdon as a very able person who had been somewhat limited by Kerr Grant not giving him the freedom to go his way.

Well, I think the real problem was there was absolutely no money. I mean, when I started in an honours year doing – trying to do some experiments with Dave Sutton, I think it was, I had to go down and plead for a small length of second-hand hook-up wire. I mean, that's an absurdity. And people were, everywhere, getting equipment from army disposal stores. I mean, to me it's a complete miracle that any good physics actually ever emerged. It said a lot for people's ingenuity. But the difficulty was that you ploughed in vast amounts of time doing things that were totally stupid.

It seems to me, looking at the work before Huxley arrived –

Yes.

– that Burdon's work on surface tension was the only thing in the whole history of Physics in Adelaide that you would describe as a research program –

That's right.

– as distinct from a set of isolated projects.

I agree with that. Yes. Well, and Graham retailed[?] to me how, you know, when he, not long after he arrived, he just called Bob Crompton and I think it was Dave Sutton and Graham into his office and said he wanted to start a research program and, 'I've got this one and this one and this one; which one would you like?' And Graham chose the meteor – the upper-atmosphere winds. And he said to me at the time, he said at the time he knew absolutely nothing about it. I mean, how often would you choose to get yourself involved in a program that you had no warning about and had no background it? And he just did the same to Bob, so Bob chose electrons in gases.

Yes. I was saying to Bob that –

It's astonishing, when you think – – –.

– it seems to me they came in cold to a new field.

Yes.

Really received very little supervision.

Oh, well, Huxley was my supervisor, my formal supervisor, and he had the Oxford technique that you, if you're going to have a PhD student, you lock them in a room for X number of years and they either produce something that's really very good or else they go slowly mad, and that was pretty much what happened. He left me almost entirely alone. He gave me, I think, two suggestions, both of which I found to be wrong, about the field that he wanted me to look at, which in that particular case was measurements of the mobility of ions in gases; and he gave me one book to look at, and that was it. And all my interactions were with Bob Crompton, who really acted – – –.

So Bob, effectively, provided the supervision.

He provided all the supervision.

Yes.

But, I mean, no-one provided me with any supervision. I mean, I could have just gone off and done anything – and Huxley would never have known, either.

But is that right? I wonder whether – did Huxley provide encouragement?

Oh, yes. The one thing that Huxley always had was enthusiasm. He was enthusiastic about what you were doing, as long as it wasn't stupid.

So it seems to me that the PhD students at that time were provided with enormous freedom about what to do and which direction to go in – effectively, no [supervision].

Completely. In fact, people would ask you, you know, 'What do you want to do?' And of course you didn't know.

Yes. Effectively, no supervision.

That's right.

They got encouragement, and then had to write a PhD thesis –

Right.

– having no models of theses to go on.

Oh, absolutely.

'What does a PhD thesis look like?' How did they know that?

Well, that wasn't my situation, of course, quite, because by the time – it was the situation for Bob and Graham and so on –

And David.

– because they were the very first PhDs to emerge out of the system.

Yes. It was Bob and David Sutton were the first two –

That's right.

– and they both put their theses in the same month –

Yes, and they both contained – – –.

– and they were the first.

What?

They were the first.

That's absolutely right. I came – '53 was when I started, so I joined the group to do a PhD four years after it was founded in '49, and Bob and David had their theses through – gee – – –.

They were conferred in '54. But they submitted in February '53.

That's right. Because that was all going on – I know there was a great hoo-ha-ha, you know, I was trying to pick up gossip all round the lab – and there was a great debate with one of the examiners, who was in Sydney, who was also working in electrons in gases.

I wonder who it was.

Whose name escapes me at the moment. Anyway, and it was – from memory, I think, in the end, Huxley sent it to a third examiner.

Now, whose thesis are you talking [about]?

This was Bob's.

Bob's thesis. I see.

Who passed it. So, you know, this – wish I could remember his name, but I can't. So he had a long-running sort of – not vendetta, but disagreement with this head of – was it head of Physics in Sydney? Can't remember.

Well, Messel had gone there by that time.

Messel I met, who was there around then, anyway. He had his office just across the road from our lab.

Because Messel –

....

– was briefly in Adelaide with Bert Green –

That's right.

– which must have been in 1951, I think.

It must be later than that. Oh, ’51. I can’t remember the dates.

Yes. Messel was there ’51–53.

That’s right. Because I remember Messel. But, of course – well, then I started at the beginning of ’53, so he was around. I mean, I just knew him to see him, that’s all. But, I mean, when I started in ’53 it was an extraordinary experience because I had no idea, you know, apart from – because I had Graham as a model and someone to talk to – but I had no idea how one started with equipment and everything, and once we’d decided to do some work on measuring ion mobilities. Bob just – I still remember it: he gave me a Ferranti DBM4A tube.

What’s that?

It’s an electrometer tube.

Oh.

Specially-built electrometer tube which had all sorts of things to have very low-leakage currents and so on, and the idea was that I then had to build an electrometer to start with, to measure the ion currents. And then the next thing was he handed me a book called Benson, which was how to design high-voltage stabilising power supplies. So then I had to read this book, design my own power supplies that would produce the 600, 700 volts DC, and produce highly-stabilised output, which wasn’t so trivial in those days. And the point is that [in] any decent research laboratory you’d just haul one of these things up and put it in a rack – I mean, nobody goes round building high-voltage power supplies. And, of course, when we got to Canberra we discovered what an internationally-competitive lab does do, and you just buy all this stuff that you need to buy. I mean, nobody would build an electrometer.

Yes, it was a – – –.

And not only that, you had to design and construct the case and get it painted and engraved (laughter) and it went on and on.

Bob talked about having retort stand clamps cast.

Oh, yes. Well, I remember, for example, they couldn't have enough money to buy a standard resistor. I wanted to be able to do very accurate measurements using a standard resistor – currents, I think I was trying to measure. It doesn't matter. Anyway, so I ended up reading up books on how to build a standard resistor, and built one, you know.

Yes. You were really measuring a potential across a standard – – –.

That's right. And I was then into things like annealing of the wires and the schedule you had to use on the oven. The thing about all this is that you learnt an awful lot, much of which came in handy at various times in my career.

Well, that's the point.

And it taught you to be incredibly independent, because there was nobody to help you. If you went down the workshop you were expected – I actually machined all the electrodes myself. So you had to learn how to handle machine tools – and there was no course to do that; you'd just go and wander over to the other side of the workshop and say, 'Could you help me with this?'

So in retrospect you'd say you were fortunate that you had to do that, would you?

Well, it was pretty handy at times when I was in places where they didn't have very good facilities, because I could then very rapidly knock something up and use it.

I think for a long time Adelaide graduates had a reputation around the world for being people who –

Could do .....

– could do things and knew how things could be done.

Yes. Well, it is one way in which you can train students; but it's an unbelievably wasteful way, in terms of physics. It took me four years to get through a PhD course. Now, at least a year or year and a half, I think, of that was doing things that were completely nonsensical.

Could we just go back a bit, too? You talked about, in first year in 1949, you did half your degree points in just one year.

Oh, yes.

So how long did it then take you to complete the degree?

Well, three years was the statutory length.

You still took [three years].

That meant I had a fantastically good social life for the next two years.

I see.

It was a real mistake to do what I did, and someone should have pointed it out to me, because I just worked like a dog for the first year. I mean, I just didn't do much else except try and cope with the volume of lectures and pracs and stuff.

So then you did honours in '52.

Yes, I did honours in '52.

So what was honours like, then?

Well, it was small. I think there were five or six of us. The projects that we had to do – we had to do a project in the honours. The projects were really Mickey Mouse things. I can't remember in great detail, but they certainly – I had to, I think, examine the emission from a filament as to how much of the current was electronic and how much was negative ions, and I pretty well never got much past trying to get the thing free of leaks. The vacuum technology was crude in the extreme.

Now, I've got you down here as doing honours in '53.

'53 – I thought I started it – – –.

Did you have Colin McGee as a – – –?

Yes. Don McKelvie.

Yes. Kath McLeod?

Don't remember her.

Eric Murray?

Eric Murray, yes.

Bernie Milton?

Yes.

David Prest?

Yes.

John Smith?

Yes. I think that's the honours year.

Yes – I've got that – – –.

Where did you get that information from?

It's out of the – – –.

Archives.

Oh, it's the document that Harry Medlin produced for the – – –.

Oh, I see.

When was it? Centenary.

Yes. Okay. Yes. How many of them – there's about seven, is it?

But even the business of having – – –.

Kath McLeod I don't remember at all.

You see, Bob Crompton spoke of there being no lectures in honours – when he did honours in '48, that they had a book, Joos, *Theoretical physics*.

Oh, god – what a book.

....

That was still around when I did it. It was one of the books that you were supposed to – and I've got a copy, still got a copy, unless I've thrown it out. Terrible book, absolutely terrible book. Honours.

But you had lectures?

Yes, because I remember one of the courses – in fact, the only one – was called a 'reading' course, and this was in Glaston. We had to understand and be on top of the whole of Glaston's atomic energy book, and that was run by Graham, which was – I mean, from time to time I think he had small tutorials or something, but basically you were on your own, you were just given this book, and of course it was terrifying because you could be asked anything out of the whole book in an exam. (laughs) And, of course, Graham was barred from – they had to make special arrangements about marking the exam papers to avoid the conflict of interest.

So who else was lecturing in honours? Was Huxley doing some of that?

Yes. Huxley took us in honours for – ah, yes, I'm sure he took us for Electromagnetism, which was one of his things he really enjoyed, which followed on, of course, from all the wartime experiences. Statistical Mechanics, Wave Mechanics – can't remember all the courses. The honours year was an extraordinarily heavy year. I can remember – I mean, I think two of the people, when we were having this orgy of three-hour exams at the end of the year, I can remember two of the people essentially broke down. I think Bernie Milton was one of them. And they were given supplementary exams. I found it an incredibly stressful year.

So with the projects, you had a supervisor for the project?

Yes. I had Dave Sutton as a supervisor during my honours year because Bob was overseas at Swansea on sabbatical leave. But it was incredibly frustrating because the vacuum technology of the period, and certainly in the labs, was crude in the extreme – I mean, everything was done with Plasticine and Q-Compound and you wouldn't believe – you know, W40 and W60, and these black waxes. I mean, it's bizarre, when you actually think of it. And I spent, I suppose, most of my time leak-detecting, because I could never do anything because the thing would always spring a leak somewhere.

So what was the project?

Oh, the idea was to – there was some concern in their apparatuses in the lab where Bob and Dave were working that they were getting negative ions emitted from coated filaments as well as electrons, and if that was true that would be a difficulty in dealing with the current ratios from which they managed to extract transport coefficient data. So the suggestion was made that I should build a little apparatus to run a filament and separate these two components, and I decided to try and separate them by a transverse electromagnetic field. It only ever partially worked. I doubt whether we ever found what was going on, largely because of the amount of time I had to waste just trying to get this thing started. But again that was one of the cases – it was all made of brass, and one had to machine it yourself and solder it up, and – – –.

But that was a project which was linked to a research program in the department.

Yes. I already sort of had a toe in the water, in a sense, and I was working closely with Dave Sutton, and I knew what was happening in the lab. So when it came to do a PhD it was a very straightforward thing. I mean, I already knew Barb, and who was the other guy in the lab then? He went to Western Australia. Died some years back. Anyway, did work on drift velocities.

You see, when I did honours in – – –.

Bob Duncan was in floating around the place, too. The legendary Bob Duncan.

..... recall that name.

Well, Bob Duncan was the instigator of the 'yellow footprint' saga.

Oh!

Together with Eric Murray. That story is now well-known amongst the aficionados. John Lowke knows a lot about that. If you ever want to follow that up for any reason, John Lowke's your man.

What was his name again?

Bob Duncan.

Oh, yes. Did honours in '51.

That's right. He was floating around. He sort of worked in the lab when I was there. One was never too sure what he was actually doing. I remember on one occasion Bob Duncan and I went off to some Student Christian Movement conference or something during the week, and we got a message from Huxley: he wanted us back. (laughs) He thought that we were just arbitrarily having fun rather than doing some physics. And it was quite amazing, because I didn't think Bob Duncan was really seriously looking at anything much, until, about six months after he left to take up a job with CSIRO, he wrote a paper on a problem that had been puzzling people in the lab as to why the drift velocities of electrons in gases using a particular technique were pressure-dependent when they shouldn't be. And Bob Duncan showed – was the first to show that this was due to pressure dependence, two surfaces when they go – a pulse goes through shutters, you can get both forward and back diffusion, which distorts the pulses. And that was the basis from which John Lowke, who came along – was one of the people who came to Canberra in '61, virtually his entire thesis was based on that – extending what Bob Duncan had done. So Bob Duncan was a bright guy and he had quite an interesting career at CSIRO.

Now, when I did honours in '62 there was an honours room and honours students had a desk in the honours room.

Nuh-uh.

That was a 'no'?. And there was an honours laboratory, and we did projects which were, by and large, not associated with a research group at all. They were just like an essay topic or something – you know, put a bit of .... together – – –.

I don't remember any of that.

So it was not like that.

I just floated round the lab and perched on any old desk I could lay my hands on.

This was the lab where Bob and – – –.

That's right – Bob and Barb and – oh, Bill Macklin was the guy I was trying to think of went to Western Australia, and he was involved in the experimental discovery of

these pressure dependences. No. And when it came to doing a – as a research student, I just simply had a desk in – a table – actually –

You actually worked.

– bang in the lab, where all around me were all the apparatuses and things.

Already in residence.

Yes. Then, when Barb went off – I think she had an 1851 exhibition or something – I moved in to share an office with Bob Crompton.

This is Barbara – – –?

Barbara Hall, she was. She's now Barbara [Possingham]. But there was no formal arrangement for – it was a bit like the research program; I mean, everyone was just sort of independently doing their thing. And Bob was terribly useful in the sense that he could tell me who to go to and ask – I mean, you had to do everything yourself. If you didn't show any initiative to do anything, just nothing would have happened. I mean, there was nobody to look over your shoulder or anything like that.

So the PhD was a test of initiative.

Yes – and, I think, mental stability and a few other things! (laughter) But the other thing is, in order to do this, you had to work day and night. I can remember the enormous amount of effort we put in at night-times. I can well remember getting over my fear of the dark – not that I ever had much of a fear of the dark – by having to feel my way out of the building at 1 o'clock in the morning without any lights on. I used to do that routinely. And Bob Crompton, of course – it was a miracle, I think, he managed to do as much research as he did to get his degree, because he had an enormous teaching load, and I think people just worked at night.

Yes. Do you remember what Bob taught in those days?

I remember he taught a course on optics, geometrical optics.

Yes – I know, too; I did it.

You didn't, did you?

Yes.

What was he like as a lecturer?

Energetic.

Yes.

My memory of Bob is that he'd write on the blackboard with one hand –

And rub it out with the other?

– and rub it out with the other, and there would be a cloud of dust.

(laughter) That sounds like Bob. And I remember he did this course – I can remember him preparing the course; he put a lot of work into getting on top of geometrical optics. Jenkins & White.

So when do you reckon he started doing that?

Oh, boy.

When are you remembering him developing that?

That would have been round about 1955, I guess – would be my guess. (break in recording)

Malcolm, you were talking about, essentially, how it was you chose your project and a link with the project that Graham was doing. What was that all about?

Well, one of the projects that Huxley picked up was when he was working at Manchester and was ....the head then at Jodrell Bank the big radio telescope – the first big one, in fact, think was – not Lovell; I've forgotten the name.

Bernard Lovell?

Bernard Lovell. Anyway, they observed blips in their signals which they understood or guessed had to be meteor trails, reflections from meteor trails, and these moved with time because the meteor, the trail, was being blown by the upper-atmosphere winds. So this immediately suggested that you could investigate the motion of winds around the Earth at 40 to 120 kilometres above the Earth. And that idea appealed

very strongly to Huxley and it was the idea that Graham took up. When it came to studying the way these trails, which are formed like a very small pencil, they subsequently diffused along their length and this can be picked up in radar reflections, and Huxley was very interested in using the theory of ambipolar diffusion, because the trails were made up of electrons and positive ions – it's an ionised trail. So he came to me and said, 'Would you like to do a study of the mobility of these ions in gases?' And that's pretty much the last connection I ever had with upper-atmosphere winds until about four years after I retired I got an email from Graham to say, 'We're using this following formula,' which he wrote out, 'for the diffusion – to represent the diffusion of the trail. What do you think of it?' And I wrote back and said, 'It's useless.' And he said, 'Well, can you do better?' And I said, 'Well, we can do better than that, and we've got sufficient data from our laboratory experiments to actually give you tabulated values.' And that started about a two-year collaboration between Graham and I and I think we got two or three papers out of that, and that's often referred to as 'the Elford brothers' in literature. (laughter) Which is pretty unusual. That was a rather fun thing to do.

But in Huxley's mind, in those early days, there was a clear link between those two fields he established in –

Oh, yes. One was connected to the other. We were supposed to sort of support the – you know, study the formation of this trail and its subsequent behaviour.

Yes. So in those early days – – –.

So it's a separate, slightly separate topic from studying the winds.

Of course, yes. But in your mind, when you were doing your PhD, did you link what you were doing to the meteor work at all?

No. Well, I started off by – well, it's an interesting story, I suppose – by taking up Huxley's suggestion that one should look at the mobility of calcium ions in oxygen. Well, I knew immediately if you run a filament in oxygen it would burn out, so the nearest thing I could get to that would be nitrogen. Then I discovered that calcium, when it gets hot, just eats any metal, so I put filament after filament coated with

some calcium salt and they'd all burn out. So then I decided calcium was too difficult. I could get as close as I can in atomic number by going to potassium, and everything will give you potassium – if you take a bare wire and just heat it in vacuum you'll get 10^{-11} amps of potassium ions. Everything's coated with potassium. So that solved that problem.

And then I looked around: so now we had potassium ions in nitrogen, but you can see it's still moving away from anything to do with meteors. And then I read the literature, and the only book that Huxley gave me was a book by A.M. Tindall written just before the War and before the group shut down, and it contained plots of potassium ions in nitrogen, and they all had kinks in them, and I realised that anything to do with gases is not going to have kinks; it's all statistical. So it seemed to me clearly there was some experimental artefact, and so my thesis really turned around, eventually, to being a study of potassium ions in nitrogen and various other gases with a view to understanding where these artefacts came from and ultimately to – then I got led onto whether it was possible to derive interaction potentials between an ion and an atom from mobility data, and in principle that could easily have been – could have been done, although there were no adequate theories at that time to do it. Subsequently, many years later, such theories were developed.

But in 1958 or whatever it was I went to England on my way back from Canada, where I'd been doing a postdoc, and I went down and arranged to see A.M. Tindall – he was still alive; very old man – and he showed me a lot of other stuff which he said he had deliberately not published because he didn't believe the kinks himself, and he agreed that it was an experimental artefact. So that was an interesting connection with history.

During the time that you were doing your PhD, did the funding available to buy equipment improve?

What funding?

Or was it always a problem?

No. No. It really didn't improve. In fact, if anything, it got steadily worse. I mean, it's hard to imagine how dreadful it was. I mean, people would cannibalise everything. If a person finished his PhD, within 24 hours somebody would have stripped the room – I mean, even taken switchboards off the walls. It was just extraordinary. So people became terribly adept at making do. It was the ultimate do-it-yourself paradise, and people got very good at scrounging old gear. We needed, for example, tungsten seals. We couldn't possibly afford to buy a tungsten seal, but you could .....[?] get magnetrons, which had tungsten seals in them, and we spent the time breaking them open and getting the tungsten seals out. I mean, this is the sort of thing – – –. When you have to go back that far, it's enormously time-consuming.

Now, when you moved to ANU in – when was that?

'61.

'61.

March '61.

You found a very different situation.

Oh, yes. (laughs)

Now, was it that the ANU was a standout institution –

Oh, yes.

– or was it that Adelaide was unusually – – –?

Well, the difficulty was – I've forgotten the name of the report that came, that Menzies had done – but the whole time –

Murray – Murray report?

– Murray report, you're right – the whole time was – I think the funding came all entirely from state sources, and they were starved for funds, there's no question about that, and the place was technically rundown to a high degree. And so this is why everyone was forced into doing these things.

So you think all the state universities would have been in the same boat?

Oh, yes, they were – and, in fact, it was out of that that triggered the Murray report, and from the Murray report, of course, the federal money started to flow. But even before that, of course, there was federal money going directly into the ANU, and lots of money.

Yes. It was set up as the premier institution.

Yes, and it was designed to – well, one of its functions was to train – quite deliberately, they said they weren't going to have any undergraduates; they'd start everybody at postgraduate level, and because of the enormous shortage of staff all over Australia at that time one of its functions written into its charter was to be a source of PhD-level people to start universities. And to do that they needed to have major research labs and to do that they needed to attract people from overseas, and they did all that. But it was like walking into an Aladdin's Cave when we arrived. You know, I went down to the workshops and stores, and I said, 'Well, what do I fill out? Where's the forms and so on?' And they just laughed, and they said, 'What you want's over in the self-service store.' And all you had there was 20 different sort of hook-up wire and tubes and every other conceivable things you might want, and you didn't sign for it, even; you just picked up whatever you wanted and walked off with it. This, coming from the Adelaide situation, where you were begging for a piece of hook-up wire was just bizarre.

Now, Huxley got some money while he was in Adelaide from the Radio Research Board, I think.

That's right. But, of course, a small amount went to Bob and a much larger amount, I think, went to Graham. But even that wasn't – they weren't large sums at all. I don't know how many there are or what they were, but Graham could probably remember. But, of course, when you're a research student you're not very cognisant of all the financial flows.

Was it apparent that Huxley was frustrated by that situation?

I think so. I think he must have been frustrated. But, of course, he'd come from a situation in England where money was tight for research, too. I mean, you probably

know some of Huxley's history and how he, in fact, went back to England because of the Depression after having worked at Mount Stromlo when the Radio Research Board almost went out of existence. But, of course, Huxley himself was no experimentalist. I mean, he was hopeless as an experimentalist. You wouldn't let Huxley anywhere near your apparatus. (laughter) But he was enthusiastic. And later on I found that I got to know him well because I was used to work through the evening and he used to do WEA lecture series on astronomy or something, and he had a couple of hours or an hour or so to spend between the end of his working day and before he had to do his night lectures. On a number of occasions he'd just come up and sit beside me on a stool and just natter about Oxford days or how Townsend invited him to go to have dinner with Langevin in Paris and all these sort of things – and it was then that I really got to know him.

I can remember students in the lab jokingly saying when they built their equipment they included what they called the 'professorial button' –

That's right.

– that did nothing and a professor could come and press it without doing any damage.

(laughter) You had to be very careful with people. Some people, you know, would destroy your apparatus without trying.

What was the technical support like in those days.

Ah! Well, most of the technical support seemed to me to be keeping the prac labs going. Obviously John knows far more about this than I do. There was part of the workshop that was open to students, research students – there was a drill and a lathe, a few things like that – and you just had to pick up what you could. I mean, you'd have to go and borrow or, from time to time, if you wanted a tool bit sharpened – and we didn't get terribly good at sharpening tool bits – you'd wait till you could find someone who was free on the other side of the workshop and they'd take pity on you, or you'd get Lindsay Hettner, who was in charge of the workshop at that time –

Oh, he was there then.

– to give you a hand to show you how to use a lathe or power drill or whatever. Again, later on, all of these things came in very handy, to be able to use machine tools; but it would have been infinitely easier if all this had been done in a structured, properly-done way, if you'd had a course on lab techniques or machine tools. I mean, there was absolutely no – – –.

Which we did later on.

I'm sure the department would have done it later on. I know at the ANU they ran courses on ultra-high-vacuum technology and some stuff like this. But none of that existed, and I had absolutely no formal training; I just had to pick whatever I could up about vacuum technology, and it was all pretty crude then.

It was very hard to get – in fact, I don't know whether we were even encouraged to do it – to get them to build any part of your equipment. They'd give you advice, but that's all. I mean, I was taught how to prepare surfaces for taking down to the Oliphant brothers for cadmium or zinc-plating or something, all that sort of stuff.

At some stage, John Gascoigne came into your laboratory.

I first met John in the workshop, when he was working down there as a machinist. And it was the expandable that brought John up from the workshop. Bob wanted him to be working closely with him on this major, major tube.

Now, what you're talking about here is that, after Bob finished his PhD, he then set about designing a new-generation apparatus.

That's right.

Drift tube.

No; it was a Townsend-Huxley chamber for measuring the ratio of the diffusion coefficient to the mobility, D/μ, and it was designed to be everything. It was designed with multiple collector segments, a series of annular rings – I mean, just the construction of those alone was a brilliant tour de force. The actual tube is still in existence in the museum, the research school. I don't know whether you've seen it. It's a very beautiful thing to see.

I've seen a photograph of it.

Well, the actual thing's really something – and I've used it. It was largely used – there were a lot of problems associated, in the early days, as to how you converted from the current ratios that you measured between the collector segments and the transport coefficient that you were trying to derive. So you had to solve the continuity or diffusion equation, and there was a lot of argument about what sort of boundary conditions you would impose on the solutions and so on. And they decided that you needed to do a whole series of measurements with a whole range of geometrical parameters – different sizes and heights and lengths and so on – and that was the purpose in building the expandable, because it could be raised and lowered on a shaft which was driven through this gearbox by an induction motor from outside. And originally the tube was held together with a seal with black wax, back in the good old days. Later on, it was upgraded and it's in a huge envelope about this size – and that's it, that's the way it is still, now. It's still operational; if you wanted to, you could turn it on now. John pumped it down hard and sealed it off before it went in the museum, so all looks in pristine condition.

So how did the business of John Gascoigne coming into the group happen?

I think Bob simply went to Hux and said, you know, that there was so much work involved in doing this tube and he needed – that so many technical problems associated with building it that the two needed to have very close contact. So he came up and brought a lathe with him and actually worked in the lab area from then on.

Yes. Was that the first time that that happened, that a technician was located?

Yes. To my knowledge, that the first what I'd call a 'lab technician' – a real lab technician – was operational.

And that was just a reassignment of his duties by the head of department. It wasn't that there was research funding to pay the salary of a technician.

I don't know how the funding was arranged at all. I assumed – I just assumed that he continued on with the – he must have had some arrangement, because I don't know

whether he was funded as a lab technician or whether even such a post existed at that time. But certainly you never saw – I don't think there was another one in the place that had the same situation as John.

Yes. That's interesting. So when would that have been?

That would have been about '54, I would have thought.

So who was in the group at that stage?

Well, there was Bill Macklin, Barbara Possingham – no; Barbara Hall, she was then, of course; Bernie Milton – he was there for about a year, did some work on mass spectrometry; Bob and I. Dave Sutton, after the first year or so, shortly after Bob got back, went off looking at rock magnetism and seismic stuff.

When did he begin doing that?

I would have thought about '54, '53. I think it was pretty clear that once they'd put – they essentially wrote – Bob and Dave, because they'd worked together to get this electrons in gases stuff done, they essentially wrote separate theses, but they contained essentially the same material, and I think – something to do with International Geophysical Year – that they needed to set up a seismic station somewhere near Adelaide and there was money for it, and I think Huxley asked Sutton to do that.

But the International Geophysical Year – I don't remember exactly, but it was around about 1957, wasn't it? Did David Sutton – – –?

Yes, that could have been. It could have been right. I mean, that's right.

Had he moved away from the slow electron work before then?

I think the seismic stuff – before that, he was into rock magnetism, because he spent a year overseas learning the techniques.

Oh, he went on study leave.

He went on study leave – I think it was University College, London. Not sure. Anyway, that's where he learnt all about the work that was being done about the

changing directions of the Earth's magnetic field and that you could learn a lot about this by looking at the directions of – – –.

So why did he do that? Would Huxley have encouraged him to do that, to get into a field, or – – –?

I don't know. I don't know. Unfortunately, of course, as you know, Dave died.

Yes.

In the mid-'60s somewhere, didn't he?

Yes, a long time ago.

Cancer. Yes. No, I don't know that I can – maybe Bob could answer that question.

But I do have the feeling that getting into the seismic route was linked to the International Geophysical Year.

Yes. But I think that came later. I think he was into rock stuff. He had this big apparatus built – beautifully built, actually – out of Perspex in the lab just across the road and across the passage from where our lab was.

Where was your lab? It was upstairs.

It was upstairs, down one end –

On the south side.

– of the original building, without any extensions which, of course, are now there. And for many years you would know where it was because it had the Faraday cage in there.

Yes, I remember.

Remember the old Faraday cage?

Yes, I remember that.

Very different when we got to the ANU. We got a Faraday cage made of sheets of copper.

Oh.

(laughs) You could afford that at the ANU.

Who else was around the department at that time?

Hervey Bagot.

Oh, yes – I saw him – – –.

Big man in the bell scene.

I saw Hervey a month ago, I suppose –

Goodness me.

– at the funeral of John Smith.

John Smith died? Okay. What happened to John Smith? Did he continue working in the department?

Oh, yes.

What, he got a lectureship, or – – –?

No. No. He ran the electronics workshop.

Ah. Okay. Right. Yes.

For a long time, provided –

That figures. Yes, that was always one of his strong aspects.

– electronics backup to research groups right around the department –

Yes.

– for many years, and produced many outstanding electronics technicians who he took on as young kids and trained them.

He was a nice bloke. I liked John.

Yes. Now, in those days, Stan Tomlin had a lab –

Yes, doing – – –.

– which was on the other side of the corridor, it was.

Other side of the corridor. He was doing low-angle X-ray diffraction with Barb Kidman.

Yes, I've talked to Barb.

She was still around – she was sort of the same era as Graham and Bob, I think.

That's right.

But she was still around when I came along. She was continuing to work in Stan Tomlin's lab.

Yes. You see, she went away for a while to England.

Aha.

Married Ren Potts.

I was going to say I remember Ren Potts very well.

And then they came back –

That's right.

– after a gap and then she took up her PhD.

I remember we had a terrible lecturer in – supposed to be teaching us in the honours year in general relativity, and he was absolutely appalling, and he would keep – we simply couldn't follow his mathematics, and so we smuggled Ren Potts into the back of the lecture theatre so that he'd give us some idea and he might be able to straighten us out, and he came out shaking his head. He said, 'He's changed his notation three times in the course of that lecture,' so it was no wonder that we were [having trouble].

Who was the lecturer?

I've forgotten what he was called, unfortunately. But he was unbelievably bad, and the only thing I ever knew about general relativity was just completely out of books. I mean, I'd learnt nothing whatever.

What – was that your honours year?

That was my honours year, yes. Ren Potts wasn't all that – I mean, he was the same era, of course, as Graham and Bob. So we were all – – –. I was thinking, actually, when we came over, the whole group came over, how young we all were. Bob was 35 when we came to Canberra, and everybody else was under 30 – the other five people, the other four people. So it was a very young research group.

Yes. Who else was working in Tomlin's lab at that time, when you were around?

I think there were a couple of people – I can't remember them: one had snowy-white hair or very blond hair, who went – in fact, they both went overseas. I think they both went to London. And they were looking at the striations in muscle fibres or something. Can't remember. But I remember the enormous efforts they used to have to put in to get any sense out of their plots, out of their photographs to allow them – – –.

This was electron microscopy, was it?

No; it was low-angle X-ray diffraction.

Oh, I see. Harry Medlin would have been there?

Harry Medlin was – well, he was in charge of that lab, of the X-ray stuff. I can't remember who else was with Tomlin, apart from Barbara. I really had almost no connection at all with any of those people.

So what was the department like as a community in that time? Did people work fairly independently, or was there – – –?

Yes, we had tea together. There was a tearoom, where we all came. Huxley would throw a social evening once or twice – once a year, I think, at Christmastime or some other time, I've forgotten when. He and Molly would entertain the staff. But even then the staff numbers were – I mean, it was a very small place. It's hard to believe what it must have been like in the 1930s, when they were down to a staff of about three.

Yes. So how did Huxley and Stan Tomlin get on together? They must have been the two senior people in the department.

Well, they were. I don't really – I don't know. I never saw, essentially – I never saw any friction, but I never – I've no awareness of how they actually related to each other.

What about the Mathematical Physics people; were you aware of them very much?

Well, Angas – – –.

Bert Green and Angas and – – –?

Well, Bert Green was very aloof and sort of pretty unapproachable, I thought. Angas, of course, was just the opposite and everybody liked Angas. And he did some work for us, actually. I well remember we had a problem solving a Laplace equation for the particular geometrical conditions we had in one of our experimental tubes and he was supposed to go the following morning, catch a boat to the UK, so he just knocked off the solution and dropped it on our desk, (laughs) you know, off the top of his hat, which we subsequently used and computed.

So if you went to the – – –.

But he was very approachable and we all knew Angas.

Yes. If you went to morning tea in the department tearoom, would Angas be there?

Oh, Angas would be there, yes.

Would Bert be there?

I don't remember seeing Bert there.

Now, we talked about technical support to the group.

Yes.

Huxley provided theoretical support, did he?

Didn't supply any theoretical support to me. He supplied theoretical support to Bob. At that stage, I found the theory – because it wasn't directly applicable to the work I was doing, which was looking at – – –. The theory of ions moving through gases is

very, very different from the theory of electrons moving through gases, although they started with the Boltzmann equation –

Yes.

– because the distribution functions of energy and velocity are so totally different you can't make the same approximations. I just picked up bits and pieces of what was the theory behind Bob's work. And Huxley was a great exponent of the mean-free path technique that he learnt from Townsend, and that requires an enormously perceptive level of intuition – I mean, it was all done with vector diagrams and in velocity space and fluxes of particles through surfaces, and all modern theories, of course, go straight through the Boltzmann equation and disappear into mathematics. Bob had a very good intuitive grasp of what was going on, but I found that all very difficult and it wasn't till much, much later that I got involved in electron theory – did a number of measurements involving electrons in gases, apart from the ion work I'd done.

Did numerical work become important in analysing the data?

Well, yes and no. Of course, it was the days before computers and all we had was a 10-figure Marchant mechanical calculator, which would whirr away on the desk. So any calculations were enormously laborious and so – this was the problem, really – you'd end up with integrals in some theory and there was no easy way to make any progress numerically, and so all sorts of averages were taken and assumptions made about cross-sections and probabilities of electrons colliding with atoms and so on in order to try and get something out of it, and it was the advent of high-speed computers, which started really from about '61, '62, at the ANU, that completely cracked all that and you could really get a connection between what you measured – the current ratios, what you measured and the final results that you were after. So that was the big turning point. And I think all this mean-free path theory, which is in a large chunk of Huxley's and Crompton's book, is really historical stuff now. I mean, nobody that I'm aware of would ever try and do theories that way, and Huxley spent quite a lot of time, and was able, to demonstrate that, in a formal sense, the

solutions, mathematical solutions, via the Boltzmann equation are formally identical with what he got. But, you know, it depended on whether you took an average before something happened or after something happened. I mean, it wasn't for the uninitiated, and I found it a real quagmire.

So how would you rate Huxley as a physicist?

Well, I think he was perceptive in picking problems that were worth doing. He was totally reliable in terms of keeping up the enthusiasm and doing the best he could for you – I mean, dig around, try and get you a grant, scrounge a bit of money here or there and that sort of thing. From time to time he would visit and show an interest and ask you what you were doing. I mean, I think he was a very able physicist; I don't think he was a great physicist.

How would you rate him as a leader?

That's hard. How would I rate him as a leader? Well, he did a lot of things, and a lot of things outside of physics. He was on the CSIRO executive; he was the Vice-Chancellor at the ANU. He was a good leader in the sense that he was very collegiate, he consulted and was interested, he listened to people – I always felt I could go and talk to him. He once said to me – I asked him – I don't know why I asked him – why he hadn't called me down to his office when he wanted to do something, and he said, 'I don't call people to my office; I go and see them, because that's how I get to know people and find out what they're doing.' And that was one of his techniques. He was good individually with people, and I learnt to respect him and I was very fond of him.

What do you think his impact on the teaching of the department and the undergraduate curriculum [was]?

I think the general standard of teaching, with few exceptions, was pretty poor, actually. And there was not a great emphasis on teaching or upgrading teaching skills or anything of that sort. The idea was you just picked up somebody they thought had passed – who had either a PhD or they got him from somewhere and he

was expected to teach. I don't think they had any idea whether he could teach or not. And there were certainly no remedial courses or anything like that.

Yes. But I feel that Huxley was, in his lecturing, was rigorous.

Absolutely.

Now, did he expect that of his staff, so that there was a new rigour in the courses?

No. I feel that everybody just did their own thing. That was my impression, anyway.

What about the innovation in curriculum in terms of introducing modern physics – quantum mechanics and relativity and those things?

Well, I don't know.

Did that occur during your time?

Yes – I mean, we had courses in wave mechanics and molecular spectroscopy, so there was a significant amount of modern physics. We had very little on solid-state physics, which was curious, because when I did a postdoc I worked for a year and a half in low-temperature solid-state physics in Canada, so when I arrived I was a babe in the woods, in Canada, because these were all very able solid-state people, so I had a (laughs) very steep learning curve. But, of course, solid-state physics – it was about the time the transistor was invented. Solid-state physics really only took off after that.

When did you do this postdoc appointment?

I think I submitted my thesis in November '57, and about a week later I got on a plane and flew to Canada and worked at the National Research Council labs in Ottowa – it's their equivalent, I suppose, of CSIRO – and worked in a group which included one Australian, which was Guy White, who was a member of the academy and so on, who was a solid-state man, low-temperature physicist, trained in Oxford. About half the group were Oxford people, and of the group of about – I suppose about eight people, one of them was a Canadian, which says something about them. They were recruiting all over the world. And the fellowships – it was really Barb

who was responsible for me going to Canada, because she'd already got one of these NRC postdoc fellowships when she was offered an 1851 scholarship, which is very prestigious, for the University College, London, and she just threw all the Canadian stuff – you know, literature – and she had a great big folder and just threw it on my desk and said, 'I don't know whether you're interested, but I got this other one; I'm pulling out of this one,' and so I flipped it over and it looked pretty interesting, a nice, exciting place to go to, and they just had a whole series of boxes to tick as to what you wanted to work on, and I didn't know a great deal about solid-state physics and it seemed like an interesting thing to do, so I went and worked with Guy White.

And then, at the end of that, you came back to Adelaide, did you?

Yes. Well, what happened was that I had about 18 months with the group. Then I went to England for six months, pretty well – doing nothing, actually; going to concerts and touring and having a good time. Part of it was that I didn't really know whether I wanted to go on, have a career as a professional physicist, or whether I did something else. But by about halfway through that period I found I was interested to go to the Science Museum and start reading scientific papers and so on, and some of the problems that had been thrown up by my thesis work badly needed looking at and I got more and more involved, and eventually I wrote to Bob and said, 'Would there be any openings if I came back to Adelaide?'

I see.

So Bob says, 'Oh, yeah.' So I came back in November '59.

In what capacity?

As another postdoc. It was my second postdoc. And it was a funny year because the money was getting tighter and tighter and tighter, and you were doing more and more do-it-yourself stuff, and I made very, very slow progress and I was getting pretty – wondering, you know, whether I'd made a mistake in coming back to what looked like a sinking ship. And that's when Bob – towards the end of that year is when – of course, Bob and Huxley were in touch, and Bob said, 'We can't go on,' effectively, and when Huxley said he would go and see his friend, Roly, who was [Roly/Roland]

Wilson, the Secretary of the Treasury; he was a personal friend; and that's how we ended up at the ANU, because he found enough money from the Department of Finance to fund us. And so, for I think seven years, (telephone rings) we were a one-line item on the end of the Federal Budget. Fancy being able to do that as a personal favour! (break in recording)

Malcolm, are you saying that the funding situation had got so critical that that whole research program couldn't continue?

Well, it could only continue by, again, doing everything yourself, and it became increasingly obvious to Bob, and I think to everybody, that we were, essentially, becoming internationally non-competitive. For example, we couldn't possibly afford to buy pure gases. We'd have to buy common garden-variety nitrogen or helium or whatever in a cylinder and clean it up ourselves. Everybody else would just put one line – you know, 'Gases used were from Matheson's' – and everybody knew exactly this had less than one part per million of whatever. We couldn't afford to do any of that.

Now, what year was that, when this realisation came?

Oh, for me that – I'd realised – I'd been to Canada. For the first time I'd seen highly-professional, well-equipped laboratories. And to go back to Adelaide was like moving back to the Middle Ages.

Yes. And all this led to a sequence of events where Huxley left – went first to CSIRO –

Correct.

– and then to the ANU as Vice-Chancellor –

Yes.

– and your group moved to the ANU. Could you describe that sequence of events? How did that all occur?

Well, the first I knew about it was that I was sitting at the desk one day and Bob just casually said, 'How would you feel about going to Canberra?'

Had Huxley already left at that point?

Huxley had left probably four or five months before that.

I see.

And I think Bob thought that, you know, we were in a competitive environment against people like the Westinghouse Research Labs and so on; we had a number of international groups working in the same field, and it was pretty obvious that it didn't matter how hard we worked or how many hours we ploughed in, you were never going to be able to move at the speed with which other people could, and you'd have to end up by doing second-rate stuff or it just wouldn't be worth doing – working on stuff that wasn't worth doing. And so it was Huxley's personal friendship with Roly Wilson which was the key to the whole thing, and they said they would buy the entire research group for I don't know how many hundreds of thousands of dollars.

What was Roly Wilson's position?

Roly Wilson was Secretary of the Treasury. His name was on the banknotes.

Yes, but – – –.

I went to someone who said, 'Who's Roly Wilson?' And I said, 'Have a look at a banknote.'

So he was undertaking to provide funding to the ANU –

ANU, directly from the government.

– from Federal Treasury.

From the Federal Treasury. And it was on the end of the Federal Budget as a one-line item.

Yes. Could we just go back a step to Huxley's departure? That, essentially, occurred quite independently of that funding crisis.

Oh, yes.

So what impact did that have on the group and the department?

I think there wasn't another appointment made. I think Stan Tomlin was the acting –

Interim.

– interim – and you had the whole place felt like it was just drifting.

Yes. That left a group of which Bob was the leader.

Yes. I was a postdoc. John Gascoigne was the head technician, and we had two PhD students, which we took with us.

Yes. So then Huxley had gone first to the CSIRO and then to the ANU –

Right.

– and at that time this arrangement with Wilson and the Treasury came.

That's when he was Vice-Chancellor at the ANU. And, of course, he also knew Oliphant well, and Oliphant said he would make space available in the area of the Research School of Physical Sciences.

So this transition came not as a shock but a way out of an impasse.

Oh, yes. Oh, yes. I don't think there's any doubt that the group would have completely stopped, eventually, without major injection of funds. It was the era, for example, that within a year or so people were starting to buy – and I was one of them – trying to build ultra-high-vacuum systems so that we could work with proper, long-term, pure gases. A lot of changes were occurring and most of them involved fairly expensive outlays – ion pumps were just coming in, a lot of barium stuff – could never have afforded that in Adelaide. And, of course, when we – eventually, before Huxley retired, he passed a motion through council making us a legal, formalised part of the Research School of Physical Sciences, but up until that time it was just a personal arrangement between, again, Oliphant and Huxley.

Oh, was it? For how long?

It was until he retired, which was –

When was that?

– oh, I think it was five or six years after we were there, something like that. I can't remember exactly when he retired.

Well, I think we've left Adelaide now.

Good.

My brief is to talk about Physics in Adelaide, so I guess it is time to stop.

Okay, fine.

It's been a very interesting conversation.

Great, Alastair. Thank you.

Thank you for taking the time. It's been most interesting.

Thank you for making me think about things that I hadn't thought about for a long time.

END OF INTERVIEW