Angas HURST

This is a conversation with Angas Hurst at his home on the 8th July 2011.

Angas, thank you for making the time to talk to me. I wonder if you could start by telling me about the appointment of Bert Green. What was Bert's background and what – – –?

Well, this is what I'm going to say. On the basis of Huxley's experience in Birmingham and with Peierles they decided to set up a Department of Mathematical Physics, separate from Mathematics and Physics.

Peierles – what was Peierles?

Peierles was a very famous theoretical physicist, and he was professor in Birmingham. Peierles and Frisch are the people who first proposed the atomic bomb.

Ah.

Anyway, they advertised it and for a long time – I mean, a job in Adelaide in mathematical physics wasn't a big deal, and they were very lucky that Bert applied. Bert could have got jobs in United States and in the UK without any trouble, but he wanted to go to Australia. And so he took the job on and he came out in 1951 and he'd been in Dublin with Schroedinger and Messel was there also working with him, and so when he came out to Adelaide he had a provision for a research support and hired Messel. And Messel came and for nine months they worked like beavers, madly constructing a theory of cosmic ray showers and publishing papers and so on and so on. But the joke is that all the work they did on the cosmic ray showers in no time was behind the times.

But one weekend Bert filled in time by devising a new theory of particles, which he called 'generalised parastatistics', and he wrote this paper up after the weekend. It's still quoted. It's one of the most widely-quoted – it is the most widely-quoted paper of Bert's, and it was done in a weekend, whereas all the big stuff he did is forgotten. (laughter) That's how life goes.

So he came out and, as I say, worked very hard with Messel. Messel left after nine months, having teed himself up a job of Professor of Physics in Sydney, very cleverly, and Bert then advertised the job again and the next applicant was John Ward. John Ward really was very unlucky not to get a Nobel Prize, because he and Salam did basic work. Well, John Ward and Bert – well, John Ward couldn't stand Bert. Bert quite liked John Ward. Did you know John Ward?

No, I didn't.

It was before your time. John Ward's objection to Bert was he didn't like the way Bert did physics. He didn't mind him personally, just that he didn't like the way he did physics, so he only stayed nine months and he went off. So the position was vacant again, and hence he gave the job to me. I came from Melbourne in 1957.

(laughs) When I came over, I wanted to get a house, of course. I'd sold my house in Melbourne. And I was told they wouldn't give me a house because the position was called a 'research fellow' and it was regarded as temporary. So I went along to see the Registrar, Vic Edgeloe, to try and persuade him to give me a permanent loan, and he said, 'Your predecessors have only each lasted nine months. How long do you intend to stay?' And I said I would stay until I got a better job. Well, I always joke, because I stayed 35 years. (laughter) And so I came over at the beginning of 1957 and worked with Bert, but soon after I came – we wrote a paper together – soon after I came, he got hooked in with – well, Roy Leipnik had been a visitor and Roy Leipnik was working with the rocket people, and they'd got Bert over at United States, and he spent a lot of time in the United States working in rocket stuff and so on, and I didn't see that much of him. But then he realised that he needed to have a research backup in the form of students, and so he started to initiate a research program and he had two research students. The first one was Marta Sved, George Sved's wife – she did an MSc with Bert – and the second was Ian McCarthy, who did a PhD with Bert. And so Bert thought we should have a proper course, and we then instituted a Mathematical Physics degree, which started in third year and went on to honours.

What did the university have in mind in setting up Bert as a separate department from the Physics Department?

Well, he was going to be a professor, and in those days a professor had to have a department.

I see.

There were no multi-professor departments, so the mere fact that he was a professor meant a separate department.

Yes.

And it was – well, initially, a department of two people, you see. Eventually it grew to four. We were joined first of all by Lindsay Dodd and then by Pat Seymour, and so we built up – oh, in the order of 15 or 20 research students at any one time, so it became quite a vibrant department with third-year lectures and fourth-year lectures as well as a PhD program, and Bert and I shared the supervision of the students.

So where did the students come from, mainly? Were they from Mathematics?

That was a good question. They were split between Mathematics and Physics. In fact, and for a while, we instituted two third-year courses. One we called Theoretical Physics and the other we called Mathematical Physics. And, well, in the sense that the actual lectures we gave were the same, but if you did it under the name of Theoretical Physics the other subject was Physics – third-year subject; if you did it as Mathematical Physics the other subject was Mathematics. So you either had a Mathematics/Mathematical Physics combination or a Physics/Mathematical Physics combination. So we had the two sorts of streams: those who were mathematically inclined and those who were physics-inclined.

Eventually, when the new Oliphant Wing went up, we got two floors, and Bert and I – well, Ian McCarthy joined the staff eventually, and so did Lindsay, so they had offices there and the floor above was where the research students were. So it was quite a flourishing department.

But then – that's the sad thing about it – there came the time when the university started looking at itself and building up planning for the future and one of the criteria

was the number of staff, and we didn't have enough staff to be regarded as viable, which was crazy because we were able to maintain a full lecturing program and research program with the four of us quite easily – plus visitors; but there was a sort of behind-the-scenes operation which wiped the department out, and the only way in which it could survive was to amalgamate with Physics. Pure Mathematics wanted us to amalgamate with them, but we thought we should amalgamate with Physics under the condition that the department became called Physics and Mathematical Physics, and the Chair of Mathematical Physics, which, after Bert retired, I took – I'd got a personal chair by then, but when Bert retired I took over his chair – and so that Chair of Mathematical Physics when I retired was supposed to be filled by somebody else, and was filled by Paul Davies.

That's right, yes.

And he lasted three years and went off, and they never – – –. They advertised again and got 50 applicants and didn't make an appointment, and so the department was wiped.

Yes. How did you see that failure to make an appointment? Was that political or to do with the quality of the applicants?

Oh! It wasn't the quality; it was political. The trouble was the appointments committee was a balance of mathematicians and physicists and the chairman of the committee was the Vice-Chancellor, was Gavin Brown, who was a mathematician but he'd written papers in mathematical physics, and they had 50 applicants and some of them – I think about five of them were full professors from elsewhere. And the mathematicians would say [of] a candidate, 'Oh, he's not mathematical enough,' and the physicists would say, 'He's too mathematical,' and they knocked back every applicant. I mean, we had people like Paul Pearce, who got a chair in Melbourne afterwards, and people from overseas. They all got knocked back because – I never really understood why they couldn't make an appointment. Bert and I went to see Gavin Brown about it and he said, 'Oh, well, tell me about it,' as if he didn't know,

when he was the chairman. And so the thing just staggered on and staggered on for a while until eventually they said, 'There's no money,' and it just ran out.

So really you're saying the politics of the situation was internal to the appointments committee rather than the university.

Completely internal. And, of course, the joke was that they've got six professors of physics now. (laughter) But the one council-appointed chair in mathematical physics was knocked off by the dean, who happened to be a physiologist.

Yes. Things have changed. 'Professor' is now a career grade, isn't it?

Yes, exactly.

It's quite a different notion –

Exactly. That's right.

– than the old professor and head.

Yes. Sort of 'god professor' almost, in those days. But by the time I was around they started – departmental government had come in and actually Bert and I shared a chairmanship, a head of the department, long before they had departmental government. But when the departmental government came in, well, then people like Lindsay and Peter took over as head of the department. But they, of course, didn't have the clout.

I think Physics led the way with departmental government, didn't it –

Yes, they did.

– and starting with Stan Tomlin, I think.

Yes. Well, of course, John Carver was very open-minded.

And then John Carver took it on.

He was very open-minded, yes. Yes, we got on very well with John Carver; we got on very well with Eric Barnes; but there was always this concealed rivalry/hostility with Applied Mathematics. Ren Potts – for some reason or other, Ren had us in the [?]. I think the reason was that he had applied for a job with Bert, and Bert gave it to

Harry Messel. And Ren never forgave him for that. And the result was that he never supported us in all the strife that went on when we were trying to keep the department going, even though he'd written some very significant papers in mathematical physics himself. It was very peculiar.

Yes. How did you look upon the appointment of Paul Davies. That seemed to be a different kind of appointment than previous ones.

That was a very interesting episode because they had an excellent, excellent panel of people apply for that occasion. Alan Carey was a top runner, I remember. But Tony Thomas, who was there by then, was very keen to have Paul Davies because he thought he would raise the profile things. Even then Paul was quite well-known as a media presenter. So he was appointed, and to start off did very well indeed; I mean, he got some really outstanding people around him, like Jim McCarthy and what's his name from New Zealand, and others. He had about four or five people, very active researchers, all working around Paul. But apparently Paul Davies felt that, being on a salary, he was too exposed to the taxation department, whereas when he was doing entrepreneurial work he could conceal it in all sorts of ways. So he resigned the position of Professor of Mathematical Physics and became an adjunct professor of natural philosophy. As I remember, you were chairman of the department then –

Yes, I was.

– and you were trying to get him to sit on a committee or do something, (laughter) and he bloody well wouldn't. He was a very personable sort of bloke, but he didn't do us any favours.

It was not an appointment in the traditional manner of mathematical physics, it seemed to me.

No, that's right. I mean, he did have quite a good record in mathematical physics, (clock chimes) so if he'd gone on the way we thought he would be, would have been fine. But by that time he was become – well, now he's talking about cancer cures.

Yes. He's a wide-ranging person.

Cancer cures and alien visits and everything, yes. So it wasn't a good choice, was it. That, of course, exposed us to get a replacement, and that's where Gavin Brown let us down, when we couldn't make an appointment.

What about the impact on teaching, the curriculum of Physics at Adelaide, with the arrival of Bert Green?

Yes. Well, relativity was a big area that he hotted up. And Bert was also, of course, very strong in solid-state – kinetic theory of gases and things like that. I remember Roy Burdon saying that he realised that Bert Green was a 'top man' – well, coming from Roy, that was pretty good. So, I mean, the Born-Green theory of liquids is a very famous theory, so Bert had a very strong thing in complex states of matter. But he's also worked on particle physics with Born. But then he started doing things in general relativity. He could do anything, really.

So was it Bert who really began the teaching of relativity at Adelaide? Had it been in the curriculum before that, do you know?

I don't know whether Don McCoy had been doing it.

Well, that would be before Don McCoy's time.

That's right. There wouldn't have been anybody to do it then. I'm sure Huxley – – –.

So it's about that time that relativity must have come into the curriculum and at about that time that quantum mechanics came in.

That's right.

And I think it might have been Stan Tomlin who got quantum mechanics courses going.

Yes. That could be right. But I think – the relativity, I think, really started with Bert, although plenty of other people took over; I mean, I did it for a while. And he gave a lot of what you'd call extra – not extracurricular, but lectures and talks outside the syllabus, which were very well-attended. So what you might say he widened the educational scope of the department. He was a good lecturer. People always enjoyed his lectures. So he was a teaching asset as well as a research asset.

Yes. He must have had some good students over the years.

Yes. Yes, well – they're all gone now. Of course, Tony Bracken and those people – yes, what has actually happened is there's a lot of our students went off to other universities and built up mathematical physics there, so that now mathematical physics is really most widely-practised in Brisbane and Sydney and Melbourne. Adelaide doesn't have mathematical physics as such.

No. So who were some of the notable students that you produced?

Ian McCarthy, of course. We had – we've collected a number of Fellows. So Ian McCarthy is a Fellow of the Academy; Jorgen Frederiksen's another Fellow of the Academy. There's somebody else as well. So people have done quite well academically. Then others have gone off into other areas, like Bob Bishop is a world leader in computing science. He runs a big firm in the United States now. I'm getting a bit rusty now on going right back. Of course, see, there was the so-called Beagle boys – there's John Corbett and Lindsay Dodd and Ken Amos, who all went into – became professorial level.

So Lindsay Dodd was a student of the department, was he?

Yes.

And did he go away before he was appointed – – –?

Yes, he went to the United States for a while, yes. And John Corbett went to Indiana. And John Corbett spent a lot of time in France as well. And who else? Ken Amos, of course, went to Melbourne. Then there's Tom Wigley, you see. Tom Wigley's one of our people, and he's a world leader on climate research.

Oh, yes?

He was one of the top people at the University of East Anglia on climate research. Then he became one of the senior academics in Boulder, in the climate study area of Boulder. So he's been in the world – he's a world name, Tom Wigley, so that's another one of our people.

Well, I'm looking at a list of Mathematical Physics appointments. There's a couple that you haven't mentioned. One is a person called Bergmann, who was there.

Otto Bergmann. Otto Bergmann was a research fellow who came. He overlapped with John Ward, and he was there for some years afterwards. He went back to Austria.

Yes.

He married an Adelaide girl, actually.

And the other one was Harvey Cohen.

(laughs) Dear old Harvey. Harvey, he went to La Trobe and I think he'd be retired by now. He came via Canberra and he was a weirdo. Harvey Cohen and Pat Seymour were a marvellous pair. Harvey Cohen was one of the most untidy people you could ever possibly meet, and Pat Seymour was one of the tidiest. The result was they used to clash like billy-o. (laughter) Their lectures were contiguous. Well, Harvey would keep Pat waiting for ages, because he'd ramble on and on and on. Harvey was a very interesting bloke, but he didn't do very much.

What about your own story, Angas? What were your main interests and what do you see as your achievements?

I gave an interview with Bob Crompton a while ago for the Academy, one of the distinguished scientists left there, and so I told the story of my life there to Bob. I started work in Cambridge in particle physics and when I came back to Melbourne I did nothing because all I did was teach. And Bert, in a sense, saved me by pulling me out of Melbourne. There were very good people in Melbourne, but no-one who knew anything whatsoever about physics. Mathematicians didn't know anything about physics, and the physicists didn't know anything about mathematics, so the result was I was, in a way, a sort of guru; but that didn't help research much. So then I came over to work with Bert, and, well, Bert and I didn't do much together.

The only one thing we did was write a book together on the Ising model, and that was a funny thing because I'd heard Onsager talk about the Ising model in Cambridge, and everybody was completely baffled by it, because Onsager was

renowned as one of the world's worst lecturers. And when I came out to Adelaide I saw a paper – written by John Ward, actually, John Ward and Mark Kac – in which they reckoned they could do the Ising model in a sort of graphical fashion, and I found a little trick to try and understand what they were doing, and Bert developed it by using what was called a Pfaffian, and so we rewrote a lot of the whole stuff on the Ising model together and produced a book. And then both of us dropped and never went back to it, really.

And I got involved after that in, essentially, mathematical physics. I had a number of absolutely outstanding people working with me: Alan Carey and Henry Grundling and Phillip Broadbridge, and they developed the mathematics of it so that we had a complete machinery for doing some of the fundamental parts of particle physics. So that was my interest. Apart from doing some work on nerve membranes – that's right; I got onto that through the physiologists, worked with a physiologist quite a while on the structure of nerve membranes, which I enjoyed doing. So I jumped around a bit – not as much as Bert. Did a lot of algebraic work in mathematical physics. So, hard to say what else I did.

You mentioned earlier the difficult relationship with Ren Potts and the Applied Maths people. There was a time when the mathematicians went off and formed their own faculty.

Yes.

What's the story around those times?

That was very interesting. The mathematicians – that was Ren and Eric – wanted to try and improve mathematical level of some of the non-scientific departments, and one of the things they wanted to do was to get more mathematics into Economics – god knows it needs it – and so they put up a proposal to the faculty that they have a subject of Mathematics for Economists, or something of that sort. And the faculty thought that was a little bit like empire-growing and they knocked them back and they wouldn't let them start an Economics mathematics subject, first-year subject. And so, I don't know, they, whether you'd call it in a huff or something like that, Eric and Ren said, 'All right. If you won't play our way we'll go off by ourselves and

form a separate department.' Well, Mathematics, in those days, was really – let me see: Pure Mathematics, Applied Mathematics, Statistics, Computing – was really four departments.

Yes.

But the university thought that that was too narrow to form a separate faculty, so they got to work on us and said, 'Will you come in with us as Mathematical Physics as an extra sort of outside group?' And by that stage we were getting a bit fed up with the way the university was handling our applications for research grants because, not being an experimental department, the Faculty of Science wasn't very interested in us. And, blow me down, our research grants had to go through the Law Faculty. Well, the lawyers were very nice, but they didn't have the faintest idea what we were talking about. So we were starting to feel, 'Well, look, we don't like where we are, either, so maybe if we go in the Mathematics Faculty it might have more chance.' So we agree to secede from Science and go into Mathematics. And the university then agreed that with Mathematical Physics was broader, really. But that's how it was. And so they put up a separate faculty and Eric Barnes was the first dean, and (laughs) [it staggers.....at one] stage Bert became dean. Well, of course, Bert's not very good at being a chairman of a committee. His deafness was pretty bad by then, but my deafness is awful now, but in those days, of course, Bert's deafness was terrible; and Peter Abbott-Young comment about Bert presiding over the Faculty of Mathematics was, 'They've got a deaf dean and a dumb faculty.' (laughter) So the Faculty of Mathematics really wasn't a very successful enterprise.

When Ren and Eric sort of dropped out of things, all the – – –. I mean, I was dean for a while and so was Ernie Tuck. But at that stage it was just the rump of a collection of departments. So it was something that was there for a while, just to essentially meet a particular crisis.

Another development in Physics was the appointment of Tony Thomas, I guess –

Yes.

– and really the growth of theoretical physics within the Physics Department. How did you see that?

I was on the committee at the time that the appointment of Tony Thomas. That's when I got a lipoma on my chin which had to be cut out – that's when I grew a beard – and so my attendance at the appointments committee meetings was a little bit sporadic at that time. But there were, essentially, two outstanding candidates for this vacant chair. One was Tony Thomas and the other was Garth Paltridge. Garth Paltridge, he worked – he was a Fellow of the Academy and he worked in geophysics, I suppose, or atmospheric physics of one sort and another. And Fred Jacka was very keen to have Garth Paltridge appointed –

Yes, he would have been.

– to the position. And I suppose I made the mistake of – yes, I did make the mistake of thinking, 'Build up our own empire,' and so I was supporting Tony Thomas's appointment, although I did say at the time, 'Why do you want to come to Adelaide to do particle physics when we haven't got any particle physics in the Physics Department in any way going where you could go to ANU or Melbourne or somewhere?' And he said, 'Oh, well' – his wife, Joan, comes from Adelaide; he'd like to come back here. So he got appointed. And so we just – sup with the Devil, you need a long spoon, and Tony Thomas had his own ideas completely, which didn't include Maths Physics, and he was a real powerhouse, you see. Completely different style from us, because he'd been strongly involved with the group in Canada and CERN and so on, and had all these overseas contacts, and well, he's booming now, the latest *Adelaidean*, whole article about his collaboration with CERN and so on. So he developed what we now call Theoretical Physics. And there never really was any meshing between us, which is a pity. I always felt that there could have been much closer interaction, but there wasn't. And when the Maths Physics Department collapsed, there was no sort of counterbalance to him when Peter was the only one left, and he didn't want to fight any sort of administrative battles and so things – Maths Physics disappeared and the sort of theoretical physics of Tony Thomas were prosecuted very, very vigorously and very successfully had

taken over. But the old-fashioned – the style of mathematical physics that we'd promoted, the only one was left was with Max Lowe, and he wasn't in the race. So it got lost.

Just going back further, to Huxley's day, what was the relationship between Physics and Mathematical Physics in that time?

Huxley was a prime mover in the establishment of the department, because, as I said, his knowledge of Peierles in Birmingham led him to think that Mathematical Physics was a very good subject. After all, Adelaide was one of the first places in the world outside the big centres to have Mathematical Physics. And Huxley was always very supportive. He always used to say that he wasn't really a theoretical physicist himself, or certainly not a mathematical physicist, but he knew enough about the subject to have a lot of respect for what we were doing. So the result was that relationships with Huxley were always very harmonious. I mean, he and I wrote a paper together on electron propagation. So, as far as we were concerned, Huxley was a great friend.

What about Stan Tomlin?

Well, Stan, he wasn't at all active in his interests. I mean, personally, we always got on very well with Stan, but he had no real scientific interest in Mathematical Physics at all. One test would be whether they'd come to our seminars; we'd go to theirs; but very few people from Physics would come to our seminars.

Yes. Then you said relations were good during the Carver years. What was John Carver's role in the atmosphere of Physics in the broad sense?

Yes. I knew John Carver in Cambridge, of course, and had contact with him from way back, so we were always on wavelength. He always had a high regard for mathematical physics because his Cambridge knowledge, background of there, so we never had any trouble talking to him about things and he was always very supportive if we wanted to make any appointments. We had a string of very high-powered physicists, and John Carver was always very helpful about bringing these sorts of people out. So he was more positive in his thinking than, say, John Prescott was.

Was he?

John Prescott wasn't – – –. Actually, to be fair, John Prescott was a bigger friend – the trouble with John Prescott is that he's got a – what would you call it? – he likes to make little sharp comments, just little digs here and there, and you've got to know that there wasn't anything behind it, but for a while you'd think he means it, and it took me a while to realise that he was actually a good friend of Mathematical Physics.

Angas, what's special about physics?

Physics is getting to the bottom of things – just, really, what are the ultimate questions and how are they put together. And, of course, there comes a stage where you have to jump from the Schroedinger equation to the behaviour of an amoeba, you know. But it's very interesting to contemplate the possibility that the behaviour of an amoeba is somehow to do with the Schroedinger equation. That sort of connection is so extreme, it makes you realise how complex this world is, and physics has to throw everything away to get down to the bottom like that. And also, of course, physics is so much more sharply-defined even than chemistry, so that you can answer questions with so much more precision in physics than anything else. And so it is the last resort, really. It's the last resort.

Yes. And from time to time makes a revolutionary impact in another discipline.

One of the things I love saying, when I go into a supermarket and the girls are going chomp, chomp, chomp, chomp, chomp: 'Did you know that that was Dirac and what's-the-name's invention in the 1930s? Coordinated photon beams? (clock chimes) Without the weird things back in the 1930s you couldn't whack things through in a supermarket chain.' And that's something that people find hard to realise, that such absolutely useless things can be something that you can't do without now.

Did the advent of computing have a big impact on mathematical physics?

Oh, yes. I'm amazed to read about the sort of questions that they can do now. You'd have no hope of doing it on a pencil and paper, but put them on a computer and you start getting almost answers – although, of course, climate control is where – or climate change is where the thing is really being pushed. I mean, climate change is just a great collection of computer models.

Yes. Computing opens up the possibility of working with nonlinear processes, doesn't it.

Oh, my word, yes. Yes.

In a new way.

And things that you could only have the vaguest ideas about before, and then you put it on the computer – – –. This is one of the things I like to talk about with mathematical physics: computing is a marvellous tool for getting to grips with seemingly intractable problems; but, in a way, you do the computing as a sort of act of faith, because you chunka-chunka-chunka-chunka-chunka-chunk away and out come some numbers. What do those numbers really mean? What are they worth? And sometimes you've got to go back and find out just how good that thinking is, and that's mathematical physics.

So if you take a complex, nonlinear process and you model it for the computer –

Yes.

– do you then understand it?

That's right. Do you really understand it? You'll get lots and lots – garbage in, garbage out quite a lot of the time if you're not careful. But that's why, somewhere along the line, some poor mug in a backroom has got to start proving some mathematical theorems which will justify these things. And it's not mathematics, either, because you've got to – the mathematics has got to be tailored to the world and not just a little game. And so I think they complement each other. There's not much point going on with these abstract things if nothing ever comes out of it, but

there's not much point thumping away at computing if you don't know what you're doing.

So do you see computational techniques as a tool of mathematical physics, or a different discipline?

Well, it develops a life of its own, of course. Well, like the proof of the four-colour problem, which I think is about 200 pages or something, you know: after a while, you really don't know just what's going on. So when I say it's a tool, but it's a tool that's got a huge life of its own. And, of course, eventually, you see, computing science, which is very strongly based on mathematical logic, develops a whole discipline of its own and you can't say it's a tool of anything else; it's got a life of its own, it almost stands on its own feet.

Do you think universities undervalue the hard disciplines like physics now with the trend towards valuing interdisciplinary work, at the expense of – – –?

I was talking to Geoff Williams about the low level of mathematical preparation that students have now, coming into university, which means that the university has got to try and do a lot of the work the schools should have done. So it's hard for them to teach to the same level of intensity as before, because they're starting from a much lower base.

Yes – the dumbing-down.

The dumbing-down, you see, because they've got to keep their failure rates down, and instead of educating them better at school, lower the standards of the university.

It's related to the number of students taken into the university, isn't it.

Yes.

In your earlier days, your students were from the top very few per cent of the – – –.

That's right. They would have some pretty hard teaching at school, where it's easier to learn things because you get so much more support. At university you're on your own much more, and so when you're thrown into these tough things without any

proper background they just get lost. I think that university – well, of course, the trouble is financial questions have meant that universities have to try and attract big numbers. When you get big numbers, you have to lower your standards a bit. So we get these huge courses in business methods and so on, which are not really the sort of discipline that I thought of universities doing, although I suppose in the end they've certainly got value; but I wish the whole weight of the university wasn't on that end. And even Physics, I think, are tending to shy away from what you might call 'useless' things.

The wheel will turn, do you think?

Yes. I don't know what will happen. Well, I think it worked, the wheel worked for Tanya Monro – I don't know anything about her work, but I've got no reason not to have the highest regard for it, because that's the sort of in-between – it's not all application; it's got a fundamental basis; but it's not actually developing fundamental basis, and so it's obvious she's making a very big impact, but it's not really changing what you might call the way the subject's put together. But I'm speaking on the basis of pure ignorance, here. I really ought to go and hear her talk, give a talk, to get some idea what she's doing, because she's obviously very good.

It's interesting that Tanya Monro and Tony Thomas are having an unspoken competition of who'd get the most millions per year. (laughter) Yes – oh, it's amazing the money they can pull in.

Well, Angas, it's been an interesting conversation. Thank you for – – –.

Well, I have rambled a bit, you know. Look, I'll get Peter's article. It's in the journal, so it's just a matter – you'll have to just read it, I think – but it's written so well that you'll have no trouble following. (break in recording)

Huxley was going on and on and on, and his eyebrows were going up and down, and Stan was listening with a pretty what you'd call 'resistant' expression on his face. When Huxley finished, Stan protested, apparently, quite strongly, and I said, 'Well, I wonder what's going on.' And I asked Stan afterwards. Apparently, Huxley was due to give a lecture immediately after Stan, so he came along but the door to

the lecture room was shut and there was a voice going on inside, so he paced up and down, thinking it was Stan still talking. And finally, in exasperation, he went back to his office, and it was in this process of – at morning tea he was berating Stan for going over time, and Stan was protesting that he wasn't. What had happened was that he'd finished his lecture at five to whatever it was and gone off, and the students were having a little rehearsal. (laughter) It was one of the students talking. Huxley had to apologise.

END OF INTERVIEW