NATIONAL LIFE STORIES

AN ORAL HISTORY OF BRITISH SCIENCE

Professor Michael McIntyre

Interviewed by Paul Merchant

C1379/72

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Interview Summary Sheet		**Title Page**	
Ref no:	C1379/72		
Collection title:	An Oral History of British Science		
Interviewee's surname:	McIntyre	Title:	Professor
Interviewee's forename:	Michael	Sex:	Male
Occupation:	Applied mathematician	Date and place of birth:	28/7/1941, Sydney, Australia
Mother's occupation:	/	Father's occupation:	Neurophysiologist
Dates of recording, tracks (from – to):			
28/03/12 (track 1-3), 29/03/12 (track 4-6), 30/03/12 (track 7-8)			
Location of interview:	Department of Applied Mathematics and Theoretical Physics, University of Cambridge		
Name of interviewer:	Dr Paul Merchant		
Type of recorder:	Marantz PMD661		
Recording format :	WAV 24 bit 48kHz		
Total no. of tracks:	8	Mono/Stereo:	Stereo
Total Duration:	9:03:31		
Additional material:	The interview transcripts for McIntyre's mother, Anne, father, Archibald Keverall and aunt, Anne Edgeworth are available for public access. Please contact the oral history section for more details.		
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[Interviewee's clarifications and corrections, finalized November 2013, are in small print as here.]

Track 1

Can I start by asking when and where you were born?

In Sydney, Australia, on 28th July 1941.

And can you tell me anything you know about the life of your father?

My father [Archibald Keverall McIntyre] was a respected scientist and his career, of course, was affected by the Second World War. So about the time I was born he, I believe, was working for the air force on aviation physiology. His main career was as a neurophysiologist, so he had medical training and some scientific training in the medical sciences. And through most of the war I believe he was stationed in England, working at one of the research establishments, and he had to do horrible things like – as a junior assistant, you know – get on to a centrifuge and experience high *g* levels, because they were developing the famous *g*-suit worn by the fighters that enabled them to turn more tightly and was obviously critical to the Battle of Britain.

[Dead wrong on two counts: (1) *g*-suits were operational only late in the War – see the A.K.McIntyre interviews by his sister Anne Edgeworth – and (2) the centrifuge experiments he was involved in were mostly done in Australia, and he went to the UK only for the last year or so of the War, when I was "just three" according to my mother Anne's interview by Barry Wise, transcript page 6.]

And what had he done before his war service?

Well, he had a childhood... well, he was born in Edinburgh, if I recall correctly, because *his* father was also a medical person. He was a... he became a gynaecologist – my father's father – and was in Edinburgh, I believe, studying for his degree. So my father was born in Edinburgh but shortly after that they moved to Tasmania. That's the, sort of, family home on the father's side: Launceston, Tasmania. And my father went to a boarding school called Barker's [Barker] College, which if I recall correctly is in the Sydney area. [Yes: Sydney's North Shore area.] I suppose that was the idea of how to get a good education, you know, playing rugby and all that stuff, and they helped build the empire and everything they did in those days. Except that was a bit tough for my father 'cause, like me, he was heavily myopic, and so playing rugby was a bit of a pain for the poor bloke [laughs].

And he stayed a lot with my great aunt Molly [Mary Edgeworth David] during that time. She lived in the suburbs of Sydney [Hornsby, near Barker College – more detail in her 1975 book *Passages of Time: An Australian Woman 1890–1974*], in a house that I know because... erm .. I knew her; she was the daughter of my [great] grandfather. You see, I'm going back another generation, aren't I? On my father's side – okay, my father's mother [née Margaret Edgeworth David] was one of the redoubtable ladies of the time, a pillar of society, a feminist before her time – and her mother in turn was the same thing. And she – that was Cara David – she was the wife of my great grandfather, Sir Edgeworth David FRS, who was a famous geologist, and also went to the Antarctic. He had his name on the South Magnetic Pole for a while, until the wretched pole moved. So there's the luck of the draw in getting your name on things [laughs].

[0:03:14]

I wondered how you know these things about family history. In other words, when in your life you discovered these things.

Well, of course, a lot of it's family folklore, but some of that's on record because Edgeworth David's life... I mean, he had a... his biography was written by his daughter, the same Aunt

Molly who lived in Sydney, and Aunt... okay, there's *that*, and there's also a book written by Cara David, Sir Edgeworth David's wife, Lady David, about their expedition to Funafuti, which is a Pacific atoll close to the equator. I think it's about eight south or is it two south, I forget. [8˚31′S, 179˚13′E.] Funafuti – they went there to drill down into the coral to see if Darwin's theory of atoll formation was correct, which is a jolly good piece of clean science to be doing.

Darwin said, okay, why do we have atolls, these strange flat things on the sea surface? Because they started on something more substantial that used to poke out of the sea surface but has become a seamount and sunk beneath the surface. But of course the coral keeps building, so it keeps the surface very near sea level where the coral can live. And so all you have to do is drill down to see if you... how far down the coral goes for starters, 'cause if you see coral a lot deeper than coral can live, you know that it must have been closer to the surface once upon a time. And finally you hit the rock and you've got the complete picture. And I believe that Funafuti expedition was the – I think it was the first to do it, or perhaps that's just family folklore. I'm not really a science historian [laughs]. [Chapter 5 of *T.W.Edgeworth David: A Life* by David Branagan (2005) gives a detailed account. It seems that they didn't 'hit the rock' but nevertheless got deep enough to find coral samples strongly supporting Darwin's theory.]

[0:04:57]

Thank you. And the life of your mother.

My mother [Anne] came from a family in Sydney that... and I know less about them in detail. They included lawyers and business people. And her father, whose name was Keith Williams, I believe lost a substantial... well, I don't know whether fortune's the right word, but anyway he lost a lot of money in the 1929 crash. Family folklore has it that he, you know, was a bit of a stock-market... is the word 'speculator' fair? I don't know. But anyway, he lost a lot in 1929. But they still, you know, lived comfortably [since luckily he'd had the good sense to give my maternal grandmother a house as a wedding present, according to Anne's interview by Barry Wise] and they provided a loving and caring grandparents' home for us. [Further back in the family there was a famous settler, James Milson Senior, who was Keith Williams' great-grandfather and who after arriving in 1806 built his home on Milson's Point, now at the north end of Sydney Harbour Bridge. See *The Life and Times of James Milson* by Roy H. Goddard (1955).]

I very much remember a visit, when I was little, to them in Sydney... well... and then before that... [Here I think I must have been conflating fragmentary memories from toddler age with clearer memories from a later visit at age 10 or 11 – still somewhat 'little'.] Of course I stayed there when my father was away at the war. So I think my memories of that place, which was in Cremorne, which is one of the suburbs of Sydney that borders the harbour – it's just at the north of the harbour, just to the east of the Harbour Bridge – Cremorne, a very pleasant place – as kids we could go down and play in the rock pools in the harbour, and fish off the wharf, and ride on the beautiful ferries.

And another memory I have from that time – and I think I must have been, well, heavens, three, four, or five, or something like that [again, I suspect I may have been conflating or merging childhood memories from a wider span of ages] – one of my memories is of the Sydney Harbour ferries and their beautiful steam engines. And I was fascinated by the steam engines. You wouldn't see that these days. The engines would all be enclosed. But in those days you could, you know, lean on a rail and see the great pistons moving up and down and hear the chooff, chooff of the steam.

And there was something called a triple-expansion engine – which was obviously a triumph of Victorian steam-engine technology – where the... that very hot steam went into a small cylinder and then it was passed into a middle-sized cylinder and then into a big one, to extract as much energy as possible from a given amount of heat. Clever stuff, that. [laughs] And of course Sydney Harbour's a very beautiful place for a kid to be. And I... you know, played with... well let's see, I'm talking about when I was very little. And I may be getting a little confused 'cause part of that time was in Tasmania and part of it was in Sydney. Because while my father was away at the war, I was... you know, we were put up in various parts of the family, the Tasmanian part for a while. And I have fond memories of that as well, and the ancestral house in Launceston, number 10 Carnarvon Street, and in Sydney this Cremorne place.

Oh, and, gosh, another memory I have. My maternal grandparents had a place out in Manly. That's on the east coast of Australia. You have to go out on the sea side of the harbour heads – I can look this up on a map. I think it's to the north [correct], but anyway, it's on the sea coast. And they had a place, I think it was just a little holiday cottage probably [or it could have been their only home at the time], but I remember being there, and on the seashore [Manly Beach], when I was very little indeed. And I actually remember – and heaven knows whether this sort of memory is accurate – but I seem to remember learning to swim. I was fooling around in the waves and I got the idea of doing a dog paddle [laughs]. [An unusually early memory for me – perhaps that's scientifically interesting in itself. The powerful thrill of a sudden bright idea – and the even greater thrill of finding that it worked – laying down an unusually vivid memory from toddler age. I also seem to remember that the idea came from noticing a dog swimming.]

And the other thing I remember is oil washing up on the surface, and I was told this was because of the war. You know, there were battles out in the Pacific against the Japanese, and when ships were blown up their oil tended to float around. And I actually got covered with some nasty sticky oily stuff, and I remember my grandmother having to wash it off rather laboriously [laughs]. So that was... I think I must have been two or three. That's probably the earliest memory I have.

[0:09:06]

What are your memories of the Tasmanian landscape and... experience?

Well, that's difficult because, of course, I've been back to Tasmania on many occasions since then. And it's a very beautiful landscape. We're talking in the Launceston area, of course. It's the low-lying eastern part, not the highland part. And, well, you know, you've got these rather semi-arid grasslands, often a yellowish colour unless it's just recently rained, and, you know, gum trees scattered around rather sparsely, and, you know, rolling hills. There's a bigger hill – mountain would be fairer – called Ben Lomond, off to the east. You can just see it from some parts of Launceston. So there's this very interesting, you know, shaped landscape. I believe it's geologically very old, but don't quote me on that [laughs].

So... but as a little kid what I mainly remember is playing in this, you know, house at 10 Carnarvon Street, where various... you know, my paternal grandparents [who owned the house – a substantial house in red-brown brick with two storeys, a big basement, and a big garden including a hen run] and various aunts and uncles would be in and out all the time and, you know, there was a lot of loving family... this and that... heads [of sub-families, I think I started to say]... I had a number of cousins there, some of whom I became very fond of.

[0:10:39]

Do you remember what you did in the house, when you say you remember playing there?

Oh, just the typical thing that I would do as a little kid, which is play with... things, but... all children are scientists at that age! You know, we always love to experiment. I remember a delightful morning when I discovered how to make some very smooth mud. You know, if I mixed it up very well I had a certain... must have had clay in it, I think, and I was fooling around with this [laughs] somewhere in the garden, and I remember the delight – the feeling that I'd got it to this very smooth texture. I suppose that must be like what a cook feels when they get the right pudding texture or whatever [laughs].

And then they had a pet dog called Sidi, which doesn't... nothing to do with seeds, but it was an Italian S-i-d-i, after some war site in Italy, I think. Was it Sidi Barrani? I'm not sure. [It probably *was* Sidi Barrani – not, however, in Italy but on the north coast of Egypt.] Anyway, Sidi was a friend of mine when I was a little kid, and I used to... you know, we used to share bones together, so I got into trouble for that [laughs].

And, well, you know, I mean, I don't think there's... and, okay... I think actually I'm getting the chronology a little clearer, because I remember toward the end of that stay in Tasmania my father was back from the war and he made me a light box. It's a simple enough thing, you know, a box with a sort of rotary switch that could turn on various lights, and the lights were behind coloured windows, so as a child you could jiggle around with the switch and have the pretty lights come on in different colours. And one of the positions gave you a buzzer, just as a surprise. And that kept me happy for quite a bit, I think [laughs].

And then there was a bit of rivalry because my uncle – one of my uncles [Tony Godfrey-Smith, Anne Edgeworth (David)'s first husband] made a light box for one of his kids, one of my cousins who were living there, and I thought it was a better light box. He made a fancier one, you see. So, you know, it's all pretty obvious.

[0:12:54]

What I then remember is moving back to Sydney. And this is quite important to me because, as I think I've told you, I'm deeply interested in music. (I was even a professional musician for a while, and I wondered whether to become a full-time professional musician, but that's jumping on.) When I was a little kid I do *clearly* remember lying in bed – and this was after we went back to Sydney – and we weren't staying at the maternal grandparents' any more; we were staying in our very own house [so to speak – actually a small house owned by the David family, I think; it was called 'Haldane' probably after John Scott Haldane, rather than his son J.B.S. Haldane], 'cause my father had got some sort of, I don't know, postdoctoral job or something, or... I don't really know. He could have been living on a bit of family money. But anyway, we, the young family, my father, my mother, myself – and that was all, for a while: my sister Margaret followed; she was two years later... so, okay, she arrived I think at that time... [Wrong! I forgot that Margaret had already been with us in Tasmania. It was only our younger brother Richard who was born when we were at Haldane.]

Anyway, we were in this little house in Hunters Hill, which is a part of Sydney that had a nice view of the harbour, and I remember seeing the rowing eights going past. They used to practise there. And... but the thing I remember most intensely is lying in bed and hearing the music that my parents used to play. They had a 78rpm record player, and they had a modest collection, a lot of which was Beethoven symphonies. In fact I remember for a while thinking that *the*, you know, Fourth Symphony, *the* Fifth symphony and *the* Sixth symphony... that was all there was, you know. And everybody knew they were by Beethoven, so you didn't have to bother to say that.

But what was complete magic was hearing the slow movement of the Fifth. Do you know the Fifth? [Hums music] Sorry, I can't sing it in tune. It's a very beautiful, very, very *simple* thing. I don't quite know to this day why it's *so* magical, but it was complete magic then, and it still is.

Do you remember that house... ?

I think I was about four. I think I was about four then – I *think*.

Do you remember that house clearly enough to take us on a tour of it?

Ooh... that's a bit of a tall order [laughs].

If not, a sort of partial tour?

Well, perhaps a quick impression would be better. It was kind of perched on the lip of this rather steep cliff that overlooked the harbour. That's how we got this wonderful view. And, well, we kids weren't allowed to go to the bottom of the garden. That was called Down Below. Don't go Down Below, we were always told, because we could have had a bad fall. And... I don't think... I don't remember much about the garden. I remember, you know, there were a few small bedrooms, a small bathroom. The uphill side of the house [facing the street] had a little garden and it had a garage actually. My parents had a beat up old wreck of a car. It was all they could afford [laughs]. I remember, once, a fire got started in that garage and there was a big panic about putting it out before the car's petrol went up, but they managed that [laughs].

So... well, there was I in this little bedroom listening to Beethoven's Fifth. One other thing I remember: there was a bigger room, which of course had the view of the harbour. I suppose it was the living room. And I remember we had this, you know, record playing system there with some quite big speakers. [Actually *one* big loudspeaker – well before the days of stereophonic sound!] My father loved music very much and he wanted to do his best to make it sound well. And another thing I *vividly* remember is, when he had the whole kit out on the floor, he had the amplifier, you know, vacuum tubes glowing and so on. This was still 1944 or '5, probably 1945, I think. So there was all this *amazing* apparatus, the like of which of course I'd never seen before. And he had the soldering iron there. And he had an oscilloscope that he'd borrowed from the lab. He was already working on neurophysiology so he had, you know, all sorts of electronic measuring instruments.

So there was this oscilloscope with the green wiggly line jiggling all over the place while the system played Beethoven's *Eighth* Symphony [hums music]. You know, marvellously life-enhancing, energetic music, the first movement of Beethoven's Eighth. And I won't forget hearing that, and seeing the green wiggly line, and understanding that that was somehow saying what the music was doing. And I became very curious, how on earth did all this work? You know – how did they get a whole orchestra into that thin little disc? [At that time

I had never seen an orchestra, but must have learnt something about them because we also had a recording of Mozart's wonderful K447 Horn Concerto, which I'm told I used to refer to as 'the horns and violins'.] What did the green wiggly line... how was it related to the sound I was hearing? Of course I couldn't have articulated those questions as a little kid, but I know that I became very curious, and interested in playing with all sorts of equipment.

And later on I remember my father helped me make an acoustic gramophone. You know... so we had some Meccano bits and pieces and an electric motor; I think this was after we went to America. He went to America as a postdoc after that, you see, shortly after that. ["Postdoc" is technically wrong, even though it was at that sort of stage in his career. Because of the war he never took a PhD.] So we had this Meccano-type kit – it was called Erector in America – including a little induction motor with a gearbox. So he was able to rig it up with the gearbox so that it would turn a turntable at approximately seventy-eight revs per minute. And we made a pickup out of an old boot-polish tin with a paper membrane and a paper cone, so you could stick a needle on to the record and hear it quite well using this thing, and... which of course made me more curious than ever about how you could get an orchestra into that disc [laughs].

So I think that was the beginning of my being scientifically curious, you see, and interested in technical things, even though I ended up as a mere theoretician... you know... I'm just a humble theoretician who tries to understand fluid dynamics. But that was how I began, I think.

[0:19:13]

What else do you remember doing with your father as a young child, perhaps in the Sydney part of your childhood?

Yes. Well, the Sydney part... I was too young to have very systematic memories. I think I have more memories of that sort of thing in America. We were in New York, in a house in Yonkers, for about a year, I think. I think this started sometime in 1946, again just from memory, so probably ’46 to ’47. [Actually a bit longer, well into 1948.] What I do remember clearly is there was a severe winter and we had deep snow that was about as tall as I was, perhaps four feet or something, and I was very excited by the snow, of course. And the local kids used to play games of making patterns in it. There was a little girl who lived next door who liked to lie on the snow and wave her arms, and that made a pattern that she called being an angel, you can see [laughs].

The other thing I remember, and this answers your question better, is that my father got me a little book on making model aeroplanes, which was a beautiful book. It was... you know, in those days, well, you could probably get kits but they were probably pretty primitive, and, er... they may have been too expensive. I know that my parents were very short of money. They were on some sort of fellowship that was funded with Australian money [sort of – see Anne’s interview by Barry Wise, transcript p 7] and the exchange rate with America meant that they were pretty hard up actually and, indeed, my father always said that one reason my myopia is so bad is that he wasn’t sure he’d got us probably nourished at that time. And they reckoned *they* weren’t properly nourished. They would tend to give us, the kids, priority, of course. But anyway, so, model aeroplane kits were probably out of the financial reach.

But he’d got this book which told you how to take bits of balsa wood and make your very own, just giving you the plans and drawings, starting with an extremely simple thing, just a stick with a rubber band on it so you could propel the thing, and wings that are simply thin balsa sheets, not very efficient aerodynamically, but the thing flew. I think that was called the First Model.

And then the Second Model was the next step up, which was the same stick fuselage and balsa planes for the tail parts, but a properly cambered wing, which you had to build, with shaped ribs and spars and struts and things. And on a, you know, drawing board, pin it down and use aeroplane glue, acetone-based glue, the sort of thing that kids these days sniff to get high on, I think. I didn't know anything about that then [laughs]. But, you know, you've got to cover it carefully with paper and stretch it nice and tight. And then there's a thing called doping, where you sort of sprayed a dilute glue on to it, and that tightened it more. So you'd make a nice wing and that would fly more slowly and stay up longer.

And there were a couple more grades of models, ending with, you know, the proper thing with a built up fuselage covered in paper, and a doubly cambered wing [oops: I meant polyhedral wing], very popular having these up-ended tips. It's funny, the jetliners have things like that these days [well, a bit like that] for reasons of fuel efficiency. But there it is. So I remember that very well because my father, I think very patiently, helped me build these planes. And I was still only, what, five years old or something, so I think he had to do quite a lot of it.

But I think I learnt, you know. I did learn to do it myself at some point. And I remember flying these things and the thrill of seeing them performing in the air. And I always after that had flying dreams, and I *always* wanted to fly myself. And I've actually ended up doing so, comparatively late in life. I mean, as an academic with a young family here – I had two stepchildren [oops, three!] – we had to be careful with money ourselves, although probably not as badly as my parents. But going flying wasn't on the agenda for ages. But finally I did, and that was a thrill too, you know, actually being in the atmosphere that I'd spent my professional life thinking about [almost *feeling* the turbulent atmosphere, through gliding in fact – 'flight without power', as Philip Wills' classic book called it – at the Cambridge Gliding Club, where I went solo in 1990].

What were the flying dreams? What did they consist of?

Oh, I don't think it was anything out of the ordinary. But, you know, you imagine that you gain the power of flight and you sort of – ah well, vaguely – flap your arms and you somehow manage to stay up through an effort of will [laughs]. You can do that in dreams. [I sense that there might be some connection with staying up in the water, as when learning to dog-paddle as a toddler on Manly Beach.] I never learnt to do lucid dreaming, by the way. It would have been wonderful to do that and get some control over the whole thing, but I never got the trick.

[0:24:08]

And memories of time spent with your mother as a young child. We've covered time spent with your father but what did you do with your mother?

Well, she left the technical scientific stuff to my father. Oh, if I may just say one other thing about the time with my father. I remember one night – it was a clear night – and he told me all about the stars and how far away they were. And whether that was in Australia or America I haven't a clue. But what I do remember is being absolutely entranced and marvelling at... I was... I probably couldn't quite grasp the distances, but I thought there was something awesome about the whole thing. Those little... pretty little points, they're really like our Sun! You can imagine [how mind-blowing I found it]. He explained that sort of thing to me, and the next time we had a clear night I said, "Daddy, please tell me all about the stars." And he was rather embarrassed 'cause he'd already told me everything he knew [laughs], being only a neurophysiologist, you see [laughs].

[0:25:11]

And as regards my mother, well, she was a basically kind and loving and beautiful young woman, who tended to be a bit restrained. I think she always, throughout her life, felt a little bit dominated by my father [intellectually, I mean – my father was a gentle person and not domineering in the usual sense]. And she had her own, you know, artistic activities. She was a very skilful painter and drawer. She did beautiful portraits of us as kids. Still kicking around somewhere. And... but, you know, as you might expect from that time, she accepted the social norm, which was that you supported your husband in his career and you looked after the house and the kids. And I think sometimes this got her down a bit. Sometimes she was a bit short with us, but, you know, not in a – not in any serious way.

You know, what you learn about dysfunctional families... I mean, I was in some kind of *paradise* compared to that sort of thing. Actually, I do remember as a kid, hearing some slightly more dysfunctional families screaming and quarrelling – and thinking I'm glad that doesn't happen in our place [laughs]. So there you are, we had a pretty civilised environment.

What did you read at this age and slightly older?

What did I read? Oh, I think I remember the penny dropping about how to read. This was probably still in the Hunters Hill house in Sydney, in probably 1945 or so. And, you know, they gave us nice kids' books with nice pictures. I remember a book called *Angus and the Cat,* and there was another American one – perhaps that's after going to America – called *Pretzel*. Pretzel was a dachshund and [the book was] a sort of comic strip. *Angus and the Cat* was a standard book with pretty pictures and narrative, a few lines of text on each page in the usual manner. And, oh, I've forgotten what it was about, something about Angus, the – Angus was a dog, he was a Scotty dog – and [about] having his favourite spot in the sun and the cat taking it over, or something like that.

But what I think I remember is... I mean, obviously we got read to from these books. And I think my parents must have been reading and, you know, pointing to the lines of text, hoping that we'd get it. And I remember a moment, a sort of eureka moment, when I did get it, and somehow managed to see what the lines of text meant. Now how this happened I wouldn't have a clue. They must have done things that I've forgotten. They must have tried to show me what the letters sounded like and that sort of thing, I expect. We call it 'phonics' these days, don't we? So there's one memory.

Who read to you?

Well, I think they both did. Er... I really don't remember which one did it more. I wonder why. From what I know of my parents, they would both naturally have wanted to.

[0:28:38]

The other thing I remember about my mother was that she was interested in, of course, my development, and that of the other brother and sister; I had one brother and one sister. The brother was born just before we went to America, my kid brother. He was a baby in arms on the plane across the Pacific, and all the passengers... he would be swaddled up with a big rusk sticking out of his mouth and all [one of the?] the passengers said, "Oh gee, he looks like Winston Churr-chill" [laughs].

But... the... so our mother was interested in our development and one thing I remember her saying... and I think she even put this down in a little notebook she wrote; she... at one point she made a little book with some photos in it and her thoughts on this and that. She called it 'Anne's Book'. [Title page reads "REFLECTIONS / Anne McIntyre / interviewed and photographed by Barry Wise 2/10/89". The text is a transcript of the interview conducted by Wise. My original copy is now deposited with the Library.] And... one thing, it had a little story about me as a kid. There was a playpen. Heavens, was I small enough? No, the playpen was probably my sister's playpen, I suspect. [Assuming that she was 1 year old, I'd have been 3.] Anyway, there was a playpen, and there was a cat who went into it and my mother said, "Oh look: so and so, the cat, has just gone through the bars." And I immediately said, apparently, "You mean *between* the bars." So at that age I was already interested in precision in the use of words, which is perhaps a bit unusual. And I've had a fetish about this all my life, as you know [laughs]. [According to page 14 of my mother's interview transcript, the words were more like the following:

Anne: “Michael, look at Pickles walking through the bars.”

Michael: “Through the holes you mean.”

It's interesting how my imperfect recollection of this story made the child's simple utterance more complicated. I have no personal memory of the incident – hardly surprising since the transcript also confirms that I was aged “between three and four”. I was recalling only how I thought Anne had told it

in later years.]

You know, just a thought that pops up from that: I always marvel that science, and lucid thinking of any sort, gets anywhere at all when you look at the forces ranged against it – including the general sloppiness in the use of language. And there are, of course, evolutionary reasons why our language instinct is sloppy. It wouldn’t do to have automatic logic-checking, would it? You know, you can easily talk about self-contradictory things without realising it and, you know, some of them are even *celebrated* and put on a pedestal. Russell’s Paradox, yes? The barber who shaves only those who don’t shave themselves. [And the set of all sets that are not members of themselves, and so on.] It’s always puzzled me. It’s flagged up as a *profound* – you know – conundrum, or a profound something or other. I suspect Russell himself didn’t really think of it that way. [He encountered it as a technical difficulty in set theory.] But, you know, people get off on all sorts of hype-ish trips on these things. All it is is a self-contradictory term, for heaven’s sake. It’s no different from saying that somebody has a ‘hairy bald head’. But the thing is, it’s not obviously – you have to think consciously – to realise it’s a self-contradictory term. You know – okay – does the barber shave himself? Oh no, the statement says he can’t. Does he *not* shave himself? Oh no, it says he does. You know... and by that time, you realise it’s the same thing [as the ‘hairy bald head’ – i.e., yet another self-contradictory term].

And as you probably realise from reading these *Lucidity* papers [my published papers on *Lucidity and Science*] ... and I get this from Jacques Monod, you see – Jacques Monod, whom I acknowledge, of course – he wrote this beautiful little book called *Chance and Necessity* and he pointed out that there are all sorts of things in genetic memory that would have enabled our ancestors to survive. And the mindless following of a great leader who gets up and says, the gods have spoken to us and, you know, "you're either with us or against us" – and all these George Dubya-ish sort of things – that's just deep in genetic memory 'cause that's how our tribes of ancestors survived and defeated other tribes in warfare.

Why did they have to do this? Well, rapid climate change was one reason, wasn't it, the seven years of famine and the seven years of plenty. That's a universal story, and with good reason, because we now know, from the ice core records and other things, that climate did change enormously within an individual's lifetime. There were things called Dansgaard-Oeschger events, where suddenly within, you know, a decade or two, you'd get a big climate change. So plainly the years of plenty could be replaced by years of famine.

The starving tribe members would have visions; a leader would arise and say the gods have spoken, you must follow me over them thar hills. And that's why the human genome's spread around the world. And I don't think there's anything particularly original about that idea, although I don't think it's as widely known as it deserves to be. 'Cause when leaders get up and try and fool us with these things, it would be a good thing if more people realised what's going on. But of course, one reason they *can* [fool us] is that we don't have this logic checker. We don't naturally see through these bogus arguments and half truths and things that happen politically, do we? [Laughs]

[0:33:30]

Thank you. Your first school, do you have memories of your first school?

Yes. I think it was in Sydney. I don't think I went to school in Tasmania. So in Sydney from the age of, well, about five. That would figure, wouldn't it? There was a kindergarten, I suppose, run by a lady called Miss Budden [sp?]. At the time I thought 'Budden' was the same thing as a button, but never mind. Oh, and I remember... well, not very much from that. I think it was probably a good school. I don't remember being terribly unhappy. I think we were allowed to play and do reasonable things for five year olds. But one thing I vividly remember was another musical experience. They had quite a good piano there, and I one day happened to wander into the room the piano was, and heard the most *magical* sounds coming out of it. There was some, you know – might have been a young teacher or anyone, some older person, not very old, but they were extracting these wonderful sounds. I don't know exactly what it was. It *could* have been a Mozart sonata, something of that sort. And I was absolutely entranced. I hadn't realised that a machine like that could be used to make such wonderful sounds.

And that sounds very stupid because actually there was a piano in the Cremorne house [though perhaps I'd never heard *that* piano played so beautifully], where I... now here's where my memory really is failing me. I told you that... part of the war I was in Sydney at the Cremorne house and at Manly, but it *could* be that it was only at Manly, and that Cremorne came later. This is the sort of thing memory does. They don't give you a reliable chronology. There was a good piano at the Cremorne house. And now that I've remembered the other thing about the piano, it could be that I hadn't encountered the Cremorne piano yet, or it could be that I was at Cremorne and they didn't have the piano there yet. This I don't know. The piano came down through the family in some manner that I don't know. It was a Bechstein [grand], a pretty good one. So there you are.

But the memory of hearing a piano [at Miss Budden's school], a beautiful piano for the first time playing beautiful music, that is quite clear, and vivid. I don't think I remember anything else about the school. Very vaguely, you know, kids' games, throwing things around, nothing interesting at all [laughs].

And then – the next school then is in America?

You see, I think the reason was I wasn't instinctively very social. I was always a bit of a loner, a bit of a maverick. I was never into this thing that I've got to have friends, I've got to be popular, I've got to have a best friend at school. If I'd been that way inclined, I might well have remembered more about the school. So your next question, sorry?

Yes. The next school then is in America, is it?

Hmm...

Perhaps we ought to establish how long you were in America for.

Yeah, it was. It was in Yonkers. And I have more memories of that, being a bit older. Okay, we went to America, I think 1946 is correct, sometime in 1946. My father... I remember the air trip across the Pacific in a Douglas DC4. I remember one of the engines cutting out. I remember looking out the window and seeing a stationary propeller. The other three luckily were still going [laughs]. And I remember seeing the wrinkled sea beneath us crawling – y'have t'remember in those days they only flew at, you know, several thousand feet, much lower than now. So you could see the waves rather well and I was fascinated by that, looking out the plane window. I remember being airsick in the early part of the journey but it took, what, at least two days, I think. We stopped at Fiji. I remember the plants grasping – there were plants that could grasp your finger, you know. You stuck your finger there and it tried to catch you. It thought you were a fly or something. And we stopped at Hawaii to fix the engine, so we were hanging around in – Honolulu, it was, for quite a while. And then we eventually arrived at San Francisco, and I remember being taken to a big play park with a *huge* spiral slide. And I remember being terrified of going down this, until finally I plucked up courage at the last minute, and then we had to go, and I wished I'd had the courage to go down it before.

But I think we carried on across the continent, and whether it was in a plane or not I just don't remember [probably a night flight, with yours truly out for the count], but we arrived in New York. We went to this place in Yonkers. My father went to be a postdoc [again, technically wrong, but it was to do research at, almost certainly, postdoctoral level] at the Rockefeller Institute in Manhattan, with a man called David Lloyd, who was a famous neurophysiologist.

And I went to school – to answer your question – at a school called PS16, Public School 16, in Yonkers or near it. And I do remember a bit about that, because I remember a rather straitjacketed, sort of rigid way of teaching arithmetic. We were doing little adding sums and that was something I could do already, but the teacher said you had to lay it out in a certain way, you know, and I thought that was very tiresome because I already knew how to add things [laughs].

And the other thing I remember about... I remember two other things immediately. One was that we had classes in what was called 'ball' and I thought that meant dancing, but actually it meant bouncing balls. It meant ball skills. And we had balls – we bounced them and threw them and so on – but we did it to *music,* so I think that's where the confusion comes from. It was probably a jolly good thing, you know, coordination and such. And I remember a little girl there called Virr-ginia, and she wanted to make friends with me and I was much too shy to make friends with her, so I sort of... I think I was a bit standoffish. There's my maverick lone-ish nature again.

The other thing I remember about the school was the American Oath of Allegiance. Every morning we had to stand up and face the flag. There was a flag in the corner of the classroom. I think this goes on till today, doesn't it? We had to "stand up and face the flag like good liddle Amerr-icans" and we, I don't know, sang My Country, 'Tis of Thee, or one of those things. So that's about all I remember, apart from the general fact that the kids were too forward... for my liking, you know, "hiya kiddo, blah, blah, blah" [laughs].

Do you remember anything else of teaching and learning there?

No, no, not really. I only remember the arithmetic thing. I suppose we were taught to write sentences or whatever. Who knows?

[0:40:10]

And where were you living while in America?

Well, we had this house in Yonkers. We were in... where were we? It was a few rooms to let, in somebody else's house, a lady called Mrs Cipollini, who was... most of them seemed to speak Italian in that neighbourhood. I hadn't a clue what goes on there. But I remember hearing the sound of Italian being spoken, and there was sort of shouting at the kids in Italian and so on. So... it didn't... oh [laughs], well, I remember... Mrs Cipollini had a big son. He must have been in his late teens probably, mid or late teens, called Aldo. And Aldo had a movie projector, and now that *was* a treat, because Aldo had films of Mickey Mouse and such, that he showed us from time to time, and that was fun. And I think I got on quite well with Aldo; he probably loved kids.

But then I remember... oh, I was taught, you know, table manners. I mean, one shouldn't talk with your mouth full and so on. And I remember, as kids will, transferring this to the Cipollinis. I think – it was either Aldo or Mrs Cipollini, I don't remember – I think it was Mrs, and she was talking with her mouth full and I said, "Mrs Cipollini, you're not allowed to talk with your mouth full." And Mrs Cipollini's response was, "Well, you ain't seen nothin' yet" [or something to that effect]. And [then] she talked with her mouth wide open and full of food [laughs]. I don't quite know how she did that without it all falling out, but it made an impression on me [especially when she stuck her tongue out] [laughs]. But she was quite a good sort, I think. You know, it was quite a jolly sort of time in some ways.

And when you think of the Yonkers house, do you have sort of mental images of your parents doing certain things in the house in certain rooms?

Not in detail. I remember it was a suburban house and there was a fair amount of open land and trees around. And that was nice for us kids ’cause we could go and play in the woods, another thing you’re not allowed to do these days. But, you know, we had fun, didn’t come to any harm. I think I slightly made friends with a few of the neighbouring kids, or we played together at least. I remember a great ice storm, you know. We had a very severe winter while we were at Yonkers. I told you about the deep snow. But at some point there was a huge ice storm that meant all the trees, which had dropped their leaves for the winter, were covered in absolutely *glistening* ice. *That* I remember – it was such an amazing sight.

And where is the next school? Do you stay in America for a while?

Well, let’s see. I think we must have been there for something of the order of a year [actually more like two years]. After that stay in Yonkers we spent a bit of time in uptown Manhattan, East Sixty-Somethingth street, I think it was. Somewhere near the Rockefeller Institute, I believe. Because I think the Cipollini rental expired, and we had to find somewhere else to live. So... I remember this was the summer, probably of 1947 [actually summer 1948]. It was very hot [and humid], as it often is in New York in summer, and I do remember lying not in, but on, the bed with nothing on and still being too hot, and not being able to sleep very well. And again the kids in the streets were, well, more aggressive, but I didn’t get into any real trouble.

And my sister broke her arm because she... she was always a bit more sociable than me and she made friends with another little girl, and they got on to this swing. There was a little playground – there was a little courtyard at the bottom of this block of apartments, and she was being swung up by this other kid and it got too much. The swinging was too violent and she screamed to stop, and then she fell off the swing and broke her arm. So there was a whole business about getting her to hospital and our parents somehow finding the means to pay for this, ’cause of course that was a very expensive thing to do in America, and I think it was a big worry for them.

That's about all I remember of that. After that we went to England, and he had another year's postdoc – here in Cambridge. [Again technically not a postdoc, and actually more like six months, according to the A.K.McIntyre interview transcript, Day 2, page 26.] I remember going to school here.

[0:44:53]

Let's – yes, let's come with you to Cambridge then.

Alright. I think that's about it, because I can't remember much more about America. So we arrived here and we had another severe winter. And I suppose it must have been the winter of '47–48. [Actually 1948–9.] You can probably check this in the met. Records – I never have – but a lot of deep frost and snow and stuff. We stayed in a... one of those big houses in Chaucer Road [No, Latham Road, one block further south], up in the attic, you know, in the old servants' quarters. And we had a terrible winter with us kids... I think everybody had nasty colds and bronchitis. I remember feeling very ill with something that was said to be bronchitis. And of course the place wasn't heated well enough.

But there was a gardener there, who worked for the owners of the house, who took a liking to us and made swings and merry-go-rounds at the bottom of the garden, so he was a lot of fun. He let us tease him too. He had this thing about lighting methylated spirits and saying "This is fairy fire," you see. And I was a bit too sophisticated to buy that, by that time. I used to tease him and say, "No, no, you're burning methylated spirits." And we used to smack his bottom as he rode his bike back up the road at the end of the day's work. That was Ralph the gardener. He must have been a very decent sort of bloke, I think.

I remember being taken to a concert. I remember being taken to two concerts in Cambridge, on the back of my father's bicycle – riding, you know, from Chaucer Road. [Latham Road!] Do you know where it is, in the south of Cambridge, coming up Trumpington Street? And I believe it's still got the same street lamps. Do you remember the street lamps in Trumpington Street? They're sort of tall diffusers, probably with fluorescents inside them now. But I think they're the same fittings. They look terribly familiar. So we rode up past those into Kings Chapel [sort of] and I remember we heard the choir singing and it was snowing a bit and it was rather beautiful, looking at the side of the chapel. So that's a definite memory. [A.K.McIntyre interview transcript, Day 2, page 26 again.]

And I also remember some sort of audio recorder there. That fascinated me, of course. It was... I think it wasn't a tape rec... it was either a tape recorder or it might even have been one of those old wire recorders, which is the same principle, but pretty low quality obviously. So somebody was recording that concert. And why do I remember it? Well, of course, it was a fascinating-looking bit of kit, with these great reels full of tape or wire or something, enormous thing. And I think I heard the rewind and, you know, on those old machines you would generally hear the signal running backwards at high speed when they rewound it [makes gobbeldy-gobbeldy rewinding sound]. So I didn't forget that.

The other concert was in the Guildhall, which again is [today] still much the same as it was, I think. And that was the first time I ever *saw* a symphony orchestra playing, as distinct from hearing a recording on a seventy-eight record. And that was fascinating, of course, with all these movements going on, the bows going up and down together. That I remember. I don't remember what they played though. Probably it wasn't familiar. I was always pretty slow at music. I always needed to hear something more than once to get it. If they played something I knew from records already, I'd get it straight away.

[0:48:29]

So what else do I remember? School, you asked about the school. I went to Newnham Croft School. I think it still exists. I remember a bit about that. Oh, I remember being taught to do an amazing thing, which is add up pounds, shillings and pence. Now there was something really elaborate. I don't think I did know how to do that before [laughs]. So all this elaborate layout and all the funny symbols: pounds, 's', 'd' and so on. I don't think I understood why pence was 'd' [laughs]. So... but... and I think it was, you know, a pretty good school. I think the teachers were kindly and helpful, and it seemed very well organised. Sometimes you'd make two classrooms into one big room by moving a partition. I don't know whether you can still do that.

What else do I remember? I vaguely remember the appearance [of the school building]. I remember how I got there. I had a little bicycle. I had to get there and back from Chaucer Road, didn't I, so I had to get on this little bike and go down the alleyways across... did I say Chaucer – no, it was in *Latham* Road. I got that wrong. We were in Latham Road actually; Chaucer is the next door one [to the north]. So I'd have to go from Latham Road through one alley, across Chaucer Road, and then on to that green area and along the edge of that, across a couple of [foot-]bridges. There are two river bridges there, before you get to Newnham, and at least one of them looks like a miniature Sydney Harbour Bridge. And I remember that, of course, taking the bikes across it, helping my sister. My sister was quite small then. I think we went together. So, well, I mean, I do remember that, I suppose, because it was – you know, took a bit of mechanical organisation to cope with the bikes and the bridges and help my sister along. That's about all.

[0:50:37]

And so as you were sort of getting to be a sort of older child, and approaching, I suppose, what we might call high school, what are you doing out of school at this stage? In the same way that as a younger child you were making planes and, for example, making the record player, how did you occupy yourself as you grew up, as you became an older child living now in Cambridge?

It's a good question. I probably... I'm guessing here. I was interested in making model planes throughout most of my childhood, probably up to, I don't know, high school age or so. Well no, beyond that, because when I was at high school – and that's jumping forward to our time in New Zealand – we actually had a project to make a radio controlled plane, but it never quite got finished. So no, I was always interested. So I may well have been making planes. But then again, I may not have, because perhaps they [my parents] couldn't even afford to buy the balsa wood, or perhaps you couldn't even get it, because that was only just after the war. Well okay, it was 1947–48 [in fact late 1948], and I think we went into '49. I think we left Cambridge in '49 and went to New Zealand. I remember the boat trip.

So in Cambridge I don't remember playing with either planes or electronics. I'm sure I would have been interested to do so if there'd been the opportunity. And at some point I'd got more interested in music. But again, that's another thread that runs through this whole time, because obviously I was deeply interested in music from an early age. I think it was about when we went to Cambridge when I was given a recorder, as musical kids often are. So I was given a recorder, and a book with little tunes. And I started, you know, composing my own little tunes, of course, at first just for the recorder. It could have been that learning the recorder was... perhaps occupied my spare time in Cambridge when I wasn't ill with bronchitis, or whatever else was going on [laughs]. The recorder – it was a black plastic cheapo [descant] recorder. It performed quite well though. You know, you could get all the notes quite well. Tuning wasn't too bad. And I think it was a made-in-England recorder. I don't think you could get that sort of thing in America in those days, could you? Probably not.

Do you remember what about, for example, playing the recorder you liked in particular? 'Cause you could imagine that one child might like – you could like a lot of things – but one child might like playing it to other people and watching their reaction, and that... another child might like, you know, hearing the sounds that it made. Was there a particular... what was the nature of your interest in it, in playing it?

Oh well, it's the second of course. As a loner and a maverick, I was never much of an exhibitionist and attention-attractor. (I think my sole attention-attracting activities would be when I wanted something, and to get something out of someone [laughs].) But no, with the recorder *my* interest was experimenting with it and hearing the sound and playing little tunes to myself, and then thinking of other tunes. So it was a rather... you know, solo activity, if you like.

You've mentioned myopia a couple of times. When did you first discover that you had problems seeing?

Well, my *parents* discovered it before I was aware of it, because – well, what they always used to say was that they were travelling with me in a bus or a train or something and I... and I was probably very little then – I mean, I may well have been three-ish – but apparently I pointed to a field with sheep in and said, "Look at all the puppy dogs." And they suddenly realised I couldn't see the sheep clearly. So of course then they had my eyes tested and the rest is... I've worn glasses [or contact lenses, in middle life] ever since. Of course that's another reason I was a maverick. I mean, a kid who wears glasses is always teased by the other kids. You know, you're called four-eyes and, you know, they're merciless – kids – if you have the *slightest* difference. So... but... [that's] perfectly ordinary [laughs].

[0:55:14]

What did you do with your brother and sister as you grew up? I mean, what sorts of things did you do with them? When not doing things on your own, what were you doing with your brothers and sisters?

Well, again, the loner-and-maverick was the default, really. They tended to play together more than with me. I tended to do my own thing. And of course as a little kid I would tend to think of them as ganging up on me. So I would get defensive about that, which of course reinforced that situation. But I didn't mind, because I had lots of things to do. By the time we got to New Zealand [more accurately, somewhat *after* we got there] I was playing with electronic circuits as well. That took hours and hours of fooling around [laughs].

What do you remember of your parents telling you that you were going to go to New Zealand, in other words, that you were going to move again?

I don't remember. I think I just took all that for granted, you know, that we'd go wherever we went. I don't think I understood much about career progression. Of course, the fact was [that] my father had got his first permanent academic job – or potentially permanent; I don't know the exact terms of reference. Funnily enough, it was... there was a very famous man as Head of Department there at the time, Sir John Eccles. He's a Nobel Laureate in neurophysiology. So I suppose my father must have thought, oh, that's a kind of a Mecca for... if I, you know, get an opening there I ought to go, I expect he thought. Before that he may well have thought about returning to Australia, I don't know. But there you are, he happened to get that job.

And that was in Dunedin, New Zealand, at the University of Otago, the oldest New Zealand university, founded by the Scots settlers. Dunedin, you know, means 'Edinburgh on the hill'. It's got all the same street names, Princes Street, Dundas Street, etc. They've even got a little Scott monument in the centre of town, a little thing, about – I don't know, it must be about a fifth the size of the real one in Edinburgh [laughs].

And how long did you stay in New Zealand as a family?

Well, okay, I seem to remember that we got there in 1949, and that, really, was my main education from then on, until graduating with a maths degree [then taking up a temporary lectureship at Otago before going to Cambridge in 1963]. That was all in Dunedin [for me, that is – my parents, brother and sister moved to Melbourne, Australia in 1962]. So we... I started at a little school called George Street Normal School, which was in North Dunedin. (There you are: George Street, that's another Edinburgh street.) And it was a decent school. I think the teachers were, you know, pretty devoted, and strict but kind. I think I've been lucky all my life with schooling.

We lived in this house on the hillside that hardly got any sun, and that got my mother down a helluva lot. She... you know, being a good Australian, was brought up in a sunny... she had a sunny childhood in Sydney. She was born in Sydney. So this rather dank and dismal sort of shaded house we lived in for several years – it wasn't good news for her. In fact we all tended to get colds and things, more than we otherwise would, I think. But I suppose it was the best we could do at the time – my father had a lecturer's salary.

And what was your mother doing while your father worked at the university?

Well, she was again being a good housewife and looking after the kids and, you know, trying to paint pictures in her spare time [if any]. At some point she took up sculpture a bit. I don't think she ever got into sculpture in a big way. Drawing and painting were always her strengths. She learnt to cook very well. You know, she helped with educating [entertaining, I meant] my father's colleagues. They ran wonderful dinner parties. Later on, when we made more friends in the academic community, they would band together to put on super dinners. You know, they had a sort of dining-club thing where, I think, three or four of the families took turns in coming up with new dishes. So I think that, you know, kept her quite... and, you know, they cared about good eating. I think we always – you know, they did their best to feed us well. We probably ate too much meat. In those days it was thought that you should eat lots of meat. But there you go, we had salads and vegetables too, you know. And it all takes a lot of work. New Zealand is, you know, very well off for these things, of course. Naturally, 're a lot of agricultural resources.

[1:00:08]

And can we go to your secondary school, if… ?

Yes indeed. Well, we had three levels. We had the primary, George Street Normal, we had the... what's called the intermediate – I think this is a kind of system you see in other parts of the world. That, if I recall, would be, you know, age eleven, twelve, thirteen-ish. Two years of intermediate school, we called it the First and Second Form. We don't have a complete [consecutive] set of grades; we start at '1' again, at age eleven or so. So that was the First and Second Form. It was called Macandrew Intermediate School, and I had to cross town to get there. I think my parents must have... you know, done quite a lot of hunting around for what they thought were the best schools.

So I went on the trams. Dunedin had a very nice tram system in those days (which I was devastated to see had been done away with, when I last went there [in the 1990s]). But anyway, it was quite a pleasant tram ride and a bit of a walk and... this was... sort of more in the western part. [More precisely, the school was about 5km south-south-west of our North-Dunedin home.] So I think it must have been about a three mile journey to school and back every day. And I remember my second year there. There was a very strict teacher called Miss Foster, who was very keen on getting us to, you know, do our grammar correctly and, well, do everything correctly. And I got into trouble with the other kids 'cause I was quite good at that, and so I was Teacher's Pet. And I gradually began to learn I'd better, you know, be a little more reticent and not show off too much in class [laughs].

What was the effect of not being reticent, of appearing to be able in front of these children?

Oh well, you know how kids behave. They resent it, you know, and so they tend to... they try to bully you. I remember suffering from, I think by today's standards, mild bullying. I mean, bullying can be a very serious problem these days. We live in a completely different culture, full of brutal video games and heaven knows what. I mean, we were free of that when I was a kid. So, you know, kids instinctively, you know, pick on other kids and try and wind them up and that, of course, went on, but I gradually learnt... this was the beginning of my social education, I suppose, from a complete loner and self-contained, almost, to someone who began to understand a bit about how to cope with other people. And the first thing I learnt was, well, ignore them and then they won't stay interested. Er... but, of course, it's obvious where the resentment comes from when they yell "Teacher's Pet" at you, isn't it? [Laughs] And... Well, there it is.

Anyway, I myself am grateful to Miss Foster, because I suppose it developed my sense of how grammar works a bit earlier than most kids, or again, [more than many do] these days. I notice with my students, some know this [know some grammar] and some don't know it at all. I have t'actually teach some of them from scratch, 'cause they haven't ever encountered it.

Any teaching of science at this school, this intermediate school?

I think – oh, now that's a good question. Did they have a science lab or not? They certainly had one at the next stage, at the high school. But at the intermediate school – I simply don't remember whether there was any lab work. I suspect probably there wasn't. So... we may have learnt a bit of science. They had quite a good music department though. I remember singing in the choir there. I still had a treble voice and I remember enjoying that.

And any striking memories of the teaching of mathematics at this school?

Good question. I suppose – well, we were still in the regime where one teacher would teach one class. So if I learnt any maths it must have been from Miss Foster. I can't remember the name of the first-year teacher, though I vaguely remember it was a he. He was a pretty good teacher, you know. I always quite liked my teachers. Maths, maths? Well, when did I begin to learn maths? Probably not very much then. I don't think we were doing much beyond arithmetic.

One thing I do remember is at some stage encountering Euclidean geometry. And that was... but exactly when that was I'm not sure. And I suspect it mightn't have [been at intermediate school]... well, I don't know. It might have been at high school. What I do remember is suddenly realising the beauty of being able to prove something by an abstract argument. You know, you have some similar triangles, or you've got a semicircle... You know, this beautiful theorem that if you take a semicircle and draw its diameter and any other two lines, that from the ends of the diameter [go] to any other point on the circumference, [then] the angle is always a right angle. "The angle in a semicircle is a right angle." That's the way it's often said. Not obvious, is it? I mean, you have to think a little bit about how the angles add up. But the beauty is that it's true of any such thing; it's not just true of the one case. So that's the big imaginative leap toward understanding what mathematics is about, which is about abstract things.

What is abstraction? It's the ability to comprehend many cases at once – usually an infinite number. And this is an example of it. So Euclidean geometry was, I think, my first, sort of, epiphany on that sort of thing. And I think it was probably... first year of high school. I mean, it's difficult to be sure because I know my parents gave me, you know, some books called Teach Yourself this and that. They gave me something called *Teach Yourself Calculus*, so I learnt a bit about differentiation and integration, I *think* before I encountered it at school. But that wasn't as exciting as the geometry [laughs].

[1:06:32]

And as you're now becoming an older child, what was – what did you discover about the religious views of your parents?

Oh, well, my vague impression was that on the whole they weren't very religious. But some time after we arrived in New Zealand, they joined the local Quaker meeting, so I would go along to that with them. I think it was driven by my mother's... [concerns]... She was on a quest for some sort of spiritual solace with her, you know, relatively restricted life. And she, by the way, loved music, but not as much as my father. So for my father, as for me, music would be enough spiritual solace. To me, music is the closest I get to the divine – and probably for him too, I think – but for my mother, not quite so much. So I think she was looking for something else.

I think what probably happened was she met some people... of course, some of our Quaker friends were very wonderful people. So you get impressed, how these people manage to be so, you know, well, wonderful. I mean, how do you put this into words? There are people who teach you that our species isn't *all* about brutality and the holocaust and all those dreadful things that you read about in the news all the time... um... genocide... it's obviously in genetic memory. But these wonderful people, and there are quite a number of them in the Quaker movement, they teach you that there are people who think and feel and act otherwise. They look for "that of God in every man" and so on. And-er... so I mean my mother was quite taken with that. So I just went along to Quaker Meeting with them. I never really... I mean, I thought this was probably a Very Good Thing, you know, especially since it's all about not joining the war machine and all of that. I think I quite liked that idea. And so I sort of went along, but it never quite grabbed me the way music does. I think people vary, don't they? I mean, some people... it's a kind of meditation, a Quaker meeting, and that does more for some people than for others, doesn't it.

You know, writing music is my best meditation. It's the best spiritual exercise I have. I don't do it as much as I'd like to, because it's time consuming. But getting into this almost trance-like state and asking the music where *it* wants to go, that to me is – that's the closest I can get to that sort of thing.

Why is it spiritual and not simply a kind of distraction? Why do you describe it as a spiritual experience?

Well, what does one mean by spiritual? What does one mean by spiritual health? I think I mean things that make for a deep-seated wellbeing, something that's deeply in accord with our instincts of... ah... goodness and wholeness, and... [communion with, or oneness with, something greater than oneself, I could have added] ... difficult to put these things into words. The things... the life-enhancing things that are countervailing forces to ordinary brutality, ordinary unkindness and competitiveness, ordinary greed. Some of us think these countervailing forces could be, you know, important. I think they always have been.

I mean, look how far we've come in a mere few centuries. We don't burn witches any more. We don't panic at the sight of a comet. It's – it's miraculous really, when you look at, you know, human nature on average. So these countervailing forces are real, and there are very great people who represent them and live them, and – you know, the Ghandis and Nelson Mandelas of this world – don't they. And so you have to... I think there is such a thing as a quest for these things. I'm sure my mother was on such a quest. She was always... that *is* something I remember; she was always concerned about all this. Well, they'd just come through the Second World War, hadn't they? They'd just become aware of the holocaust. They'd just become aware of the *dreadful* happenings in the Middle East that continue to this day. How are they... [unclear] ... would always talk about the Jews and the Arabs – how are they ever going to sort out this mess. You know, so what's new? [Laughs] So there was my mother going to the Quaker Meeting, and... so I went along, but to me it was sort of going along, and not really quite feeling that it was getting to me.

When you say you remember your mother being concerned about these sorts of – you know, sort of global or social problems, what are you thinking about? When you think that she was concerned, how did she show that she was concerned, by talking about it?

She could talk very intelligently about these problems [more in her interview transcript, e.g. on page 9, "I feel... one of the most wicked things people can do is to bring unwanted children into this world"] as well as about artistic matters. She was very interested in architecture, by the way. She was interested in things like the Bauhaus movement [and] in, you know, groups that are trying to make advances in... well, what should one call it, artistic progress? She was acutely aware that architecture is important for us, that if we have beautiful spaces to live in and work in, it can make a difference. And she actually designed a couple of the houses that my parents later occupied. I think that gave her a lot of... that was *her* spiritual solace actually, more than anything else, I suspect.

What was your response then to religious instruction in school?

Oh, oh, the ordinary rituals, yes. Well, of course we had prayers and hymns in assembly and... our... well, at high school we always had an assembly. It was a short address, a little bit of administration, you know, disciplinary stuff. [And *instructional* hymns that we all took with a grain of salt, e.g. "Teach us to rule ourselves alway [sic], Controlled and cleanly night and day" . Not very lucid – it took me a while to get the point!]

We had this marvellous headmaster, Bill Lang, who was a very redoubtable figure. He was a very good head, I think, but he was always, you know, standing up for law and order. *"There are always some fools,"* he would declaim in assembly, you know, talking about some bit of vandalism or whatever it was [laughs]. And somehow, you know, we had a school with pretty decent aesthetic [ethic, I meant] and, you know, barbaric behaviour was kept reasonably under control there, probably more than you could possibly do these days. So okay, and religious ritual, that was a kind of a routine part of it. We'd have a hymn, we'd have a short prayer. We said the Lord's Prayer every morning, if I recall. We also had a patriotic song, you know, *Men of Harlech* or *The Fishermen of England* or something. Later on I used to play the piano for these things. I got to be one of the school assembly pianists. I was never a brilliant pianist but I got just about good enough to do that [laughs].

[1:13:47]

And so could you describe what you remember of the teaching of science at the high school?

Okay. Well, we had... our teachers... again, they were all pretty decent sorts, and they did their best within a certain, I don't know, programme. They had a programme of experiments, simple chemistry and physics, you know, the usual sort of thing. I think my reaction to it was, okay, quite a lot of that's fairly interesting but, you know, I suppose I sort of... to some extent knew some of it already, from either my father or reading about it myself. And I think I remember being struck by, it's all a little bit... it's just a programme of things to do, that have to do with science. I don't think I had a sense at that time of how real science is done, how real research is done. I don't think many kids do, 'cause of course they *do* it as little kids but it's purely instinctive, you know, what... how real science is done, you know you're poking things to see how they work, you suck it and see... [as a kid] you play with, you know, toy guns and things.

At school, well, you can't do it that way. You've just got to go through a set programme. You've got to prepare for exams. You know, the teachers... yeah, we had quite a jolly time playing with bits of apparatus and... you know... but it wasn't a big deal, really. At the end of the day it was about getting marks in the exams – even then [laughs]. So you had to just memorise certain things. I didn't have any problem with it, but I didn't find it hugely exciting, the way I did my father talking about the stars and how far away they were.

What then were you doing out of school at this time?

Well, I was playing with electronics quite a lot. You know, vacuum-tube circuits. Transistors were... they were hardly with us. They were just beginning to appear on the horizon, I think, 'cause they'd been invented, but to be easily and cheaply available, that wasn't so easy. So yeah, anyway, I think I learnt some circuits, some simple vacuum-tube circuits, from some [two] older boys who lived down the road. They were the sons of a local maths professor and they... *they* had the most amazing bedrooms or workrooms or something, completely full of electronic components. They were real geeks and had loads and loads of bits of apparatus, and made their own radio receivers and transmitters, I think, probably illegal. So they showed me some circuits and gave me some bits and pieces and got me started. That was when we were still in North Dunedin, I now remember. [I think I was still attending George Street Normal School.]

And after that, well, I had a little bit of pocket money so I could go and buy a few components myself, in later years when I was at high school. I didn't tell you, but we moved house. We weren't in North Dunedin any more; we moved to the south end, which wasn't all that close to the school. It was sort of the opposite extreme; it was up in the hills [Shiel Hill, across the harbour to the southeast, nearer the sea coast], so I had this quite long bike ride to school and back with a steep hill at the end of it, which was probably good for my health. Always had a headwind [it often seemed], 'cause the [northeasterly] sea breeze would always make sure you had a headwind, very often both ways [laughs]. [The prevailing southwesterly winds tended to dominate in the mornings, and the northeasterlies in the afternoons.] But I had a little bedroom by then of my own, and I could... and a workbench, and a soldering iron, and I could play around with a few circuits – and getting electric shocks quite regularly because, you know, [for] vacuum-tube circuits... you need a couple of hundred volts or so.

[1:17:44]

Would you be able to describe something that you made at this stage, for an audience who perhaps won't know what a vacuum tube looks like or how you, you know, do circuitry yourself? But is there something that you could sort of take us through that you made?

Gosh, well... a vacuum tube, otherwise called a thermionic valve – we just called them valves in those days, um... well, the *principle* – that did fascinate me, that the principle... I think this is why I played with them, because I was just fascinated to look inside the things [and imagine how they worked]. You've got this hot cathode... so there's a little element [the cathode] that's got a little heater inside it, an electric heater, and it glows red – glows dull red – and it emits electrons. And because you have a vacuum, the electrons can happily fly off, if they're attracted to some other electrode that has a positive potential. Typically you'd have, as I say, a couple of hundred volts on your positive elec... element.

And then there were, you know, variations on this theme, in which you had the element attracting the electrons – that's called the anode – and in between you had sort of mesh-like things, or sparse coils, which are called grids. And the flow of electricity from the cathode to the anode – under your power supply of 200 volts or whatever – that [flow] could be modulated by putting a voltage on the grid. And if your... the geometry of the thing was suitably arranged, with the grid pretty close to the cathode usually, you could control the flow of electricity through the thing with a much smaller flow through the grid. In fact, you can even arrange things so there's no flow through the grid. You simply change its [average] voltage. You arrange for the grid to be slightly negative [on average] so that the electrons don't land on it very much. But you can still modulate the flow *past* the grid by just modulating its voltage. That's how you make an amplifier with these things.

So I understood that much, and I played with them, and made amplifiers, and that was fun for a kid because you could take a microphone and speak into it, and have it come out louder at the other end on a loudspeaker. Simple stuff, you know, really rudimentary. But actually feeling how it all worked, and putting it together and making it work, I found a lot of fun.

What was the extent of your father's involvement in this making at home of electronics?

Oh, I think I was pretty much self-sufficient by then. He was interested, of course. I mean, he knew all that stuff. He had to use that sort of kit for his neurophysiological research. So I'm sure he must have encouraged me, and he probably explained the principles. I'm sure I learnt that from him, rather than at school. They wouldn't have had any reason to teach you that at school. So, yeah, I expect I learnt it from him. He... you know, the sort of kit he used was more sophisticated. It would amplify a lot more. You had to amplify *tiny* signals from nerve cells, in his work, and make them come out on an oscilloscope or some recorder.

[1:21:06]

What did you know then of his work?

Well, not much, although he would take me in to show me his experimental kit, and I would come in and marvel at all the bits and pieces in the lab, and... occasionally he'd show... doing a dissection. And now I'm going to get into trouble with the animal-rights people, because to do his sort of work you had to use live animals, if you [want to] measure what's going on in a nervous system. He used cats mainly. And he actually had quite a conscience about this. He knew that this was, you know... he loved animals, and cats especially. We always had pet cats, and he absolutely loved them, so he obviously always had this feeling that... I don't like what I'm doing, and yet I have to do it if I'm to get anywhere with the science. And so he... you know, he did his best. He didn't need any regulation, as we have nowadays, to show that you minimise the suffering. He would see to that himself, and he would, you know, try and make sure they didn't feel any pain. This was all done under anaesthetic. And usually after an experiment you would sacrifice the animal because you'd had to, you know, open it up too intrusively. But, you know, as far as the cat was concerned that was, you know, the way I'd like to die, you know, a sudden oblivion, no pain.

These days, of course, you have to go through a whole bureaucratic procedure. I think it's quite right, actually, that you need to justify the need for experiments on animals. I'm, you know, completely with people like Colin Blakemore who argue for it publicly. Of course there are some things you can do without animal experiments, and you should always try and do that, but my father's work is a very good example of something you couldn't. And there will always be some work of that sort. So there it is. I remember feeling these conflicting emotions myself. You know, my father would always talk to the [experimental cats]... when they were conscious he'd talk to them and stroke them and pet them and try and see that they were comfortable. So...

Did you watch an experiment involving an animal?

I don't think I ever had the patience to watch the complete procedure. To do an experiment, it was very many hours of intricate work. And my father was apparently very good at it. I mean, his students always used to gasp with amazement: "Oh, did you see him dissect that single nerve fibre", you know. I learnt this later when I was an undergraduate, and some of my fellow students worked with my father. But I remember seeing an open dissection in the lab. The cat was unconscious, and the electrodes... he used things called micro-electrodes. This was the new thing at the time. That's what he learnt in New York, I believe. You draw out a glass tube to almost invisible fineness, and you can get the tip of it into a single nerve cell. And that was fairly new at the time.

And why was he doing this work? What was the... ?

To learn how the nervous system works – to learn how synapses work, how neural circuits work, how receptors work. How the... how is it that we can see? How do... how do the photons hitting the back of our eyes actually send signals to our brains? Of course there's a huge ocean of unknowns, even today, in that. But that was the aim of the science, to advance our knowledge of these things.

And, you know, when you think about something like Alzheimer's disease... and that *is* something I think about, because it's in my family. I may get it. It's one of the cruellest things that can happen, not so much to the person himself or herself, but to their nearest and dearest. It's very cruel indeed. And understanding how that works, you have to understand how nerve cells work, don't you, at a pretty deep level. So I wouldn't have any hesitation in saying [that] all that work is strongly justified, even though I'm not quite sure I have either the ability, or passion, to do it myself.

What were your brother and sister doing while you were making circuits at home and presumably other things?

Well, they were a law unto themselves. They played together. They... at one stage they both got interested in horse riding. And I think my parents... this was when they were a little better off, of course, and they made... this was some years after we came to New Zealand, and after we'd moved to this place in South Dunedin where we had a bit of land on this hillside [Shiel Hill], so we had room to keep a few horses – two or three horses, I think. I think we had two actually. Oh, we had three, at one point, 'cause we rented the space to someone else. They [my brother and sister] learnt to ride horses. And I think that was partly so that they would have something to do that I didn't because... sort of... I was the... sort of, um, favourite in a way, you know – being more obviously intellectual and promising. So they... ah... for my brother and sister it was always a little difficult having their own thing. And the horses were one thing.

My sister became a very accomplished sportswoman. She became a ski instructor, and she lives in Hawaii now and teaches surfboarding and... she's also a Feldenkrais therapist, you know; that's all about bodily balance and so on. It's a bit like the Alexander Technique, which I've had and was very good. Many musicians learn Alexander Technique. It helps you to play without getting too tense. But my sister's an expert on all that sort of thing.

My brother became a surgeon, and that's the way he's earned his living, but he's *passionate*... what he's really passionate about is making wine. He's a medal-winning vintner, in Australia – in the Melbourne area, Mornington Peninsula. There are lots of vineyards there. He makes a wine called Moorooduc, which I don't think you can get here at all. It's too small an operation. It tends to get snapped up by the nearby restaurants and a few customers in the region. But he's... you know, every time we go there we get fed these *marvellous* wines, not just his, but anything else he thinks is good, and I always think he knows what he's doing, even though I don't have the judgement to tell really independently.

[End of Track 1]

Track 2

Could you say more about high school, in particular relations with other children at the school?

Okay. I think my backwardness in, so to speak, social ability, emotional intelligence and all of those things – I sort of gradually had the corners knocked off me, and learnt to deal with the mild bullying that I tended to experience. I think I learnt how to be reasonably well accepted at school. Part of this was doing sport, you know. Now sport was quite a religion in New Zealand schools at the time, and at the time I thought it was a bit tiresome because I wasn't specially interested in sport as such, nor was I specially good at it – which may have been in part because of my myopia, which stopped me from naturally learning ball skills and so forth. So I never had any hope of being an outstanding sportsman. But I learnt that to be socially acceptable you had to have a bit of a crack at it, so I gradually learnt to be, you know, somewhat competent at one or two sports.

I wasn't too bad at fives actually. We played fives. We had fives courts. I believe there are two sorts of fives, Eton and the other one, whose name I forget. I think we had the other one. There were plain... these fives courts were like squash courts with open ends, and you hit the ball with your hand. And most of the time we played with tennis balls. And you could get quite skilful at this, you know, a bit like being... well, a bit like sort of one-sided tennis, I suppose. And there was a social side to it, too, because it was very much what you did in the interval and at lunchtime: you played fives. And there were different versions of it. If there weren't too many demands for the court you could play singles or doubles, which is the same as tennis singles or doubles or squash singles or doubles, I suppose.

But then if there were a lot of boys wanting to play, there was a game called Alley, where somebody threw the ball up and called a name and... let's see, have I got this right? Somebody would throw the ball up, somebody else [the boy designated as "Alley", I think] would hit it and call a name, and the idea was to hit the ball in the direction opposite to where the named person was, to make sure they couldn't return it. So that person was then out. So you eliminated people and then there was an interesting late stage where a few were left, and they managed to get the ball back a bit. And I remember one day I succeeded in... okay, what happened? I got picked on. I was in the middle of this crowd of boys in a game of Alley and one of the, sort of, you know, great sportsmen – the ones who are really good at it, who'd hit the ball really hard – he [as Alley] hit the ball and called, "McIntyre." And he kept on doing this and kept getting me out, every time, until one day I managed to return the ball, by some sort of miracle, and we had a significant rally [myself and Alley, as the rules specified]. And I remember that was the point at which I became more socially acceptable [laughs]. I'd jus.. passed some sort of initiation test. And I learnt to be, well, quite good at fives in the end. I even got to the, I think, semi-finals. No, I even got to the final, in my last year. That was a bit of a fluke really; I wasn't quite as good as that, but there you go. So I became socially acceptable that way.

Also made a few friends, who tended to be the more geeky boys, who were interested in science. I was very good friends with one boy, with whom I had a project to make a radio-controlled model aeroplane, because he had one of those little model diesel engines that you can fly a plane with, and I had a bit of knowledge of radio and electronics, so I was researching some circuits for simple transmitters and receivers. But sadly we never quite got to the end of the project, because some big exams came up and we both decided we'd better swot for the exams. But I suppose that gives some flavour of my social life.

The other thing we did was in my last year. A new music master came along, and decided we were going to put on Gilbert & Sullivan. Now that was a completely new and radical thing for this school. This, by the way, is King's High School, Dunedin, New Zealand, the only high school I went to, and... Anyway, so all of a sudden there was this demand for musical this and that, and I got the job of arranging this Gilbert & Sullivan opera for two pianos. And I and my friend – who happened to be the radio-controlled model aeroplane friend, who could also play the piano quite well – we played the two pianos in this arrangement. We couldn't afford an orchestra. But we put on... you know, it was a lot of fun... it was a great learning experience, finding out what goes into putting on a staged performance like that. We'd got people making the scenery and one of the masters, who was good at directing plays, did the directing. The music master oversaw the music and trained the singers, and my friend and I helped to train the singers. You know, we'd go to their homes and take them through their parts on the piano. So that was all a lot of fun.

It was *HMS Pinafore*, by the way. You know [half-singing], "We sail the ocean blue, and our saucy ship's a beauty," etc, etc, etc. And the boy who... and by the way, we couldn't have girls in the girls' parts because schools were very much segregated. This was boys only. And... I think it was frowned upon to get the boys and girls on one stage together. So there was a boy singing the leading lady, whose name is Josephine, I think. And she or he had a crisis, because a few weeks before the performance his voice started breaking, so he started cracking up on the high notes, which was... and, you know, a lesser spirit might have given up, but with great courage he just carried on, and got through it somehow [laughs].

[0:06:28]

There was also, I think in your final year, a particular mathematics teacher that...

Yes – that's right. We had a new mathematics teacher [Gabby Haase], who was also... he was also involved in the Gilbert & Sullivan. He was part of a new stream of vigorous young teachers, new blood that had just arrived and... he was... actually he was very good at several things. One of them: he was a brilliant hockey player. He brought the school hockey up to a higher standard than before. But he was also a very able mathematician and an able teacher. And he decided he was going to run a special class for so called Additional Mathematics. This was [at] the New Zealand counterpart of A levels. And, so, you could do this 'Add. Maths' subject, and take an extra paper. And he took the class. There was only about six of us, but it was a wonderful new thing, in which all the class was intensely interested in mathematics, and he played to this and got us all, you know, thoroughly stimulated. He was very fluent at the blackboard. He'd write things up quickly and he'd challenge us to do things in our heads rather than writing everything down. So that was, I think, quite influential on me. And I ended up with pretty good marks in the final exams, and a scholarship to the university, where I did maths.

At this stage, just going off to university from having finished the equivalent of A levels, what did you have in mind about what you might like to do career wise?

Not a clue, completely naïve, no idea at all. Except, of course, people did tell me, if you're good at maths, you won't have any difficulty finding a career. In those days it was easier to say that sort of thing. It was the years of hope after the war, and so on. Everyone was on the up. So that was fine.

And the decision to read maths, why maths?

Oh, I think it was just that you ought to do what you're good at, and I seemed to be better than average at maths, so... thought I'd go for it.

Could we then have a sense then of your starting your degree and the sort of content and structure of the first year?

Well, I remember going to morning lectures. It was the usual, you know, traditional thing we have at Cambridge too, lectures in the mornings. I remember we had some lectures at eight in the morning, that was quite tough for a sort of... eighteen-year-old, or whatever I was [laughs]. Actually my parents had the wisdom to see that it would be good for me to stay in a student residence hall, a sort of counterpart of the – you know, the closest they got to a Cambridge college – it was a place called Knox College and it was near the university [at the north end of Dunedin], and I think they forked out quite a bit of money so that I could do that, but my father was I think by then a professor, so he could afford a few more things than he used to be able to.

So I did all my undergraduate [studies] from Knox College and, when the parents were at home, I would go out to our house at the other end of Dunedin that I've already described, on the hills at the, sort of, south end of it, a place called Anderson's Bay – well, Shiel Hill, to be technically correct: Shiel Hill above Anderson's Bay. I would go there for the weekend to see them, but spend the whole week in Knox College and go to lectures and make new friends and do my homework and so on. And I remember cycling down to lectures – to eight o'clock lectures – in a tearing hurry after eating an enormous breakfast. The amount of food I ate in those days is mindboggling [laughs]. They plied you with as much food as you could eat, and I ate plenty. So I had this enormous breakfast, and I charged down the hill on the bike, through the Botanic Gardens actually – must have been terribly dangerous. And I sort of got to the lecture in time usually, and these were all lectures in pure and applied mathematics. [And physics and chemistry.] I don't remember very much about them, except that I found them quite interesting. And I took meticulous notes, and I did study the notes mostly – before exams came round – so there's nothing unusual about any of that. Some of the lecturers were better than others, of course. Some had the gift of lucidity and some were even entertaining – and others weren't [laughs]. Oh, I did physics as well, of course, in my first year. It was maths, physics and chemistry actually. I specialised in maths – I think I ended up with nothing but maths in the last year. [Last *two* years probably.] It was a four year degree actually. I think that was a bit unusual in those times. It was called Batchelor of Science with Honours.

[0:11:40]

Could you say something about the friendships developed during those four years?

Gosh – well, I had a room-mate in this college residential-hall setup. Most of the young men lived in pairs... and it was pretty generous sort of accommodation. It wasn't on staircases like Oxbridge colleges – it was, you know, long corridors – but the typical room would be a sort of small living room with a bedroom on either side; and that's what we had. And so I just sort of... we had to sort of... it was a bit of a lottery really, pairing up with whoever it was when you arrived there. But I think I was lucky. I picked, you know, a pleasant young man called Max Restieaux [or maybe he picked me], and he was a keen singer. He was interested in singing, you know, German lieder, and so on. So we had some musical interests in common. And if I recall correctly, he was studying law, so we didn't have much to talk about in terms of our respective studies. I wasn't specially interested in law, and he wasn't specially interested in mathematics. But, you know, we had a sort of... pleasant, chatty sort of relation.

And then, you know, the social life of the college would typically involve getting together over coffee in the evening with... in bigger parties, and, you know, one would have discussions on science or politics or whatever it was, in the usual way. And so I... I think I... you know, one or two people I still stay in touch with. There's a guy called John Anderson, who was, you know, intellectually interesting. He was actually a student of my father's at one stage. He was studying medical sciences – biology – and he actually married a girl whom I'd known in childhood.

[0:13:46]

I haven't said anything about our holidays in the Southern Alps in New Zealand, have I?

Ah no.

But we had this beautiful place in the Southern Alps, at the northern end of Lake Wakatipu. Lake Wakatipu looks like Loch Ness bent into an S shape or Z shape perhaps. I think it's a Z. [Wrong! More like S.] And at the northern end of that, there's a little village called Glenorchy, and if you go into the countryside north of that, you... there's a great big, sort of, alluvial glacial plain. It's got glacial terraces on it. It's flat but... a few terraces where the level changes suddenly. With mountains all around, beautiful mountains all around. The biggest mountain nearby is a double peak called Mount Earnslaw.

And sort of tucked into all that topography there's a little... beautiful little lake called Diamond Lake, and my family and a bunch of friends... another family, rather, called the Mantons, they built together this hut by the lake, and we would go there every summer. And this was when we were still fairly young kids. And so... one of the girls in the other family I was quite fond of then, as almost a childhood flame, I suppose, to the extent I had any such thing. Her name was Jenny. She later married John Anderson from Knox College, and I've kept in touch with both of them. Erm... so I don't know how much more to say about that... apart from it...

[0:15:32]

Any clubs or societies at university?

Erm, I wasn't very big on those things... except for music. I mean, I think my spare time tended to be taken up with music, because by that time I'd been studying the violin quite seriously. I began to learn the violin at age about ten; I studied piano as well. So I would tend to do the best part of an hour's practice on each, probably not quite that much, aged ten, but getting on that way. So actually throughout my teens it was a pretty big part of my spare time, because by the time I got to the late stages of high school, it seemed clear that I had more aptitude for the violin, although being able to play the piano to some extent was good and useful in all sorts of ways. But the violin was... I was tending to practise up to probably two hours a day by then. I certainly tended to do that when I was at university. So I think that answers your question.

Quite typically I would be doing a couple of hours of violin practice in the evening. And at Knox College you couldn't do that *anywhere* [anywhere you pleased], but luckily there was a big hall that was usually free. I think it belonged to the School of Divinity or some associated divinity school, and for some reason they had quite a big hall. Sometimes, you know, the master of Knox College would call everybody to an assembly there, if there was something to discuss or announce. But very often it was free in the evening and I'd just go and play the violin there. It was rather a nice acoustic. So I think that's why I don't remember much in the way of societies. [I did, however, take part in quite a number of concerts, in Dunedin and elsewhere. I was a founder member of the New Zealand National Youth Orchestra and was its leader for three years.]

Scientific interests: I think they were probably satisfied pretty well by, you know, the lectures and the lab work, because I was doing physics and chemistry, in my first year at least. I think I must have been doing physics at least in the second year as well, perhaps in the third as well. I remember doing quite a lot of physics lab work. And I was quite interested, you know. You had some serious apparatus to play with, much more so than at high school, so that sort of amused me quite a bit.

[0:18:06]

To what extent did an interest or a focus develop through your degree, an interest in a particular part of maths or a particular part of the sciences?

I tended to be interested in whatever was lectured well, and within... well, for example, on the whole we had better lecturing in the pure-mathematical subjects, and so my maths degree was more focused on the pure side, even though later on I became focused on applied, and fluid dynamics especially. But in a way it was kind of a good preparation 'cause it gave me a broader perspective. I have some sense of broader areas of maths than if I'd focused narrowly from the start. I certainly couldn't do much fluid dynamics, because fluid-dynamics lecturing was pretty much... well, it was either nonexistent or, in one year, it wasn't very good [laughs].

I didn’t really get into fluid dynamics till I came to Cambridge to do a PhD. And I did fluid dynamics *then* because it was a very strong part of the Department here. And the Head of Department more or less told me, well, you ought to do fluid dynamics here: that’s what we’re good at. And I accepted that pretty readily, because I think I’d learnt enough to realise that fluid motion is interesting and not at all obvious.

I don’t think I’d yet read Feynman’s lecture on fluids. You know, there’s a famous chapter, or there’s two chapters, in the *Feynman Lectures on Physics*.

Richard Feynman is one of my heroes, of course. I mean, he’s one of the people who exemplify what I mean by *understanding* something. It’d be nice to talk about that later. But Feynman has written these legendary lectures in physics, he and a whole team actually. He gave the lectures, then the whole team of people knocked them into shape for publication. But there’s – at the end of Volume Two, which is mostly a wonderful description of electromagnetic [theory], there’s two maverick chapters called *The Flow of Dry Water* and *The Flow of Wet Water*, where he talks about his wonder at fluid phenomena.

Well, as I say, I don’t think I knew that until [after] I got to Cambridge. But I think I already had some sense of fluid motion being nontrivial, and a possible thing to be interested in. And when the Head of Department said, “Oh, that’s what we do here,” I thought I’d give it a whirl.

[0:20:48]

So could you take us through the decision to move to do a PhD in England following your degree, how... ?

Oh well, that was terribly simple. That was just... everyone around me took it for granted that going to Cambridge or, failing that, Oxford, would be the best thing to do [laughs]. And

I'd shown some talent, and our Head of Department in Dunedin – who was a pure mathematician and a very nice man, called Desmond Sawyer – he said to me, "Well, I assume... you know, all you have to do is just do your best in the exams and I'm quite sure you'll be going on scholarship to Cambridge." And that's what happened [laughs]. And of course I knew a bit about the dreaming spires and the beautiful buildings, and so there was a certain magic and mythology about it for a kid to, you know, want to go and see it and find out what it was like. So that was all it came down to. And everybody took it for granted that, if you did all that, you probably wouldn't have any problem having some sort of career afterwards. That's as far as it went.

And you applied to the… would you have applied to the Maths Department, is that how it would have been then?

Well, you had to – I think – well okay, through most of my career here, you had to apply to two places, one to a college, and one to the university to be admitted as a research student. I think it was like that [when I myself applied]. I remember applying to a college. Somebody said, oh, the greatest college is Trinity, so you should apply there. And my parents knew someone there and they said, oh yes, fine, why not go with it. So I applied to Trinity and got in. I didn't do any shopping around. I mean, knowing what I know now, I would actually say that if you… you know, you might prefer a smaller college. Trinity is such an enormous place. It's got most of the Nobel Prizes and all of that, but it's very big, it's very rich. If you like that sort of thing, it's fine, it's a great place. If you prefer a more intimate thing, à la C.P. Snow, you might go for one of the smaller colleges perhaps.

Our present Head of Department, Peter Haynes, was a student of mine many years ago and he's always been a loyal Queens' man. Queens' is one of the smaller colleges, and it obviously has a jolly good family sense. If somebody asked me now I'd say, well, you should certainly consider Queens' – it's a very nice place.

What then was the subject of your PhD? What were you... ?

Okay. Well, I came here and I – as prospective research students normally do – went and talked to various supervisors and thought a bit about the projects they suggested. Of course you can't really choose in an intelligent way 'cause you don't really know what they're like. And there's a… of course… I th… I think what really happens is if you get some vague feeling about things you might like to do, and then the Head of Department comes and says, oh well, we've got so many places here and so many places for this and that, and sort of nudges you.

And I think actually I got nudged, because there was a new young lecturer – his name was Francis Bretherton – who'd just come and was looking for his first student. And I think the Head of Department – who was George Batchelor FRS who was a very redoubtable figure in fluid mechanics, still greatly revered in the whole international community – well, George was a sort of relatively young and vigorous Head of Department and he was keen to expand the Department's fluid-dynamical activities, and the reason Francis Bretherton had been hired was to expand into the so-called geophysical fluid dynamics area, which actually meant then the dynamics of atmospheres and oceans –fluids flowing under the influence of Coriolis forces and buoyancy forces – which gives the whole flow a rather different character from ordinary kitchen-sink flows.

So there was Francis. And George sort of nudged me and said, "Well, Francis is looking for a good student." You know, he was skilful, George, you know, flattered me a little bit. So I ended up working with Francis, which was quite an experience 'cause Francis was a very energetic young man, exploding with all sorts of ideas. In fact I didn't quite know how to handle working with him at first, because Francis's way of working was to sort of do his research by talking *at* you. So you had to sort of pick up what you could from being talked at. We always used to say that, ah, you can always tell if Francis is in the Department. All you have to do is just listen. He had this booming voice you could hear all the way down the corridor [laughs].

So at first I found this pretty confusing. But I remember, you know, after sort of struggling with it for a while and… sort of trying to have a go at one or two problems he suggested – suddenly a few pennies dropping. Because there's a peculiar thing that happens when you make the transition from being a bright undergraduate and getting good exam marks, on the one hand, to becoming a creative researcher and exploring new problems, on the other. And part of that – well, in my case anyway, I think I've seen this with other students too – the penny drops that it's no longer about just getting something right that's more or less complicated; it's also about paying attention to the very *simplest* things.

You know, the orders of magnitudes of some quantities, you know, how fast does the Earth rotate, or how long does the Gulf Stream take for a particle to get from one meander to another. And as an undergraduate schooled in sophisticated mathematics and group theory, and modern algebra, and heaven knows what – I actually knew what a Galois group was in those days [laughs] – anyway, [going on] from that you have to learn to respect what you formerly thought of as trivial things, very simple things. And actually that's something else I learnt from… where did I learn it from? I don't know. I suppose you sort of pick these things up listening to conversations.

A very important part of a research group is conversations in the coffee room and playing with ideas, and drawing things on the coffee tables and so on – and listening to other people *getting stuck* on problems. You have to realise that – you have to realise that you're not the only one who doesn't understand things.

That's actually rather important. I remember being in a fluid-dynamics seminar – and there was a strong programme of these, there was a pretty substantial talk every week on a pretty substantial research topic. And I remember sitting in a talk by a more senior research student, who had been around for I suppose a couple of years, and had done something rather complicated and sophisticated. And there he was putting up these transparencies on the screen, full of equations, all complicated stuff, and taking us through them much too fast for me, so I hardly followed anything. And, you know, feeling, oh, I'm never going to get on top of all this stuff. And when George Batchelor FRS pipes up from the front of the room, George says, "Len, Len," he says to this student, "You're going much too fast for me; go back and just explain that point more slowly" and suddenly I realised, hey, I'm not the only one in the room who doesn't understand this. George Batchelor FRS is having a problem too [laughs]. And so that was actually very encouraging. I didn't... you've got to learn not to expect to understand.

That's the essence of research; you're living your whole life as not understanding everything. That's the thing. You've got to try and live with the uncertainty, and [to] value, you know, partial steps *towards* understanding something. And this comes back to this point about noticing very simple things, 'cause sometimes understanding something does mean noticing something simple. Oh, the order of magnitude of that quantity – it's overwhelmingly smaller than the other things we need, so I can actually neglect it. It isn't exactly right, but that doesn't matter. It's a jolly good approximation – so I'll make some progress if I neglect that thing [laughs].

[0:29:50]

And so how did you – having been nudged towards a particular supervisor, how then did you decide what to study, or perhaps it was allocated, I don't... ?

Oh well, I mean, as usual for a research student, the supervisor suggests a problem. It can't be anything else, unless the student is a genius of unusual stature. Very occasionally you get that, but I wasn't one of those. I had to have a problem suggested. So, you know, it was a slight variant on something that had been done already. I think that was a perfectly reasonable suggestion. A supervisor doesn't want to load too much on the student. You've got to suggest a problem that's pretty likely to be soluble, and yet has a bit of novelty so it hasn't been done before. That's how you get started. So I sort of ground away at a problem of that sort and, yeah, I got somewhere with it. The results didn't turn out to be terribly interesting, but it made a bit of one of the chapters in my thesis, I think.

What was that?

What was the problem? Oh, this gets a bit technical. But... now we're talking about fluid dynamics.

Okay, if you think about weather systems in the atmosphere, you've got these meandering jet streams, and these cyclones and anticyclones at the Earth's surface, and there's a... they're all part of the same flow system. And the reality is extremely complicated, and you can't hope to understand every detail of it. But what is amazing is you can understand some aspects of it. And the way you begin to understand something like that is to, as it were, do simplified thought-experiments.

Okay, let's imagine that there wasn't any geography. Let's take away the land-sea contrast, let's have what people call an 'aquaplanet', nothing but ocean and atmosphere. And then on such a planet you can imagine that the flow could be just a symmetric flow around latitude circles. We do expect some sort of flow [relative to the planet], because the Sun's shining, and it must be driving some sort of motion. So... and I'm leaving a lot of stuff out because to give you the complete picture I've got to give you a whole lecture course on atmospheric circulations [laughs]. But… one of the interesting thought-experiments turns out to be the following.

Let's now just imagine, somehow a flow around latitude circles has been set up from west to east, let's say. So it's a bit like the real flow except it's been straightened out to just go around and around latitude circles. And I'm going to… now do a thought-experiment where I start that going like a great flywheel, and ask, will it stay that way or will it be unstable? Will it develop eddies or waves or something? And that's a doable problem, and it's that sort of problem I was investigating, or one aspect of that sort of problem. The answer is that it *is* unstable if you give it realistic conditions, a bit like the real Earth, you know, such as being warmer in the Tropics and colder at the Poles and so on, and rotating at about the right rate. And when you do that problem you find it is unstable, and if you start with a wavy motion, no matter how tiny the amplitude, the wave will grow, like compound interest. It grows exponentially fast on a certain timescale, a few days in the case of the Earth, you see – remembering the typical orders of magnitude, part of understanding something.

And then when the amplitude gets large, the flow tends to sort of wrap itself up into great big vortices, and these *are* the cyclones and anticyclones we see on weather maps. So there you have already a piece of understanding, which says, even if you didn't have the land-sea contrast you would still have cyclones and anticyclones. So the land-sea contrast adds other effects, but independently, as it were. Or rather... or I shouldn't say independently, because when you put the land and sea in then, of course, the cyclones and anticyclones have an interplay with the land-sea effect. So it gets more complicated.

Thank you, yes, that's very clear. And having started to do those thought-experiments, in other words to answer the question will it become unstable, in order to answer that question what did you do? Did you... you know, I can imagine perhaps you sat at a desk somewhere with a pencil, thinking and writing, or did you use a computer? What did you actually... or did you do something else, or make a model? What did you actually do in order to decide whether instability would be produced, in order to see that, yes, cyclones, anticyclones are going to be produced by these waves?

Well, you can do this sort of theory on several different levels. And of course I mustn't give the impression that I did all of that, because it's a standard problem that people had thought about for quite a time before. In fact the first people to do a problem like that did it in the late 1940s, and one of them was a man called Jule Charney. Jule, J-u-l-e – he's often misquoted as 'Jules' with an 's', but he doesn't have an 's'. And he's a very famous man in atmospheric science. He was one of the two who first did that sort of problem. And he did it I think entirely without a computer. There weren't any [available to him] – when he did that – that was his PhD in the [middle to] late 1940s. And I don't think he had any computing resources available, so he had to do it all with, you know, hand calculation, and tables of special functions. It was quite a complicated endeavour.

What I did was a slight variant on that sort of problem, which in fact arises when you think about a counterpart of it in the laboratory. Again, it was known since work in the 1950s, if I remember – beginning of the 1950s – that you could make a sort of simplified laboratory counterpart of this problem. Instead of the real atmosphere on an aquaplanet, you could take an annular container on a rotating table. So okay, you've got this container, it's got a sort of... outer cylindrical wall and inner cylindrical wall. In between you have fluid. And if you heat one wall and cool the other, you're in a crude way simulating the tropical–extratropic[al], the pole-to-equator, temperature contrast on the real Earth. And it does undergo a similar kind of instability.

So when you do this experiment you see a meandering jet stream, which is a little bit like the real jet stream. It goes wavy and parts of the flows wrap up into cyclones and anticyclones, and that's always been considered very interesting 'cause you're learning something about the fundamental fluid dynamics in a simplified context. So now... but what's different about the experiment is that these annulus... these annuli, these containers, are relatively tall, whereas the Earth's atmosphere [viewed on the scale of its cyclones and anticyclones] is very thin and shallow, and that means that the balance of forces in the vertical could be significantly different. You could get a lot more vertical accelerations as the instability arranges itself, in the lab case. And my first problem was to look at whether that made much difference to the results.

I see.

It's what's called technically 'nonhydrostatic effects' [laughs].

[0:37:52]

And in this circulating annulus, is it water that you're using, or is it dyed air? How are you... ?

Well, you could use any fluid, but people tended to use water 'cause that was convenient and had... it was about as good a thing to use as anything else. Sometimes people would put glycerine in, to make it more viscous, because you'd... it became a problem to understand how viscosity affected these instabilities. So that's becoming more of a... sort of... idealised fluid problem – which isn't especially like the Earth's atmosphere, which [latter] has got a relatively frictionless sort of motion. But yeah, people... you know, when you start studying a problem systematically, you want to vary the conditions [systematically]. And varying the viscosity was one thing that they did.

How did you go about investigating whether the depth of the fluid was important, by interacting with this piece of equipment, this annulus?

Well, I mean, my aim was a relatively modest one, which was simply to do what's called the linearised instability problem – that's to say, a mathematical theory that describes the early stages of the instability growing. So when it's beginning to be wavy but the amplitude not too great, you can do that with a simplified theory that's easier to handle, for several technical reasons. And you can do quite a lot of that just on pencil and paper; but in the case of this problem, I had to use a computer at the end. So that's answering another question. It was a combination of doing things on paper and a bit of computing as well. And I did that on a vacuum-tube computer, by the way. That's how ancient I am! The computing service here was still running a vacuum-tube computer, not a transistor one. It was called EDSAC II. And by the way, that was one of Maurice Wilkes's babies. I expect he told you about the EDSACs.

He didn't personally, but other people have told me about interaction with it because it was used by the Geophysics Department as well. Sometimes they were allowed to use it.

Oh yes, everyone used it. Some of my student friends were astrophysicists and cosmologists. I mean, there was a wonderful guy called Malcolm Longair, who's actually a Professor of Astrophysics. He's in the Cavendish Laboratory here, and he was a fellow student of mine. We flatted together, and Malcolm would come in – Malcolm was a sort of bluff Scotsman – and he would come in and enthusiastically say, "Hey! I've created anotherr univurrse!" And he did that on EDSAC [laughs].

I wonder then how you would describe your interactions with the computer in order to do this work. How... I know it will be difficult to explain technically and mathematically what you were doing, but perhaps you could describe what you were doing day-to-day, in other words how you're sort of doing the day-to-day science of this.

Hmm, well, I'm not sure about day-to-day. What I had to do was to get the equations worked out, that was the paper part, and try and put them into a form that was... well, first of all you'd try and find out as much as you can by playing with the equations and, you know, doing mathematical things. I... you know... it gets a bit technical, but you know, sometimes you can prove you've got stability just by playing around with things called integrals. But never mind that. So I would play with that sort of thing but in the end realise, I'm going to have to go to a computer if I'm to finish with this.

This, by the way, wasn't really heavy computing. If there weren't any computers, I probably could have done it by hand calculation at the cost of, you know, several weeks of hard labour, using things like log tables or whatever. I mean, that's what everybody did before computers. In fact there were specialists who... when you talked about a 'computer' in the nineteenth century you meant a *person*, who was jolly good with log tables, and if you were a great scientist you'd probably employ a whole team of these people, duplicating the calculations so that you could check one against the other. But since we had simple [electronic] computers, [such as] EDSAC, I could do that on the computer. So once I had the programming right, then it would take a relatively short time, although very long compared with today's computers.

So then the problem was getting the program exactly right, 'cause one mistake, of course, and it computes something different from what you meant it to. So I had to learn about being 100 percent accurate in programming the computer, which is not trivial. So, well, there's a lot of day-to-day work, just working on that, and y'put something in and the first thing that happens is it beeps at you and says, "you've missed out a full stop, or something, and I [the computer] don't understand this instruction at all." And then you get past that stage and then it gives you some numbers, which you can see are rubbish, and then you look for whatever mistake caused that, and eventually you get that... [get it right], especially if you try and compute things in more than one way, which I always try to do when I can. In fact I'd never believe a result of mine unless I'd got it at least two independent ways.

So eventually you reach the point where you think, yes, I do believe that result. I remember one time, when the numbers were coming out *slightly* wrong, and I couldn't understand this – it was just in the third or fourth decimal place. They weren't, you know, withstanding a little consistency check I was doing... It turned out in the end that actually one of the banks of vacuum tubes had gone wrong in the computer – so that one wasn't my fault. But usually it's the programmer's fault [laughs].

So, I mean, you can imagine a lot of hours of work doing that sort of thing, and getting it right. That's [part of] why, you know, a PhD takes, well, three years at least, as a rule. [I should have added that one also needs time to think *around* the problems, to understand their scientific significance and their relation with other people's work, and to begin to think up new problems for oneself.] I took a bit longer [than three years] 'cause I used a lot of time doing music. There was an absolutely wonderful crowd of musicians at Cambridge; I don't know whether you want to talk about that now.

[0:43:50]

Well, just in a second. But I wonder first whether you could tell me about your memories of members of that Department, the Department in which you were doing your PhD, the sorts of things you could tell us about the scientists working there that might not be obvious from obituaries or from other accounts. But you will have met, you know, not just your supervisor but other senior scientists there.

Well, I think the bottom line is you pick up things from listening to people talking at coffee or whatever. You get a sense of what kinds of mathematical tools they use, and they find important. And most of this, by the way, were things I had not learned as an undergraduate. I told you I did mostly pure mathematics as an undergraduate. So some pretty standard techniques in applied mathematics I didn't know about, so I had to pick up that people were using them. I remember Francis Bretherton with his booming voice talking to somebody else about – oh yeah, that problem, yeah, well look, what you want to do is you want to set up this and that, and then you want to "do the asymptotics." And later I learnt [that] that was an absolutely bogstandard thing, it just means you consider how the equations simplify when you let some parameter, some number, go to a limiting value – very small or very large. And it's a very, very standard way to simplify things so you can get some sort of handle on what's going on.

I mean, I would now tell my students a complete understanding of one of these problems consists in doing what you can with the equations on paper, and looking at the asymptotics – that's looking at the limiting cases where, with luck, some of the cases will be simple enough to solve completely by hand, with a simple formula even, if you're really lucky – but then to do all the in-between cases, where the parameters take more general values; they'd probably have to use a computer. And by the time you've done all that, you've kind of got the extreme cases and the intermediate ones, and you've got some sort of grip on the whole problem. So I was picking up things like that for the first time, in my first year.

And also of course, you know, noticing the personalities round the place [to come to your question.]. One of them was Fred Hoyle. You know, Fred Hoyle was still a membe r of the Department then. This was before the great fall[ing] out between Fred Hoyle and George Batchelor, where they both wanted to be Head of Department, and of course had very different ideas what they wanted to do with it. If Fred had won, then of course we wouldn't have any geophysical fluid dynamics at all, and my career would have been slightly different – if I'd had any sort of career [laughs]. Probably would have been somewhere in America. But there you go. But, you know, Fred... it was interesting to see the legendary Fred Hoyle around. I think I'd read his *Frontiers of Astronomy*, or something like that, when I was young.

What was he like as a sort of character, just as a person?

Oh, you know, Fred, sort of the bluff Yorkshireman, "you knaw, aw, what! that's a lawd of nonsense, you knaw"… I… Actually, Fred... [laughs] it's interesting about Fred. He was a brilliant creative thinker, and yet he would also go completely off the rails. I mean, if you've ever read any of his sci-fi books – the *Black Cloud* is a good example – his whole idea is this comic-strip idea of the great scientist who does everything himself. I think Fred... that was Fred's own image of himself, you know [laughs]… cracks the entire problem single handed. It's not how things really work, hardly ever. And even, you know, the *greatest* of scientists, people like Feynman, would tell you that.

But what was I going to... Fred... well, I'll tell you one thing about Fred. Later on in my career I noticed that he'd been saying things about the stratosphere – the Earth's stratosphere. He wanted the flow in the stratosphere to be of a certain kind, to fit his theory of 'panspermia', you see. It's all about how viruses get here from space, and 'flu epidemics are all to do with viruses coming down through the stratosphere from somewhere or other [laughs]. And, you know, he had his reasons for this theory, which I actually... well, lemme not... I don't really think anyone takes that seriously now. [We now have much deeper and more detailed insight into the workings of natural selection and molecular biology – which I, for one, would say makes nonsense of Fred's then line of argument.] But the thing about Fred is he did [seem to] have this idea of doing everything himself. So, with the stratosphere, he didn't actually bother to look at what people had observed was going on in the real stratosphere! He just got some idea about it, which is the flow – it's actually this flow around latitude circles I was talking about. He thought the stratosphere really was like that, and nothing much [else] was happening up there. And we know perfectly well that's wrong [laughs]. A lot of my career was getting a better handle on what really is happening in the stratosphere, and what the fluid dynamics is like.

But anyway, there was Fred, who thought it was, you know, just flow around latitude circles. And I once actually met him and tried to tell him what the real stratosphere was like but I don't think he was very receptive [laughs]. I don't remember very much about that. But I do remember talking to Hermann Bondi, who was famously a collaborator of Fred's, you know. There's a very famous paper by Bondi, Gold and Hoyle, or Bondi, Hoyle and Gold, in some order, that proposed what's called the steady-state theory of the universe. And at the time that was a perfectly good piece of theoretical science because, you know, they wanted a picture of the universe in which you could understand that the galaxies are receding as we... which is seen to be the only sensible interpretation of the observed redshifts – and yet the universe was in a steady state [their theory said]. It wasn't something that went from a Big Bang, as we now all believe. In fact, Fred invented the term 'Big Bang'. Did you know that? Fred... you see, the radio astronomers – and Malcolm Longair, whom I mentioned before, was in that Department [the Cavendish Laboratory's radio-astronomy group under Martin Ryle] – so I learnt quite a bit about this when I was a research student. And, you know, they observe these redshifts and [have] these pictures of galaxies receding and the universe expanding, and that was just building on what Hubble had done before.

And so some people thought, oh well, there must have been something like a Big Bang. But that wasn't called that till Fred called it the Big Bang. And he regarded it as a term of derision. Oh, this silly Big Bang, you know. A much better theory [he felt] would be to say it's all in a steady state, and matter is being *continuously* created to fill the voids between the retreating galaxies. And that was a perfectly good thing to suggest because it was testable. And it *was* tested, and found not to fit the observations. So that's [part of] why we believe in the Big Bang now, which does fit, in quite a lot of detail, with various recent modifications that have gone on – dark ‘energy’ and all that stuff [laughs].

But I remember Bondi... Bondi – I don’t suppose you ever heard Hermann Bondi lecture? He was – well, in a way he’s another of my heroes. He was a brilliant man, a brilliant thinker and a brilliant lecturer, who very much exemplified what I call ‘lucidity principles’. He was not only lucid – absolutely – you know, no needless words [or gratuitous variations], every word pointing clearly to what he was trying to say – but also very entertaining [laughs]. There’s some published lectures of his called the – I think it’s the Tanner lectures [actually the 1965 *Tarner Lectures*, published by Cambridge in 1967] – called *Assumption and Myth in Physical Theory*, which I recommend t’anybody who’s at all interested in science, and... He, er, [unintelligible] lots of wonderful things to say about, you know, both scientific progress and mythology – things that people believe without their being really justified – and how to understand things better. It was wonderful stuff.

But anyway, I once or twice had a chance to chat to Bondi, and I asked about Hoyle, once. [Trying to imitate Bondi’s Viennese accent:] “Ah Frred,” he said... uh... “Fred – a brrilliant mind. But he needed me to get him back on to the rrails.” [Laughs] ... Well, there’s a digression for you.

That’s wonderful, thank you.

But you asked me about personalities, you see, around the Department, so there it is. At the time, he [Hoyle] was working with Jayant Narlikar on the steady-state theory. They were, you know, trying to refine it so it would withstand all these new observations, but they had to give up in the end, because it got too artificial. I remember Narlikar writing a paper which said, oh well, the whole thing’s really in a steady state, but we’re living in just a ‘bubble’ that happens to be expanding [laughs].

[0:52:51]

And so how then did your PhD progress?

Well, not fast enough at first, 'cause I got into this crowd of musicians [laughs].

Oh yes, tell us about that.

Well, this was a wonderful thing, and I wouldn't have missed it for the world. There were, in the... student musical circles – and this is when I *did* get into student societies – there was something called the Music Club, and there was the Cambridge University Music Society. I was mainly involved in the Music *Club*, which ran chamber music concerts every weekend, and I played in quite a number of those. I was quite an accomplished violinist by then, having – doing all this practice in the evenings when I was an undergraduate [laughs]. And playing in the New Zealand National Youth Orchestra, by the way. I got to be leader of that. So I had quite a bit of encouragement as a musician. I suppose that's part of what got me practising like mad.

So here I was in Cambridge, and all of a sudden there were all these other musicians, some of them amazingly talented. I mean, one of them was Andrew Davis, who's now a well known conductor. Another is David Atherton, who is also a well known conductor, though not quite so well known, 'cause I think he's made his career more recently in places like Hong Kong. I've lost track of David. And Antony Pay, the clarinettist and conductor – he was [and is] an absolutely wonderful musician and... he's a good friend of mine now – he's one of my dearest friends – because we talk a lot about how music works and all of that. He works... he's a world-class clarinettist, and musical thinker, actually. He's got a marvellous article on how to think about playing Mozart, which was an epiphany to me. [Phrasing in Contention, in *Early Music* **24**(2), 290 (1996). Link on my website.]

But anyway, they were all there as young things, playing in various groups, and David Atherton was one of the… he was really the prime mover. He was the great organiser, and he got groups together to do this and that, and one of them [the works performed] was even *Pierrot Lunaire*, which is quite a challenging... it's a Schoenberg piece, which is really quite demanding technically and musically and everything. And Atherton had ambitions to become, you know, really into being able to perform twentieth century music. And, you know, this was the path that led him on to founding the London Sinfonietta, which is world famous for its performances of contemporary music. And Atherton was really the founder conductor of that. And quite a number of the musicians – the founder musicians – *were* this crowd I was in. So you can imagine, it was thrilling for me to get involved in that. I was the, you know, leading violinist in that crowd for quite a while. And I played in *Pierrot Lunaire*. I had to learn to play viola, because the violinist also has to double viola. So I learnt to read viola clef, which is an impediment to some violinists. It was actually easier than I thought it would be.

[0:55:53]

But that of course used a lot of my time and energy, so of course I got behind with my PhD work. You know, I was chipping away at some of these instability problems, and so forth. And I actually worked on another problem that was related to this annulus experiment I described, because working out the flow round and round the annulus before it goes unstable, that's also a nontrivial problem. You see, you've got hot air rising on one wall and cold air going down another [water, I should have said], and you've got layers on the bottom and top which feel the rotation strongly. The whole thing's on a rotating table, of course, 'cause you're trying to imitate the Earth's atmosphere.

And I was working on those problems but really getting... and I worked on another instability problem, which was a more interesting one than the first one I described to you. And I got quite excited by that, but it was all getting behind. So to cut a long story short, I took an extra year to do my PhD, after making a break with the musical crowd at something like the end of my third year. So the fourth year was just an absolutely focused and concentrated effort to finish my thesis, so... staying up all night in the computer lab... by that time they had a transistor computer, I think.

[0:57:19]

To what extent were you – had you thought about music as being something that... you say that you broke with it in order to focus on your PhD, but...

Yes. Well, I thought I should finish my PhD, it seemed crazy not to finish it. And I was still thinking, really, about a career in science. I think I already knew that... okay, what have I got? I'm gifted in modest ways. I have a modest gift for mathematics, I have a modest gift for music; neither of them are absolutely tops by world class, but both of them are enough to do something significant. And, well, in the music... well, I had the handicap of only starting the violin at ten; you know, that's far too late if you're going to be a world-class soloist or anything like that. Nevertheless I did get somewhere in the BBC violin competition. I thought I'd, you know, have a go to see how far I could get, and I got invited to play in one or two London things – but it wasn't quite enough. We did have a chamber group – we did run a chamber group that played piano trios, because I married this wonderful pianist. This was... now we're jumping forward to my postdoctoral years. Well, I met her [and we got together] in Cambridge when I was finishing my PhD, and...

Could you tell that story of meeting your future wife?

Well, it was through music. Actually, I met her quite a while before that, when I was still finishing my school days [no, undergraduate days] in New Zealand, 'cause I would go over to Australia for holidays. The whole family was in Australia by then, 'cause my father had moved to a job at Monash University in Melbourne, so – I think that was during my undergraduate years. [Yes, early 1962.] So that was another reason why I finished them in this residential student college, Knox College.

All the family was over in Australia; so I would tend to go over there for part of the summer holidays. And one summer I was visiting my parents in Melbourne, and they took it into their heads, well, we ought to get him playing with some local musicians. And I did meet quite a number of local musicians and have a wonderful time making chamber music with some of them. And one of them was my future wife. Her name was Ruth. Her maiden name was Hecht. She played piano professionally under the name Hecht, using the maiden name in the usual way. Anyway, it somehow was arranged that I would go and play some sonatas – violin and piano sonatas – with her. And I did, and we got on quite well and, you know, I think respected each other as musicians. Well, I certainly respected her. And I remember arriving at her place and hearing this wonderful sound of... a piece by Ravel that I hadn't heard. It's called *Le Tombeau de Couperin*, which is a brilliant but very elegant piano piece. And there she was playing it with extreme brilliance and elegance, and a wonderful lightness and clarity of touch; and I remember this [vividly!]. She was a very talented pianist and musical artist.

Anyway, so we had this nice time playing, but that was all it came to at the time. It was just one of the, sort of, musical episodes then. But then she came to England and came to see me while I was a research student here – about halfway through that time, I think. And she was in a bit of a state because her marriage had just broken up. So there was she with her three children – did I say I had two stepchildren? I have three stepchildren. That was a mistake: there are three of them [laughs]. But they were her children [and very much mine too, now]. And so she... I think she came to, sort of, talk it over with me, because she didn't know anybody much else in... well, she had some friends in London. They were [living] in London. But I suppose she took it into her head, maybe she'd come and look me up and tell me. I took her punting, I think, and she kind of decided to tell me all about it. And that was all, for a while.

But then... she may have planned [or vaguely hoped, who knows] to sort of suck me in from then on – for all I know – but what happened was she got a teaching job in Cambridge, a sort of part-time teaching job. So she'd come and stay in Cambridge for, oh, I don't know, one or two nights a week, I've forgotten exactly. We put her up in the spare bedroom in the attic of our student digs [which funnily enough *was* in Chaucer Road, as distinct from Latham Road]. And that was all for a while. But then she decided that she was going to move here, so she actually bought a house here, and brought herself and the kids here permanently. And then she offered me a digs room, more or less rent free [laughs]. I think there was a modest rent, but I was in this fourth year, and I was living on savings, you see, so she was trying to be helpful to me to finish my PhD. And we became lovers and we got married fairly soon after that. Well, no, it [getting married] was a year after that, because what then happened was I went to MIT to be a postdoc, and... they came over... or she came over to visit me the first Christmas of that, and then they [all four] came over to spend my second postdoc year with me.

So we found this house in, you know, a place called Brookline, just across the river from Cambridge, Massachusetts. We got it through a house swap – a rather complicated arrangement, but anyway the house in Cambridge [UK] was occupied by some American sabbatical visitors, and we occupied... it wasn't actually their house; it was the house of another... it was a sort of triangular swap. But, erm, never mind that. The lawyers involved threw up their hands and said, "We can't handle this," so we just did it on trust, would you believe it? It worked out fine. We even had a loan of their old station wagon, and we drove all the way across the US and back in that, the whole family, camping along the way. It was a wonderful time.

So there you are, that's how I acquired my three stepchildren, and they were fairly small at that time. We learnt all sorts of wonderful things like, you know, how to make and break camp without war breaking out [laughs] and so on. We did two big trips actually. We went from Boston to Florida and back, one winter. I thought we'd go down in winter and take in a bit of sunshine in the Miami district [having been invited to give a talk at the University of Miami]. And we had these plans to camp... we had some friends in Washington DC, who were actually colleagues of my father. We knew them from Dunedin, New Zealand. They were neighbours then, at Shiel Hill. And they'd moved to Washington DC, and we stayed with them. So we got a bed for the night then. By the way, it was quite a severe winter. There was a huge snowfall, and we nearly didn't get away at all. But we just managed it by a hair.

So there we were staying in Washington, and the next [step in the] plan was to get as far as South Carolina and start camping. But when we got to South Carolina, we decided it was much too cold to camp, and we just decided to carry on through the night. And then we made this wonderful discovery that, with the whole family in the car [the old station wagon – it was a Chevy 2] – and two drivers – you *can* carry on through the night, with the other driver sleeping in the back. And one of the kids had the job of staying up to help with the navigation, which they loved. So we discovered that, and got to Miami, and at last it *was* warm enough to camp [laughs]. So when we went to California and back in the summer, we used the same technique, so we could get past all the boring bits like Kansas without stopping [laughs].

[1:05:44]

Can you say something about your relations with these children, 'cause you've suddenly got stepchildren, having been a sort of fairly independent research student. And yes, by that I mean describe the sorts of things that you did with them and how you related to them and so on.

Well, I suppose it was part of my sort of retarded education in emotional intelligence. I was still going to the Quaker meeting when I was in Cambridge [UK], although again I still found that the meeting itself didn't do a huge amount for me, as I said before. But I ended up taking the children in the sort of Sunday School class, and in a funny way I enjoyed that. There were two or three kids who needed to be looked after and entertained during the meeting, and I volunteered to, you know, do some of that work. So we, oh, I don't know, did... Bible stories, and we even built a cardboard model of Solomon's Temple at one stage.

So, in a... but I suppose the real interest for me... I was never really interested in the Bible stories, except as some sort of cultural background that perhaps one should know something about, but more in the psychology of dealing with kids, which was sort of... I was getting to be old enough that it was beginning to be a little interesting, you know. Little kids these were, eight or nine year olds. So there I was, working with them. So I had some sense of, you know, to some extent enjoying working with kids.

Now when I went to see... when she [Ruth] saw me in Cambridge, she didn’t have the kids [with her], but then naturally I went down to visit them [in London], and I found I... I sort of suddenly got on with them, in a peculiar way. I remember a scene where I arrived there and the youngest one was squirting his mother [with a water pistol] – ’cause they were in a state of upset ’cause the marriage had just broken up, so they were all mixed up. And there he was squirting his mother, and I completely instinctively sort of stopped him, you see: “You don’t squirt your mother like that.” I think I even smacked him, you know. What a thing to admit these days. But it was all completely instinctive. But it seemed to be the right thing to do, because things calmed down and the upshot was that the kids began to, you know, respect me as some sort of stabilising figure, I think, and it sort of grew from there... I have to say, I now feel very privileged because they’ve all turned out marvellously. They’re all managing their lives very well. They’re wonderful people in their own ways, and we see all of them quite a lot. [Including two grandchildren now. Ruth and I count our blessings almost every day.]

Could you say more about your interest at that point in the sort of psychology, as you put it, of dealing with young children?

Well, it was all rather instinctive. I’ve, er... I’m not *very* able at emotional-intelligent things but I’m not completely stupid either, I suppose. And with this kind of – well, call... you feel that there’s a need, so you respond to it, I think. And there was a need to stabilise this family situation so, I don’t know, I seemed to be having some success with it and Ruth was, I think, grateful at the time, and we sort of grew closer as a result, I think. And I felt I, you know, was becoming part of the family. So that was – you know – I suppose for someone who’s rather distant from their own family... remember, all my folks were in Australia. I suppose... I'm only theorising, because what really happened was, some sort of instinct took me there, that's all.

Did you at this point have an interest in, as I know you now have, in how the children were growing themselves, how they were... ?

Oh, I think I was, yes. I mean, I did the sort of thing with them that my father had done with me. I helped them make the model boats and planes and stuff like that, and tried to, you know, play games. We had a Meccano set, which... Made things with motors and so on, and fooled around with that. One of the kids actually [laughs], like most... One of the kids... it's funny how they have different characters... the youngest one liked making things. He was the one that I actually made... model this-and-that, mostly.

The other one, the boy next up, he was more of a... how would I describe... I think he was a bit more disturbed by the breakup of the marriage. He was closer to his father, a bit less flexible. [And more *angry*, I could have added.] And the main thing he did with the Meccano set was set the motor spinning... it was the same [electric] motor that I made the turntable with, with my father – just the same kit – amazing how that thing survived [laughs]. But of course if you put something on to the main shaft it would spin like mad, like the propeller of an aeroplane. (Well, of course, one of the games *I* used to play was pretending it was the propeller of an aeroplane, obviously.) But anyway, he would set it up with it spinning horizontally, and he'd drop nuts and bolts on it to see them go ping, ping to the corners of the room. And I suppose that was his way of being [an] experimenter, exploring the world, you know. It was quite fun actually [laughs].

[I could have added something about my oldest stepchild, a girl, though in those early years most of her play was with her mother – my grasp of 'girlie' things being pretty limited then. She was wonderfully accepting of me as a stepfather, though, and grew up to be a kind, generous, and no-nonsense adult, a stalwart family member.]

[1:11:36]

Could you take me through the decision to do the postdoc at MIT? What I'm saying is, why do that rather than anything else?

Well, by that time I had got through the PhD, more or less, and felt I'd become qualified in, you know, these fluid... geophysical fluid dynamical problems, so called. Atmosphere–ocean dynamics is a better description. And so one naturally thought, where would one go next? And again my supervisor, Francis Bretherton, was... he thought... he suggested, I think, well, a jolly good place to go would be MIT because... look, in some ways that's... I don't know whether you'd say the centre of the universe. It has a very strong tradition in that area, and that's where the aforementioned Jule Charney was a professor. And a colleague of his called Norman Phillips was another very famous early pioneer, in numerical weather forecasting especially – did some of the very earliest computer models of the atmosphere. So there were two luminaries there and I think Francis just thought, well, that's where you should go if you want to get further in this area. So he wrote – he must have written, you know, a strong recommendation for me – because the next thing I knew was they said, oh, we'll have him, you know, the salary will be this and that, and so on, for one year with a possible extension to two, I think it said. So I went. I went by myself at first, and Ruth and the kids followed a year later, as I said.

[1:13:23]

And what was the – could you describe what you encountered when you arrived? In other words, describe the Department physically, but also its people.

Well, it was already in a sort of tall building – must have been about a twenty storey building. You know the MIT campus, it's towards the back and it's called the Green Building. 'Green' after some millionaires called Cecil and Ida Green, who gave the money, I suppose.

And, you know, it had some Earth science, geology, and so on, on the bottom half, I think, and the Meteorology Department was more or less the top half, and I was toward the bottom of the top half. I think they already had a bit of oceanography. Let's see... there's a very famous theoretical oceanographer called Carl Wunsch, and he was around. I don't... was he already an assistant professor? I'm not sure. He might have been at [the] Woods Hole [Oceanographic Institution]. There were all these connections. It had been long appreciated that fluid dynamics in the atmosphere and ocean was fundamentally similar in many ways. They're both flows of nearly frictionless fluid, with important Coriolis effects – that's effects of the Earth's rotation – and also important buoyancy effects... So it's typical to have light fluid lying over heavy. I mean, when you look out of an aeroplane window you can see all these stratified clouds. It's kind of obvious. And the ocean does [something similar]... it's almost inevitable, because there's nothing to stop it being stratified. Once you've got heavy fluid, it tends to go to the bottom. That's very fundamental. [I omitted to say that before going to MIT I participated in a summer programme at the Woods Hole Oceanographic Institution, attending lectures and doing a small research project – this was the summer of 1967 .]

And MIT had been a place where one of the great pioneers of the previous generation, a man called [Carl-Gustaf] Rossby – he's got his name on practically everything in the subject [laughs]. There's a Rossby number, and a Rossby deformation length and a Rossby adjustment problem, and a Rossby everything. But he *was* a great pioneer. And he was the Head of [the Meteorology] Department at MIT before the war. And part of the way he was able to advance the subject was by selling it as important to the economy, and to the military and everything, you know. We needed to understand the weather and climate better, obviously, and Rossby was a very eloquent advocate, and he got these [Meteorology] Departments set up and funded. And he was a brilliant thinker himself. So there you are.

So that was the tradition, where they came from. I don't think they had the Green Building in those days. But the next generation included Jule Charney and Norman Phillips; they were the, kind of, leaders, at the time I was there. And for me it was, you know, paradise, because they were old-fashioned scientists who believed in giving their young people their head, and a lot of intellectual freedom. So I was basically free to work on anything I fancied. Difficult to imagine that, these days.

So I worked on some things I fancied, and did one or two interesting problems [laughs], and had a great time, you know, finding out what it's like to live in America... and go to all these seminars at Woods Hole... they had a rotating seminar where – they had all sorts of interesting talks from oceanographers and atmospheric dynamicists. It rotated between MIT, Woods Hole, Rhode Island, [and] Yale... [and Harvard] – I think that was about it – and, you know, you'd have a talk for two hours. It was, sort of, more the Chinese style of talk where you go on for two hours, with maybe a short break in the middle, and completely free to interrupt all the time, so there was very often a lively discussion from all sorts of very interested people – some of them, you know, great luminaries in the subject. So it was wonderful for me to pick up all these cultural currents and get a broader view of what was going on in the field.

[1:17:33]

And what did you study in particular? You said you looked at a couple of... ?

Okay. Well, it gets a little technical now. I spent quite a lot of time on a problem, er... if you think about... I think I'm going to have to talk about an idealised thought-experiment. Some of these phenomena do occur in the real ocean actually. But let's think about this annulus thing, and flow round and round. And you've got gradients of temperature and/or salinity, things that make the fluid more or less buoyant... 's what make it stratified. I mean, these [this] annulus does the same thing; it stratifies the flow. You've got heavy fluid beneath, and lighter above, and there's a gradient. Now if you imagine a shear flow in that, with some parts of it going faster than others, then there are other sorts of instabilities that are not the sort I was talking about before, where it gets wavy – but there are other sorts where it simply overturns in a symmetric way without going wavy. And some people at... well, some... in particular, a man at Princeton had been doing computer experiments on that sort of flow, and there were some peculiar phenomena that weren't understood at the time, in which the overturning took place, but it sort of broke up into a number of cells. This is getting rather esoteric, you see.

I'm following it and I'll say if I don't.

But it turned out that in some of those flows.. okay, okay, let me just backtrack a moment. Instabilities of that general sort of had been studied for quite some time, but the flow in cells in this computer experiment took place under conditions that would have been predicted to be stable by the classical theories. And it turned out that, to get the instability, what you had to have was what we call a double-diffusive effect.

Now I'd better stop and explain that. I've told you that buoyancy forces are important. If the fluid moves around, it notices whether it's heavier than its surroundings; [then] it wants to sink again. That's part of the dynamics of this sort of overturning. But also it's going round – each piece of fluid is going round like a flywheel – and if you think about that you realise that if it's going awfully fast it wants to fly outwards. So there's a sort of inwards–outwards thing that's a bit like the buoyancy force, but not quite. And the upshot is that the way those two things interplay depends on how quickly you wipe out an anomaly.

Now okay, think of a ring of fluid going much faster than its surroundings. Well, it won't do that forever because it'll feel the viscous force and try to slow down. It'll try to come to the speed of its surroundings, if you give it time. Similarly, the buoyancy... if it's heavier and wants to sink down, that heaviness, that buoyancy anomaly will, if nothing else happens... will tend to diffuse away, because of its [negative] heat; if it's cold and wants to sink, it'll... heat will diffuse in from either side [and/or from above and below], from the warmer surrounding fluid, okay. It's [colder, or it could also be] warmer, than its surroundings, so you have to have that sort of contrast. [A garbled attempt to say simply that diffusion reduces the buoyancy anomaly, regardless of whether it's a cold anomaly or a warm anomaly, relative to the surroundings.]

Now it turns out that the instability that this Princeton chap had in his apparatus [in his computer experiment, I meant] depended on the flywheel motion diffusing faster than the buoyancy motion [than the buoyancy anomaly, I meant]. That's why we call it a double-diffusive instability, because it depends on two diffusivities being different. And so roughly speaking, what happens is that – if you imagine an overturning, you get the flywheel motion because, the moment fluid moves in or out, it wants to spin up or down. It's the ballerina effect. But then if you kill that spin quickly by the surrounding viscosity, then you actually reduce the sideways force effect, but keep the buoyancy part, so it's able to go unstable more easily, okay? And it turned out that's what was causing these cellular motions.

So I did a lot of rather technical, you know, complicated calculations, to study that sort of instability. And I went down to Princeton and got this chap to give me some output from his computer, so I could see whether that matched what he was getting. And it turned out that it did, so we felt we'd actually understood this kind of thing. And, in a way, that's a good example, you see, of curiosity-driven research. There was no obvious application, or practical consequence in sight. We simply wanted to understand what on earth was going on in this computer. Which is how all science worthy of the name works, you know. You're confronted with something strange, and you want to understand what's going on.

But just to finish the story, it turned out years later that people had observed things that were essentially this phenomenon in the real ocean – especially, if I recall correctly... let me... I'm going to get this wrong, you know. I'm going to stop talking about that 'cause I'm not sure I remember the details. What I can tell you though is that it was done in laboratory experiments, and people got quite excited about that. They set up an experiment where you had a rotating fluid and you made some parts rotate faster than others by having a disc. You know, you had a container rotating at one rate, and a disc that's rotating, faster I think, and you had buoyancy from salt stratification. And that's a good candidate for this sort of instability, because the salt diffuses much more slowly than the velocity. And sure enough, you got layers. You got overturning cells, like the theory leads you expect. So that was kind of nice.

But I suppose what I could fairly say is that it's just one of a class of double-diffusive instabilities, some of which are certainly important in the real ocean, and in stars too by the way. That's a recent development. I can't claim the credit for this, but I've been, you know, a bit involved in some research on the Sun's interior and its differential rotation. And as a result I've got to learn about some things that astrophysicists are doing. And one of the things they've done is shown that double-diffusive instabilities might be quite important for understanding the evolution of stars and planetary systems, which is a hot topic these days. So it's another example of... well, you know, you can't predict what.. You could never write a research proposal for doing any of this, as I said in my *BlueSci* essay. [2006, link on my home page.] If I had a crystal ball and could see in advance what the final outcome of research I'd done would be, I still couldn't write a successful proposal because, if I said I'm going to do this research and it will impact on this sort of thing, and that – on stellar dynamics and so on and so forth, and understanding the ozone hole, which is perhaps a more important thing I did since then – nobody would believe it, because you're seeing far too far ahead for anybody to see it coming. So you just can't do that.

You can't ever... you can't... when you're proposing for scientific funds... My policy has always been: always be honest. I never propose to do something I don't think I can do. But you've got to tailor it to the current culture. It's got to get past some committee, and you've got to make it intelligible to them, and make it seem plausible. And it almost... and you know that what you propose won't be the real outcome, because if it has a real outcome that's important, it will be because something unexpected has happened.

[1:26:33]

Thank you. I know that, as you say, this was work that was... as you've just gone through, this was work that was being done without a clear application, but how did it relate to the existence of numerical weather forecasting within the Department, the fact that this problem *was... you know, you were available to... it was possible to investigate this because of what was going on in terms of numerical weather prediction. Did it bear any relation to it?*

Well, we all knew that numerical weather prediction involved the sort of fluid dynamics that we are studying, but to do it in a way that's close enough to the real atmosphere to be – you know, to have practical value – you do need a big computer. That's always been the case. That was... As I think I said to you before, I actually met the leader at the... when I was still a research student [or it might have been after I got back from MIT] I met the leading, you know, scientist involved in numerical weather prediction at the Met Office; but of course their whole focus was building computer models. They didn't have time to study fluid dynamics; they just had to try and code up the equations and hope it would work.

And that wasn't where it was started. Of course, Jule Charney, my postdoc mentor at MIT, was actually very famous for his pioneering work in numerical weather prediction. The very first efforts at that were early in his career in, well, around 1950, if I recall, at Princeton, because [with] the new computers that were available after the war, they... people saw that they ought to try the numerical weather prediction problem. I mean, the *idea* of the numerical weather prediction problem had been around for ages. It was pioneered by a man called Lewis Fry Richardson, who was, you know, active – he was an Englishman – he was active in this research in the early twentieth century. He wrote a famous book called *Weather Prediction by Numerical Process* [CUP, 1922]. And he had a vision of the future, if you please, in which it was all done by hand! So he imagined something the size of the Albert Hall filled with human computers number-crunching, and all under the control of a sort of conductor-like figure in the centre, who would, you know, try and monitor how the calculation was going. And if somebody was getting behind over there, shine a red torch on them, and shine a green torch on the ones that were ahead, so they could take a break [laughs].

But of course they had no idea of electronic computation in those days, so it was only a dream. But of course the scientific point was clear enough. We think we know the equations of fluid dynamics, so in principle we should be able to predict how the atmosphere moves. And so he did quite a lot of the pioneering thinking about that. He even did a trial calculation – by hand, if you please! – on a coarse grid covering Europe, a very coarse resolution. It went completely wrong, because of some technical reasons that I could explain, about errors in the initial conditions. That might be an interesting thing to talk about but I'm not going to do that yet because it would be quite a long digression – the problem of 'initialisation of weather forecasts', and so-called 'balanced motion'.

Okay, so I was saying, Jule Charney, he was the first visionary scientist who had computing power at his disposal. This, by the way, was largely due to John von Neumann, who was the great maths genius, who very much was a mover and shaker in getting computers out into applications other than, you know, wartime decryption, essentially. So I think it's usually said that von Neumann suggested, oh, we should do weather now. He probably knew about Richardson's work. And Jule Charney was the young postdoc who came in and helped to make it happen. And Norman Phillips was another. So my two postdoc mentors were both well known – great pioneers in their field.

So when the UK Met Office started to do it, which was in my student time – that would have been at least ten years on – they were beginning to do it, they were really just catching up with the American project. People were beginning to see... Of course, to be fair, the American project was entirely experimental. To make it operational, and actually practically useful in day-to-day weather forecasting, that was still a long way off. I think it's fair to say the Met Office was about as early with that as anyone else.

[1:31:23]

What did you see of what else was going on then in the Department, including numerical weather prediction, while you were there as a postdoc? You've described what you were working on but what...

Hang on, as a postdoc at MIT do you mean, or [at] Cambridge?

Yes, at MIT, yes, sorry.

Okay. Well, people weren't specially doing numerical weather – when Jule Charney was involved in numerical weather prediction, that was in the early '50s – well, throughout the '50s I think he was more or less involved. So okay, when did I go to MIT? It was '67, wasn't it? It was August or September or late in '67 [after spending the summer at the Woods Hole Oceanographic Instituition]. And Jule was not working on numerical weather prediction then. He was working on other problems – a set of other problems. I think his work on desertification came from that time, for example. I mean, he had very broad interests. He worked on a variety of things. He was also interested in this 'balance problem' that I mentioned, this balance and initialisation, although I don't think he'd actually worked on it specially himself [at that time, I meant – he *had* worked on it in the '50s]. But we had interesting conversations.

I think it was Jule who first mentioned to me – look, the balance problem, it's related to the Lighthill theory of aerodynamic sound generation – which was a penetrating remark. This again gets rather technical. Will you remind me... when I... let's not get off on that now, 'cause it's too long a digression, but when/if we talk about balance and initialisation just remind me about Lighthill, because there's a long and convoluted technical story with that, that's lasted until recent years actually. Jule died quite a few years ago. He got cancer, poor man, but he would have been very interested in these latest developments in that field.

Erm... I'm trying to think what else. You see, he and Norman Phillips were both very happy to leave me free to do my own thing, so they didn't try and get me doing what they were doing, or anything. So the downside of that is I was probably not completely aware of everything they were doing. I just had to look up their list of publications and find out [laughs].

[1:33:59]

What was then the second problem that you worked on in your postdoc? You've described the first, involving the double diffusion and the development of the...

Okay. That was quite a... that occupied quite a lot of my first postdoctoral year. Gosh, hmm... I'm slightly stuck. Perhaps that's my memory failing me. Surely I did at least one other thing then. I was... oh well, of course, I did spend quite a bit of time writing up stuff for my thesis. Yeah, I think that might have been a substantial call on my time, because I never got any of it published before taking the oral [the PhD viva]. But I did have some results. Yes, I remember now, I wrote some of that up as a paper for publication, and also as a fellowship paper for St John's College. I was too late – having taken four years over my PhD – that excluded me from the Trinity fellowship competition [laughs], which is pretty tough anyway. So who knows, I might never have made it there. But I did get in time for the St John's research fellowship [competition] and I did win a St John's research fellowship. And that was for, I think, a pair of papers that came out of my thesis. So that plus the double-diffusive problem... it might be why I don't remember doing anything much...

Oh, wait a minute, I remember one other thing I did, which never got published. I think I did it at that time, or I began work on it at that time, so that must have taken a lot of time. You know, if the bureaucrats were on to me I'd be in trouble, because I'd spent this time on something that didn't get published [laughs] – although I learnt quite a lot of useful stuff. There's a phenomenon in the ocean in which you get... It's called the equatorial undercurrent. It's especially conspicuous in the Pacific. And what seems to happen is that the trade winds blow from east to west across the Pacific tropics, and so there's a tendency to push the water westward. But there has to be a return flow somewhere. And some of that's round the sides, but some of that return flow seems to take place along the equator underneath the surface flow. And it's quite a striking phenomenon that's quite narrow. It really is on the equator.

So that's interesting, you see, especially to a fluid dynamicist: how does it know to go along the equator, for heaven's sake? There's all sorts of topography and irregularities and islands here and there, why wouldn't it wander around like most other ocean and atmosphere currents? And so there's a lot of interest in this, and people went out and measured it. So it has this narrow jet profile, and it sticks along the equator, and so there has to be something about the Earth's rotation. That's the only way this thing can know about the equator... is [to] feel the particular pattern of Coriolis forces and – okay, it's also to do with the stratification.

Remember these flows are heavily stratified: they like to sit in layers with heavy fluid below and light above. So what happens, if you remember about Coriolis forces, the main effect... if you've got this heavy stratification, the main effect is on horizontal motion, okay? The stratification tends to keep the motion more or less horizontal. So the only effect of the rotation is the effect associated with horizontal velocity components, and that means that north of the equator, if I push a piece of atmosphere or ocean in one direction, it wants to turn to the right, and if I do it on the other side of the equator, it wants to turn to the left. So there's a change in sign of the Coriolis effect, and everybody thought, ah, that must be why the current is there, because it's noticing the Coriolis effect changing sign. I mean, it's still difficult to give any other simple explanation.

So people were trying to make more detailed theories, and I had a go at one version of this. I think that's what occupied me for quite a while. I did a thought-experiment in which I said, okay, if I push the water at one end of the Pacific, will it – will the pushed water just spread out like it would in an ordinary room – if I do that it just spreads out – or will it want to go along the equator? And I managed to get some results in... for a simplified problem in which I did a *gentle* push. This is another example of simplifying the equations by making some parameters small... er... this is the strength of the push. So if I start pushing the water [gently] – I discovered that the motion is a bit like what happens if you push water in a ditch, okay? Think of a... or a gutter at the edge of a roof. Think of some long channel with water in it, and a piston at one end. If I start the piston moving it's pretty obvious that the water level will rise just ahead of it. But then the elevated water has to go over to undisturbed water at some stage [some distance away]; and the place where that happens propagates as a wave [getting further and further away]. So if I start pushing here, and if the wave speed is fast enough, the deep water will... the region of deep water will extend continuously, [further and further away] from where I'm pushing. And furthermore, everywhere underneath the elevated surface, the water will be moving with the piston, so all this water will move.

And I concluded that's a bit like what happens under the equator, but instead of [channel] walls you have the Coriolis parameter changing sign, and so you still get the same sort of effect. I would still argue that's one simple way of beginning to understand the equatorial undercurrent. But since I worked on the problem, and failed to publish anything on it, I've lost track a little bit of what the latest, you know, judgements are on that.

So the Coriolis effect makes the equatorial bit a bit like a channel, the Coriolis changing sign allows it to be analogous with pushing something down a pipe?

Yes. Actually, the simplest version of the problem is if you start pushing from the west edge of the Pacific. I got that the wrong way round, I think. You get similar things either way, but with the west it's simpler and you can begin to see that intuitively, because if a piece of water wants to go southward, it tends to get turned back, you see, so that's the sort of... that's not a complete explanation, but it's a sort of beginning of an understanding, I would say.

[I could have added that there was yet another problem that I worked on at that time – indeed I cracked it and then talked about it for a bit – but never published the results because I heard on the grapevine that someone else, a Swedish oceanographer called Gösta Walin, who later became a good friend, had cracked it first. So I initiated a correspondence in which I pushed him to publish instead! This is what's called the 'stratified spinup' problem – a problem involving the interplay of Coriolis, buoyancy and viscous forces – regarding which there had been an unresolved issue while I was at MIT,in fact two claimed solutions *both* of which turned out to be wrong!]

[1:41:31]

What decisions were you making at this point about what you were going to do next or what you might do next, or what your options were or what it would be interesting or valuable to work on?

Well, again I just fell into it. I'm a classic case of things happening to me, rather than the reverse, in every respect. So I had this two years of funding at MIT, and then there was an opening at Cambridge. It's much tougher for the young things these days; there aren't so many openings. There are more talented young scientists competing for permanent positions. Well, this wasn't quite a permanent position, but it was a sort of, I don't know, semi-permanent perhaps. It was called 'Assistant Director of Research'.

I think one reason I got it was that I'd won the research fellowship at St John's, so there were some, you know, referees who thought I was promising, at least, and maybe had accomplished a bit, so they decided to take me on for this position. And it involved a bit of teaching, but not as much as a full lectureship. And I think the money [unintelligible] probably arose from George Batchelor's efforts to expand the Department into this geophysical area, but I don't really know. But anyway, that was there, and I got the position. And then a lectureship came up, and I got that too.

You know, I was so naïve at that age. I – I didn't immediately jump at the lectureship. I said to George, "I'd rather spend more time on research, and less on teaching." And George said, "Don't be silly, a lecturer is a more secure position." So I took it [laughs] and the rest is history. That was a tenure-track position, so I had essentially a permanent career at Cambridge. And this Department has always been a stimulating place, and it's a source of good students, so there never was any reason to move – even when in later years I was offered what looked like tempting Directorships and things. But I never – I always – I looked at some of them, but I always ended up saying, I don't have that sort of talent, I can't handle the politics, and the bureaucracy, and the fundraising. I'm not good at that stuff, so I don't want to be a Director even if it sounds rather grand [laughs].

So in what year was the Assistant Director of Research post?

Yes, so called... I mean, and you'd... it's really a research assistantship. It started in October 1969, so that was after the two postdoc years at MIT, straight in. So how lucky was I, you know? I've been lucky all my life, Paul [laughs].

Do you remember the interview for that post as Assistant Director?

I don't think they even bothered with an interview. They must have consulted some referees. I'm sure Francis Bretherton was one of them. Francis was still here in the Department. I expect he had a say on the relevant committee. And they probably asked for referee letters from Jule Charney and Norman Phillips, I should *think*, but I don't know. I mean, what else could they have done? So Charney and Phillips must have said, well, he's done some quite interesting stuff. I don't know, they must have thought this [laughs] double-diffusive thing was, you know, sort of interesting in its peculiar way. [And I might have got some credit for the unpublished work too, which probably got some local attention.]

[1:45:07]

I didn't tell you the other thing I did for my thesis, which I then worked up and worked a bit more on for publication, and which won me the [St John's] fellowship. And that was another wavy instability problem that was technically quite difficult at the time, and of... a lot greater interest. Now how can I explain this? It's worth having a shot, because it connects with things I did later, and am still interested in. If we go back to this thought-experiment where we start with flow around latitude circles, and we let it go unstable in this wavy mode, which doesn't, by the way, depend on diffusivity at all. It's a completely different phenomenon – the sort of instability that makes cyclone and anticyclones.

Now there are different versions of this problem that are technically harder or easier, and one of the harder ones is one in which the flow you start with has a jet-like structure. So there's some middle latitude where it's strongest, okay, and the strength dies off to either side. And by the way, these flows we're talking about always have differential rotation in the vertical. So, to get realistic anticyclones and so on, you need the flow to increase with height. It's got what we call vertical shear, and that goes for all the problems I've been talking about without exception, including the diffusive ones. So we've got vertical shear, but now we've also got horizontal shear, because of this horizontal jet-like structure, which is a step toward the real thing, which is always jet-like.

Now if you... to solve that problem it gets technically harder mathematically, for reasons that are difficult to explain... But... okay, the... my first thesis problem, remember, I could solve it by a combination of paper mathematics and [a] rather simple computer program, on this vacuum-tube thing called EDSAC II. This new problem is... takes... well, again, I tackled it by a combination of paper and computer techniques. And it's a good illustration of what I was saying about asymptotics, because there's a case where you can get somewhere just on paper, in which you say that the horizontal shear is weak. So you say it's only a small departure from the original problem, which had only vertical shear.

Could you just remind us of the difference between vertical shear and horizontal shear? What is the vertical shear?

Yes, of course. You've got to imagine the Earth – and vertical means radial, of course, upwards, away from the centre of the Earth. And the vertical shear means that the basic velocity, the velocity I started with in this thought-experiment, round latitude circles... as I go up, it's increasing. If I imagine flying in a balloon and going up, I would be in stronger and stronger wind.

Okay, yep.

That's vertical shear. And... so... the problem with only vertical shear is simpler because you have to work out the structure only in the vertical dimension, whereas if you've got horizontal shear as well, it's got structure in the horizontal as well; that's not trivial. And that's a much bigger computational task. And indeed, at the time it wasn't really clear how best to tackle it. So I took an intermediate step, which was to say, let's try and solve it for the... for weak [horizontal] shear.

By the way, I wasn't the first to do this. There was an earlier paper, funnily enough from one of Jule Charney's students. This might have been part of what got me the fellowship, you see, because this student of Charney's, who's actually a very famous senior man now... his name is Joseph Pedlosky. (He just [now] got a big prize at the American Geophysical Union.) But, yeah, he's very famous for his work, mostly on ocean currents actually. But he was Jule's student and he did... he was not the first, but the first to do rather systematically problems with both horizontal and vertical shear, of the kind I'm talking about. That was his thesis. [He made progress by drastically simplifying the vertical structure, while allowing continuous horizontal shear.] But then he... *he*, actually, was the first to think of this idea: let's try and get at this [now with *continuous* vertical structure and] with weak horizontal shear, and do the asymptotics for that. And he got it wrong! This was a paper he worked out after his thesis, so that was one of his postdoc things, I suppose.

And he published this paper; it was technically rather formidable looking. He brings in some quantum-mechanical formalism and it looks all very impressive, but actually he got it wrong [laughs]. And I realised this. So that was my chance, you see. So I did that problem and got it right. Nobody's questioned it since then, but actually there are a lot of checks, so there's no real doubt that I got it right. So I had the effect of weak horizontal shear. But then I took it further, you see, because the technique used for that, the asymptotic technique... you can get the first effects of weak vertical shear, but then you can get what's called the further corrections, and you can keep on going and refining and refining the results, by calculating more and more terms in the mathematical equation [giving the answer as what's called a 'series' of terms].

It gets more and more complicated. But you can at least see whether, you know, the thing – now it gets technical here – I was going to say converges, but there's a technicality about what you really mean by converging. Let's say, there's an issue about whether you're going to get close to the real answer by taking a certain number of terms, alright [laughs]. And I managed to show that you could get close in that sense, by taking... I think I ended up taking, oh... perhaps some... I think I... I forget how many terms. There was quite a number though. [Eleven, actually – but that was just playing safe; the last few made hardly any difference.] What I had to do at that point was to bring in the computer, and use it to help me calculate the more complicated terms, because they got too cumbersome to write out.

So this was a sort of intimate combination of paper mathematics and computing, and that was what was keeping me up all night in the computer lab in my last year, trying to get this damn thing to work. And I wouldn't... with something so complicated, I would never believe that – ever – unless I had it two independent ways. And eventually I got it, the same answer by two independent routes, and I said, "Eureka, I've got it!" And then I knew I had my thesis in the bag, and I went home and caught up with my sleep [laughs].

How did you know that you were getting close to the real answer?

There are consistency tests. You add a term and you see it doesn't make much difference. And... oh, wait a minute, I've just remembered something about that. I actually did a piece of pure mathematics, which I don't very often, but in this case it was complicated enough I felt I should try and actually *prove* that this thing converges. I was talking about asymptotics. You see, there's a technical thing that... asymptotics in the technical sense is not the same thing as convergence. But actually this thing does converge. That means that if you take [an] infinite number of terms – you keep on taking them forever – it gets closer and closer to some definite answer. That's convergence. And I proved that that was the case with this series. I felt I had to go to those lengths, which is a bit unusual in an applied-maths paper, because the great Joe Pedlosky had published a different result, you see [laughs]. And I was just a young upstart trying to make my name. But that's all on the record, a rather big paper in the *Journal of Fluid Mechanics* [laughs]. [*J. Fluid Mech.* **40**, 273–306 (1970).]

[1:53:13]

Thank you.

I haven't told you why horizontal shear's interesting.

Oh yes, yes.

But we can come to that later. Just remind me to talk about that later. There's a deeply interesting reason why we should want to understand the horizontal shear.

Well, I was just going to ask, at the point that you were doing this, in other words in the final year of your PhD, staying up in the computer laboratory, what was the... now, without wanting to say that, you know, there has to be a particular kind of applied outcome, what was the context? Why was this thought to be an important thing or an interesting thing to study?

To do this thing with horizontal shear?

Yes.

Oh well, that was thing I was going to come to. The reason is that, if you look at the real Earth's atmosphere with its cyclones and anticyclones and so on, and furthermore if you look at computer models of the same thing in aquaplanets, you always get this result that in my time as a postdoc was considered a huge mystery, which is that the jets in the real atmosphere tend to sharpen themselves. And that, you've got to realise, is very different from what an ordinary domestic jet does. If I blow candles out, I've got to have the candles pretty close, because the ordinary jet I blow from my mouth spreads out quite fast. If domestic jets behaved like geophysical jets – these are the atmospheric jet streams, the Gulf Stream, all those great currents in our Earth system – I could blow candles out all the way across the room because the jet hangs together. It keeps itself together; it keeps sharpening itself. It doesn't spread out, you see. And this means there's got to be some sort of... I like to call it *anti-frictional* effect. Some people called it 'negative viscosity' at the time I was a postdoc. And it's on record that it was [then] regarded as a big unsolved mystery.

And... now these instability problems with horizontal shear... many of them showed this same tendency for anti-frictional effects. Let me try and make that a little clearer. The instability makes the flow wavy, and the waviness has a certain structure, and it turns out that the structure is such that momentum – you know what momentum is, it's the motion of the fluid, let's say, the motion of fluid mass – tends to get more concentrated in the centre of the flow. Because whenever something's moving toward the centre, it's moving eastward and when it's... and vice versa, you see. So there's a sort of actual... almost a herringbone pattern of motion like this, that tends to concentrate momentum and keep the jet sharp.

And people were very interested in this, because it had been shown from observations that the atmosphere does it. There's a long history going back to Harold Jeffreys' work [in the 1920s]. But Victor Starr, who was another great luminary at MIT – I met him personally; he was more in the twilight of his career then, I guess – but he'd done work in the '50s taking the newly available observations from upper-air soundings. Because, as a result of the war, people did these upper-air balloon soundings, so for the first time you had some sort of coverage of upper-air motions. And he showed that these motions always had these anti-frictional effects, that the jets were always sharpening themselves.

And the great Edward N. Lorenz, who's another luminary, who was at MIT there – I rubbed shoulders with a lot of famous people! Ed Lorenz is one of the great legends, as the father of chaos theory. And he was beginning to do that work then. And he was a great guy too, one of these people who were after understanding and lucidity – is a most beautiful writer. I'm sure he influenced me, my aspirations. Well, Lorenz wrote a famous book on the general circulation of the atmosphere, and at the end of that he says, there's this strange thing where the eddies concentrate the momentum into the jets, as if you had negative viscosity, which is what Starr called it actually. "Today we haven't the faintest idea how this works" [said Lorenz in his book]. And Starr published a book that had a rather pretentious title, I think, the *Physics of Negative Viscosity Phenomena*. But what that book says is the same thing – that we see this thing happening, and we haven't the faintest idea how [laughs]. No, they didn't claim to understand it at all.

So these instability problems with horizontal shear, they showed the same sort of thing. So a lot of us thought, that's interesting to study because it looks like the beginning of an understanding of negative viscosity. Now there's a further chapter to that story, which I perhaps should talk about later, 'cause there's a lot of stuff in between [resulting in, today, a very clear understanding of how 'negative viscosity' works.]

Okay, yeah. So we'll pick that up later. But we've got to the point where you've been appointed a research assistant, although it's got a different name, in 1969. So we ought to explore your decisions about what you studied. You said you've got limited lecturing.

Yes, let's...

[End of Track 2]

Track 3

Could you say something about the development of your research having moved back to the UK as assistant director of research in 1969?

Okay. Well, my memories of that time aren't very complete or accurate, but I think I spent some time, you know, floundering around a bit, looking for new problems. And I also had this pull toward a musical career. We did have a serious musical enterprise, I think it's fair to say. It was a chamber group, a piano trio. My wife is an absolutely wonderful pianist, and we knew a wonderful cellist so we formed a piano trio and I think we delivered some world-class performances. And I did for a time think of having a second career alongside my scientific career, but I found in the end it was too much. I didn't have the time or the energy to really hold up both. So that was part of the uncertainty at the time. I mean, we did keep the concerts going for several years, and I wouldn't have missed it for worlds. It was an absolutely wonderful time.

[0:01:15]

As regards science, I was, as I say, looking round for some new problem areas. And I think what actually happened, looking back at my publications at the time, was that – being in this environment at Cambridge where all sorts of interesting fluid-dynamical problems were discussed in the weekly seminars – I learnt about some problems of flow of air over mountains, and their laboratory counterparts. And there was an interesting technical issue in the laboratory counterparts.

Okay, what sort of problem is this? You have... This is a problem in which the Earth's rotation is unimportant; everything happens too fast. But you have in the atmosphere a stratified flow over a mountain. It forms waves in the lee of the mountain; this is well known to glider pilots, who use them to gain altitude. In the laboratory, there were various simplified versions of this, and a famous one was due to a man called Robert R. Long, who worked at John Hopkins University; and he did some famous experiments in a long [no pun intended] laboratory tanks [tank: a single tank of length about 6 metres]. And he put a stratified fluid in. It was stratified with salt, I think. And you let it settle down, and then you move a mountain along. So it's like flow past a mountain, except in a different frame of reference. So you get waves behind the mountain that depend on the stable stratification, like the real lee waves in the atmosphere.

But then a technical question arose about whether there was any disturbance ahead of the mountain or not. This was called the problem of upstream influence. And there was a lot of argument about it in these seminars at Cambridge. So I was picking up that this was an interesting piece of fluid dynamics. And then one of the, you know, rather famous fluid dynamists in our group came up with a proof, as he claimed, that there was always such an upstream influence, in this laboratory version of the mountain-wave problem. And that means that something called 'Long's hypothesis' was wrong. Long's hypothesis was something that said there was no upstream influence; and he [Long] wanted this to be true because it led him [permitted him] to do some nice theory of waves of finite amplitude.

But anyway, this famous man – his name was Brooke Benjamin; he died some years ago sadly – he was a very clever creative thinker about fluids, and then rather famous, and he was a Fellow of the Royal Society ages ago, and his name was Brooke Benjamin [as I said]; and he also composed music, although I never heard any of it. But he came up with the claim that he'd proved that you always had upstream influence. And I got absolutely fascinated by this, because his proof didn't give you the slightest clue about how it happened. But there were hints that it happened in a subtle way that wasn't just to do with the mountain hitting the fluid; it was a much more, sort of, indirect thing. And to cut a long story short, I studied this problem intensely and found that, yes, you usually did get upstream influence but not always – so the famous Brooke Benjamin was actually wrong on one point. And the way it happened was extraordinarily interesting because to see... to get it to happen, you had to do the problem of the wave train *developing*. You actually had to consider the time-dependent problem of how the waves started downstream of the mountain, and stretched out for increasingly large distances.

And it turned out that all the action was in the tail of the wave train – where the wave trains ended [further and further downstream of the mountain]. There, there was a significant interaction that sent a signal back to [toward] the mountain, and further upstream in some cases, but not in all cases. So I think I actually sorted out how that problem worked. And it was exciting, because nobody had the faintest clue, before that, how it happened. And I published a paper called "On Long's hypothesis of no upstream influence", etc, etc. That was in 1972 that it came out [again in the *Journal of Fluid Mechanics*].

Sorry, how in practice did you prove that there was this signal going from the point where the waves end, back past the mountain?

Well, it was another example of doing the asymptotics. I based the theory on the wave amplitude being small; so that's a shallow mountain, if you will. And if you do that... if you do that to what we call leading order, if you do the – you know, if you neglect everything that's of smaller order [also called 'linearised theory', or just 'linear theory'] – then you just get the result that the lee waves grow behind the mountain, and extend for longer and longer distances. I mean, this is... you're imagining an infinitely long tank, if you will. But nothing else happens. To get the upstream influence you've got to get the next correction, which is of the order of the square of the wave amplitude, which is much smaller. And at that order there's this interaction in the tail of the lee wave train that sends upstream a long non-wavy disturbance, except that it's a bit like this equatorial undercurrent thing, you see. So the head of it propagates like a wave, but the rest is non-wavy and joins all the way to the back [the tail] of the lee wave train. It's quite a complicated thing to imagine.

And did you do this... I mean, how did you do that calculation?

Oh, well, technically... well, I actually did it two different ways. One was by solving what's called an initial value problem, where you suddenly introduce the mountain, and that gets quite technical. You have to use things called integral transforms. But I also did it by a method that makes things simpler, in which you introduce the mountain gradually. And then you've got what's called a separation of timescales. There's the timescale for growing your mountain, and you can make that a lot longer than the time for a particle to go through one lee wave. So there are two timescales in the problem. Now if you make that your small parameter – the short time of the lee wave divided by the long time of growing the mountain – then you get significant simplifications. And I ended up doing most of it that way, 'cause that was enough to get the essential result. As I recall, I showed [that] you get essentially the same result anyway. So I had it two ways.

[0:08:09]

And how at this time did you organise home and work life, if you like? You were presumably living in Cambridge and working in the Department? I don't know if that's the right way to put it. But how did home and work relate to each other? How did you organise your day, if you... ?

Well, when I think about it, I must... it boggles the mind how I managed to do everything I did, because I had these three stepchildren at home and I had to try and, you know, give them some attention and my wife some attention. And we still had this secondary career in music as well, so I had to make sure I did a certain amount of violin practice every evening, especially if a concert was approaching [laughs]. And I think I still had my college research fellowship or... At some point around then, I switched to a teaching fellowship. So for a while I was doing college supervisions as well. And the answer to your question is, I haven't a clue how I managed all that. I mean, young people have a lot more energy than us oldies [laughs]. I couldn't do it now, for sure.

And what was your wife doing while you were... ?

Well, she was looking after the kids and the home, and practising the piano for our concerts, and doing quite a bit of piano teaching as well. [And doing all the shopping.] So again, that boggles the mind. I don't know how she did it either.

[0:09:31]

And then after the mountain jet work, what was next in your research?

Okay. Well, this mountain – this sort of mountain lee-wave problem – this got me interested in these subtle effects when waves of a certain amplitude produce very different effects at the next order, as I've described. And I realised that there was a lot of interesting and subtle stuff going on. And I think it must have been around then that I remembered that when I was still a research student Francis Bretherton had another student, called Chris Garrett – who's now a famous theoretical oceanographer – but his PhD thesis was almost contemporary with mine; he was working on these subtle wave effects. So I remembered that. And I also ran into a paper of Francis himself, where he had discussed some aspects of how mountain lee waves would have systematic mean effects. And my lee-wave tail effect was actually a slight variation on that theme. It was really a bit different but it was another example of what we call second-order mean effects of waves.

And... to cut a long story short, it began gradually to dawn on me that effects of this sort might really be significant in the atmosphere. I think I owe this to Francis. Francis recognised it. That's why he worked on the problem, because... alright, mountain lee waves... some of them go upwards as well as downstream, so some of them penetrate to great heights. And there *are* systematic mean effects. And, in a nutshell, what it... what they mean is that the atmosphere can feel a systematic force at great heights where those waves dissipate, even though the force is being exerted by the mountains. So you exert a force in one place, and the atmosphere –at least as far as its mean motion is concerned – the atmosphere feels it in another place, which might be much higher up. And it turns out that that's often *very* important for understanding even the gross features of the circulation. By the time you get up to, you know, fifty or a hundred kilometres, these effects can be very important.

And I think that's probably what began to get me interested in the upper atmosphere, meaning the stratosphere and, you know, those sorts of higher altitudes, as well as the wave problems. And in fact, *my* first research student, who was a marvellous guy called Adrian Simmons, who's now a luminary in the numerical weather forecasting business – he's a leader at the European Centre for Medium Range Weather Forecasts – and I got him to work on problems of large-scale waves in the stratosphere, not mountain lee waves but something called Rossby waves, because while I was still at MIT I'd learnt of some work by... well [backtracking], there was a pioneering paper by Jule Charney again – who'd pioneered a lot of things – he and a man called Philip Drazin wrote a famous paper on these large-scale Rossby waves that go up into the stratosphere. I'll have to explain what those are later, I think. But, you know, if you think of the planetary scale, if you think of a wave whose wavelength is, you know, halfway round the Earth or something like that, that's... and... um... [getting back to the main point] then a guy at MIT who was... I think he was still a student of Victor Starr's, or he might have been a postdoc, very smart guy... His name was Bob – he's still alive; he's still doing interesting work – his name is Bob Dickinson. And he was interested in this kind of problem. And he wrote a paper called, what was it, "Planetary-scale Rossby waves propagating in [vertically through] weak westerly [wind] waveguides".

And he was interested in how these waves might get into the real stratosphere in... taking a step further than Charney and Drazin, in making more realistic models of these waves getting up. Because it was beginning to be clear that understanding this was very important for understanding things you observe about the stratosphere, in particular the difference between the winter stratosphere and the summer stratosphere. [The winter stratosphere is highly disturbed – nothing like a simple flow around latitude circles – whereas the summer stratosphere is remarkably *undisturbed* and close to being around latitude circles, at altitudes roughly 20–40 kilometres.]

This, by the way, is the thing that Fred Hoyle got wrong. He didn't know there was any such difference. But anyway, the winter stratosphere is much more disturbed, and even today we still... it's still clear that that's because of these planetary-scale Rossby waves, even though we [now] know far more about how they work in detail. And that I'll come to later.

So anyway, there was this paper by Dickinson. It turned out that Dickinson had sort of done the problem half right, but not quite. So I set Adrian Simmons on to this problem, and he did a more accurate version of Dickinson's work, and got us a bit closer to realistic models of these Rossby waves in the stratosphere. And by the way, Adrian was an absolutely ideal research student. Here was I, as a naïve young Assistant Director of Research, as I was then, taking on *my* first research student. And I would suggest a problem, and Adrian would go away, and come back a week later with a neatly worked out answer to whatever the question was. And he was very self-contained, and organised himself very well, and it went that way throughout his [PhD] work.

And when I got my next research student, whose name I forget [laughs], it was a terrible shock 'cause I thought that all Cambridge research students were like Adrian Simmons, and would all just perfectly do whatever you suggested [laughs]. As I said, Adrian is now a great luminary in numerical weather forecasting. He was very much involved in developing the models on which today's weather forecasts depend, which are, I think everybody would agree, not perfect [inevitably, thanks to the Ed Lorenz 'butterfly effect'], but very, very much better than they used to be when Adrian and I were young. So that's another great scientific and technical story.

And so you directed him in work on the Rossby waves?

Yeah, he did these stratospheric Rossby waves. So that must have been, I think, the real beginning of my interest in the stratosphere. I did publish a little paper [also apropos of the stratosphere] on this thing that Jule Charney actually got wrong [laughs]. It turned out not to be the most important aspect, but there was another instability problem that some people thought was important for the stratosphere. And there was a paper by Charney and Stern that's very famous, published in 1962, that said that this stability problem... this instability didn't work. He thought he'd proved that this particular shear flow was stable, and I realised that was wrong because there was a sign error in the... I could do this because I'd studied these shear instability problems enough, by then, to have a pretty good grasp of how they worked. I could see it was wrong, so I published a little paper with the correct solution in it, in the *Quarterly Journal of the Royal Meteorological Society*. That came out in '70... when was it? '71, I think, was it? Or was it '72? Yeah, it was '72 – okay – which was actually the same year as the paper I published on the upstream-influence effect of mountain lee waves. So I had these two different things I was interested in [– the lee-wave problem, and an embryonic interest in the stratosphere as such. I should add that the 1962 Charney and Stern paper was correct, and interesting, in many other ways!]

But I think the bottom line here is that I was beginning to be more and more interested in the mean effects of waves. Not just the waves themselves, as in Adrian Simmons' thesis, but actually what systematic mean effects they might have. And just to cut... fast forward for a moment, it turns out that these big Rossby waves are absolutely *critical* to understanding practically everything about the stratospheric circulation – such as how the ozone moves around, and all the other chemicals, how the ozone hole forms – all that story. Those Rossby waves are basic to that. But you have to take it further, and understand their systematic mean effects. Now I didn't really understand all that then, but I was beginning to be fascinated by the systematic mean effects, because they were *mathematically* interesting.

[0:17:56]

Could you just define systematic mean effect? What is a... what would be the systematic mean effect of a wave?

That's a good question. You know the... okay. It's a little subtle how that arises in the lee-wave problem, but you remember I said that the tails of the lee waves send this non-wavy disturbance. So by comparison to the waves, that's sort of a mean – a change in the mean flow, this upstream-influence thing. So that's one clue. But perhaps a simpler illustration is something that anyone can do in the kitchen sink, which is, you put a thin layer of water in the kitchen sink, and you sprinkle some dry tea leaves on it and you jiggle with a little wavemaker [a fat highlighter pen would do] at about five [cycles] per second, so you make short waves on the surface. And it's very easy to show that, when you do that, you generate a strong mean flow. The tea leaves move away from the wavemaker. And it's very conspicuous, and very robust.

I do this in lectures quite often and I get the audience's attention by saying, "This experimental demonstration *never fails*," I say. And I can see the ears pricking up. And then I do it, and it doesn't fail, 'cause it *is* very robust. And what's happening is that the waves, which are, at leading order... if you do the linear theory, the leading-order theory [for small wave amplitude], you just get that the fluid particles jiggle back and forth – this is what you read in any physics textbook – but if you do it to the next order you see, actually, there's a systematic mean motion. And some of that's due to the waves dissipating, and some of it's due to something called the 'Stokes drift', which is a technicality which I don't think we need to bother with. What's more important is that the dissipation of the waves gives a systematic mean effect, which is a lot of this... part of this jet that goes away from the wavemaker. Just in case someone knows about Stokes drifts, let me add that if you stop the wavemaker the mean flow carries on. That proves it's not [entirely] a Stokes drift. A Stokes drift is a temporary mean flow that depends on the waves being present.

So this – in the example then of the tea on the surface of the water, you've got a jet which is moving the little leaves of tea...

Yes.

… which shows that the waves aren't simply sort of... when you do the waves with a wavemaker, it's not just simply circling and producing no general motion in a particular direction.

Yeah, the particles aren't... they are jiggling, but they're also drifting along, carrying the tea leaves with them, so there's a systematic mean flow. That's what I mean.

And it's quite fast in relation to the... it seems quite... when you look at the demonstration that you showed me, they seem to move quite quickly, away...

Yes. When you do the theory it looks like a small [second-order] correction, but when you do the experiment you're actually using quite a large wave amplitude, so actually that correction is quite significant. And really to do it accurately you need higher corrections. But the second order is enough to show you the qualitative nature of the effect. And there's a beautiful, you know, general theory of this, which is something I worked on quite hard in the following years.

[End of Track 3]

Track 4

At the end of the last session we'd got to the point where you'd been working outside of science to some extent, in professional music, as well as being employed to research in the Department of Mathematics at Cambridge. And we'd considered some work on air moving past a mountain. And I wondered whether you could continue to describe what it is you were focusing on in your research, at this time.

Yes. Well, to be honest, at that time I wasn't completely focused on one thing. There's a slight digression – let me mention – I was working on musical acoustics with a wonderful research student called Jim Woodhouse, who still works on that, and is [now] a Professor in the Engineering Department here. But I'd better not get off on that, because it'll be a distraction.

Certainly one of my interests were these... what I think we'll have to call 'second-order mean effects' of waves. Remember, 'order' in this technical jargon means [refers to] the wave amplitude. So a first-order theory [a linear theory] is the sort you see in physics textbooks, where you assume the wave amplitude is small, and don't try to compute any corrections. And in that sort of theory, if you have water waves, for example, the theory says that the particles of water go round in little circles, or ellipses, and... just... closed orbits. And the water doesn't move anywhere, on average.

And if you do the second-order correction, which means corrections of the order of the square of the wave amplitude – this is a standard mathematical technique you can use to get a refinement of the theory – well, I was interested in the fact that, when you do that, you get all sorts of new and interesting effects, one of which is this mean flow that you see created by the wavemaker in the kitchen sink. And I've often demonstrated that in lectures, and [as I said last time] I always say to the audience, "This experimental demonstration never fails. It's very robust." And I always get their attention 'cause everybody knows experiments done in lectures can often go wrong. But it does never fail, because it *is* robust, and it's easy for anyone to do. And I recommend anyone interested to try it because... I always think one of the most wonderful things about science is that some things are things that can be verified by anyone.

So, okay, I'm talking about the mean flows generated by waves. Remember, the textbooks say, the particles go round in little circles or ellipses. The second-order correction refines that and says, no, no, the particles actually drift along. They're still going round in almost little circles, but they drift along. And now come the subtleties. There is a... more than one sort of second-order mean effects, that give rise to mean currents. And some of them depend on the waves being there. There's a piece of technical jargon, they're called Stokes drifts. But there's another component that adds to the Stokes drift that depends on the waves dissipating, and that's actually of greater interest because, as long as the waves are dissipating, this effect is, as it were, cumulative. If I do a thought-experiment where I start sending waves in somewhere – and they dissipate somewhere else – where the waves are dissipating there is a systematic force being exerted that depends on the wave dissipation and, if nothing else is happening, the fluid there accelerates. There's a growing mean flow.

So even if the wave amplitude is small, if you wait long enough, you can still get a significant mean flow. So we focus attention on that kind of thing as a highly significant kind of second-order mean effect, and it is important, we now know, for understanding practically everything about large-scale circulations in the atmosphere, and almost certainly the oceans as well, although it's far less well developed for the oceans. That's an active research area. So I was beginning to see that point in the early '70s, and getting interested in these problems, following the lead of my erstwhile supervisor, Francis Bretherton, who had previously seen the point about these problems and started working on them.

And so what I did was... take it up where he left off, and [working with a student of mine, David Andrews] develop a general theoretical framework that proved to be very powerful in understanding these cumulative mean effects of waves. It's got its own jargon. It's nowadays called the 'generalised Lagrangian-mean theory' of wave mean flow interaction. Sorry, that's a terrible mouthful.

[0:04:51]

But it's very interesting for a theoretician like me because, to get the powerful results and see what's general – and what's special to a particular problem – you need to use what we call the Lagrangian description of fluid motion. Okay, this is a short tutorial. This is standard fluid mechanics [laughs].

If you... there are two... essentially two basic ways of describing fluid motion. One is what a physicist would call field theory [and fluid dynamicists call it the Eulerian description]. You watch the fluid going past, you focus attention on one point at a time, and at each point in the fluid there's a pressure and a density and a velocity and a temperature, and so on. And you describe everything in terms of what all those things are *at each point*. And that's what's called field theory – just how you do electromagnetism, where you have a magnetic field at a certain point, and so on.

But in fluids there's another natural way to describe things, which is to follow individual particles [and it's called the Lagrangian description], and in these wave problems, of course, the particles are jiggling back and forth as well as possibly drifting systematically. So now there's a nontrivial problem. You want to capture the jiggling part in a Lagrangian way – it turns out that if you follow the fluid particles, all sorts of theoretical results become far, far simpler. I can give you some specific examples where, to do a particular theory [a calculation for a particular case of wave–mean interaction], it might take, you know, several dozen pages of complicated equations to do it from the Eulerian perspective, but only half a page to do it from the sort of Lagrangian perspective that we developed. [My favourite example is what's called the Craik–Leibovich theory of Langmuir circulations.]

We at first... was... well, Francis Bretherton really began it, but he didn't take it very far, and I and my student David Andrews took it a lot further and reached this systematic general theoretical framework, this so-called generalised Lagrangian-mean theory. And the trick was to describe the waves in a Lagrangian way, but the mean flow in an Eulerian way, so you could then have a picture, and a set of equations that agreed with that picture, in which the... you could watch a point and its neighbourhood and see the fluid particles oscillating, but also drifting past. So you could talk about the wave displacement, which is a Lagrangian quantity [describing the jiggling back and forth], but do this trick of describing the displacement, and the mean flow, 'at' a particular point as well. So technically it's what we call a hybrid, Eulerian–Lagrangian, description. And as I say, this proved very powerful, for various reasons.

This is going to get awfully technical but, let me just mention, the most basic [reason] is something called Kelvin's circulation theorem. And it's important because if you have a wave motion that isn't dissipating, there's all sorts of things that can't happen – you know, you can't get some of these mean effects that I was talking about. And the reason is this thing called Kelvin's circulation theorem. And what it says is that if you take a... not just a particle, but a line of particles in the fluid [a so-called 'material contour'], which would simply undulate according to a linear wave theory, there's a quantity you can compute, which is something like a measure of the mean velocity along that line. So you're taking an average along a wiggly line, you see. And that quantity has to be a constant [for non-dissipative fluid motion]. This was proved by Lord Kelvin back in the nineteenth century, and it's basic to a whole lot of other fluid problems as well. So the hybrid Eulerian–Lagrangian description captured the effects of Kelvin's circulation theorem in a rather neat and simple way, and that's why it was powerful in saying whether you get, you know, cumulative effects from wave dissipation or not.

And by the way, it also... it did something else that's very fundamental. It told you how to think of wave breaking, in a fundamental way that applies to all cases. If you think of breaking waves... obviously if waves break, as on an ocean beach, that's a case of how waves dissipate. And so you could sort of guess that that's going to be significant for mean-flow problems. But it turns out that the most fundamental and penetrating way to describe wave breaking is to consider the material contours that would simply undulate in [according to] a linear wave theory, and decide whether or not they deform irreversibly [in nonlinear reality].

If they're deforming irreversibly – getting twisted up like spaghetti on a fork, or churned around in a horribly complicated way by the turbulence in the wave breaking – if that's happening, Kelvin's circulation theorem, or at least its simple application, fails. That's one way to see *when* you're going to get significant dissipative mean flow effects. So... together with a colleague of mine, Tim Palmer, who is actually now a luminary, and he's a big name, in numerical weather prediction – he helped to pioneer the ensemble prediction that allows us to assess the uncertainty of weather [forecasts] – he's a very smart guy, he's done all sorts of interesting things, including some fascinating work on the foundations of quantum mechanics. Well, in those days when Tim and I were young, we worked together on this idea of wave breaking as applied to Rossby waves. This is jumping ahead, now, to the Rossby waves in the stratosphere.

And perhaps I should come back to that, 'cause it's a great scientific story, but the bottom line is we used this idea, that Kelvin's circulation theorem, and generalised Lagrangian-mean theory, tells you how to decide whether a wave is breaking or not. And that criterion applies to *all* the sorts of waves that are important in the atmosphere and ocean, so it's quite important.

Yes, so we'll come back to that when that happens, when this work with Tim Palmer happens.

Yes.

[0:10:57]

And so – but in terms of your work at the moment, we've got up to working on this generalised Lagrangian-mean theory with David Andrews...

Yes.

And I was wondering, when you said that this was another way to see waves, I wondered to what extent you were experimenting in a laboratory or elsewhere with fluids and liquids.

Well, okay. You must remember, I'm just a humble theoretician and I don't... I'm not really a professional experimenter. But on the other hand, I *am* a scientifically-minded person, so I like to do little experiments of my own, including this kitchen sink thing, and the little things I do in lectures, which are actually a wonderful way to make something vivid and more understandable. So... I'm always doing thought-experiments. That's a terribly important thing for a theoretician to do, because part of our job, it always seems to me, is not just to advance understanding but to try and discern what's robust about a simplified theory. And you've got to do thought-experiments to see that kind of point.

I mean, okay, I was talking about pictures. I was beginning to talk about equations, I think. And let me just say, I would argue that understanding a problem – you know, practically any scientific problem – should consist in seeing it from as many angles as possible: from equations, from pictures, from feelings, from doing thought-experiments and real experiments. It's just a child playing, poking things to see how they work. All good science is like that. And *I* say this, but I'm in good company because people like Richard Feynman have said it. And if anyone's interested, there's an absolutely wonderful description of this in James Gleick's book on Feynman. It's called *Genius,* and there's a wonderful passage in there where he describes Feynman trying to understand something. Feynman himself talks about this quite a bit in his popular lectures. But it's exactly this thing of seeing it from several angles, trying to make mental pictures, trying to see it from the equations, and so on. Einstein always talked about feeling things, as well as calculating them and visualising them.

[0:13:13]

If we could sort of hover above you then unknown in the late '60s and early '70s when you're developing generalised Lagrangian-mean theory, what would we see you doing?

Oh, you'd be bored to tears. What you'd see would be me sort of scribbling lots of equations on papers, and banging my head and... uttering grunts of frustration. Because technically this theory was quite complicated. We had to get – this is both David Andrews and I – we had to get our head around quite a lot of at first unfamiliar mathematics. It grew out of, as I said, the Lagrangian description. That's a classic and well known thing. But in fact it was an effort to get on top of it, because it wasn't very often used. I think that's one reason why what we did was a significant bit of pioneering, because we were applying this to a... in a somewhat new area, as I say, a hybrid of Eulerian and Lagrangian.

So we had to get our heads around how to say that mathematically, which isn't trivial. I mean, now you can get beautiful... or at least one beautiful book about it, by my ex-student Oliver Bühler. There's a... it *is* a beautiful book. It's called *Waves and Mean Flows*. It's a book that... well, I said at first, the book I meant to write. For years I planned to write a book on this but never got round to finishing it; and as Oliver's book developed I first said, "Ah, this is the book I wished I'd written." But when he got to the end it was, "Ah, this is the book I wish I *could* have written," because Oliver had a great flair for putting things in a vivid and witty and interesting way that I don't have myself. So if anyone is really interested in this topic I recommend his book, *Waves and Mean Flows*. It was published in 2009, if I recall correctly [laughs]. So you could say the subject has come to a certain maturity now, and it's sort of... a number of people do understand how to use the theory, and it is being used on new problems now, but it took a long time.

[0:15:17]

And as well as seeing you sitting with bits of paper, banging your head, were there other places where you went to do this work or... ?

Well, that's an interesting question. Some of it I did on a sidewalk café in Nice. I was already getting interested in solar astrophysics – although none of that came to fruition till a long time later – but I did get pulled into one or two conferences. And at the Observatoire de Nice they had a beautiful library where they held a conference one year that I was at, something about the Sun's vibrations. That's another great scientific story, by the way, which I may... I'd like to say a bit about that later on.

But we loved Nice, you know, the south of France, all the sunshine and the wonderful food and the frothy coffee and everything – the beautiful buildings. There's a wonderful museum. It's called the Chagall Museum [the home of Marc Chagall's *Message Biblique* paintings]. It's got Chagall stained glass [a whole sidewall made of blueish stained glass] in a little concert room. We played a concert in there once, by the way. They had a beautiful piano. In fact I think it was part of this conference. We got engaged – we were still... still had our professional piano trio – and we were engaged to play a concert for the conference in the little concert room in the Chagall Museum. That was a wonderful occasion [laughs].

But anyway, so... you asked where would I work, you see, and I know one of the ideas I had that popped up and helped us get our heads round this theory did occur to me on a sidewalk café in Nice. It was something called the 'transformed Eulerian mean', which actually grew into another important bit of theory that's widely used, which I don't think I'm even... going to even try to describe. It was just a mathematical... a rather simple mathematical trick that has proved to be very useful.

How do you know that it was there that this occurred to you in your memory? When you say that it occurred... that this developed...

Well, that's my memory. Memory can play tricks. But I do remember sitting in this café and having a eureka moment and saying, oh gosh, let's make this transformation, that'll simplify everything in a very nice way. And it did [laughs].

Who were you with?

I was by myself.

And were you sitting thinking or actually working with... ?

Oh, I had this nice cup of big frothy coffee, a grand café au lait [or grand café crème], or whatever they call it [laughs], and sort of mulling over some theoretical puzzles. This is... it always works this way. You struggle with something, and you bang your head, and you get stuck, and then you've got to take a break. And you might be doing something completely different like sitting on a sidewalk café, or in the shower, or the loo, or whatever it is, and suddenly an idea pops up. I think anyone who does creative work has this experience, a sort of ping-pong between the conscious and the unconscious. You've got to have the frustration of getting stuck, to stimulate the unconscious to do something. And that's, I think, what happened on this occasion; it happened with something else as well in the GLM, the generalised Lagr... can I call it GLM theory?

Yes, referring to generalised Lagrarian-mean theory.

Lagrangian-mean, yes, generalised Lagrangian-mean. I'm going to call it GLM, probably, from now on.

GLM, yeah.

And there was a little thing slotted into place in that, too [GLM], when I was... I don't know whether it was the same session on the sidewalk café, but I just have this memory of a nice Nice sunshine and the pleasant weather [and frothy coffee!].

So... you know... If you're interested in this sort of thing [creativity, innovation], there's a lot of wonderful stuff about it in Littlewood's *Mathematician's Miscellany*. The great mathematical analyst John E Littlewood wrote this little book of snippets of witty observations on this and that, but he's got some... a section called *The Mathematician's Life of Work*, I think [actually *The Mathematician's Art of Work* – in the Bollobás edition only], and he talks about how different individuals work well in different situations. Littlewood says, I work best when going for a walk, you know. [That's only part of it; he would also work "all out" on paper at a desk.] It's the same thing though; you struggle with something and get stuck, and then you go for a walk. And he's got one story where he recalls he almost heard his subconscious *shouting* at him, try this, you fool [laughs]. And, he says, the tension, until I could get back and verify – 'cause he had to get it on paper and really see that it worked – the tension was nevertheless... considerable, before I could get back and verify, he says. This, by the way, has been republished under the editorship of a man called Bollobás, Béla Bollobás. He's a pure mathematician here [at Cambridge] who published Littlewood's... the original thing was out of print long ago, and this is a republication with extra bits added, including a picture of Darwin playing his – this is Erasmus Darwin – playing the trombone to his tulips [what he called a 'damn-fool experiment', something that a scientist should try every so often]. Might come to that later [laughs].

[On checking the Bollobás edition of the *Miscellany* I find that my memory of the story just recounted, while correct in essence, draws on two stories from different chapters. In *The Mathematician's Art of Work* Littlewood writes, "I had a sense that my subconscious was saying, 'Are you *never* going to do it, confound you; try this'." And in a separate but similar story at the end of the chapter entitled *A Mathematical Education*, "... a flooding certainty came into my mind that the thing was done. The 40 minutes before I got back and could verify were none the less tense."]

When you try to... one of the things that you say when you're thinking about what it really means to understand something, and thinking about Feynman as well, is that you try to... you said that you try to see things and calculate them and feel them, do you... does that involve... doing... understanding in different ways like that – does that involve doing different things?

It does sort of, although most of it... for a theoretician, mostly the 'doing' is in one's head – thought-experiments, if you will. You say, okay, I've just got an equation, what does this *mean*? It means that if I do this, something else will happen. So you try to understand the implication of the equation. You know, that's different from just reading the terms in the equation, and checking that the signs are right, and so on.

[0:20:50]

Thank you. So having worked out GLM, where did that take you in terms of your work next? How did you use this GLM to do something else, to move things... understanding on?

Yes, okay. Well, of course, having developed this general framework, I was naturally interested in how it applied in particular cases. And, gosh, here's where memory is slightly letting me down. I'm sure what I must have done was sort of look around all over the place, and see if I could spot any interesting problems [beyond those I'd already encountered, such as mountain lee waves and stratospheric Rossby waves]. I know I thought about water waves, ordinary surface gravity waves, the sort that come into ocean beaches, 'cause they're quite interesting in many ways. And the standard theories of those, at the time, tended to neglect the sort of mean effects from dissipating waves that our theory gave us a better handle on. So I did think a bit about those.

I think I published – well, I published at least one paper on water waves. The theory, for instance, told you that, if you build up waves on the surface of the ocean, the mean surface height changes. This was of interest to people doing radar altimetry of the sea surface. I've lost track of the subject a bit, so I'm not sure whether that proved really important. But I have a little paper called *On the Divergence Effect...* [Full title in gory detail: *A note on the divergence effect and the Lagrangian-mean surface elevation in water waves*]. This was more from a theoretician's viewpoint, about... Oh gosh, I'd better talk technical for a moment. I don't think it's overwhelmingly important, but divergence means... okay, if you think of the flow of a gas, it can expand and get less dense, so the velocity can, as it were, go outwards from a particular particle. That's what we call divergence. It has a precise mathematical meaning. However, if the fluid is not a gas but more like water, it's more nearly incompressible, so it's very hard for it to be divergent in that sense. And we call this 'incompressible flow', and we often assume that it's exactly incompressible. That's quite an accurate idealisation for things like water waves.

And the Lagrangian-mean, the GLM, theory tells you that, even though the actual flow is incompressible, and free of divergence, the mean flow can nevertheless be divergent, which is a bit of a surprise. It surprised, you know, people... actually, you're going to interview Michael Longuet-Higgins. It surprised him like anything [laughs]. But anyway... so, as a result, I published a little paper showing that that was the case. It was easy enough to see from the [GLM] equations. And, well, that's about all.

[0:23:37]

In deciding to think about, for example, waves breaking on beaches, was it necessary to go to beaches, or was the choice of that as something to apply GLM to inspired by being at a beach, or seeing a beach to any extent?

Well, I've always used the image of waves breaking on a beach as a kind of conceptual peg to hang quite a lot of other things on, 'cause they're such... they're so visible, these ocean waves. Everyone has a mental picture of those. And, okay, here's an example. There was a huge number of papers written on something called critical-layer theory, and that's about... mostly about the mountain sort of waves that we were talking about before – the waves whose... that owe their existence to the fluid being stably stratified, the gradient of... technically it's something called potential density. Never mind: heavy fluid below light fluid, with a continuous gradient. And so that [the fluid] wants to lie on flat [horizontal] surfaces; so if you disturb it there's a wave motion. We call them 'internal gravity waves'.

Now... if you send these gravity waves into a shear flow – remember, we talked about vertical shear – well, if you have such a shear flow, and if it happens that the flow speed at a certain altitude agrees with the horizontal phase speed of the waves... What does that mean? It means, if you take a horizontal slice through the fluid, you'll see moving wave crests, and that that [the crests'] velocity is what I mean by the 'horizontal phase speed'... the [there's a] technical issue about defining that carefully. Now if that agrees with the fluid flow speed at a certain altitude, we have what's called a critical level. And if you do the linear wave theory in that case – and as a matter of fact, my supervisor, Francis Bretherton, wrote one of the important papers on that – if you do that linear theory, you get this mysterious-sounding result that the waves seem to be absorbed at the critical layer, without having to dissipate them.

But actually that's a deceptive thing, because the theory, if you look more carefully, predicts its own breakdown – which is often one of the most interesting things about a theoretician's work, noticing this. And what it's telling you is that the waves are actually going to break *before* they get to the critical layer – yes? And you can see this if you go into the detail of the wave dynamics. So it isn't so mysterious any more.

Two points, one is, the waves break, and therefore they really do dissipate, even though you thought at first the theory wasn't allowing for that. And second, the waves don't actually reach the critical layer; they break *before* they get there. And still, in the literature, there's a myth that keeps persisting, that talks about waves being absorbed 'at' critical layers [critical *levels*, I should have said], 'cause people haven't looked any further than the simplistic linear theory [laughs]. But, you see, the ocean-beach image is helpful in understanding that, because the way you get the waves apparently being absorbed at the critical layer is, you assume the amplitude is very small; it's a first-order theory. But for it to really apply accurately, the wave [amplitude]... it has to be *microscopically* small. The smallness requirement is very, very severe. So it's like the ocean beach where you had *tiny* waves that don't break until they just about reach your toes, at the water's edge. That's what the critical-layer [theory] is talking about. And now you understand: in the real world, that's not very interesting. In the real world, you've got a whacking great surf zone of violently breaking waves, much further out. And similarly, for gravity-wave critical layers, you've got violently breaking waves *before* they get to the critical layer [laughs].

[0:27:27]

I see. Yes, okay. So having done a bit of applying it in that way, where else did you apply GLM?

Well, one of the things that... here's where it starts to kind of spread out a bit, because, you know, some other people were interested, and I'm beginning to lose track of exactly who was working on what. But the sort of thing that we, you know, gradually started to understand was... well, let's take this gravity-wave case with the waves breaking, before they get to critical layer, or for some other reason. There are other reasons why they can break, such as the density falling off. And so the thought-experiment is, send the waves up... and this, by the way, is a *useful* thought-experiment for understanding the effects of real mountain waves that exert real forces higher up in the real atmosphere. And this is actually built into weather-forecasting models these days, and has a significant effect. So it *is* important to understand it.

So think of a... do a thought-experiment where we idealise a bit. We... well, the simplest version of this is take an infinite row of mountains in an infinite channel, so you get waves that just undulate in the downwind direction, or the... I shouldn't say downwind, I should... it's actually upwind technically. It's in the... along the channel [upwind *and* downwind]. You've got undulations, and a simple mathematical model – it's just a sine function – so you've got sinusoidal mountains extending from, let's say, [from] x is minus infinity to x [is] plus infinity [laughs]. And you turn on these mountains... you do a thought-experiment where you start this going with no waves in the fluid, and the waves propagate into the fluid and propagate upwards. And if there's a critical layer, or a decrease in density, they'll break at some point.

Now the GLM theory tells you the following thing, that... well, let's say two things. It says that, where the waves are breaking – if you keep sending them up persistently – the mean flow will accelerate cumulatively. It'll be a cumulative change. This is what I was saying before, that the net change in the mean flow can become significant if you wait long enough, even if the waves are of small amplitude – small but finite amplitude – so they are still breaking. So you've got this cumulative... [cumulatively-changed] mean flow. And of course that in turn can refract the waves, and you get interesting interactions, an interplay between the waves and the mean flow.

And the GLM theory... okay, now here comes a technical difficulty. Breaking waves are incredibly complicated fluid motions. They're turbulent fluid motions in one sense or another. And this means that we don't have an accurate theory to describe the details. The best you can hope to do is a computer simulation. And computers, even today, are not powerful enough to simulate motions like that accurately. So it's really important to be able to say something about the problem independently of those details. And GLM allows you to say the following thing, that when the waves... think of the layer where the waves are breaking. Beneath that layer, the waves are not breaking, and indeed the linear wave theory is a good first approximation to understanding the rate at which momentum is being fed into the wave-breaking layer. So that tells you the... at least the average rate at which the air is accelerating in that layer.

So the theory tells you, you can compute that quite independently of the wave-breaking details. That's an important insight. And most of our practical, you know, computer codes that try and put this into weather forecasting models make assumptions of that sort. They use a linear wave theory, then they say they [the waves] break at a certain level, and there, in some... there's some... range of altitudes there, where the waves are breaking, where the mean flow feels an accelerating force.

By the way, that's not the end of the story, because if you don't have such a simplified experiment, and you have a more realistic three-dimensional situation, there are very subtle extra effects. We call them 'remote recoil' effects, and note, they were only recently understood. They're in Oliver Bühler's book, though, if anyone's interested [laughs]. Um... but, gosh, they're really quite hard to describe in a few words. So that's one example.

[31:55]

Let me tell you another example where this... all this was important. And actually David Andrews and I were working on this at the same time as developing the generalised [GLM] framework. And that's something that's called the quasi-biennial oscillation of the zonal winds in the equatorial stratosphere. QBO for short. And that's another great bit of science. It was... gosh, I think it's a great story. Why don't I try and tell it in a nutshell?

Yes. I wonder... but just before you do, would you be able to say why it is at this time... you are... working in this... you're looking at the stratosphere? Why did you not... why are you not looking at, you know, this part of the... this other part of the atmosphere, or looking at this or working... looking at the oceans. Why is it that at this point in your career, which, I suppose we're now talking – we're in the '70s – why is it the stratosphere that you are looking at?

Well, the short answer is scientific opportunism. Scientists who do anything significant have to be opportunistic, because most problems are much too hard to solve. This is why the political–bureaucratic imperative, to dictate to scientists what they should work on, doesn't work, usually – because what the politicians would like us to solve is usually too difficult to solve. But what really happens is incremental progress, where you spot something where you *can* make progress. That's opportunism.

Well, this QBO thing was a good example. And I wasn't the only one involved, because quite a lot of progress had already been made. But it was one of the great problems of the time, and I was aware that theories of wave mean flow interaction, of which GLM is... was an example, did have something to say about that sort of problem. So there it was. In fact, as I say, we were actually working on that, already, during David's thesis. And I suppose if you'd asked me at the time I'd have said, well, one of the points of developing GLM is to get a better way of doing the theory of the QBO. And now, shall I take a few minutes to describe... ?

Yes. This is the quasi-biennial oscillation of the...

zonal winds in the equatorial stratosphere. Zonal means east or... eastward or westward. And it's a bit like that other picture I was showing about zonal winds in the stratosphere that aren't at the equator. But the thing about the equator – this is the same point that came up with this little thought-experiment of pushing from the western edge of the Pacific, remember, and sending an undercurrent across the Pacific – the equator is dynamically special because the Coriolis parameter vanishes, the vertical component of the Earth's rotation vector [multiplied by 2, to be technically correct]. Or to put it another way, the Coriolis effect changes sign when you go across the equator.

Now one of the outcomes of this is that flow in some band around the equator – in practice this is a, you know, ten or fifteen... plus or minus ten or fifteen degrees of latitude – that flow is a bit like flow in a channel. And if you have a second-order wave-induced acceleration of the kind I've been talking about, you can actually accelerate that, so that it first... so that it goes, you know, more toward the west, or more toward the east, or whatever.

And now comes the really interesting thing. By the way, I'm jumping ahead in the story, but let me just continue this point. We now know – we have very high confidence, I should say – that the QBO is a wave-driven mean flow. What we observe is that it goes first toward the west, and then toward the east, and it takes about thirteen months for the flow to reverse. And there's a certain pattern in which it reverses earlier at higher altitudes. It's quite characteristic.

And, well, the *end* of the scientific story is, there is only one credible explanation for this. And that *is* second-order wave-induced mean effects. And the waves involved... there's still some uncertainty about exactly which set of waves are involved, but we're pretty confident that some of them [especially what are called 'equatorial Kelvin waves'] are definitely involved. And then there's a bit of a grey area, because we don't have good enough observations to pin down all the waves. Some of these waves are gravity-wave-like things, they've relatively high frequencies. It's rather expensive to observe them in detail. Progress is being made on this. I mean, in ten years' time, somebody might be able to get up and say, oh yes, we do know all the main waves involved in driving the QBO.

But what happens in all these... so we've got various theoretical models of various sets of waves. But what's common to all of them is the following two ingredients. One is the wave-induced cumulative mean-flow changes due to wave dissipation that I've been talking about. The other ingredient is refraction of the waves by the shear in the mean flow. 'Cause I told you it reversed earlier at higher altitudes; so there's always some vertical shear. So a wave that's going up from the, you know, massive lower atmosphere, generated by thunderstorms or whatever it might be, those waves are going to get refracted by the vertical shear. And you can make a pretty good... a pretty convincing model of the QBO, if you simplify and say, oh, there's just two sets of waves, one of them propagating eastward and the other one propagating westward, that go up and get refracted.

Now remember, when the waves dissipate, there's a completely general tendency for the induced acceleration to be in the same direction as the wave phase speed. So the westward-propagating waves will try and make the mean flow go westward, and vice versa. And that's actually one of the things we know is generally true from things like GLM theory. So now, take my two waves. What happens is that... suppose the flow is going one way, it makes it easier for the wave that happens to be going the opposite way to penetrate higher. But sooner or later, it's going to break or otherwise dissipate. There's a... other dissipation mechanisms like infrared damping; let's not get too technical. The wave dissipates somehow, and it does so high up, and begins to accelerate the mean flow in its own direction.

So if it's an eastward-propagating wave... [unintelligible: "penetrating highest, in a flow initially going westward," I should have said], the flow goes more and more toward the east, higher up. And then you can see that there's a shear layer... there's a vertical shear... it's still going toward the west lower down. And the wave keeps on breaking, and it... because it's tending toward critical-layer conditions... You see, it doesn't actually matter whether you actually have a critical layer [critical level, I should have said], or not, it's just *toward* critical-layer conditions that makes it more likely for the waves to break – like my ocean [beach] surf zone. You don't have to be right at the edge, you see.

If the waves are breaking, the eastward acceleration takes place at lower and lower altitudes, and... until the eastward-moving winds get all the way down to the bottom of the stratosphere. And at about that point, the westward-propagating wave suddenly realises that it can penetrate high – because remember, the wave penetrates higher when the mean flow is going in the opposite direction to the wave phase speed. It's *far* from critical-layer conditions, if you like. So now the westward wave does the same trick. It starts accelerating the high flow in the *westward* direction, and the whole cycle repeats itself in the same manner. So the wind keeps on reversing back and forth. How fast it does depends on [is roughly proportional to] the square of the wave amplitude, okay.

So using your theory you were able to explain why it does this reversal...

Yeah... by the way, I mustn't give the impression, that's my theory. I haven't told you the early scientific story. There was a theory that said that, already on the books. David Andrews and I were really refining that theory, and trying to put it on a firmer theoretical basis, using GLM. There was a technical issue about the particular waves involved, and at the time people thought it was mainly two sorts of waves that are trapped in the tropical regions. Let me be technical for a moment. One is called an equatorial Kelvin wave, and the other is called an equatorial Rossby–gravity wave because it has a sort of... it shares... it's... well, let me try and be accurate. It's a single mode, but as the wavenumber and frequency vary, it can variously take on the character of being more like a Rossby wave, or more like a gravity wave. So we call it "Rossby-hyphen-gravity wave". And that was thought to be the other main wave, the westward-propagating one [though we now know that that's an oversimplification]. And so the first theory of the QBO – sorry, no, there was an earlier theory which just supposed they were gravity waves – but the one that really took hold was the one where you had the Rossby–gravity wave and the Kelvin wave, because they'd been observed, you see. That was *really* connecting the theory with things that had been observed.

Yes, that was my question. How had these QBOs, the... sorry...

QBO is the mean flow, reversing mean flow.

Yes. How had these reversals every thirteen months been seen? You know, how did we know... ?

Oh, how were they observed?

Mm.

Oh, that's a good question. You know, I'll come back to that when I tell you the whole scientific story. I'm going to fast-wind backwards, and say how this was discovered. But just to finish the theoretical point... okay, I'm just trying to say, I didn't think of this first. I can't claim credit for that. But what I can claim credit is that the GLM theory and its... various spinoffs from that, which include this transformed-Eulerian-mean business – never mind the technicalities – but they gave you a better handle on how these peculiar equatorially-trapped waves really worked.

It was quite hard to work out the details. It was one of these problems where doing it from the GLM point of view simplified the calculation, whereas the Eulerian was rather cumbersome [though the transformed Eulerian mean was also helpful] and that had been done [*attempted*, I should have said] by other people, before, but, you know, masses and ma... whole pages of equations, and nobody understood it really [laughs]. [And there were some details in the literature that were actually wrong.]

[0:42:11]

So now... okay, so that's the technical side. But the scientific history went like this, that after the Second World War, as I said before, we had this network of upper-air observations, and this was extended into the tropics. I *think* it was related to bomb testing, or it became related to bomb testing, 'cause it was important to understand what was going on in the tropical stratosphere if you put nuclear mushroom clouds up there. But, in one way or another, we had radiosonde stations in the deep tropics, on various islands.

And before that, even, there had been a few observations in the tropics. One of them was by a German meteorologist called Berson. I believe that was the late nineteenth century. Here my history's getting a bit wonky. But Berson observed the flow in the tropical stratosphere to be what we call westerly, which means eastward [usually]. Terrible confusion in the terminology [laughs]. [I have long urged my colleagues to *ban* the terms 'westerly' and 'easterly' as gratuitously confusing – after all, we never say 'downerly' to mean 'upward'.] I'll call them eastward for clarity, but they're called the Berson Westerlies [laughs]. And then when Krakatoa blew its top in 1883, lots of people observed that the flow wasn't... it was in the other direction [laughs]. So those were called the Krakatoa Easterlies – which, remember, means westward – but that was very conspicuous from the volcanic debris. So for a long time it was in the textbooks that there were the Berson Westerlies, and the Krakatoa Easterlies lying above them; and [that] that was the flow in the equatorial stratosphere.

But now, after the Second World War, upper-air observations became routine, rather than just special expeditions now and again. And they quickly discovered... this was the early '50s... a man at Seattle called Dick Reed, whom I [later] knew personally – a great guy, Dick, a great observational meteorologist and thinker – Dick Reed with two 'e's, R-e-e-d. And another man called Veryard, who I think was at the UK Met Office. I never met him. So this... the work of Reed and Veryard was the first *to notice that this flow is time-dependent* – that it was toward the west, part of the time, and toward the east, e.g. thirteen-months-ish later.

And so of course then attention focused on this, and they said, ah, we've discovered a new 'biennial oscillation', you see, because obviously [they seem to have thought] this is going to be synchronised with the annual cycle. But then the observations went on for a number of years, and they realised it *wasn't* synchronising with the annual cycle. And then it became called the *quasi*-biennial [laughs] cycle. And that's what it's called today – quasi-biennial *oscillation* rather – QBO. So... in a way the rest... observationally, the rest is history. You can download a picture showing the wind reversing again and again and again, around every thirteen months. The oscillation is not completely regular, but it's one of the most nearly-regular, orderly things to emerge out of the chaos of atmospheric dynamics – which is always an interesting dynamical-systems [i.e. 'chaos theory'] point. It's one of the most predictable... it is actually *the* most predictable thing in the atmosphere [on such long timescales], aside from the seasonal cycle – predictable by simply extrapolating ahead.

[0:45:36]

Okay, so there there was this observed fact. And when I was a postdoc at MIT under Jule Charney and Norman Phillips this was a great... let me get this right though... yes it was... that was 1967. Yes, it was still a great enigma. That was a great enigma;

the apparent negative viscosity in the upper troposphere was [another] great enigma – I spoke of that before. And by the way, the QBO is an example of negative viscosity, if you like. You're driving the system away from solid rotation somehow. It's the opposite effect from ordinary viscosity. Okay, so there it was, this mystery, how did this happen?

[0:46:23]

Now the next important thing in the history is something that's seldom mentioned. *I* think it's important, because it's like the Michelson–Morley experiment. Some colleagues of mine at Seattle, where Dick Reed worked, were very interested in this. And their names are Jim Holton and Mike Wallace. Jim sadly died a few years ago – a heart attack. Mike Wallace is still around. They're two of the great names in our subject – just as Seattle is one of the great research centres in our subject – the University of Washington at Seattle. So [back in 1968] Mike Wallace and Jim Holton published a paper in which they ran a simple numerical model to see if they could understand these wind reversals. And they concluded, in the paper, that it [the QBO phenomenon] was incomprehensible. [The paper said:] "we've put in all the effects we know about, including eddy effects, such as were known" – they weren't known very well observationally – "and we couldn't get the sort of momentum transport that would account for these wind reversals." So the result of this paper is negative. It was just like Michelson and Morley failing to find the aether wind [in the late 19th century]. And I always mention this because our bureaucrats and paymasters are always telling us, you can't publish negative results. All scientists must discover positive results all the time. [What's more, you must now tell your bureaucrats what the results are going to be, *before* you get them: that's called specifying the 'deliverables'.] My reply is, no, negative results can be overwhelmingly important. And this is a good example.

So... in fact, they [Wallace and Holton] said a little more in this paper. They said, you can explain it only if you assume there's a mysterious force that somehow mysteriously descends, in time, to follow the pattern of the changing winds [and changing wind-shear]. But they had no clue what this force could be. Then another colleague called Dick Lindzen, who's another famous name – he'd been reading the Booker and Bretherton paper, the Bretherton critical-layer work that I'd mentioned before, it was by Bretherton and a postdoc, I think, called Booker; and Dick was very interested in wave theory – and he immediately saw that these sort of critical-layer-type effects in mean flows generated by waves, they *could*... give you exactly this mysterious descending force. Because they would tend to go with the wind shear, you see.

And the rest is history, because Holton and Lindzen published a paper on... saying *that* [that the descending force could come from the wave-shear interactions] although, at first, with a too-simpl... simplistic a model of the waves. And then they published another paper with the two waves I talked about, the Rossby–gravity wave and the Kelvin wave, which, you know, we knew were there observationally. So that was a big step forward. And they got a reasonable simulation of the QBO, and... for the reasons that I've explained.

But I guess I'm trying to say it this way because it seems to me that being able to discover that [the explanation from wave–mean interaction] did depend on the Michelson–Morley stage, where they were stuck, you know. There were other attempts, by the way, to explain the QBO. Bob Dickinson published a paper trying to explain it without waves, and failed again, you see. So there it is. So by the time I came along, it was widely accepted that waves were involved, and that the sort of theory I'm talking about would be pertinent. So we [David Andrews and I] took that to a further stage of theoretical sophistication, which is a relatively modest thing to do, but I think quite significant at the time [and advancing our understanding of a far wider range of problems, I could have added].

[0:49:51]

And at this time, are you working to any extent at home?

Well, I've always tried to work partly at home and partly in the office, because I've, as I've told you, been a bit of a loner and a maverick, and I like solitude and the chance to think deeply. And getting away from the hurly-burly... it's important to be able to do that. So I've always, you know, done that, sometimes to the great annoyance of the wife and family. Occasionally one gets away to libraries, as I said yesterday [laughs]. Being a theoretician you've got your pencil and paper, and that's about it, for a lot of the time.

Did you have in your house in Cambridge a room where you could work, in other words where you could escape from the hurly-burly of family life without leaving the house?

Yes, I've always tried to have a study at home. I think most academics – serious scholars – have that sort of arrangement. Some of them take over the whole house, you know. Karl Popper has... you know, bookshelves everywhere. I'm not allowed to do that. I mean, this is fair really. I mean, we have to share the house with the family.

And when we moved to Cambridge, we had a little house in De Freville Avenue, a little semi-detached thing, and for a while I had my study up in the attic. I actually improvised a sort of little cubbyhole in the attic where I sat and did things. It wasn't very good, though, because you couldn't pace around, and being able to pace around is important. Later on I had a little study on the second floor, quite a small room but better than the attic cubbyhole. And since we moved from there to this house in Windsor Road, which is a bigger house, I've had quite a lot of room for my studies – 'f course I tend to abuse it 'cause I tend to fill it up with junk, like this office!

I'm in the middle of trying to clear it out now, because now I've got most significant things on the computer. You see, when I started there was no alternative but having paper. If I were starting now, I would realise the danger and get things on to the computer as fast as I could. Do you know that little article, *The Social Life of Paper* by Malcolm Gladwell – came out in the *New Yorker* – talking about most creative workers needing to play around with things on paper. Unless you're like Mozart and have an eidetic memory, then you can do the same thing in your head, which is very convenient. But most of us have to fiddle around on paper. There's this wondrous cartoon of this guy scratching his head with piles of paper everywhere. I'll show it to you later. So that's the way I've always had to work, 'cause my memory has always been capricious.

When did you move to the Windsor Road house?

'79, sometime around the end of our professional musical career, apart from the memorial concert for David Crighton [in 2001] . I told you about David Crighton , our visionary Head of Department. I think *he* had an eidetic memory. He was a walking encyclopaedia of who's who in applied mathematics, and... I remember him coming to talk to me and, you know, he wanted to learn something in... my community that I knew about. And he asked me the questions and listened carefully. I feel it was like talking to this tape recorder. He listened carefully, and at the end said thank you and went away. I think he remembered every word. And he showed he knew this stuff, because he made all sorts of brilliant new appointments in the Department, and was a visionary leader as well, the sort of guy who, you know, encourages everyone to give their best. He was a great man. And a brilliant applied mathematician. He did wonderful work on aeroacoustics and shockwaves and things like that.

[0:53:38]

So after... QBO... in the stratosphere...

Okay, so we got several papers out of that. We had a couple of papers on the QBO and the wave structures, that they were, you know, this Rossby–gravity and Kelvin wave, and the mean effects [of those waves], and... the transformed Eulerian mean. And also the GLM theory. That was a couple more papers that came out later, because that was technically tougher, so we took longer to write those papers.

And where does that take us to, time-wise... ?

Well, those two papers were ’78, weren’t they? So let’s see. I’m going to have to look at my CV to remember what happened next.

[End of Track 4]

Track 5

Could you describe continuing work on the stratosphere?

Okay. Well, as I remember, I became gradually more and more interested in the stratosphere through a sort of accumulation of effects. Okay, I'd been interested in the QBO, and that was enough to get me to a number of conferences on the stratosphere, to report that work. And so I naturally heard about other stratospheric concerns.

One of the big hot topics was something called the stratospheric sudden warming. This had been discovered by observers at the Free University of Berlin some time before, just from balloon-borne soundings [radiosondes], where you suddenly saw the temperature [at high latitudes and] at stratospheric altitudes – probably twenty-five to thirty kilometres – suddenly rise, over a day or two, by many degrees Celsius. And this was remarkable because it was known that the timescales for temperatures to change purely by solar heating, and infrared radiative cooling to space, were slower – quite a lot slower. So it was obvious straight away to everyone that dynamics was involved, but nobody had a clue about what sort of dynamics, fluid dynamics I mean. So there was quite a big community interested in these results from Berlin, and [from] other observers.

And one of the landmark papers on this was published before I got involved. It was by a wonderful man who's now, you know, a very eminent senior person in the Japanese meteorological community. His name was Taroh Matsuno. I [now] know him very well personally. He's a lovely man and a great scientist. And in 1970-ish he published a pair of papers, one of which showed what looked like a remark ably realistic simulation of a stratospheric sudden warming. And what happens is that, in the winter stratosphere, there's a spinning mass of air called the polar-night vortex. And we sometimes call it the polar night jet, because the velocity profile tends to be concentrated around the edge at, you know, something like sixty north on average. And, in a sudden warming, what happens is that this vortex either gets enormously displaced off the pole, and partly or completely destroyed, or it splits into two parts.

Now Matsuno made a spectacular simulation of the second sort where it splits into two – like an amoeba splitting. And he did this with a theory that was at first sight far too simple to be able to do this because, although these were very large-amplitude motions, he used what was [and is] called the linear theory of Rossby waves, in which you pretend that the flow in the stratosphere consists of a mean flow round and round latitude circles – if you will, a polar vortex that's undisturbed – and then you disturb it a little bit, with a small-amplitude, large-scale wave. These waves are called Rossby waves, and if we have time we can talk a bit about their dynamics, although it's a standard and well known thing. But these Rossby waves distort the vortex, and if you have a Rossby wave that has a zonal wavenumber two – that means there are two wavelengths around each latitude circle – and if it reaches large amplitudes, then the vortex can split into two in the way I described, like an amoeba. So you can think of it as a very-large-amplitude, wave-two Rossby wave.

Now Matsuno's theory shouldn't really have succeeded in simulating that kind of thing, because it was a linearised theory [therefore strictly valid for small amplitude only]; but he did do one thing [beyond standard linearised theory], which is let the mean flow change in response to the waves. So this theory had built into it the sort of wave–mean interaction that I'd been studying in these other problems I've talked about. So, when the waves reached large amplitude, the mean flow changed. And the net effect, when you added the changed mean flow and the waves together, was this astonishingly realistic pattern that looked rather like a real stratospheric warming of wavenumber two.

And so that of course got everyone, including me, excited and interested and [we] wondered how such a trick could be done because, really, waves of such large amplitude should not be describable by linear theory. And, well, I'd already thought a bit about nonlinear wave effects of other sorts. Well, indeed, they were known in other contexts. Nonlinear, meaning that you can't assume the wave amplitude is small any more. You've got to take the second-order corrections, and perhaps higher corrections, and put them all together, to get a true picture of the fluid motion. So there we were. We [the research community] had a theory that worked better than it should have, and I was curious how this could be.

[0:05:38]

Now around the same time, and here I'm not quite sure of the chronology... Well, of course, the other thing in the background, I should say, is that there was growing concern about the ozone layer. In the mid-1970s, if I recall, there was a concern that the ozone layer might be affected by supersonic aircraft exhausts, so there was a programme of, uh, chemical research initiated, that began to look into this. Our American colleagues were involved; the UK Met Office was involved. And the atmospheric chemists were beginning on this long road of understanding the rather great complexities of atmospheric chemistry: how all the trace constituents and nitric oxides and so on might affect ozone. And of course it was well known that the ozone layer protected us from solar ultraviolet. And so clearly it was important to understand if it would hang together or not.

And then there was the work of Molina and Rowland [two of the chemists]; they... who pointed out that it mightn't just be nitric oxides. It might also be chlorine radicals coming from man-made chlorofluorocarbons [CFCs] that, when they get into the stratosphere, are broken up by the hard solar ultraviolet – that they might have an effect on ozone depletion as well, or perhaps more [than the nitric oxides]. And as I recall it, the point they made was that you could have *catalytic action*.

The CFCs, especially back then, were present in the atmosphere in very small amounts, measured in parts per trillion. A decade later they were getting into the, you know, one or two hundred parts per trillion, as I recall. And of course the professional disinformers – whose business was to try and discredit any science about this, because it might affect the chemical industry – they were arguing [for instance], oh, how can such *tiny* amounts of chlorofluorocarbons *possibly* affect the ozone layer in this *vast* sky above us.

And the reply, first articulated by Molina and Rowland, I believe, was, look, you can have catalytic action. That means that one chlorine atom can destroy many ozone molecules, 'cause it gets recycled again and again. And... so, anyway... but it was many years until that was resolved, by the discovery of the ozone hole and the recognition of new sorts of chemical reactions, which turned out to be more efficient at depleting ozone catalytically. But fundamentally it was just that kind of thing.

And I was very much involved in the work to understand how the whole jigsaw fitted together: the fluid dynamics, the sudden warmings, the chemistry, and the radiation. It was an interdisciplinary collaborative effort, the outcome of which was a pretty good understanding of how the ozone hole formed – and how it was caused by man-made [chloro]fluorocarbons [mostly]. And the depth of that understanding and the crosschecks – how it all fitted together and was checked by observations, including some famous airborne expeditions one of which I went on myself – the outcome was a level of scientific confidence sufficiently high that the disinformers went quiet. [Or so I thought; see 'Tobacco Smoke' on page 171 below.]

They turned their attention to the climate problem, and today they're working hard to discredit *climate* science. But they went quiet on ozone because the evidence became too overwhelming [and possibly also because the short-term financial stakes seemed lower]. And, as a result, we have the Montreal Protocol and the... prohibition... the... capping of emissions of the chlorine-[containing] and other halogen-containing chemicals that are dangerous for the ozone layer. And a by-product of that, by the way, is that the climate greenhouse effect is now less severe than it would otherwise have been. But that was just... that's just a lucky spinoff, so... I'm getting ahead of myself, aren't I, because my role in this was to understand the fluid dynamics better.

[0:10:04]

And... it sort of grew out of my theoretical interest in wave–mean interaction, because these papers of Matsuno seemed to capture some aspects of the sudden warming phenomenon, using a rather simplistic wave–mean interaction model. But I was curious as to – you know, how could we understand that better. Now it was already clear that the large-scale wave motions involved in the sudden warmings were what we call planetary-scale Rossby waves. And they're... something that, you know, is standard knowledge these days, how they work; but it's quite tricky to explain how, at a layman's level.

But let's try and say it this way. You've got motions with timescales of a few days, of large scale, in a stratified, rotating fluid. And it turns out that the dynamically important aspects of that motion all turn on the distributions of something called potential vorticity. And this goes back to some work of Carl-Gustaf Rossby in the 1930s, and it was added to by a German theoretician called Hans Ertel. Actually Rossby had most of it, but let me not get into an argument about the history of understanding potential vorticity [which goes back all the way back to the 19th century, especially to Lord Kelvin's discovery of the circulation theorem already mentioned.]

How can we say what it is [the potential vorticity]? It's a measure of the horizontal rotation of the fluid. Remember that it's all stably stratified. You have heavy fluid under light; you have vertical density gradients. The fluid likes to move nearly horizontally in layers. And so the dynamically important things on these timescales are horizontal motions along stratification surfaces that carry the, as it were, rotational information. That includes the information about the *Earth's* rotation as well. And it turns out that you can capture almost everything about the dynamics by constructing this thing called potential vorticity.

And one way to think of it – which actually Rossby very clearly described [in a paper published in 1938] – is to think of a small chain of fluid particles [a small material contour]; here we go with the Lagrangian description of fluid motion again. You fix attention on a little closed loop of fluid particles lying in one of the stable-stratification surfaces. So it would be horizontal if there were no disturbance. And around this loop, there's something that we call the Kelvin circulation. It's a kind of average of the part of the velocity field that goes round and round this loop. [By 'part' I meant what's technically called the vector 'component' pointing along the loop, at any point on the loop.] So for instance, if this loop were in the middle of a strong vortex, it would have a nonzero value of this [Kelvin] circulation, simply 'cause the flow is going round and round the loop. And again, if you have a jet, for example – a shear flow – and you put your little contour [the loop] on one side of it, then you get a nonzero value of this circulation thing because, on the side of the circuit [the loop] nearest the jet, you get a bigger contribution to adding up all these contributions from the velocity directed around this loop. Technically this is called a line integral, the line integral of **u.**d**s**, where d**s** is an arc element along the loop. [And **u** is the velocity field relative to the stars, i.e. including the Earth's rotation. The Kelvin circulation can be defined for *any* loop, large or small, including the undulating 'wiggly line', the material contour already mentioned in connection with the GLM theory.]

Now this Kelvin circulation... If you think of the circulation around a *small* loop – where small means smaller than any of the significant scales of motion – that's [that's the potential vorticity] up to a, you know, normalising constant, a multiplicative constant. You can choose that depending on what units you want to measure it in. Apart from that kind of thing, that is what we mean by the potential vorticity.

[For a clearer discussion, see my article *Potential Vorticity,* in press for the 2nd edition of the *Encyclopedia of Atmospheric Sciences* – copy available, with permission, on my website. And I forgot to emphasise the crucial point that, for non-dissipative or ideal-fluid motion, Kelvin's circulation theorem implies that the potential vorticity is a *material invariant*: it is constant following a fluid particle.]

Now... this potential vorticity, as I think I said before, contains all the dynamical information, to good approximation. And there are a lot of nontrivial technicalities associated with that, but roughly speaking, if it's slow motion – on the sort of timescale, of several days, of these Rossby waves – you capture almost all the dynamical information by looking at the distributions of potential vorticity on each stratification surface in the stratosphere.

[0:14:35]

Now what is a Rossby wave? A Rossby wave is a sideways motion that depends on having a large-scale gradient of potential vorticity. So what you have in the real stratosphere is high values of potential vorticity at [or near] the pole, because that's where the Earth's rotation makes the strongest contribution, to this [Kelvin] circulation thing. Remember, the Earth's rotation has to be counted, as well as the relative motion. So the potential vorticity tends to be highest near the pole, and lowest at the equator where the circuit's at right angles to the Earth's rotation; and it changes sign if you go into the southern hemisphere. So there's pole-to-equator-to-pole gradient of this thing. And as soon as you've got a gradient of potential vorticity on each stratification surface, you have a new wave propagation mechanism. That's what we call Rossby waves.

And the way it works is roughly as follows. If I think about an undisturbed stratosphere with the flow just around latitude circles, in the sort of way we thought of before, and we recognise that the potential vorticity is high on the poleward side and low on the equatorward side [with all the constant-potential-vorticity contours lying parallel to latitude circles], then if I now disturb this a little bit, with a large-scale [non-dissipative] motion in which my contour undulates, and bends toward the equator in one place [location], and toward the pole in another, then [because of material invariance] I've brought high potential vorticity toward the equator in the first place [location], and low potential vorticity in the second. So now, along a latitude circle there's a change [a variation in the values] of the potential vorticity: plus, minus, plus, minus... For Matsuno's wave two you'd have plus, minus, plus, minus, and then you'd be back to where you started.

Now, as I said, the potential vorticity contains all the dynamical information. So the moment I say I've got this distribution of potential vorticity, I can *deduce* – technically we call this 'potential vorticity inversion'; we say it's 'invertible'; we can do a mathematical operation called inversion and *deduce* the wind field – and when you do that with this particular case, you find that the part of your contour that's furthest southward – is displaced furthes' southward – has zero velocity. And on either side... if you go toward the northward-displaced part on the *left* [i.e. westward], you get a southward velocity. And if you go toward the northward-displaced part of the contour on the *right* [i.e. eastward], you get a northward velocity. And now what you have to do is make a movie in your head of what will happen to that contour when it's advected by that velocity field. And, if you get the sign right, you can see that the undulation will propagate toward the west. Or to put it another way [and more generally], it propagates with the high potential vorticity on the right – okay?

And straight away, to anyone educated in physics, this is absolutely weird. Here is a wave that can only go one way, you see. It can only go 'westward' [meaning with high potential vorticity on its right]. All classical waves – sound waves, gravity waves, practically anything you could think of – they can happily go either way. There's a mathematical reason, which is in the equations for the [classical] wave motion there are what we call two time derivatives. Well, in this Rossby-wave theory, based on potential-vorticity invertibility, you have only one time derivative, because it's the one that tells you how things change when the potential vorticity is advected – is carried, is moved from place to place by the fluid motion. So if you have wave propagation it *can* only be one-way. So straight away that's interesting. Of course there's no mystery. It's not as if you've violated the basic time-reversibility of physics. I'm talking about dissipationless fluid motion. It's one-way only because it's noticing which way the Earth is rotating. If I really reverse time, the Earth would have to rotate the other way, and the waves *would* go the other way, so that's alright [laughs]. So that's a Rossby wave.

[0:18:44]

Now if you want to understand sudden warmings... now I'm going to jump. I think I'm going to have to jump ahead. I've already made the point that to begin to understand Rossby waves the first thing you've got to do is look at the distributions of potential vorticity, PV we always call it in the trade. ('Rossby–Ertel potential vorticity' is a bit of a mouthful; I'm going to call it PV.) And you need to look at the distribution of that on each stratification surface. And at the time I began to get interested in this, it was thought... there was a conventional wisdom that said, you can't do that, because the observational data aren't good enough. One very eminent scientist was reputed to have gone round saying that "anyone who computes [from observational data] a quantity so highly differentiated as potential vorticity *is a fool!*"

Nevertheless, a colleague of mine at the Met Office, or some colleagues of mine – one of them was Dr Tim Palmer, whom I've mentioned before – decided they were going to try and compute it anyway. That's what Erasmus Darwin would have called a 'damn-fool experiment'. Often it doesn't work, but when it does it can be terrific.

And this one *was* terrific, because what they got, from this attempt to compute the PV on stratification surfaces [during a stratospheric sudden warming], was what we afterwards called 'a blurred view of reality seen through knobbly glass'. And of course – fast-forwarding to today – now we have a very fine-scale view of the same thing, from the full might of weather-forecasting technology and four-dimensional data assimilation. It's a very high-tech subject these days. And now we can see the potential vorticity in the stratosphere in a lot of detail. And I have a movie of this that I tend to show in recent talks, 'cause for me it's exciting to have seen this blurred view [long ago] and, afterwards [today], seen it in fine detail. [This movie is in slide 29 of my recent *Haurwitz Lecture,* available from a link on my home page.]

[0:20:48]

Can I just ask a couple of questions before you go on?

Please. Sorry, I'm talking too much [laughs].

No, no, no. One is, how did you establish contacts at the Met Office? What's the origin of your relationship with the Met Office, given that you... ?

Oh goodness. Well, of course, being interested in atmospheric flows, I was naturally interested in making contacts – at first for no special purpose. In fact I remember once going there [very early in my career], and being told by a redoubtable member of the weather-forecasting [team]... you know, they were just beginning to develop operational numerical weather forecasting then, and they were very practical people who were writing things in machine code, and doing all the huge labour – beginning to try and think out how to assimilate the data – it was all a very... primitive in those days. But I remember going there and trying to make friends with some of these people, and being told that my interest in wave mean flow interaction work... [that it] belonged to "cloud-cuckoo land" [laughs]. But... being a bit of a maverick, as you know, I sort of went – and being interested in it, and luckily having an academic position where I had intellectual freedom – I could get on... workin' in my cloud-cuckoo land anyway. And it's a perfect example of how curiosity-driven research later has important scientific implications. But that was... okay, that was my *first* contact with the Met Office, by the way. I had many others since then.

Yes.

And this... the... y'know Tim Palmer and his colleagues, Sid Clough and others, they... in fact after our... I wrote... I published a couple of papers with Palmer on the 'blurred view of reality' [in fact 3 papers in 1983,4,5] and then Sid Clough, Alan O'Neill and... somebody else called Grahame... there was a [1985] paper by Clough, Grahame and O'Neill that followed that up, with a better-quality analysis where they took more care. They used data from two satellites instead of one, and it was quite an impressive piece of work at the time. So gradually this view got clearer. And that was my contact with the Met Office then. And I must give Tim Palmer some credit for that, 'cause that arose because he – being an enterprising and very bright young man – decided he'd come up and talk to me about stratified rotating fluid dynamics [around 1980]. 'Cause he'd just got to the Met Office having done a degree in general relatively with Roger Penrose; so he wanted to learn about the stuff I knew about, so that was the beginning of [or rather, precursor to] this rather fruitful collaboration.

[0:23:15]

Why would they... why would this individual at the Met Office – and I think you implied that this was a more general view at the Met Office among those who were beginning to do numerical weather forecasting – why would they have regarded work on generalised mean flow resulting in the interaction of waves as belonging to cloud cuckoo land? What was it about it that would have been seen by them as different from what they were interested in?

Yeah, okay. I think I'm conflating two different periods. The earlier period where I heard this cloud-cuckoo-land remark – that was very early in my career – the later phase [about 1980 onward] when Tim Palmer and others got in touch...

Oh yes, yes, I realise this is from the earlier period, well before you were working with Tim Palmer – but why then would it have been seen as not what they were interested in?

Why? Well, they were, of course, focused on building these codes to do the numerical weather forecasting, you know, and getting the computers, the relatively primitive computers... it was a tough engineering job to get anywhere at all with that. So I think they were a bit impatient with young upstarts like me who came in talking about fancy fluid-dynamical concepts [laughs] – which I naturally did – having come from Cambridge with our fluid-dynamics group where we did this... you know, you heard about these things every Friday. So there was a sort of communication gap there.

And what was it necessary... in order to... with Tim Palmer, to produce these first maps of potential vorticity on each stratified layer, what was it necessary to measure, or to... what data was it necessary to look at in order to produce them?

Well, remarkably few data. Actually, the fact that you can get away with small amounts of... well, let me be more precise. It's mostly temperature data, because the main data source was an infrared sensor on a satellite. This was one of the earliest meteorological... y'know, operational infrared sensors. I mean, various visionary people could see that observing the atmosphere from space had a lot of potential. I think some of this was pioneered at Oxford, where they... But anyway, this satellite was an operational meteorological satellite. I think it was called TIROS. And what that stands for I've long forgotten. But they had these data from that satellite – they were essentially temperature fields in the stratosphere – and at first sight you'd think that that would be hopelessly inadequate, but the... for the same reason that the potential vorticity contains all the dynamical information, you can actually use other datasets; and they contain much more dynamical information than you would think at first.

It's related to the nature of the slow motions – being very dominated by the Earth's rotation and the Coriolis forces. There's a tendency for the Coriolis force to come into balance with the pressure gradient. So, you see, that relates the velocity field to the pressure field, and therefore to the temperature field, because, also, hydrostatic balance in the vertical is a good approximation. So you can begin to see why you can get away with that small amount of information. It's called the 'balance condition'; and that's been another interest of mine. It can get quite technically intricate, trying to construct balance conditions as accurately as possible. You can refine all this. This is why potential-vorticity inversion actually works a lot better than it should. In principle [in practice, quite often, I should have said], it's quite accurate.

[0:26:53]

And so could you then describe the actual business of practically working with Tim Palmer on this? In other words, did you meet up? If you didn't meet up, how did you work, and if you did meet up what did you do when you were together?

Well, I think what happened was something like the following. I... a couple of years... no, about a year before that [1982], I'd published a big review article on stratospheric sudden warmings. You know, I think this was inspired, a lot of it, by Matsuno's work. I was intensely interested. So I somehow got invited to write this big review article, for a special issue of the Journal of the Meteorological Society of Japan. And that was one thing where I, well, if you like, got lucky. You know, in the science game you get lucky, or not, with whether this or that work is influential. This *was* an influential paper. (And in fact it was brought back in the Japan conference I was at – just the other month – where I gave a slightly nostalgic talk about how things had developed since then.)

So this [the old review article] was published in 1982. And it had a line in it that went something like, if only we could [see the actual PV distributions...] ... You know, it was already clear to me that something... it was... well, everybody knew that Rossby waves were involved, but I'd begun to realise that you really had to think of them as *breaking* waves. And that... remember, I described a simple Rossby wave in terms of a contour of constant potential vorticity *undulating* northward or southward. But now if you imagine the wave getting to a large amplitude, you can imagine that contour deforming irreversibly – curling around – all in the horizontal.

And it... especially because of the GLM theory, which already focused attention on material contours, and, in a frictionless adiabatic motion – an ideal [i.e. dissipationless] fluid motion – a potential-vorticity contour on a stratification surface *is* a material contour. So if that deforms irreversibly, that's an example of the conditions for significant wave–mean effects to occur, just like breaking ocean waves on an ocean beach. Zoologically very *different* – morphologically different – but *fundamentally similar* because in both cases you are irreversibly deforming these material contours. And Tim Palmer and I actually ended up advertising this as... as, it ought to be the fundamental way to define wave breaking, in a very general way.

But anyway, in this [1982] review I sort of recognised this must be going on. This was – I have to acknowledge – with the help of somebody who'd been following up on Matsuno's work in Seattle, my colleague Jim Holton. I've mentioned him already in connection with the QBO. He had a student called Flossie Hsu – Chi-ping Hsu – who was a very bright student. She did some simulations like Matsuno's, but she started to look at how those simulations describe fluid particles moving around. So I already had a reminder that material contours *could* deform irreversibly. So I was able to put that together conceptually with the wave–mean ideas and say, hey, look, we should really recognise this as a significant phenomenon – and call it 'breaking Rossby waves' – and recognise that the irreversible effects that you observe in a sudden warming actually have to do with that.

And just to inject a little bit of insight, the reason Matsuno's work worked better than it should, is that by following the wave and mean flow evolution together, it managed to capture a bit of the dynamics of breaking waves. It couldn't possibly describe it correctly in detail, but it captured enough of it to actually represent something of what was going on – which was a small miracle, in my view.

[0:30:48]

What is the material boundary? Is it between different levels of potential vorticity?

Okay. Remember this potential vorticity thing, this PV, it's... in ideal fluid flow it's a material invariant. It stays the same on a given piece of fluid – fluid particle if you will. So if now you redo this thought-experiment with the – y'know, starting with the symmetric flow, symmetric around latitude circles – and then undulate the contours, these contours are contours of constant PV. So they're also material contours if the flow is ideal. It's only approximately true in the real stratosphere, but it's not a bad approximation. So you've got these material contours.

And, well, my review article talked about this quite a lot because I had to, sort of, imagine this. I had to have a, sort of, overheated imagination of this going on 'cause nobody had seen it for real. All I'd done was done some idealised models. It's called 'Rossby-wave critical layer theory'. I'd picked up on some very interesting work of my colleagues, Stewartson, Tom Warn and Helen Warn – Keith Stewartson, Tom Warn and Helen Warn – they'd done this rather idealised problem [now called the SWW theory]. But I was interested [and had taken the SWW theory further, in collaboration with the late Peter Killworth] because it was the simplest way you could model mathematically, in a rather precise way, a situation where Rossby waves propagated from one place and broke in another.

So it was a kind of conceptual crutch for understanding the real stratosphere. And that's actually the only reason my name got on to the paper with Tim Palmer, you see, because Palmer and his colleagues had actually constructed the PV maps at the Met Office. They had the data and the resources to do it. And the first idea was, well, we should publish a pair of papers together, in which I describe the theory – how the theory was telling us that what they were seeing – this blurred view of reality – was probably real, in its gross features at least. But we ended up thinking, oh, it's much more powerful to have a joint paper, in which you pull all that discussion together in one paper. And that was the right thing, because that *was* an influential paper.

And how far did you write it together, literally together, in the same room?

Well, I think we did a lot of... well, we did some of it in the same room, but a lot of it was, you know, passing drafts back and forth... I think I did more of the writing myself, because I was slightly more up to speed on the scientific background at that time. If we did it now it would probably be Tim, because he's [now] a great luminary with enormous knowledge, across practically everything about numerical [and real] weather behaviour.

And could you tell the story of first seeing this blurred view of reality, including your personal reaction to it?

Oh gosh, that was *very* exciting, because what happened was I somehow had sight of this thing – I think Tim and I *had* been in touch. Probably my review had... I think it probably influenced him… [Anyway, my memory of his showing me the PV maps for the first time is one of wild excitement – essentially recognizing, in a flash, what had previously existed only in my overheated imagination, with help from the SWW theory.]

It also [indirectly] influenced the group at Reading, who started using PV maps to describe, you know, ordinary weather – cyclones and anticyclones. That was another important, you know, spinoff from the work. [I should try to say that more carefully. I think my review influenced Tim, and then Tim's and my work, in turn, influenced the Reading group, as described next:]

There's another luminary in our field, Professor Brian Hoskins [at Reading, now Sir Brian] – he's a great scientist and he'd done all sorts of other important things – but he had a student called Andy Robertson, at Reading, and Andy got very excited when he saw the first draft of the first McIntyre and Palmer paper. And he... I think that's what – or at least that's what Brian told me – that he [Andy] was just switched on by, you know, the enthusiasm of our... because our paper was full of enthusiasm because we knew we'd made something of a breakthrough with this. And… y'know, we'd checked things carefully, we'd done trajectory studies [material-particle paths – including some that I calculated by hand myself, from the observational data], we'd… it wasn't *just* my theoretical work [following the SWW work] that really helped make the case. [And it added up to a strong defence against the charge of being "fools"!] So there was Andy Robertson saying, hey, why don't we do this... I think Brian had already started him on something like this, but this made him take off and really go for it.

So as a result of that, he and I and Brian Hoskins [later] wrote a review paper, which was also very influential – Hoskins, McIntyre and Robertson (1985) – in which we showed how looking at isentropic distributions of PV helped you to understand how synoptic-scale weather systems work [cyclogenesis etc]. And by the way, I said before that these weather systems don't look wavelike at first sight. They look like very nonlinear, turbulent... sort of large-scale turbulent things. But actually wave motion is very much involved, and is surprisingly like what happens in the stratosphere. That was another big surprise. And it first came out [a few years earlier] in another paper that I wrote with Brian Hoskins and a guy called Harry Edmon, who was at Seattle, in Jim Holton's group, and Harry... or it might have been Mike Wallace's group, I think. But they had the observational data, and we were able to show how this wave–mean interaction theory could be brought into touch with observational data in a new way, by constructing something called 'Eliassen–Palm cross-sections'. There's a terrible bit of jargon for you. But that's actually now a standard tool that people use. [It's intimately related to the transformed Eulerian mean.]

[0:36:09]

And how did this dynamical structure in the stratosphere at the different levels and the breaking of the distinctions between them – how did that affect weather in the... how did it relate to weather? How was this useful for someone at the Met Office, and for someone in the Meteorology Department at Reading?

Well, it's funny you should mention that, because that today is a very hot topic: how do things in the middle stratosphere, like sudden warmings, affect weather down below? And climate trends, and everything else? And that question has still not been answered very clearly, although there now seems to be no doubt that there is an influence [downward from the stratosphere]. And that's a relatively recent thing that depends on, you know, our far, you know, *finer* view we get from assimilated data. We have a much more accurate picture of how the atmosphere has evolved over several decades, because these, you know, weather forecasting systems that I talk about – data assimilation and so forth – they've been re-run over many decades using the best data they can get, and the best models and data assimilation they can get. It's called the Reanalysis Project. So now there are long records of the best that our technology can say happened in the atmosphere, and you can see things that look like climate-change trends in these datasets [though of course there are serious issues about long-term calibration of the observing systems]. And you can see signs of the stratosphere influencing the troposphere.

Now let me just backtrack a moment. When I began to work on this, the standard paradigm was: the stratosphere is relatively less dense, the air is much thinner than the lower atmosphere, the bottom ten-ish kilometres (we call it the troposphere, where most of the weather sits)... so, to a good first approximation – and you could still say this is reasonable – you could think of the tropospheric motion as given, and afterwards compute the response of the stratosphere as a passive response. And that's the way we used to think about sudden warmings and it gets you... that's the paradigm upon which Matsuno's work was built, and it gets you quite a long way in understanding them. However, there is a relatively weak back-reaction of... where what happens aloft does react back on the troposphere, especially if you're looking at subtle, you know, slowish things going on, like, you know, just how long does a severe winter last. That sort of thing involves a lot of complicated fluid motions *and* radiation and chemistry. It's getting more like the ozone-hole problem, to get a full handle on this. It's very sensitive [for one thing] to how the water vapour's carried around in the atmosphere. So that's a very complicated problem, but the evidence is becoming fairly clear, that there *is* an influence from the stratosphere.

That of course is why modern weather forecasting models routinely include the stratosphere – which is why our blurred view of reality [as it then seemed] can now be seen very clearly [relatively *un*-blurred] in weather forecasting model output, as you can see in recent talks of mine [e.g. the *Haurwitz Lecture* already mentioned]. Courtesy of Adrian Simmons, by the way. Adrian Simmons – remember, I said – was my first research student, who fooled me into thinking that all Cambridge research students were as brilliant as he was [laughs].

[0:39:33]

And could you then tell the story of your involvement in ozone science?

Well, of course, I'm just a fluid dynamicist, but of course to crack the ozone-hole problem we needed communication between the different specialists, so I learnt, you know, quite a bit about the most important side of the chemistry and radiation problems.

Radiation is technically intricate, but it's very well developed. And the people who work on it have very complicated computer codes. The reason [*one* of the reasons] is that the greenhouse gas molecules… any molecule with more than two atoms is a greenhouse gas. That's a simple consequence of the frequency range of floppy vibrations of the molecules that interact with infrared radiation. But actually getting the details right is an intricate quantum problem, and it has to be checked by careful lab work. So there's a whole community who try and get all that right. And we think it's done fairly accurately in today's models. That's about the least of our problems in the models.

The chemistry is very complicated, but people who understand it better than I do have discovered how to simplify it, and put all the... you know, there are hundreds of reactions going on, hundreds of 'constituents' [chemical species], but they can group them into 'families' [by recognizing different timescales – fast and slow reactions] and get simplified models. It's a bit like understanding the dynamics by looking at just the PV. You know, you spot what the simplifying features are, and get a better intuitive grasp of it. Well, they've done that. So I have a bit of second-hand understanding of that side of it.

But when did you first feel that you were working on that problem, that you were sort of consciously… if you were… deciding to apply what you knew to… engaging with discussions of ozone depletion and... ?

Yes. I don't... let's see. I think my awareness of that side of it developed gradually. You could hardly go to a meeting on stratospheric warmings and such, without hearing something about the chemistry. Because the chemistry was significant for observing, don't forget. By... The basic way of observing the stratosphere from space is by clever infrared spectrometers. And you can learn a lot about the chemical composition from those, because all the molecules have their own infrared signatures, thanks to this same complicated quantum business. And so you can look at how the methane and the ozone and various other things… some nitric oxides… And quite a number of constituents can be sensed from space.

So there would always be talks about... you know, here's the distribution of such and such a chemical, how do we understand that; and fluid dynamicists like me would get involved because quite a lot of them… you had to understand them because of the way... because of the mean *circulation* of the stratosphere. This is, broadly speaking, a rather simple mean flow, where… By 'mean' I mean the mean around latitude circles. If you average around latitude circles [and use the 'transformed Eulerian mean'], you get this systematic rising motion in the tropics, and poleward motion and then downward near the poles. It's called the Brewer–Dobson circulation, because Dobson [Gordon M. B. Dobson] was a pioneer in ozone observations from the ground, and began to suspect that there had to be such a circulation to account for the observed ozone. You see a lot more ozone in places quite different from where you expect it to be produced by, you know, [ultraviolet] sunlight photolysing oxygen. So Dobson had this clue. And then Brewer – Alan Brewer – was a very clever observer, who made the first credible observations of stratospheric water vapour.

This was... I think it was actually during the Second World War. He [Brewer] was put on to this problem because, in the war, they were concerned about condensation trails from high-flying bombers, and so they cared a lot about how moist the air was at high altitudes. And quite a lot of the time these bombers were in the stratosphere. And so of course if you want to fly without leaving a contrail you try and get into the stratosphere. And knowing when to do that was all to do with having a good weather forecast, of course. Anyway, from this, Brewer developed a way of measuring the moisture in... the humidity in the stratosphere, which is technically quite difficult, because the stratosphere is very dry. And he made the first credible measurements of that, and verified, yes, it is very dry. It really *is* very dry. How on earth does it get so dry?

And the only way he could think of was this circulation. Somehow the air must be getting up in the tropics, where the lower stratosphere is very cold, and the water vapour's freezing-out there – forming cirrus clouds and such – and [the remaining air] carrying on into the rest of the stratosphere in a much drier state. And so his name is, in my view quite rightly, attached to the idea of this circulation. He clearly said there had to be such a circulation, or we can't make sense of what we observe.

But he also, being an honest scientist, said, I haven't a clue how this works! Because at first sight it violates angular-momentum conservation – okay? Because if you had nothing but that circulation... just visualise, if you've got upward motion in the tropics, and then poleward motion, now if that's all that was going on, the whole stratosphere would spin up like crazy – from a huge ballerina effect, you see – and the wind speeds would be far stronger than they are observed to be. And that [the stratospheric winds *not* being so strong] was already known, I think, from some balloons and such… aircraft measurements. So Dobson – sorry, Brewer – realised that that didn't make sense. But he said so in the paper: "I haven't a clue why this is." And it was a long time before, you know, people like me understood why – which was exactly the momentum transport by waves, in this case mainly Rossby waves – that's what cracks *that* problem as well.

[0:45:42]

And I did mention already, didn't I, this thing called 'gyroscopic pumping', remember?

Yes, yes.

Well, I'm talking about Einstein's 'tea-leaves experiment', and you saw the video of that.

Yes.

And so you have a rotating mass of fluid. If by any means you exert a retrograde force on it, you will gyroscopically *pump* the fluid toward the rotation axis. [Viewed in a frame of reference rotating with the mass of fluid, the pumping action can be thought of as a Coriolis effect. You persistently push the fluid retrogradely, and the Coriolis force persistently turns it inward.]

That is exactly what these breaking Rossby waves do, because if you go into the theory of that, and the consequence of rearranging the PV on all the stratification surfaces, you rearrange it and you invert… one of the consequences is that the... effectively, the stratosphere feels a retrograde force, because of the breaking waves. It's just like the force due to breaking ocean waves on a beach – that exert a force and generate a longshore current. The details are quite different, but fundamentally it's similar [as regards the genesis of the force]. So you get this retrograde force from breaking Rossby waves, and that [force] gyroscopically pumps fluid poleward. And that's what drives this Brewer–Dobson circulation. And I can't claim that I'm the only one to have understood this, but I *can* claim that the sort of fluid dynamics I'm talking about is absolutely central to understanding it.

[0:46:52]

So this pumping is what takes ozone from the tropics, where... from... I understand from what you're saying, more of it is likely to be produced by the action of light on oxygen.

That's right.

And that explains why there's so much ozone observed in the poles, where you wouldn't expect to see as much as that, because of the pumping of fluid, and therefore the chemicals that it contains, in this case ozone, to the pole.

Yes. That's a very important part of that story. You could say it's perhaps the... to a first approximation, how it works. The ozone is formed most rapidly in the upper tropical stratosphere. Or summer actually; I mean, at solstice the sun[shine] is strongest at the summer *pole*, remember. The Earth is tilted enough that actually it goes all the way to the pole. But all the... so in the sunlit upper stratosphere you're forming ozone at a great rate. [Especially at altitudes around 40–50 kilometres in the summer hemisphere.]

Now I have to be a little careful how I say this, because if I do nothing but move the air from there to somewhere else, and I do it slowly enough that the... I maintain chemical equilibrium... okay, if I take a piece of air with solar ultraviolet hitting it, there's a certain amount of ozone, because there's an equilibrium between the rate of formation and the rate of destruction. This goes back to work by Sydney Chapman in the 1930s, and now we have... well, we have a very sophisticated picture of all this, taking account of all the other chemicals. But the bottom line is that, in the brightly sunlit upper regions, you've got a fast chemical equilibrium time. So if you just move air around, nothing happens, except that it just adjusts to its new equilibrium when it gets into less sunlight.

But there comes a point where it's getting out of the sunlight, and the chemical reaction timescales are slowing, and becoming more comparable with the fluid transport timescale. So there's a subtle transition to a situation where a fluid parcel can carry the last ozone concentration that was established by the sunlight – carries it into the shade – and from then on the ozone is more or less inert. And, from then on, it can be carried all the way down into the lower stratosphere by the descending – this is in the polar stratosphere – by the descending branch of the circulation. So that's, you know, part of that story. I think everyone would agree, the circulation is critical. Getting the circulation right is critical to getting everything else right in that problem.

Now this, by the way, is classical ozone chemistry. It's nothing to do with the ozone hole, 'cause the ozone hole depends on another set of chemical reactions that come mainly from man-made chlorofluorocarbons [in a different chemical-reaction setting]. (The evidence from that is overwhelming, and crosschecked in very many ways [as I said before], even though the professional disinformers kept trying to discredit it; but never mind, that's past history now.) So that's another set of reactions, that depend on having solid or liquid surfaces, these *hazes* that you get in the very cold lower stratosphere – especially in the Antarctic, but it's happening in the Arctic too, a bit, especially in the colder winters.

[0:50:04]

Could you explain why Joseph Farman's spectrometer at Halley Bay and the other one at Argentine Islands in the Antarctic... why they were in a particularly good place to spot ozone depletion?

That's a very good question, and the answer is that the main ozone depletion takes place in the core of the polar vortex. Now I haven't talked about the jet going round the edge of the polar vortex very much. But this whole business of Rossby-wave breaking, and mixing of potential vorticity especially outside the vortex, in this case – which Tim Palmer and I *saw* with our very own eyes, and which we can now see in fine detail – that very same thing makes the jet sharper. And that has two consequences. One is that the Rossby-wave *elasticity*, if you will – it's a slightly loose term, but [I'm referring to] the wave mechanism – is concentrated at the vortex edge where the steepest gradients of potential vorticity are concentrated, after this mixing process. Remember, you're mixing only [or mainly] outside, so you're actually sharpening the [PV] gradients in the jet core [i.e. at the vortex *edge*]. So you're sharpening the jet [making its velocity profile narrower]. And that, by the way, is an anti-frictional effect. It looks like complete nonsense by all classical turbulence theories. That's another great story... I might come to a bit more.

But you're sharpening the jet... so, you're making the edge more elastic, 'cause the PV gradients are more concentrated. (Remember I said that 'PV gradient' is [approximately] a horizontal gradient in the stratification surface.) They [the PV gradients] are the source of the Rossby-wave mechanism, so they have a sort of restoring effect: if you push the edge of the vortex, it wants to bounce back in a peculiar elastic way, because of the Rossby-wave mechanism.

Now that mechanism is most effective on the largest scales, as it turns out. That's because of the peculiarities of PV inversion. But there's another thing, which is the shear. By sharpening the jet, you've created more shear near the edge. And that supplements the Rossby elasticity by shredding pieces of air. So if a blob of air with different chemistry wants to try and get across the edge, it tends to get shredded before it gets there. So the two effects, in combination, create what we call an 'eddy-transport barrier'. It's very hard for chemicals to get across the edge, especially from the outside into the inside. So we can think of the inside of the vortex as a sort of chemical 'containment vessel', as I once called it, in which the ozone-depletion chemistry can carry on without much contamination from outside. And that's now recognised, I think, as a part of that story. So there's two things. One is, it's relatively isolated – it's difficult to mix stuff in[to] there. And [secondly] it's also cold [promoting haze formation], just because of the dynamics of the vortex swirling round, and the radiation [the cooling-to-space that created the vortex in the first place]...

Why does that vortex form over where Joe had his mass spectrometer?

Oh, I haven't answered your question. It just happens that, because of the geography of the southern hemisphere, the polar vortex [core – whose area is comparable to that of the Antarctic continent] tends to be displaced a little bit toward the Halley Bay [area] – of course you can make all these observations perfectly well at the South Pole – it isn't displaced all that much; but it is displaced away from the American station at McMurdo Sound. So actually Halley Bay, which is on the sort of South American, the Atlantic, side, that's actually not a bad place for – quite often – seeing the vortex core, inside this eddy-transport barrier. So that's why they saw the depletion there.

And could you explain why it forms at all, why this vortex forms there at all?

Oh right. Well, okay, that's first of all down to radiation. The jet round the edge is often called the 'polar-night jet'… for the very good reason that in the winter stratosphere the polar cap sees no sunlight at all, it *is* the polar night. And that means that the heat radiation balance still involves infrared cooling to space, but no compensating heating from the Sun. And, well, that means that the core of the vortex tends to get cold. And that would happen without any of the fluid dynamics. So even with no fluid dynamics you still get some kind of vortex, but it would be a more broad, spread-out sort of thing with a broader jet. What the fluid dynamics does... well, it actually does two things: one is to sharpen the jet by this Rossby wave-breaking process, and the other is to... actually make the vortex less strong than it would otherwise be. And that's because of the Rossby-wave breaking, and dissipation by infrared damping as well, because the infrared radiation acts as a damping effect. It's a sort of thermal relaxation – hotter air radiates more strongly to space, so temperature anomalies tend to get ironed out – and that's a damping effect on the Rossby waves. And when you damp Rossby waves, either by breaking or by infrared damping, you irreversibly transport angular momentum. There is another wave–mean interaction effect, and the sign of that is such that you slow down the vortex. Remember, it's a retrograde force. It's the same thing that's doing the gyroscopic pumping; and it also slows the vortex.

And so could you explain then how this vortex breaks up? 'Cause I understand that it's the breaking up of the vortex that's involved in the depletion, partly.

Yes... well... mm... the sudden warmings, the strongest ones especially, are events in which the vortex more or less breaks up. And you could think of that as a Rossby wave whose amplitude has got even bigger than before, so it, you know, completely destroys the very thing it propagates on, if you will. It stops... it starts being a bit futile to talk about waves at that point. It's probably better to talk about the whole thing as nonlinear vortex interactions. What you see is... one thing you see is the Matsuno-type scenario, where the vortex splits like an amoeba, and if it splits far enough up, it may well get largely shredded up, or greatly reduced by the shear flow it gets into. The other flavour of warming, which is actually a commoner one, is where the vortex gets displaced an enormous distance off the pole. It's a wave-one disturbance, if you will. But again, if it's too strong, then it tends to break up. It [the vortex core] tends to get seriously eroded. You get intermediate cases where it gets displaced, and eroded, and worn – you know, the core is made smaller; it... the breaking mixes stuff off the edge, it erodes the whole thing – but then the remnant of it gets back over the pole. So, you know, there's a whole variety of scenarios that the observers study.

[I could have added, to answer the question better, that most of the ozone depletion takes place while the vortex is still more or less intact. It takes place within the vortex core, i.e. inside the surrounding eddy-transport barrier. After that, when the vortex breaks up – usually in the late spring in the case of the Antarctic – pieces of ozone-depleted air wander around, and can arrive overhead in Australia, for instance, posing a hazard to sunbathers.]

[0:57:14]

What was the nature and extent of your family's interest in your work at this time? And by this time, what we're saying now is the '70s and early '80s, I suppose. So you've got children who are getting older, and your wife, Ruth, as well. To what extent do you... yes, well, the first question really, what is the nature and extent of their interest in what you're doing, or even involvement in what you're doing?

Well, I have to say, most of the time they weren't very interested. But you can... I can sympathise with them because, as you can see from this interview, it's quite hard to describe the technicalities of my sort of work. You know, if I was just an observer and produced lots of maps and... hey, look at this spectacular event here, they could have followed it more easily. But there you go. You know, I tried to, you know, reach some sort of work–life balance. They would claim, of course, it was too much work and not enough life, but there you go.

I mean, our concertising in the 1970s was part of that balance, which actually I found I couldn't sustain. So I don't know, I... on a broader front, I mean, I think my two boys, my stepsons, were both interested in science in a more general way. The younger one, Peter, is now a venture capitalist, and he seems to have a brilliant way of grasping the essentials of some new technology, and judging whether to try and get it to market. That takes a very flexible and scientifically-aware sort of mind, and he's always been... well, in a way, a bit of a maverick like me.

He got rather impatient with school [in his mid-teens], and went off to New Zealand and [laughs] I always said, that's when his real education started. He went and worked for the tax department. What better training for getting into business can you imagine – actually seeing how the real bureaucracy works? And he did all sorts of other things: worked in a steam-engine museum, and... oh... played around with old cars. He's a great guy. He's got a lovely family now, and they're doing very well with venture capitalism [*and* their family life]. He's been... you know, he talks to me from time to time about things he does. Some medical advances, better ways of treating disc problems in the back, and so on, using new plastics technologies. Better ways of cleaning water without wasting so much energy, by being a little cleverer with the chemistry – amazing stuff.

And my other stepson, Jonathan, he's more of a computer guy. He maintains the IT, or part of the IT system, in a business. He became a Buddhist, and so it's a Buddhist-run business – a lot of lovely people there, who have a high ethic, and they work together well. So he... we see him quite regularly. He spends about three days a week in Cambridge, because that's where the headquarters is. He spends the rest of the time in London. And he... you know, he tells us a bit about their travails with trying to get these systems working robustly. It's very complicated. It's a terribly stressful thing actually. They're commissioning a big new system now. To try to judge whose services to buy and who to avoid – whom to avoid I should say [laughs]...

And the daughter [Miriam], she's not scientifically minded at all, but she's a lovely person and she's had quite a hard life actually. She, well, mainly supports her husband, I think. Works as a secretary. [She also does genealogical research in her spare time, and has recently published a book on this: *Plashet – Gone, But Not Forgotten*] They're out in Australia. He's a food safety inspector. So there you are. So the answer to your question is, not all that much, but perhaps in a broader sense some influence, I hope.

What happened when you did try to... did you try to explain what you were doing and what happened if you did?

Oh, I always... I believe, especially with youngish children, that you've got to go with the flow. You've got to try and sense what they're interested in, and do your best to satisfy that. With the younger one, Peter, I... you know... I've told you about my interest in model aeroplanes and electronics. Well, he went the same way, mostly with electronics, when he was little. So, you know, I played with those things with him. The older one, he was more of a sort of intellectual, and in those days he hadn't developed his interest in computers especially. He's... how would one describe this? You could imagine he might have become a lawyer. He was interested in arguing, and in logic, you know, and in beating people in debates, and so on. In fact he was jolly cruel to his little brother for a while. We had to kind of get him to hold off, because he could always dominate the discussion by being clever with the, you know, argy-bargy [laughs].

And what about your wife, Ruth?

Well, she's a great artist with music... well, she's a wonderful artist in a number of ways. She does lovely paintings. She's had phases that she does one thing or another. She's become a great *culinary* artist. She follows *MasterChef* assiduously, and she creates wonderful things to eat. She threw a huge seventieth birthday party for me last summer, and we had about fifty people along to the village hall. And she masterminded quite an elaborate spread, of a buffet lunch, with many delicious things in it. I kept saying... the planning for this lasted months and months and I kept... 'Darling, we should... look, we should just splash out and get a good caterer in, shouldn't we?' But no, she had the bit between her teeth. It was a creative project. And she still plays music, although not professionally any more; but she's got a lovely amateur group that plays around [and gives informal concerts] with clarinet-cello-and-piano trios, in fact.

[1:03:17]

Looking through your list of publications, as we did at lunch... Every now and again, fairly regularly, appears a publication which is not about atmospheric dynamics, but which is about acoustics, and about the production of music. And so I wonder whether you could talk about... well, describe that work, but indicate in that also the origins of it, how it was that you were combining those different kinds of research.

Yes. Well, they... it started quite simply... of course I was still playing the violin professionally; so I was interested in the finer points of violin sound, and playability, and so on... how to get different tone colours... And suddenly along comes a young man called Jim Woodhouse, who is now – well, he's now a Professor of Engineering at Cambridge. But Jim came to me and told me that he made violins. He was in the local violinmaking class. He was obviously a skilled craftsman, and he made pretty good violins. And he wanted to do for his PhD a study of the acoustics of violins.

And I said, "Well, I'd love to get involved in that, even though, you must be warned, I haven't ever really thought about it. If you don't mind [us] learning the subject together, I'll be your supervisor." So... and that was the right way to jump – although I had no way of knowing – 'cause Jim was actually one of the most *brilliant* research students I've ever had: [among] the most brilliant, well, two or three, I suppose. He had, just instinctively, a flair for how to do research. He could quickly put his finger on what was significant. And if he didn't know about it, he had a way of finding out who did know about it, and going and picking their brains. So not just me, but other people; and very quickly we had a lab setup – because he went and talked to people in the Control Engineering lab, and persuaded them into letting him use their computers, and analogue-to-digital converters, and so on, which weren't... you know, they were real research tools. And they're sort of common as dirt nowadays, but in those days you had to be a little ingenious to get a setup like that.

So we rather quickly had a setup where we were thinking about violin vibrations, and measuring some aspects. And the work developed into two strands. One was understanding how the bowed string work[s], and the other about wood vibrations, so I learnt a bit about the elastic theory of anisotropic materials like wood. I mean, every violinmaker knows that the wood properties are critical to making a good violin – especially the front of the instrument.

Does anisotropic mean it's got sort of complicated internal structure?

It does sort of mean that, but what it really means is that different directions have different properties. So it's just the fact that the wood has a grain, so it's stiffer to bend in one direction than another, that's all. And the point about violin fronts is, it's unusually stiff and unusually light. There are only three or four sorts of wood that are used for violin fronts – things like sitka spruce – that have this lightness-and-stiffness combination. And that helps to get a stronger coupling to the air. The air, of course, is very light.

So... and this was all known... but there was a wonderful woman called Carleen Hutchins who'd been trying to *open up* violin acoustics. She was trying to do the thing that was done for chemistry in the Renaissance, you know – because, before she came along, mostly violinmakers worked in secret [alchemist-like], and wanted to find the 'secret of Stradivarius' on their own. But Carleen Hutchins – along with some savvy colleagues who knew physics and circuit theory and so on – they wanted to understand violin vibrations better, and to learn how to build good instruments with unconventional dimensions: one of her big projects was to produce a 'consort of violins' that had, in some sense, optimal acoustic properties over a whole range of sizes. So there was a tiny sopranino violin that was an octave higher than the ordinary violin, and a huge contrabass thing that was lower than an ordinary double bass [I should have said, with lower *acoustic resonances* than an ordinary bass] –

and everything in between. And, you know, some of it... it's got its own musical character, and music gets written for this. I was quite involved for a time in trying to get it... that [the consort] known in Britain... going to conferences on violin acoustics [and, with help from Charles Taylor and Roddy Skeaping, starting a project at the Royal College of Music in London].

The other thing that... Carleen Hutchins, as I said... was trying to open it up to scientific... to open science... to get people involved in trying to understand violin vibrations [scientifically] and not be secretive about it, but rather, they [would do better to] have meetings and learn off each other's progress. And she founded a journal called the *Journal of the Catgut Acoustical Society*, which is very much an icon, still, for serious work in this area. So Jim and I got to know that community, and made some contributions. We got somewhere with understanding wood vibration – how to do this mathematically, which is somewhat complicated even if you do... I mean, linear theory is rather good for the body vibrations. They're small amplitude, so nonlinearity isn't an issue.

But with the bowed string it's quite different, because nonlinearity is essential. It's critical to how the whole thing works. And that's been known for a long time. This goes back to Helmholtz and Raman, the same Raman who famously got his name into spectroscopic quantum effects. (There's something called the Raman effect, on which some kinds of tuneable lasers are based.) But this same Raman, in his youth, worked on bowed-string dynamics, 'cause he was interested in the violin. So we were able to take the theory beyond their contributions.

Oh, there was a man called Lothar Cremer, whom we got to know personally. He was a famous acoustician, with a side interest in violin acoustics, and he made some progress in models of the bowed string. We were able to take that further, too, because all those models, up to now [up to Cremer's work, I should have said], didn't predict an effect with which I was familiar as a musician, which is that if you press too hard with the bow the note... the pitch drops. [We called it the 'flattening effect'.] And all the standard ideas about string vibrations didn't predict that. So Jim and I discovered how that worked, by a combination of *simple* experiments and simple mathematical models, and it was all a lot of fun. It was a real advance of fundamental understanding.

And the lab work was... you know, in a single afternoon in the lab, you could learn something significant. Contrast that with my other interests, which were to observe [vicariously! – and to understand] the stratosphere. To do that, people had to spend half their careers developing one instrument, and getting through all the fundraising, and bureaucracy, and development work that allowed that instrument to fly – something that I hugely admire, people who can have that sort of persistence and skill – because I have none of that. I could never do that, any more than jump to the moon [laughs]. Too impatient.

[1:10:44]

Could you describe what you did with the violin bow in a laboratory in order to answer some of these questions? I mean, in detail. So if we were standing at the lab bench with the two of you or with one of you, what would we see you doing with the violin bow?

Oh well, should have brought my violin, shouldn't I? But perhaps not. The short answer is a boring one. I would play notes, and we would take measurements – because we were thinking about how the string vibrations worked – and we had some simple ways of measuring forces. I think we had a piezoelectric crystal under a leg of the bridge, at one point, so we were taking the signal from that, digitising that and processing it in various ways, you know, looking at the frequency components; this is Fourier analysis again. But looking at the waveforms as well.

And to me that was the most interesting part, because understanding several things, including this 'flattening effect' where the pitch goes down [is best done directly from the shape of the waveform, rather than from the frequency content] – and one or two other things too, such as the noise that you get, by... I mean, if you hear, y'know, first class violin-playing, sometimes the player will use quite a noisy regime. You hear a certain amount of 'grit' in the sound. [This is a strong, bright sound made with the bow pressed firmly, and accurately close to the bridge.] And we discovered the basic mechanism for that, which is to do with the finite width of the bow.

I don't think, before our work, anybody had done... other than consider the bow as a single line, you know – at one point on the string. And you can get a fair way with understanding the dynamics that way, but you can't understand this noise – this particular sort of noise. And it turned out that you could begin to understand that with a two-hair model – not the many hairs of the real bow – but two hairs is enough that the short section of string, between the two hairs, can slip and stick in an irregular way. [See "Aperiodicity in bowed-string motion: on the differential-slipping mechanism", *Acustica* **50**, 294 (1982).]

Remember... I haven't even begun to explain the basics, but there's a stick-slip mechanism. The rosin on the bow has a tendency to stick and then break free – and the usual layman's description of how a bowed string works is actually wrong [or half right at best] – it says, oh, what happens is the bow grips the string and then the string slips and then it grips again. That's only half the story, because the other half, that is missed out of that explanation, is that the timing of the stick-slip transitions is controlled by a pulse that propagates back and forth on the string. So the transitions are induced by this pulse passing the bow – okay? And that was more or less understood, from Helmholtz onwards, by people who thought carefully about it.

But creating a nice model of it wasn't so easy. Cremer, I think, was the first who really did that. And we took that further because we created a model that allowed for the stick-slip transitions being different. They exhibited something called hysteresis. And that meant that the time delay involved in the transition was different when it went from stick to slip versus slip to stick. That's why the note flattens when you press too hard – the hysteretical difference.

But, okay, now if you have two bow hairs, you see, you complicate all that, because now you can get a more irregular set of transitions. The little section of string between the two bow hairs can behave a little more like the layman's explanation, and... sort of behave in a way that is not entirely timed by this pulse. By the way, you can demonstrate this by pressing *far* too hard. And then you get a horrible scrunching sound instead of a musical note. That is the sound that corresponds to the layman's description of bowed-string motion [imitates sound] [not very well; it's really much more raucous]. And so what... the way you get this grit in the sound is by the short section of the string doing a slight scrunch, but the whole thing still being well timed.

Well, that was quite subtle and interesting. It's just curiosity-driven research. I don't think it had much impact, as such, on violinmaking or whatever. We did consider some things that do matter to violinmaking, like how 'wolf notes' work. We even made some advances in understanding that [and verifying some ideas of Raman; see "On the oscillations of musical instruments", *J. Acoust. Soc. Amer.,* **74**, 1325 (1983)]. A wolf note is when you... especially a problem on 'cellos, where the string vibration is coupled too tightly to the body vibrations, and there's a nonlinear interaction that gives self-excited oscillations, a bit like the QBO in a way, two things interacting. So the string builds up body vibrations, and the body vibrations modifies the string behaviour, and it flips back and forth from one state to another. And the audible effect of that is, the note – instead of being steady – gets a sort of brrrrr or irregularity, wulululuh, some sort of nasty thing you don't usually want musically. And of course, all 'cellists know that you can buy something called a wolf eliminator, which is a little weight you put on one of the short sections of string on the other side of the bridge, and that reduces – if you tune it to the wolf note – it'll be quite effective at reducing the amplitude that... [the amplitude] of body vibrations that give rise to the wolf at that frequency. But we made s... our models actually captured that sort of behaviour quite nicely.

[End of Track 5]

Track 6

You were going to say a little more on the acoustic work.

Yes. Well, I just wanted to say that Jim Woodhouse – the brilliant student with whom I worked on violin acoustics – had a... subsequently had a brilliant career; he worked for a consultancy firm called Topexpress for a number of years; and now he's a professor in the Engineering Department here at Cambridge. And he's done significant work on other topics, such as noise in structures, which is quite an important topic for... in various engineering problems, architecture and so on, and...

[For instance he found, and refined, a significant application of the idea of 'Anderson localisation' to the noise in complex structures.]

He still takes students in musical acoustics, among other topics. Musical acoustics is a wonderful way to get a young person, scientifically-minded person, interested. It's a challenge to, you know, do lab measurements, do calculations that bear on them, all on a small scale and in a c... a compact format that's rather suitable for PhDs. And it forces you to think hard, because everybody starts thinking acoustics is trivial – which is the impression you get from the average physics textbook – and you realise that to understand acoustics at a level that matters to, you know, music or architecture – room acoustics for example – it really is quite complicated and challenging. So he's had a wonderful career doing all of those things.

[0:01:30]

Could you say something more generally about the enthusiasms of fellow mathematicians for music in Cambridge? And perhaps not just in the Department of Maths and Theoretical Physics, but in mathematical sciences more generally? This may involve friends that you know who were also... who combined interests in maths and music.

Yes. Well, it's a sort of... seems to be a generic thing, that mathematically-minded people are quite often also musically-minded. I have a personal theory that this is something to do with what I call our 'unconscious power of abstraction'. This is fundamental to how perception works. I've written about this a little bit in the *Lucidity* papers. Comes in at the end of the first paper, for example... but... And I actually think that the connections between mathematics and music are deeper than you usually hear about. I mean, there are brilliant people, like Marcus du Sautoy, who talks very entertainingly about these things. He's a very clever guy; he knows a lot of mathematics, and he's a musician as well. But usually you hear the things about numerical patterns, and not so much the abstract side of it, which interests *me* because – what is abstraction? It's the ability to handle very many cases at once. That's the essence of a mathematical proof. You prove something that applies to *all* triangles enclosed in a semicircle, and things like that.

And… if you think about how perception works… it must involve an unconscious power of abstraction. The brain must be fitting models to the incoming data, and it must be considering very many models at once. It must somehow be handling them all at once, just as a mathematician handles many cases at once. Part of how this works seems to be a kind of sensitivity to certain patterns, certain generic *kinds* of pattern. You see, this is abstraction – this is many things at once.

For example, organically-changing patterns, patterns in which some things stay the same and others change, usually by small amounts – that's a generic type of pattern to which we're very sensitive perceptually. And if you understand *that*, you can understand a great deal about how music works [including the way harmony works, if you recognize that "small" has two distinct meanings for musical pitch] – for instance – and other forms of art as well.

You can see organic change in the visual arts, in poetry of course, and in, well, practically *any* skilful use of language – er, this is one of my bête noirs – most of us are taught at school never to use the same word for the same thing. "Never repeat yourself," you know. But actually, powerful use of language almost always does involve [some] words being repeated, but other things changing – organically-changing patterns, in other words. Just as in music. Most music that works – I would dare to claim, *all* music that really works – has repeated elements. How did I get on to that?

[0:04:50]

The connection between music and maths! Well... mathematics, a lot of abstract mathematics, for example, is concerned with invariant elements. You consider a large class of objects – they might be numerical patterns, or other sorts of patterns, geometric or anything – and common to all these is some... something that stays the same. I mean, [the] Kelvin circulation [that] we were talking about is actually a good example of this. You take all the conceivable ideal-fluid motions of stratified, rotating fluids, with all the possible choices of material contours – chains of fluid particles in the stratification surfaces – and, for *every one* of these, this circulation thing I was talking about, this line-integral thing – you can calculate it by a perfectly definite formula – that's invariant, for all these fluid motions.

So I think that's an example of what I mean by *abstract* – you're grasping lots of cases at once. PV inversion is an[other] example. The invertibility principle – you take a very large class of PV distributions – for all of them, it's true, in a wide range of circumstances. (I have to say [to avoid giving a misleading impression], this is accurately true [only] in a certain range of parameter conditions; I mean, you can think of extreme cases where it isn't true. If an asteroid came by and pulled off half our atmosphere, then you'd be in a different sort of fluid dynamics.) But – in the ordinary fluid dynamics we get in the stratosphere, it's almost always true that you can deduce from the PV distributions everything else – velocity fields, temperatures, pressures, and so on. So that's another rather abstract thing, but *useful* because we know we can apply it to so many different cases of Rossby waves, whether they're breaking or not, etc, etc. [And a host of other large-scale flow problems, including cyclogenesis.]

[0:06:46]

So could you just clarify in that sense how music is an abstraction, is abstract?

Well, okay. Now music is... [skirting around the point, trying to set the context first:] I mean, it's very often used as part of telling some sort of story. The most popular music is always telling a love story or something or other – personal angst, or whatever it is. And classical art songs, the lieder of Schubert – it's the same sort of thing. And music... you have opera, you have tone poems, you have all sorts of... you have film music, very much music in aid of a narrative... [the] *Star Wars* music – that's a beautiful example of the skilful use of the language of classical music [in aid of a narrative]. The opening gets you all excited, oh, there's going to be a great adventure and great clashes of powerful forces; the music sorts of sets the scene for that.

However, if you become deeply interested in music, as I am, you also notice that some genres of music appeal to you on what you might call a 'purely' musical basis. Much of the work of J.S. Bach is a good example – the Preludes and Fugues, the music that's... you know, I'm not thinking about his [overtly] religious output, which does tell stories, but pure music. The instruments are playing just for the sheer joy of... of being immersed in these patterns, and admiring their beauty and *feeling* their beauty. Music is a very 'feeling' thing.

(I actually have a theory that great musicians probably had mothers who sang and danced while they were still in the womb, because great musical performances always seem to involve this unconscious being, this inner game, this inner musician who is both a singer and a dancer. And my experience performing music... you know, little things like "I can't make this piece of music come to life"... but, well, if you play the violin you have this luxury of being able to walk around. And if you walk around, and try and *move with it*, suddenly you feel it coming to life.)

So... and the way a lot of that works seems to be quite independent of whether it's helping to tell a story or not. There seems to be a joy in the beauty of it, the energy of it, the feel of it, that's completely independent of everything else. So I think there is such a thing as *pure* music. And if you accept that, you have to admit that it is a rather abstract thing! It's all about beautiful patterns and their interplay. [And their unexpected twists.] And that's very much like abstract mathematics.

There was a wonderful mathematician called Erdős, who's famous for having collaborated with many people round the world. His whole life career was avoiding bureaucracy and politics, and not even earning a salary. His entire life was visiting fellow-mathematicians and collaborating. And he was such a fertile source of ideas that he managed to live his whole life this way. And when he or somebody else found a particularly beautiful theorem he would say, "It's in the book!" He meant [something like] the book of heaven. It's just – beautiful. It was exactly the sort of reaction we have to a wonderful piece of music, it seems to me – and I think for fundamentally similar reasons, that we find a beauty in the *sheer abstract patterns*. [The more so if there's an unexpected twist or connection, musical or mathematical. And in some cases there's furthermore a feeling of transcendence, of awe – a sense of something far greater than oneself, with huge emotional power.]

Now I think there's a biological reason for all this, because being interested [unconsciously, deeply, and emotionally] in patterns [as such, as abstractions] – especially organically-changing patterns – is a key to survival. [That's again because of the role of organically-changing patterns in the way perception works – part of our survival kit – not least in, for instance, perceiving the difference between living things and dead things.] I think it was selected for. I mean, I think Darwin's [and Alfred Russel Wallace's] theory of natural selection is a very powerful key to understanding how the living world is, and how we are, in particular. (And of course music [also] had particular purposes, such as keeping groups together – you know, tribal solidarity, keeping together, and fighting the next tribe and so on. But let's not get too far into that.) Music is one of the mechanisms involved... but... [and here's what I think is more to the point], the ability to survive, and live life, always involves *play*.

This is most conspicuous with young animals – of our species and other species. All of them play. What is juvenile play? It's the very *serious* rehearsal for real life. It's developing your skills in the things you need to be skilful in. So, if you're a young cat, you have to be skilful in grabbing things and catching them and chasing them – and kittens at play are doing all those things, learning. We humans have to become skilful in many things, including language, but also social skills of solidarity – and skills of grasping abstract concepts... because *we're* unusually smart at adapting in creative ways. And part of those skills, I think, involves... a sensitivity to abstract patterns. [And playing with them gets us emotionally engaged – here's the emotional power again – and music is one form of *playing* with abstract patterns.]

I think that's *part* of why music is important [and why even 'pure' music has emotional power]... It's difficult to say more, because to take that beyond the level of speculation and plausible argument, you need to start to get systematic, and do psychological testing. And there's a whole community interested in 'music and science'. There's a wonderful man called Ian Cross who works on that here. He's a walking encyclopaedia of lots of stuff that's been done on this, which is beyond my knowledge. So... and to take that further you'd have to talk to someone like him!

[0:13:05]

Now since you started working at Cambridge, and particularly during the 1980s, the atmosphere as a whole, including the stratosphere, becomes more and more and more the focus for people who are concerned about global warming and climate change. And I wondered – leaving aside the ozone concern, which we've covered to some extent – to what extent did the concern for global warming... so the... here is the atmosphere becoming something that scientists and politicians are concerned about, rather than just something that is circulating around the Earth. When and to what extent did that begin to impinge on your work or... ?

Yes. Well, I've always been on the fringe of the climate-science community, although I have watched it quite closely. And I talk to people who are more closely involved in it, scientists who contribute to the IPCC reports; and all the ones I know personally are very fine and honest scientists – contrary to what many of the disinformers are saying these days. [I should add that I myself have never received research funding for climate science, as such.] So I'm quite clear about my position: that there is honest climate science [and that it's reported by IPCC, and elsewhere in the peer-reviewed literature]. It's very difficult and complicated, because if now we're considering the Earth system almost as a whole, what it looks like depends very much on the timescale you view it.

And there's been a tendency to focus on the next century, although my own personal view, having gradually, you know, got to know more about this, and talked to colleagues who work on palaeoclimate especially – and reading about it – is to gradually realise there are some very *simple* things about the Earth system that we need to take on board more clearly. There's a little piece on my website about this. And it goes something like this.

Remember I said, in connection with the ozone problem, which of course is part of the climate system, by the way; it isn't separate... but there was this... you know, when the... when Molina and Rowland first pointed out there was this possibility of catalytic destruction of ozone by man-made [chlorine-containing] chemicals – or their photolysis products – the reaction of industry and politicians tended to be, oh, this is... you know, this is a load of nonsense. There's a lot of chlorine in the atmosphere from natural causes – comes out of volcanoes, and so on. The man-made part can't possibly be significant. Look how *tiny* the amount is, they would tend to say. And there are some people who [seem to] have made their entire career going around giving lectures to that effect – and I can tell you, one or two of those, whose names I happen to know, are now giving lectures about how climate science is rubbish – because they've given up on discrediting ozone science. [And their approach is well illustrated by what they said about chlorine – talking naïvely about total amounts – distracting attention from the precise mechanisms, and the detailed insights about *where* different chlorine species go including the fact that some of them go into the stratosphere, where their effects are greatly amplified by catalysis.]

So now it's the same thing [with climate]. Big money says, we've got to try and discredit this, because it's jolly inconvenient – this whole business about greenhouse-gas emissions, and the change in the climate. And some people, including eminent scientists like Freeman Dyson, for instance – I have to say, I'm a Freeman Dyson *admirer*; he's a great physicist, he did brilliant work on quantum mechanics in his youth, and he's done all sorts of brilliant things since then and he writes very interestingly about many problems – but he seems to have decided, in a way that to me seems not quite rational, that climate science is rubbish, and climate models are terribly bad, and therefore we should pay no real attention to those. [And even if in reality there were to be a problem, then] we should concentrate on measures to change the climate system artificially, just by engineering and technology. He seems to have a kind of... to me, I would say somewhat naïve, faith that this can be done.

But of course someone of his eminence gets listened to a lot. And then there are of course the... there's the, you know, people who are purely political, and driven by money, who argue that climate change isn't a problem. And they're very skilful at playing havoc with what scientists are trying to say – [not only because of their highly-paid professional skills with words, but also] because the reality is very complicated – and the [big climate] models [used by IPCC] *are* very imperfect, and their predictions *can't* be entirely right [and so no honest scientist can possibly sound as simple, clear and definite as a skilful propagandist with a particular agenda].

One of the greatest difficulties is being correct about *regional* climate change. Now these models contain fluid-dynamical modules that do the sort of fluid dynamics I've been talking about [though still not resolved in sufficient detail]; but they've also got to have everything else, including the chemistry and the moisture, and... things... and the cloud–radiation effects – that's another very difficult thing, because it's very difficult to model that accurately. [And another huge difficulty, important for longer-term predictions, is modelling the so-called carbon cycle – which for one thing requires the oceans to be modelled in *far* finer detail than is possible today.]

And this is one reason why, in my own thinking about this in recent years, I've tended to say, let's step back from the climate models a bit, and try and get a bit of perspective on one or two things that are actually quite *simple* about the Earth system, if you expand your view and look on a longer timescale. And [this is the main point on my website] we have a lot of very *hard* evidence that's completely independent of the climate models, that comes from the past – well, the best part of the past million years, 800,000 years to be precise – which are measurements of gas bubbles [and so-called 'clathrates'] in ice cores, which tell us... a lot about atmospheric composition over that time. And there were – how many – seven or eight 'ice ages' during that time, when the climate went between a state a bit like the present state, into an ice age, and back out again [the so-called glacials and interglacials]. And every time that happened, the atmospheric carbon dioxide [CO_2] went between two very different values. And in round numbers they were [respectively, in glacials and interglacials] 180 parts per million by volume and 280 parts per million by volume.

This happened again and again. So you can say that this range, 180 to 280 – of atmospheric CO_2 – is the natural range of variability, of atmospheric CO_2 in the recent geological past, with the Earth system in something like its present state or the ice-age state... much closer to [the present]... you know... it's been much more different from that, if you go back further in time. (The early Eocene [about 55 million years ago], for example, was very hot. There were no great ice sheets. I think there's a lot of interesting things to be learnt from that as well.)

However, the time for which we have the really *hard* information is this recent 800,000 years. And so if you now say, look, we know that carbon dioxide is the most im[portant – see below]... is chemically very stable in the atmosphere. Other things like methane – practically every other carbon-containing gas, with very few exceptions, will tend to turn into carbon dioxide. Carbon dioxide is the stable end result of most chemical reactions involving carbon in the Earth's atmosphere. For example, methane goes into carbon dioxide, in the lower atmosphere, in a decade or two. So if we're looking in the longer timescale, it… putting methane in is almost the same as putting carbon dioxide in.

Now the other thing we know with very high confidence... as a scientist I never say we know an absolute truth, remember – that's for ayatollahs [sorry, I mean something different: 'fundamentalists' of any sort] – not scientists – but with high confidence we know *a lot* about the radiation physics of carbon dioxide. We know it's an important greenhouse gas, as well as being chemically stable. And a quick way to say... how… the significance [of CO_2] is to say that carbon dioxide is the... is the most important *non-condensing* greenhouse gas. And climate scientists have been a bit slow to point this out. But it's an extremely important distinction [condensing versus non-condensing], because the professional disinformers keep saying, oh, look at all the water vapour. Water vapour is a very powerful greenhouse gas, and there's more of it than carbon dioxide, so it must be more important. But that's completely wrong. [The same naïveté! Arguing from total amounts alone, with no insight into mechanisms.]

And this is a very simple point, which is that water vapour can condense or freeze, and carbon dioxide can't, under terrestrial conditions. So I say again, [CO_2 is] the most important *non-condensing* greenhouse gas in the atmosphere. That gives it a very special case [significance] in regulating the climate system. And you can see how it works over the past 800,000 years by tracing what accompanied those changes in atmospheric CO_2 between 180 and 280 parts per million by volume. Accompanying those changes were what we would regard as *enormous* climate changes, with the sea going up and down by more than 100 metres, that's [more than] 300 feet. That's much more than the single metre or two that we're worried about for the end of the next century, about which there's a lot of uncertainty [though the uncertainty is mostly about the *timing* of the response, as distinct from its ultimate magnitude].

But we [therefore] know that changing atmospheric CO_2 by that sort of amount can't be a small effect. The professional disinformers keep arguing, oh, it's much less than water vapour, so it must be unimportant. No… putting in extra CO_2 … the palaeo record clearly tells us that it's like injecting an input signal into an amplifier. The whole system is very *sensitive* to atmospheric CO_2 , for very well known reasons of radiation physics. And what have we done now? We've taken it – in the recent preindustrial past it was 280 parts per million in round numbers – we've now taken it up to more than 390. [Ands now 400, as I correct this transcript in 2013. So the increase *already* exceeds the natural range of variation, 100, that goes hand-in-hand with very large sea-level changes, sooner or later.] And it's going to be almost certainly 600 or 700 before it becomes politically possible to do anything serious. [So the bottom line is that a very large response is practically certain – sooner or later.]

Now I'm absolute rubbish at politics. I don't know what to do politically. But I suspect that, when the sea has come up a certain amount, people will realise that the problem is, after all, serious, and that they've been told a lot of lies by the disinformers, backed by bigtime money, and that we have to take seriously this business of CO_2 emissions and other greenhouse gases. And, by then, it'll be more a question of adaptation than mitigation, to use a bit of jargon. But there are still possibilities for mitigation. You can bring in various carbon-capture technologies, including taking it back out of the atmosphere. That's quite expensive but not impossible – no more difficult then building the Great Wall of China was for the ancient Chinese empire. If there were the political will to do it, it could be done. And my eminent colleague, Wally Broecker, who has made these points for a long time, would argue that in his book *Fixing Climate*.

I'd better stop talking about this, 'cause I've got some chapter and verse on my website. But the point to stay with is that the Earth system is a powerful amplifier – although with a very slow response [and internally very noisy] – and one of its input signals is injecting extra carbon dioxide. And putting in as much as we've put already is already a *large* input signal, and that is going to become a lot bigger before we've finished. And that's kind of basic fact.

If there were a disinformer here they'd say, oh, this is alarmist, you mustn't go scaremongering. My reply is, it's nothing of the kind; I'm just trying to make an honest statement of what we know [with very high scientific confidence] about the Earth system now. What politicians and society does about it is outside my competence. I just want to try and convey an understanding of one or two basic things that are quite independent of the dodgy climate models [in particular, the *large* response to human activity that, sooner or later, can be expected from the noisy but sensitive 'climate-system amplifier'].

[0:25:47]

If you've met any of the disinformers, as you call them, and I think you may have met some of them personally, what can you... what light can you shed on their motives, having met them *personally? What can you tell us about the reasons for saying what they do, from the point of view of someone who's met them?*

I know one or two of them personally. One of them is Dick Lindzen. He's absolutely convinced that the climate problem is a non-problem. I don't personally have much insight into why. [Actually I think Dick holds a sincere, if mistaken, belief, rather than being a *professional* propagandist or disinformer.] I've gained more insight into that from a book by a man called Ross Gelbspan. This came out, ooh, a decade or so ago. It's called *The Heat is On*, if I remember. And it's quite a nice summary of the political arguments that go on, including Dick Lindzen's testimony to Congress – testimonies, plural – and a personal interview that Gelbspan had with Lindzen. And, well, perhaps I should leave anyone interested to look at that book. But the bottom line is, Gelbspan says Lindzen... well, he... I have *some* awareness of this. He [Lindzen] does keep changing his reasons why mainstream climate science is wrong, because as people sort of nail down one point, he'll switch to another. But I think... well – from what Gelbspan says, and I suppose a bit from my personal knowledge of Dick – and by the way, he's a charming man, and a very clever man, and he's done some great science – but on the climate problem, I *think*... it's like Freeman Dyson. He simply has a faith – it's [something like] a religious faith – that somehow we'll come through it, through science and technology – and the one thing we *mustn't ever do* is to stop Business As Usual. [Or even slightly disturb it.]

Business As Usual is the Holy Grail. It's all about capitalist economics… economy – making money. I don't know what he would say about the recent *failures* of capitalist economy, the bank bust-ups and all of that. I don't know whether he would argue that… still argue… I mean, it's a kind of religious faith, isn't it, that says Market Forces Are the Answer to Everything, in capital letters [regardless of the evidence]. I think that's basically what he probably believes.

(What a lot of people who believe that... I don't think, think through clearly, is that to give market forces a chance to work well, you've got to give them a level playing field [and supply them with correct information!!] and... unfortunately you don't have that, because [as well as massive *dis*information] you have all sorts of dirty dealings behind the scenes, where politicians create perverse subsidies, and subsidise some capitalists more than others. So... but, as I say, what to do about that – I don't have any political genius – I don't know.)

[Much earlier I said that on ozone "the disinformers went quiet"; but it might be more accurate to say that on ozone they're no longer listened to – in responsible circles at least. I recently came across a well-documented book showing that their professional campaigns continue in *all* the scientific research areas that they wish to discredit – for commercial or ideological reasons it seems – including, still, the ozone research: *Merchants of Doubt: How a Handful of Scientists Obscured the Truth on Issues from Tobacco Smoke to Global Warming*, by Naomi Oreskes and Erik M. Conway, Bloomsbury, 2010. It's a soberly argued, evidence-based discussion and, where it overlaps with my personal knowledge, I find it to be substantially correct. And it seems that my use of the word 'professional' was no exaggeration. Indeed, something I hadn't fully appreciated is that all the campaigns appear to have revolved, largely, around the *same* small community of well-paid propagandists, some of them eminent ex-scientists, whose history and well-honed disinformation techniques go back seamlessly to the earliest campaigns on tobacco and cancer.]

[0:28:44]

You said a number of times that you couldn't yourself be a politician, but I wondered what your own personal political views, voting habits, possibly even engagements were, in the '70s and '80s, when you looked out on what was going on.

Well, I suppose I... I've never been intensely political; but I suppose I'm going to be shoved into some sort of left-wing pigeonhole if it's going to get political, aren't I?

[There we are – straight away into that tired old false dichotomy, 'left' versus 'right'.] Because I don't actually believe in solving problems by bombing people out of existence, like the neocons seem to have done when they went into Iraq [riding on another piece of highly implausible disinformation, that Saddam Hussein was allied to al-Qaeda]. I don't agree with that sort of approach. I would rather people, you know, got smart about negotiating what's in everyone's mutual interests. We're in a very bad place at the moment because the neocon type of mentality seems to have had it all their own way in the last decade or two.

So how do you recover from that; how do you rebuild trust between cultures? You know, we... and... And our journalists have some[thing to answer for]... again, I don't know what to do about this. You sell newspapers by trivialising and... sensationalising things and, you know... [for instance] instead of recognising that all the main religious groups have their moderates, you label a given religion as all extremists, don't you, 'cause it sells more newspapers, I think. I don't know what to do about that. If I were more skilful at debate and public speaking, I might *try* to speak on the side of moderation and, I don't know, some sort of... I'm not saying I want to go to a purely 'socialist' command economy like the old Soviet Union, because I think I agree with Winston Churchill that "democracy is the worst political system, apart from all the rest." So the old Soviet Union was definitely worse.

But, you know, we live in interesting times, don't we? We have the emergence of a powerful capitalist economy, China, which is going to dominate the world in many ways. And they're not a democracy, yet, but there is an argument that says they'll have to become more democratic 'cause otherwise they'll implode. It'll be interesting to see what happens. I couldn't predict.

[0:31:15]

Can you say something about personal relations with palaeoclimatologists, possibly Cambridge ones but perhaps others?

Well, I've talked a lot to... well, [for instance] I knew Nick Shackleton, who was a very famous palaeoclimatologist. I knew him quite well personally. That was partly through music, but of course I was interested in the [palaeoclimatological] sort of thing he does [did]. And this is again this line of research about the past million or so years, the Quaternary epoch of the Pleistocene.

Nick's own work was more on looking at ocean sediments, and the small animals embedded in them called foraminifera. And they're a good source of information 'cause you can... grab their shells. And you know that some of them live near the surface of the sea, and some of them lived all their lives on the bottom, and if you analyse the isotopes in their shells – of [oxygen and] carbon and magnesium and calcium and so on – you can get some information about temperatures and ocean composition [and ice-sheet volume]. It's very hard to disentangle all this information, but people have made progress [not least Nick himself]. I admire the meticulous, laborious, and careful work they do.

And so we have some idea of... you know, it's not just the carbon dioxide in ice-core bubbles [and clathrates]. That's correlated with a lot of other information from ocean sediments and other sources, like... deposits in caves, and pollen on the bottom of lakes, and all sorts of things. So the answer to your question is, I don't know many of the people who do this, but I look on their work from afar and admire them.

Nick Shackleton's last student is a very bright young man called Luke Skinner, who is now a rising star in the field. He's doing a lot of work on ocean radiocarbon, and he works with foraminifera. Being Nick's student, that would be a natural takeoff point. And we've talked a lot about... you know, how did the ocean swallow all that CO_2 in glacial times? Remember it goes from 280 to 180 in the atmosphere; that's a lot of CO_2 to put somewhere. And everybody believes the abyssal ocean is the place where most of it went. [This is the ocean below depths of a kilometre or so, all the way down to the bottom, mostly at around 5 kilometres. The abyssal ocean is the only place where there seems to be enough capacity to store so much CO_2 , as bicarbonate ions etc.]

And that interests me as a fluid dynamicist, because it's [partly] a fluid-dynamical problem: understanding how it got into the ocean but then how it got [almost] sealed up there, for a long time [very many millennia], until finally it came out... most recently about, you know, 15 to 10 thousand years ago. That was the last deglaciation, that led to our present climate.

And, you know, there's a subtle feedback process going on... that the climate disinformers... [would like to ignore – they] pick out particular time series and say, oh, look, look, the temperature went up before the CO_2 there. *Therefore*, they say, the temperature must be *causing* the CO_2 rather than the reverse. It's like

taking a complex web of cause and effect – think of a complex integrated circuit in which all sorts of electrical stuff is going on – it's all interconnected; you pick two places [within the circuit] where one signal is slightly ahead of another in time, and it's... oh, that one *causes* the other one, you say. Which is terribly naïve, because [to understand such a complex circuit – this is my noisy amplifier again] you've got to consider all the *other* thousands of signals, and how their interplay goes on. [For instance, the two signals you're looking at might both be following some other signal, or signals, that you're ignoring.]

And I think a well-informed person on these matters would say, what's actually happening is the Earth-system amplifier has another small input, which comes from subtle changes in the Sun's rays, because the Earth's orientation and position in the Earth's orbit – which is slightly elliptical – changes in time. And this is well known from celestial mechanics... And the upshot is that things happened like, the poles are [sometimes] more strongly illuminated in winter [and at other times more weakly]. And if you're in a glacial time with a big ice sheet in polar latitudes, it can matter a lot exactly how much sunshine arrives in polar latitudes [especially northern latitudes]. And that changes a bit, because of the Earth's orbital changes.

So any well educated person... I'm sure Luke Skinner would agree, that the sort of thing that's happening [in a deglaciation] is that the orbital change begins to melt the ice sheet [in northern latitudes], and then other things follow in a complicated chain of events, including changes in ocean circulation [e.g. from meltwater pouring into the North Atlantic], and changes in sea-ice cover in the Antarctic, and all sorts of things... that result in temperature changes *and* CO_2 changes. But… the CO_2 that comes out rein[forces]… It may be that the temperature changes first [e.g. on a northern ice sheet], but then the CO_2 comes out of the [abyssal] ocean [mainly via the Southern Ocean, we think – for good fluid-dynamical reasons], and atmospheric CO_2 builds up – and straight away there's a reinforcement of the original warming. And that'll produce further warming. It's a positive feedback.

And the whole deglaciation process probably depends on this whole complex feedback chain, which we do *not* understand in full detail – but we know it happened

[and we can follow, in great detail, the stages in which atmospheric CO_2 increased from around 180 all the way up to 280 parts per million by volume]. And we know that CO_2 is a powerful greenhouse gas [and that] it is chemically stable, as I said before. So anyone who thinks that the atmospheric CO_2 isn't playing a key role in these events is

[to coin a phrase] *a fool* !

[0:36:38]

And are you able to tell us anything about Nick Shackleton that we might not know from just the sort of ordinary obituaries and that sort of thing, from you having known him?

Oh gosh, it's probably a little difficult to say much more. He was a lovely man, and very honest scientist. He's the sort of person you could talk to about anything, and have an argument about... where you disagreed on the point. And he would argue back, and the whole thing would be to try and get it right – there was nothing personal. That's part of the joy of being a scientist, that you've got colleagues who care more about arriving at good science than they care about winning arguments, or personal egos. So… I can name other people like... Nick was certainly one of those. And he was a humble man, he always deferred to the evidence. He always strove hard to get better evidence [and with a completely open mind].

[I'll never forget the first time I heard him give a research seminar – several decades ago, probably in the 1970s. That was the first time that I heard about his celebrated work, with Jim Hays and John Imbrie, dating and intercomparing a number of ocean-sediment cores to produce our first really clear evidence for the effectiveness of what's called the "Milanković pacemaker". That's the scientific name for the small input to the climate-system amplifier from the Earth's orbital changes. Our knowledge of those changes over the past million years or more had been refined, around that time, by the work of André Berger. In listening to Nick I was immediately struck by the careful, meticulous, and highly ingenious way in which he had approached the whole problem, and the modest but lucid way in which he described the progress they'd made.]

And he was also a sensitive musician [a clarinettist – also a well known expert on the history of clarinet-making], and we had lovely times making music with him occasionally. His wife who – they both died of cancer, you know – he had a lovely wife [Vivien Law], who was a very [well] respected linguistics expert. I think she was a Fellow of the British Academy. [She was indeed. And she knew more than 100 languages.] But she was also a wonderful musician – played the flute very well. I remember a time we played Bach's Fifth Brandenburg Concerto in a chamber-music arrangement, with her playing the flute, and me playing the violin and Ruth playing the piano. And she [they, both Vivien and Ruth] played just wonderfully. So that was the personal dimension, as far as it went.

[End of Track 6]

Track 7

Could you tell me about relations with your parents over the period that we were discussing yesterday, which is really from appointment at Cambridge onwards, through the ’70s and into the ’80s?

Well, it... obviously, living at the opposite end of the Earth – they were still in Melbourne – that wasn’t… easy... In those days, of course, I mean, long-distance telephone calls were a significant expense, unlike today. But, you know, I’d ring them up from time to time; we’d write from time to time. They were always pleased when I, y’know, got on… or... did something I thought was exciting. I think... I don’t think there’s anything specially profound, apart from the fact that, as always, they were wonderful supportive parents.

And, you know, it was always a great pleasure to visit them occasionally. Occasionally one would make the trip to Australia. And I did admire my mother’s *architecture*. They had a... let’s see. There were three... or two and a half examples would be fair. (When they moved to Melbourne, my father was the founder Professor of Physiology at Monash University; and they promised all sorts of things to tempt him there and then reneged on most of the promises afterwards, so he had to go on all the committees and fight for every inch. But never mind, he bravely did it.) But they first lived in a house – this is in a part of East Melbourne called... well, an eastern *suburb*, I mean, called Clayton [wrong: Clayton was later – the first house was in another eastern suburb called Mount Waverley]. And they had a very beautiful house there [in Mount Waverley] that was designed, I think, by a professional architect, but heavily under my mother’s influence.

And after a number of years there, they moved to another house that was chosen to be closer to work [this *was* in Clayton, near Monash University]; so it… I think it was within walking distance. And *that* she designed most of [if not all]. And when my father retired [in 1978], they went to live in Tasmania – his old home town of Launceston that I’ve spoken of before – and they built a beautiful… another beautiful house that, again, my mother mostly designed. Sitting on a hilltop, commanding wonderful views of the Tasmanian hills and valleys. You could see Ben Lomond just on the [eastern] horizon, if I remember correctly.

What was your mother's style? In other words, how would you tell that she'd had an influence over the architecture?

How would I tell? I'm not a sufficient connoisseur of architecture to be able to tell, you know.

I wondered what her particular enthusiasms were, in terms of designing.

Oh well, I mean, she had a very sensitive eye for form, and shape, and texture, and colour. And everything she did was beautiful to me. She, I think, admired things like the Bauhaus movement, some of the modernist stuff where you tried to be sparse. You tried to omit needless details. You weren't too ornate. It's like good writing where you omit needless words, isn't it? [Laughs] And... so... her houses had a certain slight austerity but also, I would say, warmth... y'know, she was a real artist in what she did – in *everything* she did. [Her ideals and preoccupations – which included a preoccupation with functionality as well as with form and simplicity – are discussed at some length in her interview by Barry Wise, transcript pages 27–36, where she speaks of having designed *six* houses, "three for others and three for us". Among them she counted our Shiel Hill house, saying "I didn't know anything then, but it worked – probably because of my feeling for spaces." I would concur. The 'other' three – none of which I saw – included one for an impecunious but imaginative academic family with six children.]

What did your father do in retirement?

Well, he, like I do now, tried to stay active. He had quite a few years in which he *was* active, and did collaborative research with his former students or anyone else who was interested. So he would go and visit Professor Dick Mark in Canberra quite a lot, I remember, and they worked on a special receptor that… the... there's an animal called the echidna, which is, you know, one of the marsupials of Australia; it's… or… otherwise called the spiny ant-eater. It has a long snuffly nose, and a long tongue that it can schloop up ants with, of course. But it has receptors that are sensitive to electric currents. And those, among others, were my father's interests at that time.

He also did something on receptors that cats have in their foot[pads]... in their paws. They have very sensitive vibration receptors – and I don't think anyone knows for sure why, but probably it's to feel ground tremors. If you're a predator you probably want to feel if some prey is walking around nearby. But anyway, they've certainly got these very sensitive receptors.

In fact, one of the ways my father used to tell our pet cats to get off the dining table would be just to bang the surface – which apparently the cats found rather a big hit on their paw receptors; so they'd usually jump off [laughs]. So he was, you know, basically still active in his area, and still a respected researcher. And I think he felt he was to some extent catching up on research, after a pretty *gruelling* period as Head of Department and… administrator at Monash.

And what was the nature and extent of his interest and understanding of what you were doing in science?

Oh well, he'd naturally ask me, and I'd try to explain, in the sort of way I'm trying in these interviews. And he was quite interested, or he'd always throw up his hands and say, "I can't get my head round the technicalities." But then again, I probably could never do the sort of wonderfully skilled dissections and measurements he did on nervous systems.

[0:06:02]

Thank you. You mentioned yesterday that you'd taken part in a flight soon after the ozone hole was recognised by Joe Farman and his group.

Yes.

Could you first of all talk about the origins of your involvement in that?

Well, as I think I began to say last time, the… we all, everybody working on the stratosphere, became increasingly aware that the ozone problem might become, you know, a serious concern – not just a scientifically interesting problem. And of course the discovery of the ozone hole, the Antarctic ozone hole, in 1985 – or at least it was *published* in '85, by Joe Farman and Jonathan Shanklin and Brian Gardiner – that was a landmark in the subject. Immediately the Americans verified it, because they took that line of code out of their data processing [which said that very low ozone values had to be ignored, as unrealistic] and then their satellite images showed the ozone hole in all its spectacular intensity [laughs].

And, from then on, everybody involved – fluid dynamicists, chemists, radiation people – were beavering away, trying to figure out the cause of this, because the chemists hadn't thought of the kind of reaction that was responsible, as I said before. It was [as recognized subsequently] on solid or liquid surfaces in these 'polar stratospheric clouds', so called, for which haze would be a better word – sort of thin fog, sort of thing. Some of them were ice crystals, some of them were nitric acid trihydrate, and then you've got liquid ones [tiny droplets] with both sulphuric and nitric acid in them. And they all provided new sites for chemical reactions. And that made all the difference, and made the [catalytic] ozone destruction *far* stronger.

So that's... but how is it that you actually went on one of these flights, how did that... ?

Oh well, that was... well, for me that was a sort of optional-extra luxury. It wasn't really essential to my theoretical work, and the modelling work that people in my research group were doing with computers. But, well, anyone who's scientifically-minded, and involved in these problems, wants to see… wants to learn a bit more about how the others involved go about their business – in particular the observers. And what was critical at the time was to gather information about the chemical constituents in the lower stratosphere. [And I was on the *second* of a sequence of airborne expeditions to do this, which got started as follows:]

You know, according to this or that theory of ozone destruction, you expect to see this or that chemical species. One of the important ones was something called chlorine monoxide, which is a rather unstable chemical with one chlorine and one oxygen bound together. And a group under Jim Anderson, an American group – at Harvard, if I recall – they had a team that created this instrument to measure chlorine monoxide, and they flew it on NASA's ER-2 aircraft. Now the *first* expedition to do that kind of thing with the aircraft was in, if I remember correctly, 1987 – or was it 1988? Now I'm forgetting. [In fact August–September 1987.] But this is very much on the history books. They went down to an airbase in Chile – Punta Arenas – and based the ER-2 aircraft, and the NASA DC-8, which is a bigger, of course... a bigger aircraft, a flying laboratory with all sorts of instruments, including lidars that could see the polar stratospheric clouds [*above* the flight path]... and measured quite a few chemicals at the altitudes at which it could fly [11–12 kilometres].

On the whole, the ER-2 was the more important because it could get higher. The ER-2 is a descendent of the U-2 spy plane. And it's got long wings like a glider, and a single jet engine, a beautiful aircraft. I sat in the cockpit once, but I wasn't allowed to fly it because [joking] it's a lot harder to fly than a glider, I'm told [laughs]. And actually the people who flew it from Punta Arenas into the Antarctic ozone hole – to pick up the chemical signatures, including the chlorine monoxide – they were, you know, 'right stuff' pilot people, who actually had to bend the safety rules. You know, if they'd been properly bureaucratic they wouldn't have gone in, because it was quite dangerous. You had this one jet engine; you didn't have much in the way of safe emergency landing sites. They were flying down the Antarctic Peninsula – if you remember the map – which stretches up towards the tip of South America. Not much in the way of landing places. Furthermore... flying into conditions they'd never encountered before, so they weren't quite sure how the one jet engine would really behave, in air so cold. Suppose it flamed out and just dumped them? But they went in anyway, because they knew it was important.

That's what I call the 'right stuff' [laughs]. I met some of these people. I admire them and salute them, and I mention this when I give talks. So they went in, behind that single engine. You're flying this thing at the sort of altitude – we're talking about 18, 19 kilometres – and the air's so thin you've really got to wear almost a spacesuit. You've got to wear a pressure suit – looks [and functions] like a spacesuit. And there's not much room in that cockpit if you're wearing this bulky thing, so you've got to sort of somehow sort of cope with that long lonely flight, and operate all the instruments – do everything all by yourself. And they brought back critical information. It's usually called the 'smoking gun'. Yes, there was lots of chlorine monoxide! There were various other things. It all fitted the new picture of [catalytic] ozone depletion *due to reactions on hazy particle surfaces* – heterogeneous chemistry as it was called. And the rest is history.

That's basically our understanding today of how it all happened. By the way, that's the only final... that's [the heterogeneous chemistry is] only the final stage in a long chain of events, which starts with the man-made chlorofluorocarbons – the old aerosol-can and refrigerator chemicals, stable as they are, just circulating around in the lower atmosphere and not changing at all – but some of them leaking up into the stratosphere through the tropics. I said last time, the mean circulation is always upwards in the tropics, so they get in there; and then they go polewards and downwards. And on that journey they're broken up by the solar ultraviolet, which is much more energetic at those [stratospheric] altitudes. If you sunbathed up there, you'd be burnt to a cinder very quickly. And it breaks up these otherwise very stable [chlorofluorocarbon] molecules.

And then you've got free radicals containing chlorine and fluorine, which undergo another complex chain of chemical events before finally they end up in the Antarctic lower stratosphere, and the Arctic as well – equally – but, in the Antarctic, conditions are generally colder, so you've got more of these polar stratospheric clouds, and so, more scope for heterogeneous chemistry. And finally you've got to have the sunlight, to finish the whole thing off. And that happens in the spring in the Antarctic, and that's why the ozone hole suddenly appears in the Antarctic spring.

[0:13:17]

And so when was... I think you described the first flight there, didn't you, through the ozone hole? When was the flight which you took part in?

Okay, so I was talking about the first airborne expedition to make these measurements. Well, there was a second expedition, and I *was* on that. That went to the Arctic, to see how much of the same sorts of things went on in the Arctic. The answer was, we could see the beginnings of them. We could see definite signs of all the same things, but much less of them, because the Antarctic, sorry, the Arctic – I'm talking about the Arctic – the Arctic lower stratosphere is usually not as cold as the Antarctic lower stratosphere. But we learnt a lot of useful stuff.

I went along as a sort of consultant theoretician; and I don't think I did an awful lot that's scientifically significant beyond, you know, doing a bit of educating... educating the chemists a bit about the sort of fluid dynamics, and why you'd expect the vortex core, which we spoke of last time – where you have the ozone hole in the Antarctic – the core of the stratospheric polar vortex is a... more or less isolated from its surroundings, so it can have its own chemistry. So I talked a lot about that, and showed them some of the early computer simulations from our group. I suppose it may have had some influence. And many of them weren't surprised, because they'd seen observational evidence that supported the idea that the vortex core was isolated.

People who look at aerosols, so called, these are the usual aerosols – this again is a sort of haze. Most of the lower stratosphere has, all the time, a haze of sulphuric acid droplets. That comes mainly from volcanic eruptions, but there are other sources. But that's a stable species that hangs around, tiny droplets, only a few microns across. Um... a few microns? They're even smaller, many of them, a *fraction* of a micron. Anyway, they're small enough [that] they don't settle out quickly. So they hang around in the lower stratosphere. And people had been flying satellite instruments to observe these aerosols for quite a few years. And when I started talking about the fluid dynamics of isolating the vortex core, they tended to say, oh yes, we knew that anyway because the aerosols are always different: they have a different character inside the vortex than outside the vortex [core].

I think Joe Farman had noticed the isolation of it through the eruption of a particular volcano.

Yes, Joe was very much an observational scientist, so he was aware of all this. Joe once told me that even Dobson, the pioneer of ozone observation in the, you know, first half of the twentieth century... Dobson... pretty much worked out... I'm not quite sure how strong the evidence was, but Farman claimed that Dobson knew about the difference between the vortex core and the vortex exterior, from his ozone measurements. And they used to... I mean, they recognised [intuitively – and we can now say correctly, with detailed justification] that the fluid dynamics must be a bit like the classic fluid dynamics of smoke rings. You see, it is fundamentally a similar phenomenon. You blow a smoke ring – and in those days you saw them all the time in college combination rooms, and everywhere [laughs]. And it goes back to Lord Kelvin. Lord Kelvin would have been *very* familiar with smoke rings. That's why he got interested in vortex dynamics. That's why he proved his circulation theorem that we've spoken of.

If you think of a smoke ring, it's got a core that's filled with smoke, and a surrounding that's relatively smoke-free, so that precisely illustrates this same thing. The core tends to keep itself isolated, and the [Kelvin] circulation... the contour you choose, for the circulation of that, is one that just surrounds the core, you see. Kelvin worked out most of the theory of those things... a very smart guy. (He thought it was a key to understanding atoms. He thought atoms might actually be miniature vortex structures. It was a brilliant idea, even though it was wrong. It was a marvellous idea. You couldn't do any better than that at the time. Didn't have quantum mechanics, did we.)

[0:17:33]

What did you see on the flight?

Oh, well, I of course couldn't go on the ER-2 since it's a single seater, but I did go on the DC-8 a couple of times. In fact on both occasions it went to the North Pole – so I've been to the North Pole twice! And on one of those occasions they flew around the North Pole at, I don't know, ninety-nine point something north, so they flew in a big circle *around* the pole, so we had two Thursdays and two Fridays, you see. So I made a bureaucratic joke: we should claim two extra days of expenses. (The way bureaucrats sometimes behave, I sometimes think they deserve that.) But I don't think anyone took up the suggestion [laughs]. Another interesting thing was, I was allowed to go on the flight deck, as well as look at all the instruments working. And it was all fascinating stuff to me 'cause I'd never seen any of this done, you know, lidars that looked up at the clouds, and things that measured chemicals [in tiny concentrations].

And on the flight deck I was interested of course in the navigational instruments. And one amusing thing was that the gyroscope, the inertial navigation system, worked perfectly fine all the way up to the pole; but another system called Omega, which is a radio location system based on longwave radio – it's been around for ages and ages; it's a very standard instrument – when we got to about eighty-nine and a half north, it suddenly went crazy. I thought, why on earth is this? Long radio waves don't know about the pole specially; they should work just as well at the pole as anywhere else [laughs]. But then I realised, ah, must be a programming bug. The programmer didn't realise that the 'plane would ever go to the pole, and didn't bother to look... y'know, take care of the possibility that the latitude might be ninety degrees. And then of course, you know... the cosine of ninety degrees vanishes.

So somewhere in the program I'm sure it must have divided by that, and they didn't think about, sort of, treating that as a special case.

And was it possible to look out of the window and see... anything... ?

Oh yes, oh yes. It was an ordinary DC-8 so you could... you had a nice view out of the window, especially if you were on the flight deck. And, well, we saw... well, I remember seeing some of the polar stratospheric clouds coming... let's see. Of course, we were in the polar night most of the time. This was pretty much in the depths of winter, which is the interesting time to observe. But coming out of the polar night, or coming toward the dawn, coming toward the edge of the polar night, I remember *seeing* these polar stratospheric clouds, you know, layers of misty stuff, a peculiar brown colour if I remember correctly. You know, these are very small particles; they can do interesting optical things and make strange colours – no trouble at all [laughs]. And I saw some auroras – beautiful coloured auroras, near the pole. They weren't very big ones. I don't think it was a time of enormous solar activity. [On checking I find that, on the contrary, the Sun *was* fairly active at that time. So probably it just wasn't, then, sending too many particles out on paths intersecting the Earth.] I suppose that's about all I saw, apart from sea below and so on [laughs] – or the sea ice, rather.

[0:20:52]

Was it useful to you, as someone who was investigating the air at this level, to see it? You know, was it in any way useful to someone who was thinking about it?

Mm... well, er... strictly speaking, I... it probably didn't make a huge difference to what I was able to do with the theory, but it probably... I mean, it was exciting. And, well, as I said several times, I always admire the people who get it together to make these technically complicated observations: space-based, aircraft-based, there are other people who fly enormous balloons; there are all sorts of clever things they do. And it takes an awful lot of patience, ’cause the number of ways for these systems to go wrong is combinatorially large, and I always marvel when they work at all [laughs]. So I admire it, and love seeing and meeting the people who do it, and chatting to them about their work and how they cope with all the problems. And seeing the instruments in operation is exciting.

But you might say that’s [just] cultural background really. It kind of intensifies my interest – that’s about everything. Of course there were some more specific points, because these lidar instruments – lidar means a light-radar; it’s like radar: you shoot a light beam and you get echoes back and because the wavelength of light is much smaller than [that of] radio waves, you can get echoes back off the cloud particles – and so you get rather good pictures of where the clouds are in the vortex. And I remember one picture showing rather clearly the sloping edge of the vortex, and the clouds inside and not outside. And so in a way I could say – well, surprise surprise – I know from the fluid dynamics that that kind of thing *should* happen. Nevertheless, it’s wonderful to see it.

I mean, I would say the same thing about a picture on my website [home page at the string “gyroscopic pump in action”], which shows, from space-based observation – this was later work from an instrument called CRISTA that flew on the space shuttle – but it shows the vortex and the breaking Rossby waves doing their thing, almost as clearly as you could see on that weather-forecasting [movie] construction that I showed you yesterday. You can see all that fluid dynamics going on for real, with no help, no model at all, just by looking at one of the chemical constituents in the stratosphere, which marks the – you know, it acts like a dye and marks the fluid motion – something called nitrous oxide. And for me that’s very exciting, to see, for real, what had only been in my heated imagination, in the old days when I’d first worked with Tim Palmer and written my review article.

Does it help to convince you of the rightness of your work to see... to physically see in that way things that you’d sort of calculated or... you know...

I know what you mean. Well, the answer's, well, basically yes. Anyone who cares about doing good science wants as many crosschecks as possible, to boost one's confidence that one understands what's going on. And everything... you know, I looked at that picture on my website when I first saw it with, well, slight trepidation. It wasn't *much* trepidation, because we'd done so much theoretical and numerical modelling by then we were very confident we knew what was going on anyway. Still, it is reassuring when you see the data from the raw observations of the real atmosphere actually showing the same thing, isn't it? So you say, ah yes, yes – no surprise there – but it helps my confidence.

And what's the role of it in convincing others?

Oh now, if you're trying to give a lecture – it depends on the audience. If it's a lay audience you have to try and spell out a lot of this stuff in a way you wouldn't to a professional colleague who knows it already. Yes, you want to... I try to talk about the ways in which our conclusions have been checked. I gave a lecture; I think you said you went to it – the, sort of, celebration of the discovery of the ozone hole – where I *tried* to do that sort of thing.

It's quite difficult, in a talk that might only be twenty minutes or half an hour, to give the whole story. But you can give some sense of what sort of crosschecking has been done – you know, I mean... in all the aspects. I suppose... you know, we often, in my... I think many scientists do this – we often talk about a hierarchy of models, or a complex of models. If you want to understand anything that's seriously complicated, like the Earth's atmosphere, you really do need some sort of hierarchy, at the top of which are the biggest computer models that *try* to simulate everything at once.

And as I said before, they fall short in many ways. You *can't* simulate everything at once – especially if you're trying to simulate the whole climate system. But you can get some of the details quite well, and you can check them against observations; and that's the exactly the sort of thing I've been talking about. You can, you know, do a lot – you can simulate the [stratospheric ozone] chemistry quite well now, although it's still only approximate – but again you can check it against observations. So [with chemistry, fluid dynamics and radiation all incorporated] that's the top end of the hierarchy of models; and that carries a heavy burden of direct comparison with observations.

But then you see various phenomena, such as the chemical isolation of the vortex core, and the sharpening of the jet around it, and they're all interconnected things. So you try to make, as it were, simplified models of those aspects, and that sort of thing's been a lot of what I've done – and other theoretical fluid dynamicists. I mean, quite a few people have done stuff on this for quite a while now, of course. So you take cases like that two-layer model that I showed in one of my talk slides, in which you can see a jet sharpening itself, and it's actually a two-layer model that disturbs itself through what we call baroclinic instability. That's the shear instability that I was talking about early on. So you've isolated the instability and jet-sharpening aspects, and you get a sense of how that works, because there's nothing else to complicate what's going on. And you can see that in that case you can understand most of it in terms of the properties of potential vorticity – its mixability, its invertibility – so that if you rearrange the potential vorticity, you inevitably rearrange [change, I should have said] the velocity profile, in a manner that's anti-frictional. You sharpen the jet. You do the opposite of what ordinary viscosity would do. That's why people call it anti-frictional, or negative viscosity – the opposite of what a domestic jet does when you try and blow out your [birthday] candles.

So that's an example of an intermediate-level model. It's still quite complicated, because you've got to take the fully-nonlinear fluid dynamics into account. You've still got to rely on a computer for a lot of it. But then you use your theoretical knowledge to choose what to look at. And the potential-vorticity field is a good example of that.

And then there are models that are still simpler: I mentioned one yesterday, I think, the [Stewartson–Warn–Warn] Rossby-wave critical layer problem. That's a problem a lot of which you can do purely on paper. And that, again, shows the kind of interplay between Rossby-wave motion and Rossby-wave breaking that's essential to all these problems. But it shows it in such a simple way [that] you can do most of the maths by hand. So that's another level of the hierarchy.

And then, higher up, between the most comprehensive models and the sort of idealised models I'm talking about, there would be what people call simplified or 'intermediate' general circulation models. So they try to do the atmosphere on the whole sphere, and they try to... but they simplify how the radiation and chemistry are represented. There may be no chemistry at all. And you might represent the radiation by a rather simplified heating and cooling function. And when you do that... and by the way, the first time that was ever done was by Norman Phillips. I've mentioned him. He was one of my postdoctoral mentors at MIT. And that was a great piece of pioneering 'cause to do anything like that on the computers then available [the mid-1950s] was a pretty tall order. And the hard labour of coding everything in machine code, in assem… [assembly language or symbolic machine code, I started to say], and so on.

So… but now that's the kind of tool that many people use: most research groups working on global-scale atmospheric dynamics will have simplified general circulation models at this intermediate level. And you can study things like, how do the tropics and the extratropics interact. You can begin to get a handle on what effect would the... you know, an El Niño cycle have on the extratropical weather.

Would it have anything to do with our recent severe winters, for example? Some people think they've shown that that is the case – if it's the right sort of El Niño, you know. There are subtleties: there's a... several sorts of El Niños, depending on the sea-surface temperature pattern. And in an intermediate model you would probably not try to simulate the ocean actively; you would just prescribe the sea-surface temperature, and see how the atmosphere responds. So you're doing a thought-experiment where you're isolating half of the mechanism, in order to understand it better.

[0:30:37]

In the... when... you can imagine that when NASA switched on the bit of their satellite that allowed them to actually produce an image showing the ozone hole, that that was quite a convincing image if you were going to say to people of all sorts of different kinds that there is an ozone hole. And when Joseph Farman was talking about the Nature paper that he published in 1985, he was telling me about decisions he made in displaying the graph. And the way in which he constructed the graph was to try and make it very clear what was going on. I think he said something like, I call it the grandma question, you know, will grandma understand it. So... making the graph look visually clear, if you like.

Very important.

Yes. And I was wondering whether, in a similar way, depending on the audience you're talking to, do you make your arguments using different kinds of display? So for example, if you're talking to fellow mathematicians, might it be more convincing to... simply use the equations, or the theory, or the thinking, whereas if you were talking, perhaps, to a student, are you more likely to use visualisation? If you're talking to a lay audience are you more likely to show something... So really I'm asking about how you go about convincing others what you may know in a certain way that you're attempting to... convince them that this is right.

Well, I try, of course, to discover ways of showing things that are most likely to be understood by different audiences. Professional colleagues... they're familiar with equations, so I'll use equations – *as well as* pictures. I always feel *I* don't understand anything unless I've at least got equations and pictures that agree with each other – words too if possible – which is always difficult 'cause human language is fraught with built-in contradictions. So… and people like James Lighthill – I mentioned him before – he's another of my heroes. He was a great artist at using words, pictures and equations and making the whole thing into a coherent, self-consistent picture. That's the approach I'd use for my professional colleagues.

For a lay audience, of course, you can't use equations, on the whole – maybe a very, very simple one. I mean, if I had $E = mc^2$ [or something equally simple] I might… if I had something like that, I might show that. But I wouldn't make a big deal of it, because people do get frightened by equations. They kind of switch off straight away.

But everyone has powerful vision, pretty much. Human vision is the most amazing… amazing, miraculous and powerful system. And so I use that as best I can. And as Joe said, working out the right sort of graph, or whatever, is terribly important. There are so many choices; and some work better than others.

It might be a graph; it might be a geometric picture showing the shape of something. In my… In the sort of fluid dynamics I'm talking about, potential-vorticity maps [and movies, animated maps] are always central because, not only do they contain nearly all the dynamical information, but they're also easy to understand [and make visual sense of] because, to a first approximation, different values of the potential vorticity are... [they] move, go with the fluid. It's a material invariant. It's... um... so, if you give [different] colours for different values, it behaves like a dye. And that is much easier to understand than most things in science. So I try and take advantage of that.

[0:34:27]

Thank you. Are you able to tell us more about your relations with Joseph Farman either at the time of the ozone-hole work, or before, if these relation started before, what you remember of, you know, knowing him personally?

It's a good question. When did I get to know Joe? I think the answer is a bit boring because, you know, I was working away at the fluid dynamics, he was working away at the observations, and there wasn't any special reason for us to meet [at first]. I did meet him by chance, I vaguely remember, long before the ozone hole. I mean, we did have contacts with the British Antarctic Survey for one reason or another. This is going rather back in the mists of time.

That's okay.

But Joe... I think... well, Joe, you know... well, I'm sure you realise from the review [the interview], he's a bit of a maverick and, sort of, has lots of bees in his bonnets – [evcn] more than me perhaps – and is happy to talk about them to anyone who'll listen. I remember him holding forth on something or other; and I think it was about something meteorological. It might have been about the final warming, the Antarctic [stratospheric] warming or... something where, you know, you observe from the weather maps some of what happens... I don't remember much more, because it didn't really connect especially strongly with things I was doing at the time. And he didn't know much about the fluid-dynamical theory, so there wasn't much point in talking in detail about that. It would have been a bit of... outside what he was interested in then.

But then of course, after the ozone hole business... the whole thing became a hot topic and there was much more interdisciplinary collaboration. Then, naturally, I'd run into him at meetings now and again, or here [in Cambridge] and, you know, have a chat about how things were going. I mean, he would tell me how the Montreal Protocol was going. He was more closely involved in the thing... er, the politics and bureaucracy of that. And that was a terribly important thing to do. I have to say, he often came over rather pessimistically. He tended to be impatient with the ponderousness, and the imperfection, of the politics and bureaucracy and, you know, "if people had any sense they'd do this and that," he would tend to say irritably [laughs].

And, you know, when we think about the climate problem, I think most of us would tend to be irritable. The money involved is so much bigger, the political difficulties are so much bigger. The problem is so much bigger. It's very difficult to know what to do until nature shows us *clearly* enough that we've [human societies have] been making a big mistake.

[0:37:17]

To what extent have you been involved in the IPCC process, which... ?

I've never been in that. I know [some of those involved]... some of my best friends are IPCC scientists [laughs]. [The work is mostly *voluntary*, by the way – IPCC is a lot of hard work, mostly unpaid.] And, as I think I said before, I enormously admire those of them that I know. And I absolutely resent the disinformers' campaign to discredit them and to paint them as dishonest. You know, there's two kinds of sceptics in this world. There are honest scientists, who must be honest sceptics. You wouldn't be a scientist if you weren't a sceptic and always asked, "is my idea... does it stand up... does it stack up against the evidence?" – but considered honestly.

Then there are... many of the climate 'sceptics'... some are honest sceptics and just, I would claim, deluded; they haven't seen [or considered] some of the key evidence – for instance, carbon dioxide in ice cores – and thought about the implications of that [laughs]. Others, I would claim, are dishonest sceptics. The word [sceptic] has been hijacked by *anyone* who is being highly paid to discredit climate science. I think it's pretty obvious that that's going on. And the big money is not in scientific hands. The big money is in the hands of hedge-fund managers, bankers and all these people; and not all of them specially want climate scientists to be heard, do they? It puts... they can't carry on with their business as usual and being fat cats, can they?

[0:38:50]

Why is that you haven't been involved in it, that your friends have but you... ?

Well... erm... two reasons, I suppose. One is I'm not specially in the [big] modelling business. IPCC, of necessity, has to focus on the big models. That's of course one of its *problems*, because of the big models' imperfections. But we can't do any better; we have to *try* with these models. We have to try and make them better. There's a huge labour in doing that, 'cause these models are... they're not as complex as the Earth system, but, by golly, they are complex. They're much too complex for a single person to understand the entire code; so you've got to have big teams of people that are responsible for different parts of the code.

And I've never been in that business, (a) because I'm not specially good at computer programming; I tend to make too many mistakes. [Sorry, long digression for the rest of this paragraph:] Anybody who's ever programmed a computer knows how easy it is, you know [to make programming mistakes]: you count something wrong, you do something one too many times, or whatever. It's very hard to get absolutely everything right. It's another case where the number of ways to go wrong is combinatorially large. (That's one reason, by the way, why government IT projects usually fall into the sand, and come in ten times over budget, and all of that, because politicians don't understand combinatorial largeness – the number of ways to go wrong – the sheer consequences of having an idea in the bath and saying, oh, let's do this system another way. Of course they're always doing that, because the political time horizon is so short, and they don't realise that changing the spec of... y'know... the NHS IT system is a good example [laughs]... it's going to make... add years and years of struggle to get it right. And the teams may have almost got it right, when suddenly they [the politicians] change their mind, and you've got to throw a lot of it out, and start again.) But I mean, even getting a complex computer system right when you've got clear specifications, and no political interference – that's hard enough, for heaven's sake. So anyway, that's not particularly a gift of *mine*, to be good at that.

[And (b)] the other thing I'm not specially good at is teamwork. Being a bit of a maverick, I wasn't ever a very good manager, beyond encouraging the young people to, you know, be creative themselves – which of course is successful with some of them. So... and, you know, it's well known that if you get McIntyre involved in some team, the chances are he'll fail to meet some deadline for getting something written [just as I've taken a small age to check and correct this transcript]. And that's no good in an operation like IPCC, is it?

But in a way it gives me a natural advantage, because I can, sort of, be a more dispassionate observer of what goes on, and I can think in a scientifically independent way, and I can make judgements about what makes sense and what doesn't.

And this is one reason why I'm banging on a bit about paying more attention to the paleo[climate] record. I'm not the only one. I mean, that *is* getting into IPCC. I've been bending the ears of everyone I know in IPCC [for what it's worth]: for God's sake, expand the time horizon – start talking about what happens to the Earth system for many centuries or millennia – because it's very illuminating to understand what happens then. You have to bring in the whole carbon cycle, and all of that, which is too complex to model accurately, but whose gross properties we know very well from these ice-core records.

I'm very interested in why it... why the paleoclimate work hasn't had the status within the IPCC. In other words, why it's necessary for you to bend the ear of these people.

Hmm, well, you know, there's a political imperative [combined with practical limitations]. I mean, they started with what seemed a perfectly reasonable aim. We can hardly hope to predict further ahead than one century [it seemed reasonable to say]; so one century is something that's kind of a neat marker that you can grasp; so let's try and predict a century ahead. And you could even say, well, it doesn't much matter projecting further ahead [than a century] because you can't possibly plan for that, anyway.

But then again, you see, my answer is, well, in the case of the climate system, that's perhaps not enough, because if, as is likely [in my judgement], the Earth system is already on its way towards an Eocene-like state [*far* hotter than today, with no great ice sheets], it may take many centuries, or even a few millennia, to get there. That depends on ice-flow dynamics, and storm-track dynamics and snow accumulation, and we don't model any of those things well enough. But the sort of timescale it might take is certainly multi-century or more.

And it gives you an insight that I think does inform the debate, because if the models are even roughly right – and the projections of sea-level rise are roughly right – once the sea starts rising there's a pretty high chance that it'll keep on rising, unstoppably, for very many centuries, until it's very *many* metres. I mean, the potential is something like seventy or eighty metres high[er], just from melting the great ice sheets. So the one metre at the end of the century is, in a way, a pretty small part of the story. [I said "unstoppably", but that could change once carbon-capture technologies are taken seriously – including air capture, for which there are other good reasons, including mitigation of ocean food-chain disasters from ocean acidification.]

So, although you can't really make detailed projections of this, 'cause you don't know the rates – the precise rates at which all these things are happening – you could say there's a certain [very high] confidence about the sort of thing that *will*, sooner or later, happen. And that should [in my opinion] be part of the background to this debate – and a good answer to people who say, well, you know, climate change is so uncertain that it's not a problem [and] we should just carry on with Business as Usual.

[0:44:34]

You know, [the eminent economist] Lord Stern wrote this report on... what's it called, *Blueprint for a Safer Planet*. There was a report, and then there was a book – a sort of second edition of it in the form of a book. And he argues that if we were to take all this seriously, people could make money. But he's not the only one, by the way. Richard Branson agrees. Richard Branson's a smart guy as well as a business tycoon. He started something called the Carbon War Room – which you can easily find on the web – and its message is, look, climate change *is* a serious problem and, look, if you're a business entrepreneur, it's a jolly good chance to make money. It allows you to see further ahead than your competitors, just spot the... you know, the low-hanging fruit – how we can move toward a low-carbon economy – and you'll make your fortune. And that may be the way that we eventually do a bit of mitigation.

I can't see it happening through politics. Politicians think in far too short-term a way.

[0:45:36]

Has there been any effect of... the kinds of scientists who were those involved in setting up the IPCC, has that had an effect on the relative exclusion of paleoclimate studies from it?

Oh, I think it's more [a matter of unavailability than exclusion]... I mean, the people involved, some of them are *very* high quality thinkers [who wouldn't have tried to *exclude* any relevant information]. I mean, Sir John Houghton is an example. I've known him for a long time, ever since he was the leader of the group at Oxford that pioneered some of the kinds of infrared sensing of the stratosphere, so I had a professional involvement with *their* work. Houghton was really... I think he was the founder chairperson of IPCC [yes, back in 1988]. But, look, the answer to your question is that... everybody... well, there are two answers really.

One is that, when IPCC started, the paleo record wasn't so clear. I mean, people have laboured mightily to refine the time resolution [and to increase greatly the number of lines of evidence]. This business of the Dansgaard-Oeschger warming events, you know, in the Quaternary time, they... the fact that they're so *fast* that they... you can get significant climate change within one individual's lifetime. That wasn't known then [in 1988] because, you know, you're faced with this ice core, or mud core or whatever it is, and you've got to get all the little shells out, or analyse the chemistry of the air bubbles, and... It's a huge labour to do any of that; and so of course they did it with coarser time resolution at first. You know, multi-century [at best]. So getting down to decadal times was a huge amount of work; and that's only been available in, oh, I don't know, the last ten years or so probably. So when Sir John Houghton started IPCC, he wouldn't have known quite how vivid that record... how very insightful it was going to be. I don't think even the ice-core CO_2 measurements... they weren't... I think they were beginning, but I don't think we knew an awful lot about that, back then.

So [and this is the second answer] there wasn't anything else they could do other than say, let's try and build the best climate models we can, and do the best predictions we can [restricted to a century ahead], and be very honest about the uncertainties. And they always were. There's always been a large range of uncertainty. And that's in itself such a huge task [the model-building *and* the assessment against recent and contemporary observations], they wouldn't have had the time or energy to do anything else.

[0:47:56]

And could you tell me about your work with John Houghton's group, because John Houghton is one of the other interviewees on this project.

Yes.

And so he's told me about his work in developing the satellite instruments. But it would be nice to see it all from your point of view, as well as from his.

Well, my involvement... I mean, I didn't ever have a collaborative project, not at that time anyway. But since I was getting interested in stratospheric dynamics, I naturally paid attention to their results, 'cause they were direct observations. In fact, if you look at the *Nature* paper by Palmer and myself you... I think the opening line, almost, is [that, thanks to the satellite instruments] "the stratosphere is a wonderful *outdoor laboratory*, where nature does great fluid-dynamical experiments, from which we have much to learn." Those weren't the exact words. But that was because of this remote-sensing business that was pioneered at... a lot of it was pioneered at Oxford [starting in the late 1960s]. There were some American groups doing similar things at the time. I recall that the Oxford group got ahead... with the infrared sensing of the stratosphere ’cause they thought of this rather clever trick of the gas cell.

’Cause everybody knew that what you had to do was look at the infrared coming off the CO_2 and later on other chemical [species]... other greenhouse gases, but CO_2 was sort of the strongest one. And if you look at the spectrum of wavelengths that come off, it’s incredibly complicated. You know, you’ve got this graph that looks like fur, you know, all the peaks and valleys of adjacent wavelengths being more, or less, strongly interacting with the CO_2 molecule. And to understand all of that, you’ve got to do the full quantum mechanics and it’s quite complicated. But... to measure that, you see... You see, one thing it means is the following, that if you look at wavelengths where the CO_2 is... less... [or] is more opaque – where it strongly interacts with the infrared photons – you... the whole atmosphere looks opaque, so you see the top of... you see the *upper* layers. If you look at the opposite extreme, where the CO_2 is more transparent, then you see deeper. Now if you can organise [observations at] a number of wavelengths, you can get vertical profiles of things [such as temperature] – which is very clever to start with. But what the Oxford group did, and at the time I think it was the brightest idea anyone could have, is that they said, let’s use CO_2 gas itself as the filter.

And so they had a cell with CO_2 gas in it, so its spectrum matched exactly that of the infrared coming off. But then they used technical tricks like pressure modulation – now here I’m getting a bit at the fringe of my knowledge. But you’ve got to somehow vary the properties of the absorbing cell so that it sees to different depths; but they managed to do that. And [laughs] so that was, I think, a first for them, making that technique work [the gas cell technique, in one form or another]. I think, these days, optical filters and sensors and, you know, purely artificially-constructed things do pretty well, as well, because the computing power is so much bigger; the resolution of everything is so much better. So I’m not actually sure whether they still use gas cells. You might find if you looked into it that they use a combination of that and sophisticated sensors. [On checking, I find that gas cells are indeed still used, filled with whatever trace gas is being observed in the atmosphere. The technical term is ‘gas correlation radiometry’.]

But, you know, it's a huge... it's a very high-technological art now. I mean, this CRISTA instrument that I mentioned, that can see in remarkable detail [with high spatial resolution] – it can even see breaking Rossby waves in the real stratosphere. That's, you know, a later development of that sort of technology, but I've forgotten exactly whether they use gas cells or just sensors. What I do know is that it's a helium-cooled instrument. It's the same sort of technology that was used to... that... cosmologists used to sense the microwave background of the universe. You've got to have a very cold detector 'cause otherwise the local thermal fluctuations overwhelm your signal. So they've applied that sort of thing to sensing the stratosphere, and got much more sensitivity and much more resolution and everything. That's why we can see the breaking Rossby waves now.

To what extent was your and Tim Palmer's blurred view of the stratosphere dependent on this CO_2 *cell in the radiometer?*

Oh well, that was... our *Nature* paper was 1983, and the Oxford group was... I think they were beginning to get results in the... well the second half of the 1970s, if I recall. [Wrong! Oxford's first remote-sensing instrument flew much earlier, in 1970, on the Nimbus 4 satellite. And the first results were reported in *Nature* the same year.] So those results had been around. And as I mentioned yesterday, there were also operational meteorological satellites, which were actually the ones [TIROS-N] that Tim and I used because that's... the Met Office had the data from those. [And, to answer the question, I find on checking that the TIROS-N instruments *did* use gas cells and indeed were, technically, direct descendents of the original Oxford instruments.]

[0:53:13]

You know, in those days – this is an aside, but an important one – in those days quite a lot of the meteorological data were proprietary. You couldn't get hold of it without either working in that place or buying it. And, you know, the whole climate-science community has been in terrible trouble recently because of data not being open-access. And I think now there's a big push to make *everything* open-access – the way John Sulston did with the genome data –

which at first sight sounds like a bad business plan, but actually on second thoughts works better, because then things can't be grabbed by the corporate world [restricting and inhibiting research on, e.g., the hugely complex, and poorly-understood, causes of cancer].

And the same goes for meteorological data. You know, there's money to be made with meteorological data. Some people want to keep it private. But then it opens the way for the climate 'sceptics' to accuse people of hiding things. And we now know, that's completely unacceptable. Everybody *must* make their data open-access. So that's another change, today as distinct from there [then].

So where you were asking about the satellite data that we based our work on, well, that was the TIROS[-N] operational satellite but the technology was a similar... it was certainly an infrared sensor. Whether they used gas cells or not... I really... I don't think I ever knew. So there you are. As a humble theoretician I don't... I'm not familiar with everything about the observing techniques. [But as noted above, I do know, now, that the TIROS-N instruments did use gas cells.]

[0:54:38]

Now one of the things that's well known about John Houghton is that his faith is very strong, and he draws links between his faith and science very openly and very clearly. I wondered how apparent that was when you knew him, at the time that he was working on these things.

When I first knew John I didn't know he was a devout Christian. I learnt that later. In fact, [it was when] I once heard his very familiar voice on the Sunday... whatever it is on Sunday morning on Radio 4. They have a religious news programme. And suddenly there was this familiar voice saying, "Look, God calls on us to be good stewards for the Earth." I said, "Good for you". You know, I mean, I don't know whether I... I don't know whether you want to ask me about my, sort, of personal convictions. I'm not specially religious. I'd call myself a... I told you before, I did go to Quaker meetings as a young person, but it never quite did as much for me as, for example, music. I think I went mainly 'cause I knew some very wonderful people who were Quakers, marvellous people. I do *admire* the Quaker movement. It's just that their particular rituals and way of worship didn't really give me enough spiritual solace to be worth… to… well, you have to devote a lot of time and energy to it, of course.

I'd call myself a 'reverent agnostic'. I strongly disagree with people like Richard Dawkins [and, even more, Peter Atkins]. I have to say this. Dawkins is a *brilliant* thinker and communicator, and teaches us a lot about the biological world, most of which I buy. I mean, I'm not a professional biologist, but a lot of it makes... I think I know how to think scientifically, and a lot of it does make sense [to me]. I'm aware, of course, as with all science, that our knowledge is incomplete. But there's Dawkins saying he knows *for sure* that God doesn't exist. Now to me that's just another *fundamentalist* belief system. If you 'know' an absolute 'truth' that bears no contradiction – then you're just being like an ayatollah [a fundamentalist one] or an American religious-right [and in this regard you're not a scientist, I'd argue] or... look, it doesn't matter what the particular religion is, it's the *same* sort of extremism.

And I'm sure Richard would be horrified if he heard me insinuating that he was an extremist [laughs]. But in one... in this one regard, I *am* critical. Because I don't see how he can be *sure* that God doesn't exist – or something like God – God in some interpretation or another. I mean, he's probably thinking of the naïve, you know, Biblical – an old man with the beard, in the sky, and so on. He's probably thinking of something like that. And I would more or less agree but I would reply, how do you know there isn't some reality *behind* this thing? And... what *I* come down to in the end is saying, okay, I don't *really* think there's an entity in the sky. I think the whole thing is much more likely in genetic memory, that we have... if you like, a gift, or an ability, or a propensity – for some of us at least – to believe in [and *experience*] such a thing; and it's very important for human societies, because it's very important for some people to stay hopeful [in this particular way] – and staying hopeful is terribly important to all of us – whether or not we're believers.

So I'm... I suppose my position is quite close to Martin Rees's. He explains it very well, in his brilliantly lucid way. I'm not religious myself, but I'm not critical of people who are religious – *especially* if they don't insist that I believe what they believe – if they're *not* fundamentalist. If they have a personal experience of spiritual solace, of being part of something larger than themselves that inspires them and gives them hope, I'm all in favour of it. You know, good for them, I say. And I've met people of whom that's true, and they're wonderful people.

Do you have any sort of view on why there's... why there is something rather than nothing though, why... ?

Oh well, now that's... now we're getting into the multiverse, aren't we? I mean [laughs]... Why there's something rather than nothing? It's a rather Hegelian philosophical question, I think [laughs]. I have to say, I regard questions like that as uninteresting, because there isn't a hope of giving a coherent answer. I would just start from saying, we are here, things are as they are. What's interesting to me is to understand how things work – which includes how human psychology works, incidentally, and why it was naturally selected [for], to work the way it does. And I think you can get quite a lot of insight about that. And that includes religious belief.

[0:59:23]

Yes, we'll come back to that when we talk about your recent writings on lucidity and science, which is a key part of that, I think, isn't it? And I know that you'll be able to say that this developed throughout your career, the thinking behind that.

Very gradually, yes. As I say, going back to proofreading George Batchelor's great textbook on fluid dynamics [and even further back, perhaps, to Pickles walking 'through' the bars]. And there are parts of that [Batchelor's textbook] I know are free of typos [laughs]. Erm... and... noticing that lucid writing [Batchelor's for instance, and Lord Rayleigh's] uses repetition – judiciously! There's a distinction between repetition and repetitiousness, isn't there? There's such a thing as overdoing repetition. But it's equally ridiculous to avoid it completely, because what we're sensitive to is organic change, and so, very often, the *invariant element* in an organically-changing pattern of words *is* something like a repeated noun. "We will be serious if you are serious." That's a stronger sentence than "We will be serious if you are." The repeated "serious" adds strength and lucidity. [And conversely, if you want to make it weak and muddy, just go with "the minor novelists and the reporters" – to quote H.W. Fowler – and use gratuitous or pointless *variation*: "We will be serious if you are not frivolous."]

Well, perhaps now is the time then for you to say how you think this early interest, which you... I think you said you think begins editing George Batchelor's...

Only proofreading.

Proofreading George Batchelor's work – how this interest in lucidity developed through your career to the point where, recently, you've been writing three major papers on it – in a series of three which are available on the website.

Oh, I think I can answer that easily. Having noticed one or two techniques of lucid writing, quite early on, of course I *was* interested. Because I had an urge... I thought I'd discovered some interesting things, like the 'upstream influence' or whatever it was – anything in the early papers. Why would I bother to write those big papers if I weren't intensely interested in having understood something? Well... after that, you want to communicate it to others. I found that what I was best at was getting it clear in the written form. I always had a problem being clear [when] speaking [as this interview well illustrates!] because one of the things about good writing – strong writing, lucid writing – is omitting the needless words;

but when you're speaking, if you utter a needless word – as I've done quite often in this interview – there's nothing you can do about it [laughs].

So... let's see... So... the interest in these techniques grew gradually, and I had the struggle of trying to write my papers, and gradually getting a bit better at it. I was lucky that I noticed not to be afraid of repetition, at an early stage. That saved a lot of bother. But then of course I started taking on more and more research students – graduate students or whatever you call them these days [laughs] – and of course you have to read their thesis drafts. And I began to notice that the, you know, errors of the novice tended to be... there used to be, well, there were a number that were always the same. One of them was avoiding repetition, and a few other things like that – not being sufficiently aware of how word-patterns work. And so I gradually got bored with writing the same comments in these drafts, so I decided I... there's some... you know, and I suppose... let's see... Another ingredient of this was becoming interested in perception psychology.

Now I can't quite get the chronology of that, but I remember a wonderful book by Richard Gregory, that lovely man who's very well known for his work on perception psychology. This book is called *The Intelligent Eye*. This came out quite a while ago [1970]. I probably read it, I don't know, quite early in my career. And it was making a very interesting point, which is... well, how I now express it is to say that "perception works by model-fitting." He talks about 'object hypotheses' – but he meant the internal mental models that are [actively but unconsciously] fitted to the incoming data from your eyes, or ears, or whatever. [And 'models' are, of course, partial and approximate *representations* of reality – whether expressed in equations, pictures, computer codes, neural firing patterns, or in any other way.] There are very often many [data] sources – touch, hearing, seeing. All those data are being fitted to internal models, 'cause when you see, hear, or touch something and you think, er... "Ha, that's a stapling machine" – see, I'm sensing it in... all the ways I'm talking about [makes clicking noises with stapler]... different things it's made of... I'm... what the subjective experience is, I see a stapler here – I see, hear, and feel a stapler – but the only way I can do that is that this internal mental model is fitting all those data. The perceived reality *is* the internal model.

Once you understand that, you understand a whole lot of other interesting things – such as why there can be such things as hallucinations. Why, if I were in a certain mental state, perhaps drug-induced, might I see the Devil with all his horns sitting on that chair, with every whisker perfectly real? People have experiences like that. I haven't actually had any such vivid experience, but they're often reported, aren't they? People with schizophrenia hear voices. And those voices are perfectly real. Okay – so the perceived reality *is* the internal model.

Once you understand that you understand a whole lot of things about how perception works and – because I was always interested in *music* – I began to understand a lot of things about how music works, as well. [*That's*, of course, how they got the whole orchestra into that "thin little disc"!] And that, naturally, spun off into how strong writing works. And that's how the *Lucidity* papers originated. I wanted to try and put down a coherent account of all this, in a way that would interest a young scientist – my students in particular [laughs] – so that they wouldn't keep boring me with these drafts that kept making the same mistakes.

You know, one of the most brilliant students I ever had – he's one of the two or three best; I class him with Jim Woodhouse and one or two others – his name was Rupert Ford. He died tragically, actually, that was a terrible thing – just when he was getting launched into a meteoric scientific career. And Rupert was very smart. And, you know, he was one of those [rare] students with whom one tended to feel early on that he was supervising me, rather than the reverse. You know, I couldn't get anything past *him* without having a jolly strong argument for it [laughs]. And it was very stimulating.

And I remember Rupert presenting me with a draft of one of his early bits of writing, and I saw he was making some of these novice mistakes – including avoiding repetition. [Another was *not* being about twice as explicit as he felt necessary.] And you see, when you're young and very smart, you always [tend to] make this [kind of] mistake. You don't want to insult the senior person's intelligence, do you? You don't want to make things too obvious. To which the reply is: no, you want to make things *very* obvious and trivial. And... remind me to tell you the story of the Adams–Airy Affair; that's one of Littlewood's stories.

But anyway, I pointed... I got Rupert to read... I think it was just Part I of *Lucidity and Science*, which I think I had a draft of by then. This was the early '90s. Yeah, I probably had an early draft of that. And I said, "Read that and take it in, and then have a go at rewriting your draft." Next day, ping [snaps fingers], suddenly it was lucid. The penny dropped immediately, with him. He could see the point, and he knew what to do with the draft. I didn't write any detailed comments at all [laughs]. So that was a great joy. And, you know, from time to time I meet people who say they find it helpful.

[1:07:14]

Has undergraduate teaching had any effect on the development of these interests?

Well, of course, the task of having to give lectures... and I wasn't naturally good at that because, as I say, I'm better at writing, when you have a chance to redraft things. But, you know, I gradually learnt to give a moderately decent lecture [laughs]. I ended up... I mean, there were incidental reasons. As well as not being instantly clear what to say next, I tended to make mistakes on the blackboard as well, so I had to proofread my blackboard writing, which was all a bit of a... a bit tedious for all concerned. And the students are never quick to point out mistakes on a blackboard, so sometimes you get into quite a tangle. So I ended up always producing printed handout notes.

And by the time I got to that point, I think I was quite successful as a lecturer. In fact on my website you'll see some notes on group theory that I'm quite proud of, 'cause that... it was an aspect of group theory that's quite difficult to get clear. [It's called 'representation theory'.] And I inherited some very complicated lecture notes written by experts. In a way... You see, I'm not an expert on group theory but, in a way, it's *better* to lecture on something you're not an expert on 'cause then you see it more like the students do. So I was able, I think, to produce a *much* more lucid version of those notes, which is still on my webpage, in case anyone's interested [laughs].

And what I did in the lectures was hand out the notes for that lecture, and put a copy up on the overhead projector, and have a pointer on the bit I was talking about. So there was no doubt... nobody was hunting around to see where I was. (There's always this danger when you've got a lot of stuff in front of you, you don't quite know where to look.) So I was always pointing to where they should look and I was... the lecture consisted of highlighting the highlights [aurally and visually, at the same time] – underlining the key bits of it that they should not miss. And probably the students taking notes... most of them, I think, would have underlined the same bit. And I think that would have been helpful to them after the lecture.

How do you feel that your verbal performance or, you know, the more performative aspects of it improved? One of the things you've said is that you produced lucid handouts, instead of trying to write up on the blackboard as you were going. But moving from someone who you said that you didn't feel was good at it, to being someone who was good at it, what did that involve in actually doing it at the front?

A long struggle [laughs]. I mean, it's not trivial, preparing a good lecture course. It's in a way harder than even writing a book, because you've got to make it short enough to fit into the time available. You've got to find useful abbreviations, economical ways to write the equations, judge which equations to leave out. You can't put every detail down. You've got to put enough down, though, that they can reconstruct it afterwards. So, as I say – a long struggle, that ended up with the handout-note technique, in my case. I know its... some of my colleagues are brilliant at the blackboard. They are fluent, they can write clearly and yet faster than I can, and somehow seem to avoid making mistakes. I think they've probably got clear notes that they work from, although I can tell you one exception to that.

One of my pure-maths lecturers back in Dunedin, when I was an undergraduate, gave perfect lectures [Dr Warren Wong]. I never caught him out in a mistake. He wrote out... this was lectures on modern algebra: abstract algebra, matrix theory, and minimal polynomials and all of those things. He gave lectures, on that, completely out of his head. He must have had an eidetic memory, like David Crighton or Wolfgang Amadeus Mozart. He just had it in his head, and he wrote it neatly out on the board, and talked about it at a nice leisurely pace. It was quite easy to take notes. But [as said earlier] I don't have that sort of memory, or... well organised... I don't get anything well organised until I've struggled with it and rearranged it quite a number of times [on paper or computer, or both].

[I think Crighton and Mozart did the same sort of rearrangement, but mostly in their heads – though I do have a Mozart autograph showing that, contrary to folklore, he sometimes had nontrivial second thoughts *after* writing things out. In the second movement of the great F major piano concerto, K 459, there are messy crossings-out where he completely changed the shape and rhythm of bars 66–78.]

[1:11:10]

You asked me to remind you of the Adams–Airy affair, a Little...

Yeah, because that's one of the stories Littlewood tells in his *Miscellany*, which I've mentioned before – which is wonderfully entertaining if... [for] anyone interested in... the adventures of the intellect, as it were... I mean... of any sort. I mean, Littlewood was a mathematic[al] function theorist, but... it applies to any, you know, endeavour at getting... at getting any depth of understanding. So...

And he has bits about what I call 'lucidity principles'. He's got a little bit about the inexperienced research student writing something that's almost unintelligible, because he [the student] thinks he's just got to put down all the gory details, and hasn't thought of a way of, you know, saying what the main point is. He's [Littlewood has] got a little passage where he says the... a well written... and I can't remember the exact words, but something that's well written... is organised in such a way that you can take in the essential idea at a glance.

If you want to check a detail, you're given a little problem to solve, which you can, if you want to be sure. Or you can skip it without losing the thread. The inexperienced writer gives the reader "no such chance," says Littlewood. And then he gives an example, this absolutely gory [mass of equations and symbols], proving a certain theorem of Weierstrass [laughs]. And it's a complete *haystack* in which there are all sorts of needles – including misprints – so you have to... you can't get the essential point without teasing your way through every tiny detail. And then he contrasts it with a 'civilised proof'.

He says, "A civilised proof is as follows." And he starts with a picture, of course, because Littlewood... I mean, he always said, he always thought in pictures whenever possible, but of course you have to use symbols as well; so his stance on that is rather like [Lighthill's and] mine. So he presents a little picture. You've got to consider this part of the plane [he says] and, you know, you have a certain function in this rectangle, and you have to match it to something else. So the idea is perfectly clear. And then you relegate the technical details to, you know, a few little well-defined statements that can be checked if you so wish. So that's his take on 'lucidity principles'.

But the other thing is... this other story, the Adams–Airy Affair [as recounted by Littlewood, and imperfectly remembered by me:] – this is John Couch Adams, the... who was a brilliant... like my Rupert Ford, a brilliant theoretician, who had all the mathematical details clear in his head, and he did all these complicated celestial-mechanics calculations. And he did a calculation about why the planet Uranus was going slightly differently from what it would if it were the outermost planet. You know, it would slow down here and speed up there. And this was, y'know, known because of very accurate observations that were by then available. And Adams looked at this, and worked out that there has to be another planet, there has to be a planet outside the orbit of Uranus. And that's... is the planet that we now call Neptune.

So this was about the discovery of Neptune. And famously, not only Adams but a Frenchman called Le Verrier did this calculation, and what actually happened was that Le Verrier asked the French Astronomical... you know, the observatory [in fact the German astronomer Johann Galle in Berlin] to point the telescope to find this planet, and they did [in

1846]. So he... they had the glory of the first discovery of Neptune. Adams was trying to get our Astronomer Royal, who was Sir George Biddell Airy, he of Airy functions – trying to get him to point the telescope. And Airy was sympathetic and, you know, he could see that, you know, perhaps they ought to consider interrupting their important routine programmes of observations to accommodate this young man's bright idea. But first of all he had better be convinced that the young man knew what he was talking about. So [in a letter from Airy to Adams]: Mr Adams, could you possibly tell me this, that and the other about the technicalities of the calculation? [laughs].

And Adams made his fatal error because Adams – being afraid to insult the great man's intelligence – wrote back to say, oh, that's just a trivial matter, you don't have to worry, or something like that [actually worse: according to Littlewood, Adams sent no reply at all – saying later, in private, that he'd considered Airy's question trivial], whereas what Adams should have done was to try to answer the question seriously in full detail. But the twist in the tale is that, if Adams had done that, he would have discovered that what he'd said in the first place [or, rather, what he'd *thought* at the time – about Airy's question – and said during the Royal Astronomical Society's post-mortem a year later] was wrong [laughs]. He'd actually made a slip. And that's the other point I always make to my students. Just by trying to write something – to get a fully lucid description of something you've done – is valuable to *you* because you will pick up your own mistakes that way [laughs].

[Another interesting point about Adams' slip is that he seems to have been *too sophisticated* – failing to look at the problem from more than one viewpoint, such as "noticing very simple things" as I put it earlier.]

[1:16:16]

You said that when you were younger you dreamt of... you had flight dreams, and a great interest in flight, and that you'd been able to realise that later, but I wonder whether you *could tell the story of that. I gather that you might have learnt to fly; perhaps I misunderstood?*

Yeah, yeah. [Answer is yes!] I think I've said enough about childhood flying dreams. I mean, you know, silly in a way, just trying to get airborne by some amazing physical effort, and in the dream succeeding of course, and staying up and loving the feeling of it, although it was always hard work, you know [laughs]. But that was all there was to it really. I didn't have any great adventures. I didn't go to fairyland or, you know, go anywhere in particular. I simply was getting airborne, that was all there was.

But what I was interested in is the story of how you...

So now... that was just part of my psychological background for ages and ages. And of course I read books on gliding, and occasionally watched gliders in the air, and had a vague longing, I'd love to try that. I think my father... you know, it goes back... well, of course, when I was a little kid I was interested in model aeroplanes, and I think my father gave me a book on gliding. [Or maybe the book was just there, on a bookshelf at home.] Yes, it was called *Flight without Power,* by Philip Wills. Philip Wills was a well known, I think British, glider pilot... Probably this goes back to the 1930s... But it was a beautiful book, with photos of wonderful sleek gliders, and clouds of various sorts – you know, you have to get very familiar with different sorts of clouds if you want to do gliding.

And in those days there weren't any two-seaters, if I remember, so when you were a learner you had to just go by yourself, you had to go solo straight away. But there was a sort of low-level glider that was a bit like my stick model aircraft – almost. Didn't have much of a proper fuselage, just a few girders and things, and a rather crude wing, so it was enough to get airborne in. But if the pupil crashed it, it wouldn't be too big a loss, I think the thinking was [laughs]. So... and you had that level, and then you had another level and, finally, you had the high-performance things that you would graduate to.

Now... so, these were just dreams, most of my working life, because I didn't consider I could afford... gliding seemed like an expensive luxury, both in time and in money. The family would come first, of course. But when the family were grown up... I was once on a visit to colleagues in America, and one of them, still a friend of mine, a man called Murry Salby – I have a disagreement with him about the climate system, but that's by the by. Murry and I are good friends, and he was a glider pilot and took me up, and let me do quite a bit of the flying. So I had the feel of actually flying in thermals and things, from that. And that sort of heightened my desire to learn to fly. And I was freer to do it by then, and decided to have a shot at gliding at the Cambridge Gliding Club.

And what really got me hooked... oh, I missed out something. I did have an Acorn microcomputer. Do you remember those... the BBC Microcomputer made by Acorn? This kind of takes us back to, I suppose, the '80s, doesn't it? When did they... it was sometime around then. It was a great success in the schools and everything. Anyway, I happened to have one of these things, and a piece of software called Aviator that actually simulated flight dynamics moderately well... Must have been some very clever programming to get it into that tiny computer. So there was a crude graphical display of the view from the flight deck, and you had a little joystick. So you could fly on the joystick. And I never got around to building a rudder... some rudder pedals [were allowed for], but it flew all right just on the joystick. Some planes do this better than others, by the way. With a real glider you *must* have rudder pedals, otherwise you skew all over the sky. You've got to use the pedals and the stick together. That's because the aileron drag on the long wings is so enormous [or rather, its moment arm, hence torque].

But anyway, I learnt to land this thing [the flight simulator]. It was actually much more difficult to land the computer plane than a real plane, because you have much less visual input – much less accurate sense of your distance from the ground, which is a bit critical [laughs]. Anyway, I did a lot of practice on this thing, rather obsessively, for a few years, and finally went to the Cambridge Gliding Club and got myself a trial flight. And the weather was beautiful. The view was enchanting. You know, it was one of those showery light... well, fair-weather cumulus rather, not showery. But, you know, the dappled sunshine on the green fields was a beautiful sight. So it was just magical.

And so I... once we'd got off the ground, I was allowed to take the controls and stooge around, and try a turn and so on. And by the time we had to come in to land, he actually let me land the glider myself! This was a huge surprise. I thought you'd have to have many sessions before you'd be allowed to do that. But actually it was amazing how well instinct serves you... You know, in... you know how gliding works. There's a standard routine for landing. You first fly downwind to just beyond the end of the airfield, then you make two turns to come back the other way, into the wind, and land safely. And on the final turn, that's a little tricky: you've got to get that right. You might stall or something nasty, and you're quite near the ground by then [laughs]. So I... that was a bit wobbly when I did that, so the instructor sort of said, "Oh, I'll straighten her out" and he seized the controls for a moment, and got her heading nicely in the right direction, and then he let *me* do the rest of the landing! He just said, "Okay, now you can land her." And the sheer confidence[-building value]... I thought... and I sort of managed to land without too much of a mishap. And that was such a thrill – complete, wonderful surprise.

And that got me hooked, you see. I joined the club. And I first went solo in 1990, same year as I got into the Royal Society. 1990 was a red-letter year for me in two ways [laughs]. So I went gliding quite often for, I don't know, ten years or so, I think, before I finally felt I didn't really have time for it any more. I had one or two negative experiences. There was one instructor who... you know, the trouble with it is that you've got to spend a lot of time standing around and pushing gliders, and helping other people get launched. And that's fun for a while, but after a while you begin to be conscious that the time you spend flying is a rather small fraction of the time you devote to the sport.

And there was one occasion when I was, you know, doing a flight with an instructor, I don't even remember whether... it must have been after going solo. They must have been doing some sort of check [which they do routinely, of course]. I really don't remember. But for one reason or another this instructor grabbed the controls; he said, "You're at the wrong angle," he said, "Do it like this." And he grabbed the controls. And that I really resented, because it was depriving me of those precious few minutes of practice when I could have, with *my* hands on, corrected the fault [and of course *learned* about it much better]. I could have gone steeper if he'd just given me another second or two, or shallower, or whatever it had to be. (This is a winch launch. You go up very steeply – it's rather thrilling [laughs] – and it all happens very quickly, of course.) So this guy... I really resented that. Here, I'd been hanging around for an hour and a half or so, and got my first flight, and all of a sudden I wasn't getting the practice from those precious few minutes. And so I sort of got turned off it a bit more. I kept thinking, well, if I can find another club, or perhaps just do it in summer camps or whatever – but that was just one little incident, I suppose. I mean, the rest of the time my experience was pretty positive. But I suppose what really happens is, you have enough of it to satisfy the original longing and urge and so you... your attention turns to other things. I think it happens to all of us.

And how did you find the experience of flight itself?

Oh, thrilling – especially the first time I went solo. I mean, that was another huge thrill – that they would trust me to take a little glider up all by myself – of course, you know, trying to be careful to get things right and so on. They don't let you go solo till you've gone through quite a lot of routine... including getting out of spins, very importantly.

Did it key in in any way to... I know you worked on the stratosphere, and you weren't gliding in the stratosphere, but did it key in in any way to your scientific knowledge of dynamics?

Well, generally it was a thrill to be actually feeling the motion of the atmosphere that I'd thought about for so long, even though the things you feel are much smaller-scale motions than the ones my professional work has been about. But it's part of the background to know about those smaller-scale motions. I mean, when you're gliding you're in the convection currents. In this part of the world [near Cambridge] that's about the only way you can stay up, thermal convection. There aren't enough hills for any significant hill, you know, lee-wave or [gravity-wave] lift or anything like that.

And can you just say a little bit more about... I think you said that you obsessively played the Aviator or ran the Aviator?

Aviator was this program – this flight-simulator program... But... by today's standards rather a crude thing, but it did simulate the dynamics of an aeroplane in quite a reasonable way. I think it was solving the equations of motion.

And why did you say that you practised that obsessively?

Because I was... that was before I had done much [real] flying. I think I'd gone flying with Murry in America. Or it might have even been before that. I was still subject to this longing to fly – for one reason or another – and doing it on the computer was the only available way at the time.

So how often would you go on the computer?

Well, because the computer was sitting there at home, I could easily spend half an hour on it every evening, without losing too much momentum in other directions. You know, if I had lecture-preparation pressures I probably wouldn't fire up the computer, but, you know, there are plenty of odd moments when you can. We weren't playing musical concerts by then, so I didn't have the need to practise in the evenings.

[1:27:13]

And when you did stop gliding, I think you said around 2000, did anything take over as the sort of next hobby or next interest?

Hmm, it's a... I'm trying to think about the history. Gliding sort of gradually faded out. I actually kept paying my membership, but I discovered I wasn't bothering to go out there any more for, you know, two or three years, and then finally I resigned my membership. I still had an idea I might go on a summer camp and finish my Silver C. You know, the Silver C is the first big qualification. I got the Bronze C, that's the level you've got to reach in order to be allowed to fly cross-country.

But then I was stuck, because our car didn't have a proper tow bar on it. So I couldn't offer to retrieve other people's gliders. When you go cross-country, you've got to make a deal with a fellow glider pilot and say – okay – if you pick me up if I've got to land out – you know, part of the training is how to land in any field – then I'd do the same for him or her. And I couldn't offer that, 'cause our car didn't have the right bit on the back [laughs]. So I was a little stuck. I wasn't sure how to do that.

I had this dream of perhaps going on a summer course where that part of it would be organised for us. But I never got around to doing it, because it was quite hard to find a club where you'd get enough flying. You know, you expect to pay more, obviously; but they have to be very well organised for you to get enough hours in the air. And, you know, you might go on the camp for a week, but would you get suitable cross-country weather or not? It's a bit of a gamble. I ended up not getting around to it [laughs].

I've got all my Silver C apart from the [cross-]country distance. You've got to do a height gain. I got that locally, 'cause the cloud base was high enough one day and one of the instructors said, "Hey, why don't you go for the height today? Look how high the clouds are.” And I did. And those... this was quite a while ago... you had to smoke this piece of paper, and wind it up on a mechanical barograph [drum] to prove you’d been so high.

You don’t do that now; it’s all done on GPS, of course [laughs]. That makes cross-country flying a lot easier too. In the old days you had to be sure you could recognise particular landmarks; they’re called turning points. You had to fly to a known landmark, go around it, and photograph the landmark during the turn, so it was plain that you’d got past that point. But again GPS, I think, takes care of that sort of thing now.

And so did another enthusiasm replace gliding once you’d... ?

Well, it’s a good question. I’m not aware of any... gosh, what happened? Because the gliding... interest in the gliding sort of faded gradually. I think part of old age is that your desire to do these things gets less intense. Alright, at some point... well okay, we did have this burst of musical activity. I told you, didn’t I? When David Crighton died, our visionary Head of Department – have I spoken of that before?

You’ve said a little, not of the music that happened.

Yeah. I think I mentioned that David was a visionary Department leader, and an inspiring leader – one of these people who believe in life-enhancing things, including encouraging everyone to give their best professionally. And including a love of music, ’cause music was very important [to him]. He always used to say, he really wanted to be a musician. And being an applied mathematician was the second-best thing. But he was an absolutely brilliant applied mathematician, I have to say, as well as Department leader – the sort of person who could cope with the bureaucracy in the way I can’t possibly.

You know, he actually enjoyed playing their silly games, and being a few steps ahead of them on a committee. You know, David was very skilful... A piece of nonsense starting to rear its head... he'd put it down with a few beautifully chosen words. He would keep us on target about the things that matter.

So that was David. He got cancer and died in, I think, 2000, and his widow asked us to play a memorial concert, 'cause she knew we'd played professionally in the past. And *we* wanted to give David the best concert we could, so we worked very hard at that, and we gave the concert in May of 2001 if I remember correctly. Well, it's on my webpage, if you're interested. There's still a few CDs of it left!

But anyway... he... you know... *that* would have been fully eating up my spare time around then, and I think that might be the answer to your question. [Or part of the answer. I now also remember that part of the buildup to the Crighton concert, over several years, was a big effort to deepen my knowledge of musical composition. I think it was bound up with the struggle to write the *Lucidity* papers.] That would have completely killed off any remaining interest in gliding, doing that concert. 'Cause you see, we... it wasn't just workin'm up. We thought we must do our best. It's a long time since we'd played to a high standard; we'd better go and, you know, try it out in front of, you know, people who know what they're doing. And in fact, we went to... Two summers in succession, we went to a sort of summer school in the north of France, where members of the Lindsay Quartet were the coaches, and we got some lovely coaching from them. Bernard Gregor-Smith, the 'cellist, and Ronnie what's-his-name [Birks] who played second violin, especially, very good and encouraging. [On checking my records, I find that only the first of those occasions was before the Crighton concert. Evidently we were still switched-on enough to go for the following summer as well – working on an amazing piece, Villa-Lobos' Piano Trio no. 3.]

And then we did another session of being coached by Andrew Watkinson, the leader of the Endellion Quartet. Andrew's an artistic genius. He has a wonderful way with the violin, of widening the range of voices it can speak in, and so I've always... we've always admired him very much. And so we got him to listen through the things for this [Crighton] concert, when we'd got them to an advanced stage of preparation. And he was very good, of course, in honing up some parts of the performance. And then we finally gave the performance, and it was a great success and it was even good enough to have a recording issued afterwards. It'd help with the fundraising. Because Mrs Crighton's idea was to raise some funds in David's memory. You know, there was a mathematics fund, and a music fund, and there was... well, some of the time we were raising money for both, and some of the time we were raising money just for the music – that's a detail. And that was a wonderful occasion. It was very well attended because David, of course, was very well loved and admired by very many of our colleagues, so we had a full concert hall in the West Road Concert Hall over there. It's a beautiful acoustic. And that was a great occasion.

[1:34:10]

So that... now, you might ask what I was doing after that. We did have... having worked ourselves up to a professional standard of performance again, we did play in one or two other things. Well, a friend of ours... they had a wedding anniversary, a fortieth wedding anniversary, I think, and they invited us to play a little concert for them, which was a lot of fun. They're very special friends. There was at least one other such occasion, I've forgotten. [And we did make quite a decent recording of the Villa-Lobos trio, around then.]

So for a while we were all playing music together as in the old days; but finally that died off. I think we realised we weren't going to be able to keep up the intensity of that. You know, we'd been there, we'd done that, we wouldn't have missed it for worlds – but then it was time to move on to, you know, facing old age, and perhaps seeing that the kids were okay, and seeing that the house was okay.

In recent years I've spent quite a lot of time on, you know, just things around the house. I love using my hands. I was very keen to get, you know, a bit of renewable energy into the house. It's an old house. [Fairly old – 1938.] The insulation standards will never be properly passive or anything. Y'know, it does have a cavity wall that's been pumped with foam, but that's not really [indeed, not *nearly*] good enough by what's... we need to do. But, you know, we did all the standard things, like huge loft insulation. And we've got solar photovoltaic panels . We managed to get in while the government was giving a generous so called 'feed-in tariff'. So – actually that was like using... putting some of our savings into an ISA that's about double the usual interest rate [quadruple now, as the bankers continue their power games], so that was a good deal, financially, as well as being... helping a little bit with the cultural sea-change that we need for renewable energy – a tiny, tiny drop in that ocean.

We've got a beautiful solar-thermal system as well, developed in Germany. It uses the available solar power very cleverly, mostly for hot water of course, but a bit for central heating as well – and a beautiful piece of engineering. [Including the way it manages the fluid dynamics!] So that's been fun, getting to know that, in a slightly perverse way because the... it's funny, it's a beautiful system, but the manuals are written in Martian – like the average computer manual – and I had to spend a lot of time discovering how the system works, by experimentation [laughs].

Go on, say how you did that.

Oh, it's boring really. I mean, you know, you'd read these manuals, you'd manage to decipher some of it. They... Perfect example of anti-lucidity, you know – highly technical stuff, but always using different names for the same thing, and the same name for different things, all over the place. [And there's a *huge* number of technical things!] So you had... it was like decoding the Rosetta stone. So I... my copies have huge handwritten notes, written everywhere. So I did that as much [as I could]... and then, of course, they don't... there were some things they just don't tell you; it was not complete either. So there are some things... the only way to find out is to try it, you know, try different settings and see what happens. Boring though, you know, you've just got to try and stick at it.

But I've reached the point where I pretty much know how to work it. I like to play games with the outside [temperature] sensor. The idea of how it controls the house temperature is beautiful. It's a very smooth control – a closely accurate control that's based on sensing the outside temperature, with a little bit of input from room temperature in the main area. And it's got a lot of artificial intelligence that translates that into the amount of heat output, again, in ways that I had to discover by experimentation. But I've got it working, nicely tuned up. [The outside sensor has to be positioned, and radiation-shielded, entirely differently from what the manual says!] It keeps the temperature nice and even, and... copes with the frost.

That was another problem: the installers mis-installed the pipes to the solar panels. This system works by draining. When solar power isn't available, it just drains all the water out, so you don't need any nasty anti-freeze chemicals. You just store the heat in a huge tank, which is quite well insulated. But the installers didn't realise that the pipes from... between the panels and the tank had to be downhill all the way, otherwise water would accumulate and freeze in the frosty conditions – and not just stop it working, but possibly damage the pipes as well. But luckily we got away with that. I managed to get the pipes re-sloped *myself*. I would get a... got a ladder and got up on the roof, and fiddled around with aluminium angle, and struts, and this and that, until I had the pipes properly sloping. So it works fine now [laughs].

I like doing that sort of thing. It's... you know, it's a low level of experimentation, if you like; but it's satisfying to see something working properly.

And have you used personal computers – other than for making music and briefly for flight simulation? Are there other sort of leisure uses of personal computers?

Hmm, well, we all use computers for practically everything these days. The answer is essentially, yes, I've always had to use them for professional work. I mean, you can't go to a conference without a computerised talk these days. I was one of the last to go over from the old overhead transparencies to computerised stuff. But, well, I mean, I've done that for ten years, I suppose, by now. So I've always had some sort of laptop or other to do that, and naturally you... you know, when your kids say there's this or that on YouTube, you can't... mustn't miss it, I'll use it to see that. So you just use it for whatever comes up.

I don't do twittering, tweeting and facebooking. I *was* on Facebook for a little while, but I discovered it wasn't helping me, because the people who wanted to know me were the people I knew already, so I'd have their emails anyway, so there's not much point. I think if you're very young you have to, 'cause that's what all your friends do. They don't have email, they have Facebook instead, don't they, so that's a different world. It's terrifying, though, because the owners of Facebook owns your information. They have copyright in everything you do. I don't like that. They've got much too much power over people's personal lives, potentially.

[1:40:24]

And what was the effect on your career of becoming an FRS?

Oh, well, not huge. Of course it's awfully pleasing to be elected to the Royal Society. It's a big honour. It's a mark that things you've... some of what you've done is regarded as having been important and influential. But... And of course it brings some responsibilities. You then have to be prepared to, you know, act as a referee in ways that you wouldn't have before. But a lot of that's very positive. I mean, I've helped to get other people in who, I've strongly felt, deserved to be FRSs. One of them was David Crighton actually. It's amazing that he didn't get in before me, because he was an absolutely brilliant... but, you know, it's rough justice [at best]. There's a certain chanciness about these things, and it just happened that I got in first. So I had the pleasure of helping to get him in [laughs]. [And anyone who suspects mere cronyism need only read David's papers – it was very easy for me to make the case. And by the way, no-one gets in unless *several* international referees are strongly positive.]

What is involved in helping to get someone in? What's the sort of criteria?

Well, you see, I'll tell you why it was a pleasure. It's because what you have to do... you have... well, you might or might not get asked, but I *was* asked to be one of the referees for his election. And, well, it's like being referee for anything else, but you've got to argue that the guy [or gal]'s done something to a very high standard of originality, as well as technical prowess and creativity. [And honesty and integrity counts heavily too.] With David, that was not difficult. So I mean, most of his work was a bit... you know, it wasn't quite in my area, so I had to work quite hard, reading quite a few of his papers. But that was a great pleasure, because he was a lucid thinker and writer, and what I read – a lot of it – had all sorts of exquisite surprises and wonderful things, little twists of, you know, understanding something. That sort of thing gives me great pleasure. And so I was able to say, look, this guy does this kind of thing, so he certainly deserves to be elected. That's the way it usually works.

You know, if you're electing an experimenter you'd say... talk about the clever experimental technique, or how he developed this way of observing something that had never been observed before, or whatever. You know... it's always an argument about something new – something that's not just clever, but actually has some importance – [something that] makes a difference to scientific progress.

[Susan Solomon, an American atmospheric chemist, was another outstanding case among several I can think of. It was not only for key contributions to understanding many of the complexities of atmospheric chemistry, including ozone-hole chemistry, but also for chairing a recent IPCC report with fierce integrity and great tenacity and courage, always up against the power of the professional disinformers – clearly and soberly speaking truth to power. And, to my great delight, as a result of all this she was recently elected a Foreign Member of the Royal Society, a very high honour indeed.]

[End of Track 7]

Track 8

Could you say something about the use of potential vorticity in numerical weather forecasting?

Yes. Well, I've always had this... dream... er, a pipe-dream... a sort of wish list – because in my work it's been conspicuous for ages that you get a lot of insight by just looking at potential-vorticity distributions. And the sort of dynamics that that informs you about is exactly the sort of dynamics that applies to the development of weather systems. And so you'd think that it would help a weather forecaster to be able to see the PV distributions, and... Of course there's a little technical proviso: you've got to include surface temperature as well. (For systems like... weather cyclones, fronts, and anticyclones the temperature near the ground is actually of comparable importance to the PV. In fact, we think of it as part of the PV field.)

But anyway, you've got... visualising those things is insightful about the dynamics, at least qualitatively. Now... so this made me think... and I actually wrote an essay on this called *A Dream* [actually *Vision*] *of the Future* [in fact three versions were published, of which the last was called *Numerical weather prediction: a vision of the future, updated still further*], in which I imagined that the full potential of this idea was realised with, as it were, *ace forecasters*, who become real experts at seeing the complicated three-dimensional features of the potential-vorticity and surface-temperature fields – and aided by modern computerised visualisation. Perhaps some day in the future it could all be done holographically, or something. Somehow you could get the three-dimensional picture in a very quick and intuitive way. And that perhaps there will be ace forecasters part of whose work would be to – almost – fly among these features and grasp their three-dimensional nature and, through their long experience and intuition, see... spot the cases where the forecasts needed correcting [and how to correct them].

And this is rather against the usual culture in the weather-forecasting world, which quite reasonably says that... well no, these things are too complicated for human perception to have any role at all; and the whole thing has to be automated and computerised. And to some extent they're right. It *is* complicated, and indeed the progress that's been made with computerised systems and, as I said before, four-dimensional data assimilation... a lot of that really has to be automated.

However, of course if you go into it a bit more deeply and, you know, if you talk to people like Adrian Simmons, who's in that business – my first student long ago – and... he'll... you learn that actually human intervention still goes on, because... of course [for one thing] the data that comes in isn't all correct. You've got to be able to detect when people report a radiosonde balloon sounding that they didn't really do, you know, because some bureaucrat paid them to put in a report, but they didn't happen to have the balloon to do it, or whatever the reason. And so the people in the business actually know that some stations are suspect, and they have sort of a balance of probabilities on how much – which data you believe more – and that tends to get built into the system. [But kept under review, i.e., subject to human judgement and intervention.] And, you know, looking at potential-vorticity fields at least has a potential for detecting that sort of thing as well, though I don't really know how useful it would be.

However, that was my dream of the future. I don't think very much has come of it as yet, but there are sort of signs – slight signs – here and there. There's a chap in the French Meteorological Service called Philippe Arbogast, who has constructed a PV inverter. Of course you'd need a PV inverter [an accurate one] if you were to use this operationally, because you'd need to do things like... hey, my experience tells me this feature should have a different shape: I'm going to redraw it and I'm going to invert. Now I'm going to have a different set of wind fields. I'm going to re-run the forecast with a slightly different wind field, and see if it looks better. This is all assuming that great human experience, and high human skills, *can* make a contribution, doing that sort of thing. Well, he [Arbogast] and one or two others, I think, are sort of working at it and hoping for some developments. That's all I know. I haven't been close to that business in recent years.

[0:04:48]

What makes you think that it is a combination of the human and the computer that is needed, rather than the computer?

Okay. That's a very good question; and I think it comes from recognising that human perception and intelligence is still much more powerful than our electronic computers *in some ways*. And a good way to make that point is to point out that computers don't yet drive taxis, do they? Artificial-intelligence people have been *hoping* for machine vision to reach that point, but it's been very, very hard to get it there. And you can... you know, if you think about how perception works you can, I think, have some insight into why. It's because it requires massively parallel computing. You have to deal with a combinatorially large number of possibilities at once. That's all related to this unconscious power of abstraction that we have. And electronic computers aren't yet as good as we are, at some of those things. So one day perhaps computers will be built with the sort of massive parallelism that our brains have. But it's a tall order because, you know... well, if you read Kevin Warwick's book, which uses the old naïve view of the nervous system as nothing but neurons and synapses and nerve fibres, [with the neurons as] simple switches – that's only the tip of the computational iceberg. The real nervous system is much, much more sophisticated [and its massively-parallel power much, much harder to match than the book suggests].

It has many levels of computation, all the way down to the molecular level, in which computation is done by molecules called allosteric enzymes, which are protein molecules that can go into different states depending on what's attached to them, and then in turn catalyse different chemical reactions. So they're like transistors, you see. [Or 'logic element' might be a better term.] And these days people who work on these problems talk about protein 'circuits', meaning... using this sort of logic element. So that... of course you've got lots of levels of that going on within a *single* synapse. So our massively parallel computing – we've barely scratched the surface of understanding how it works. I think it's an interesting open question as to what combination of human and machine intelligence is most powerful.

I would argue that some combination *is* [sure to be] powerful. And if we do things that use our powers of visual perception – which is a very sophisticated part of our perceptual apparatus – then there's scope for doing better than we could do either by ourselves, or the machines by themselves. So I still have that dream, although it's, you know, hardly got going as yet.

[0:07:42]

Could you now tell the story of your... the development of your interest in the Sun and...

Yes, okay. Well, that's another thing that had gradual beginnings, purely by chance, I suppose. I've always been personal friends with one or two of, you know, the great thinkers in solar physics and astrophysics. One of them is a man called Edward Spiegel, who ages ago taught me... began to teach me what I know about solar physics. And the other is Douglas Gough, who is now known as one of the great gurus of what's called helioseismology, and that is the ability to sense the structure of the Sun using the sound waves that bounce around inside it.

Now it's a piece of scientific good luck that the Sun is a very... what we call a high-*Q* acoustic oscillator. It means that the surface – it's somewhere near the visible surface of the Sun – is an *excellent* reflector of sound waves. That's just an accident really of how the, you know, profiles of temperature and pressure work out. So if you start a sound wave inside the Sun, it tends to bounce around a lot. And it's like... to excite the Sun is like hitting a gong, you know, it rings for a long time. And if you observe accurately these oscillations – and there are very sophisticated instruments that have been doing this for many decades now – you can analyse their frequencies, and sort out which modes of vibration the Sun is oscillating in. It's a bit like a violin string, which has many modes of oscillation. But the Sun has even more. So you've got an awful lot of information about those frequencies of acoustic oscillations, and what you can do, from that, is to back out the internal structure of the Sun in an amazing amount of detail. And that's one of the most powerful constraints on our theories of the Sun's structure, and... on what we base things like how much longer is it going to last, which is a few thousand million years. All of that – things we believe we know about the Sun – we know with very high confidence because the models of the Sun, the computer models, are all checked [very accurately] against these helioseismic observations.

So that's a great scientific story that my friend Douglas was... has made a large part of his career. (And he's another lovely scientist. You can talk about anything with Douglas and, you know [as with

Nick Shackleton and others I could name], have an argument – "oh, that's rubbish," you know, and sort of get to the truth of the matter – without any [fear of] personal resentment.)

So, now, one of the things you can get about the Sun is its differential rotation. Now that leads us to a fluid-dynamical problem in which I got involved... you know, many years after first learning about the Sun. And... if you picture the Sun as a sort of orange with an extremely thick rind – and the rind is meant to be the convection zone, and the core is the interior, and... what you have is... [trying first to clarify the significance of the rind-versus-interior distinction:] There are two different ways the heat gets out of the Sun. Okay, where's the heat coming from? It's nuclear reactions near the centre. And in most of the... in the interior, the heat comes out purely by radiation. That's to say, photons sort of push and jiggle their way outward. And the flow of heat isn't helped by any fluid motion at all. However, as you get nearer the surface, conditions change in such a way that the most efficient way to get the heat out is by convection. And that means great gobs of fluid are... it starts boiling. There's a layer – this we call the convection zone; that's the very thick rind of my notional orange – in which you have great gobs of gas sort of boiling upwards and downwards. And that's the main way the heat is carried in that part of the Sun.

[0:12:13]

And what you observe... and now, helioseismology has shown us something very interesting. You can tell the rotation rates of these different parts, because they refract the sound waves – y'know, like ordinary [surface] waves being refracted by a bathtub vortex, if you will. So it changes the frequencies of the observed modes. And from that information they can tell how fast the different parts are rotating. And the... a result that surprised everyone – and this first became known, I think, about twenty years ago and it's been well verified ever since – the result was that the interior where the heat gets out by radiation – let's call it the radiative interior – that is almost exactly in solid rotation, to within experimental error. We can't see any significant departure from solid rotation. It's as if the whole core of the Sun [the whole radiative interior] were solid, all rotating at the same rate, like a tennis ball. Cricket ball would be a better analogy [not being hollow].

But the convection zone is differentially rotating, some parts faster than others. In fact, the polar parts are rotating more slowly than the equatorial parts, which is what you've... which you observe at the surface. It's been known for a long time that the surface – where the motion is marked by, you know, features being carried along, including sunspots – it's been long known that the surface rotates faster at the equator. But what helioseismology shows is that that differential rotation carries through all the way to the bottom of the convection zone. And then suddenly, at the bottom of the convection zone, where it goes over into the calm radiative interior, you've got a huge amount of vertical shear, because the different rotation rates of the convection zone can't agree with the interior everywhere. So in the polar regions, for instance, the interior is going faster, and the convection zone slower. So there's a lot of shear. Shear, remember, means the change in velocity. The horizontal velocity changes a lot as you move position, in this case as you go up or down. And this shear layer is called the *tachocline*, which means gradient of speed. ('Tacho' is speed and 'cline' is gradient.) Which is a reasonably good thing to call it, and... [laughs]

And this was a great surprise because none of the fluid-dynamical ideas that people had [had previously] predicted this. There'd been all sorts of papers on the... you know, the way the Sun spun down over its lifetime. We know the Sun used to rotate a lot faster than it does now, and it spins down by throwing particles out. These are these same solar storms that might disrupt our electric power networks [if unusually large numbers of particles come our way] – I heard a bit about that on the news recently. So it's always throwing these particles out; and [being charged particles, mostly electrons and protons] they have to travel along magnetic field lines. And that means it's like a wheel with rigid spokes poking out, that... with weights being beads on those wires, being thrown outwards. And if you have that happening to a rotating wheel, you slow it down, because you've got an anti-ballerina effect. And the magnitude of that is such that the Sun's rotation has probably diminished by, you know... we don't know this very accurately, but a factor of ten or so wouldn't be a bad guess – since its early lifetime, a few thousand million years ago.

Now... so people were interested in, how did that diminution in *overall* rotation rate communicate itself to the interior? And there was an episode in the... I think the 1960s, if I remember, where one of the... a physicist called Robert Dicke suggested that the Sun's interior was spinning much faster than the outside, because then the Sun would have a slightly different shape, and then you could explain the behaviour of the planet Mercury in a way that didn't depend on Einstein's general relativity, as most people believed then [to be the correct explanation] – and still believe today, I have to say – the precession of the perihelion of Mercury. You could explain it by a different theory if the Sun's interior were rotating faster – which was of course the Brans–Dicke theory, that he wanted to replace Einstein with.

So, for quite a while, there was a lot of interest in whether the spindown of the Sun could have left the interior rotating much faster than the outer parts. And all the papers on that – there was quite a bit of work on that, and – it all gave you the impression that the Sun's interior was likely to have some differential rotation, even if it wasn't as much as Dicke had hoped.

So when the helioseismic observation showed the Sun's interior actually – as far as we could see – was rotating solidly, it was a great surprise. And then my other astrophysical friend Edward Spiegel, together with a colleague called Jean-Paul Zahn, wrote a paper in 1992 in which they argued that the interior is in solid rotation because it's stably stratified. I didn't say this, did I, but the radiative interior is stably stratified. That's part of why it's very calm. It's like our stratosphere [fluid-dynamically speaking, I mean]. And if you didn't disturb it at all, it would just sit there in its horizontal layers. But if you disturb it a bit, you might expect to get 'horizontal turbulence' – the sort of thing we see in the stratosphere – where the motion is heavily constrained by the stratification. It wants to be mostly horizontal, but can still eddy around in the sort of way that we see outside the ozone hole, for instance. And Spiegel and Zahn argued that that sort of horizontal turbulent motion would have a frictional effect that tended to make the interior go toward solid rotation. And that was their explanation of the tachocline: the convection zone wants to differentially rotate 'cause of its own convection dynamics – and that seems to be reproduced by computer models – and so they argued, the interior wants to be solid because of horizontal turbulence, and the tachocline was the shear layer that has to connect the two, okay.

Now, the moment I heard about that work, I knew it couldn't be right, because I was familiar with what 'horizontal turbulence' really is like, in our Earth's stratosphere. And, as I've said before, the characteristic thing that happens is what you might call anti-friction. It's almost the opposite thing from a viscous or frictional effect. It wants to drive the system *away* from solid rotation, and not toward it. So I knew that idea didn't make sense.

So I started scratching my head about what would account for the Sun's solid interior rotation, and I couldn't think of anything – and ever since then no one else has thought of anything – that can do it, other than the one thing the stratosphere doesn't have. And that is an internal magnetic field... the Sun's interior is very hot, it's an ionised plasma, and that means it's a very good conductor of electricity, and that means that any magnetic fields down there will... if they're the right shape – and that's a not-trivial question – but certain kinds of global-scale magnetic fields in the interior can hold it in solid rotation [or, to say it more carefully, can play a crucial role in so doing]. So... and I actually dared to pipe up and say that in a conference paper in 1994.

And I took it further with Douglas Gough, with whom I published a paper in *Nature* in 1997 [oops, 1998], in which we made a strong argument that not only could the Sun have an interior global-scale magnetic field – and that was no surprise because everybody... it had been long well known that stars *can* have fields like that. But what wasn't known was whether they *must* have them, because there were some theories of how such fields might get expelled when the star was forming, and other theories where they might get enhanced by dynamo action. It was really completely uncertain whether they had them or not.

So Douglas and I said in this paper, look, the observed solid rotation means the Sun *must* have an interior field like this. It's the only way you can explain the solid rotation. Why? Because the only other available explanation, from horizontal turbulence, won't work. That's not what stratified horizontal turbulence is like. And we know that very well from the Earth's stratosphere. So, okay, so far so good. The paper was called something like "Inevitability of an interior field in the Sun". [Actually it was called "Inevitability of a magnetic field in the Sun's radiative interior," *Nature* **394**, 755 (1998).]

But then we still had the problem of how does the Sun organise this field to be of the right shape, to do this job of holding the interior in solid rotation. And I got involved in thinking about that. And... part of the reason is that... well, to cut a long story short... I wrote a number of conference papers and things about this, but, to cut a long story short, we ended up finally with a consistent theory of how this really could happen. And the other interesting feature is, it depends on something else we learnt from the stratosphere, which I've spoken of before, and that is, that if you exert a retrograde force on a rotating fluid you get this 'gyroscopic pumping' effect. And if the force is retrograde – that is, against the rotation – you systematically pump stuff toward the rotation axis. So now if you can... and it doesn't matter what causes the force. It might be friction, as in the Einstein's tea-leaves experiment, or it might be breaking Rossby waves in the stratosphere. In the Sun we think it's actually the turbulent stresses in the convection zone [and perhaps also in some turbulent upper portion of the tachocline], which can exert retrograde forces.

And our proposal for the Sun – and it isn't fully worked out yet – but we believe we have the first parts of a consistent picture of how all this fits together, and part of it is that there's a retrograde force in the, y'know, high-latitude convection zone that pumps fluid polewards, which then downwells on to the [lowermost] tachocline

[where it meets the much calmer interior] from the convection zone [and/or from any other turbulent layers overlying the lowermost tachocline]. So you've got this downwelling at both poles, which is actually... Apart from the origin of the force driving it, it's actually like the gyroscopically-pumped polar downwelling in the Earth's stratosphere. There's an obvious point of similarity; remember, that's what carries the ozone down to the lower stratosphere. So if you say that's happening [the polar downwelling in the Sun], then immediately you've got the possibility of holding the interior magnetic field in the required shape. [Douglas and I had already drawn attention to this possibility; but at the time we had no fully-consistent theory of the effect of the polar downwelling.]

Now I should explain that, if you didn't have this downwelling, there's a chance that the interior field would want to diffuse outwards. It would want to poke its magnetic lines out through both poles, and look more like the Earth's magnetic field. And if it did that, it wouldn't be able to hold all of the interior in solid rotation any more. So the name of the game is stopping that happening. And our proposal is that the downwelling at the poles *will* confine the field because, as the field tries to diffuse out, the downwelling, even though it's extremely weak – it's a tiny fraction of a centimetre per second – it's still enough to counter that diffusion, and hold this field in the shape required to impose solid rotation. Remember, this has to be something that lasts the whole lifetime of the Sun. You've got to have something that holds the field in this particular shape for thousands of millions of years. And that's plenty of time for it to diffuse out, and change its shape, if it weren't for this downwelling.

And very recently I was... lucky to have acquired a very bright research student, a young man called Toby Wood, who's now postdoc-ing in California [and Leeds from mid-2013]; and Toby and I together worked on a theory of exactly *how* the downwelling could hold the field in the right shape near the poles. And we succeeded in finding some beautiful mathematical solutions that described how that part of the jigsaw fits together, which is not trivial because the... you have to take into account that the Sun is rotating. There are, for this purpose, extremely strong Coriolis effects. On such a slow flow, the Coriolis effects are overwhelmingly important.

Furthermore, the magnetic forces are overwhelmingly important. So it's a combination of rotational and magnetic effects, all fitting together to confine the field, just in response to this downwelling. [We called such flows *confinement layers*, by analogy with the classical boundary layers on, for instance, aeroplane wings.] And so that, I think, is going to be seen as an important part of the solution to the problem, even though other work has to be done to check how that fits into the rest of the global-scale picture. [Again, it's like classical boundary layers seen as part of another jigsaw, the entire flow around an aeroplane. We published a big and closely-argued paper on the confinement-layer dynamics, "Polar confinement of the Sun's interior magnetic field by laminar magnetostrophic flow", *J. Fluid Mech.* **677**, 445 (2011).]

In this work at any point, to what extent are you using observational data?

Well, of course, the quick answer is helioseismic data – the people who work with that data, and do all the complicated calculations to deduce that the interior rotates in a certain way, that is. Those *are* the data involved. And that's pretty much it. Of course we know other things about the Sun, such as its surface temperature, and, y'know, that's old knowledge that's been known for a long time. But you might say that those observations are part of the picture, 'cause they're another part of what constrains our models of the Sun.

[0:26:10]

Thank you. And could you now describe recent work on the jet systems of Jupiter?

Yes. Well, that's been another recent interest. The Sun and the Jupiter problem have been the two reasons why I have drifted rather away from the Earth's stratosphere – apart from a feeling that I've probably made my best contribution there, long ago. But I have spoken of jets a few times and the fact that they... in the Earth's atmosphere and oceans, they tend to sharpen themselves, and that this is a sort of anti-frictional effect. This is the very same point that I made about the Sun's interior. If the jet keeps itself sharp it's... the fastest moving fluid is not spreading out, the way it would under ordinary friction. And, okay, we have a pretty good idea of how the jets in the Earth's atmosphere and ocean keep themselves sharp, and a lot of people have tended to think that other jet systems are similar.

(One of them is actually inside fusion power machines. This is rather surprising, but the big machines that confine hot gases by magnetic fields – they're called tokamaks, or the most important kind is called a tokamak – and there's a hope that these machines might develop to give us enormous new low-carbon energy sources. But it's technically complicated. One of the foundations of that hope is that there are jet-like flows inside those tokamaks, and that they help to keep the heat in, and make conditions more conducive to fusion power generation. [There seems to be an eddy-transport-barrier effect, perhaps somewhat like that of the polar-night jet at the edge of the ozone hole.] And I was recently asked to give a lecture to an audience of plasma physicists [the Marshall Rosenbluth Lecture], telling them what I knew about atmosphere–ocean jets. I have to say, it's not clear to me yet just how similar the tokamak jets are.   But, okay, so that's one kind of jet system.)

Yet another... are the jets on Jupiter. Anyone who's seen those marvellous pictures of Jupiter's cloud layer is aware that there are great jet streams, or currents, flowing eastward or westward. And many people in my community tend to think that all these jet systems are terribly similar to each other. And some people even think they're all due to one particular mechanism; it's called the Rhines mechanism.

I actually tend to disagree with that idea. There are actually several different mechanisms that can generate jets. In fact, the nature of these systems is such that practically any disturbance will generate jets. So it isn't really clear that just because you have jets it must be one mechanism or another. But the thing that struck me about Jupiter, in recent years, is that, in contrast to the familiar [strong] jets in the ocean and atmosphere – where I think we do understand fairly well how they work, in terms of mixing potential vorticity – the jets on Jupiter don't meander. They meander hardly at all. They're extremely straight [especially the eastward or prograde jets.]. They... almost everywhere, they flow almost exactly along latitude circles – even at high latitudes, we now know from the spacecraft observations. The main exception to that statement is where they skirt around the Great Red Spot, and one or two other big vortices. But otherwise they're remarkably straight, and very *unlike* terrestrial jets.

So that makes me think: almost certainly it's a different mechanism, probably not PV mixing. I've actually changed my mind about that in recent years. And I've had two students working on this, one a few years ago, who is driving me mad by not finishing his thesis [though we published a beautiful general theorem together, in 2010]. But the other one I acquired just recently. And we're starting to work on a new idea about the mechanism behind Jupiter's jets, which I think is extremely promising. And I don't want to say much more about that, because it's early days and it might or might not work out. But he's started to play with a simple numerical model where you can test these ideas. It's a good example of an intermediate level of modelling – not trying to simulate the whole planet. Efforts to do that [at other research centres, including Oxford] are in their very early stages still. And it's very difficult 'cause we don't know enough about the planet's interior, actually.

But this intermediate-modelling level, I think, is going to teach us quite a bit about how do you make straight jets, using excitation mechanisms that are much more like the ones on Jupiter, particularly the thunderstorms that are injected from below. And we know there are thunderstorms on Jupiter. That's very clear. You can see the lightning. You can see the tops of the [thunder]clouds, and all of that. And we know there's enough water vapour there to get moist convection going – rather like you have in the Earth's atmosphere, especially in the tropics. But it's difficult to say more, because we don't have a good enough handle on the fluid dynamics yet. And that's what I'm going for with this new student [Stephen Thomson, who is now, in November 2013, making excellent progress with the problem].

[0:31:35]

You have a personal website, and I wonder whether you could give us a potted history of the creation and development of that, including perhaps your motivations for starting it, as well as continuing it.

Well, as I recall, the way it started – and this happened in the early days of the internet, shortly after it became possible to write web pages in simple HTML code – we had a computer bloke in the group who showed us how to write such code, and what some of the conventions were. And I realised that that's a good way to put up prepublication papers. Before that, the usual thing would be to send what we call a preprint, a prepublication copy of a paper. If I thought a paper of mine might interest some colleagues, I would tend to xerox copies, and post them around the place. When you think about the carbon footprint of that these days you... rather terrifying [laughs].

But that's what everybody did. So when the internet came along we realised that, well, the internet could do that job for us, and it's qu... nicer in a way. You could... it would become well known that recent publications would be on your website. You'd just put it up, and if somebody *were* interested they could look. And if they weren't, they needn't bother. So I started putting up prepublication papers on the website.

And then... well, I realised it was a way to... I suppose it was an early form of blog. I'm not quite sure what 'blog' means these days. It often means an interactive site, where you say things on your blog, but you've got... there's a slot for people to come back and comment. Well, this isn't a blog in that sense, but it is a sort of place where... well, if you look at my home page it says, "an opportunity to allow the bees in one's bonnet to buzz even more noisily than usual" [laughs], which is a quotation from Hermann Bondi's wonderful Tarner Lectures.

And, well, I have a few bees in my bonnet about things like the scientific ideal and the scientific ethic, and what good science is, and what's going to be important in the future – including this business about lucidity principles. So in fact you can find my website by googling the exact phrase "lucidity principles", provided you spell 'principles' correctly and... [laughs]. It's no good just doing 'lucidity' because then you get all the lucid-dreaming stuff, which is wonderful stuff but irrelevant. So that's one way to find my stuff. You'll find a little page on 'lucidity principles [in brief'], a quick read on what I mean by that, based on perception psychology. And at the bottom, back to my home page, and then you can go to my home page and you'll see various things – some people might call them rants, although I try to do it in a moderate tone – such as the business about carbon dioxide being a *big* input to the Earth-system amplifier, and how clear that is from paleoclimatology [and some undisputed physics and chemistry]. You can find that by looking for ["lucidity principles" simultaneously with] "carbon dioxide". And of course you can find all sorts of publications and preprints still.

It's all a bit haphazard, because maintaining a thing like that [in one old codger's spare time] is something you've just got to do hand-to-mouth, as and when the opportunity arises. So I'm afraid the layout of the website is *not* especially lucid, and I apologise for that. But since you can always use a good search engine like google *on that site* (site, colon, url) [e.g. "carbon dioxide" site:www.atm.damtp.cam.ac.uk/people/mem/], it's not hard to find things really.

And of course, because of its haphazard growth, one of the problems with it is that there are probably things buried in there – needles in the whole haystack – that would give copyright lawyers nightmares, and administrators nightmares. So what you can be quite sure of is that the website as a whole is never going to be transferred to any official archive like DSpace. And what you can also be sure of is that I'm not going to go through the haystack, and try and pull out everything that might be okay for copyright, and put that into DSpace, 'cause there's a huge bureaucratic hassle – well, not only doing that, but actually determining what *is* copyright and what isn't, because in principle almost everything is. You can't sing *Happy Birthday* to someone without violating copyright, you realise. It's a copyright tune.

So actually the resources to go into that, and sort out that problem, simply aren't available to me. I don't have the millions of dollars I'd have to pay the copyright lawyers. And even if I did pay them, they probably wouldn't come up with a clear answer as to what's safe and what isn't. So there you are. In the academic world, we have reasonable rules of 'fair use' where we say, if it's for research and personal edification, on the whole it's okay to reproduce things [in small chunks], provided you acknowledge them properly – and that, of course, I try to do.

For instance, in the *Lucidity* papers there is a copyright tune; but there *is* a rule that says [that quoting it is] 'fair use'. It's *Oh What a Beautiful Morning*, actually. Because I'm illustrating organic change, aren't I? It's a nice illustration of how organic change works in music. It's familiar to everyone. And I actually paid $100 for the permission to publish it originally. But now the papers are on my website, and I'm not really sure whether that... but there is this fair-use rule. I did go into this. As far as I could tell, from publications on copyright, it's okay to quote a short piece of music, without paying anything, so long as it's for research purposes or scholarly purposes, and not reproducing the whole thing and making money out of it – which I'm certainly not doing. So I think that's probably okay, although I'm sure there's a lawyer somewhere who might argue that it isn't. Anyway, right now you can get those papers off my website. You can also get them off the journal site because, in their infinite wisdom, they've made them open-access, I'm very pleased to see.

But if you get *my* copies off my website you'll get them with some handwritten corrections and extra annotations, which you might like to have. And there are many other things like that. So what I really hope will happen is that enough people will find my website interesting enough to want to mirror it, 'cause if enough people mirror it, then basically it becomes... it's outside the reach of the bureaucrats, and it's available to posterity. So please, dear listener, if you think there's anything interesting there, that's worth mirroring, do so, just copy all the files on to another computer. By the time you hear this, it'll be dirt cheap to do this, because of Moore's Law. So please feel free to go ahead. *I'm* just interested in communicating things I've discovered, and giving them to people to make use of.

[0:38:30]

And on the... if you do go... by googling "lucidity principles", you'll see an animation of a... well, perhaps you could describe it and say what it is.

Well, I call it the 'walking lights'. This is a great classic in experimental psychology. You see twelve moving dots, but everyone with normal vision who looks at these moving dots sees a person walking. And that tells you quite a number of things, I think. One thing it tells you is that perception works by model-fitting [as discussed earlier]. The [unconscious] brain is doing what statisticians call a Bayesian-inference operation – or something pretty like it.

There are actually some beautiful theorems that tell you that Bayesian inference is optimal. [There's a little essay on this entitled "On thinking probabilistically" on my home page; the theorems are due to Richard Threlkeld Cox.] And therefore natural selection is likely to have built it into our brains. But, in one way or another, those twelve moving dots are being interpreted by our brains as a certain kind of three-dimensional, piecewise-rigid motion. The brain has built an internal model of that three-dimensional motion, and is fitting it to those data. So, perception works by model-fitting; if you understand that, you understand [as suggested earlier] a hell of a number of things about... other things about how perception works, and, you know, [for instance] identity... mistaken identity in the law courts [remembering that the perceived reality *is* the internal model]. There's a huge number of things that are illuminated by that insight. You can also understand a lot about how music works, which interests me, and you can understand how to develop better communication skills, which is part of the point about 'lucidity principles'. [Sorry, I said most of that earlier. Must be getting tired!]

The other thing that this animation teaches us, or reminds us of, is this business of perceptual sensitivity to organically-changing patterns. The walking lights is a good example of an organically-changing pattern. Some things are changing, more or less continuously in this case, and other things stay the same – such as the number of limbs, and the number of dots. In fact if you watch... and psychologists have done a lot of work on this – if you suddenly take away some of the dots, you feel a sort of jolt, a discontinuity, but you... if you don't... if you can leave enough of them there [visible], you still see a person walking, as if suddenly somebody suddenly covered up the image, or part of it. So that's a jump *away* from organic change; and we're sensitive to when that happens as well.

What else is invariant? Well, the model you're fitting to the moving dots has invariant features, such as the distances between the principal joints. It's a piecewise rigid motion of a certain kind of structure. So there you are. Organic change, as I said before, is important to our ancestors' survival. Being sensitive to that means you can tell that you're in the path of a charging rhinoceros. That's an organically changing pattern, isn't it. The number of legs is invariant; the shape has all sorts of invariant features – but all sorts of [other] things are changing, and you'd better get out of the way quickly, hadn't you. Ahead of conscious thought [laughs].

[0:41:28]

Are there places that a listener might go to consult non-digital materials? In other words, do you have any... have you already or do you have plans to put paper records anywhere for future use?

Yes, to some extent, although I realise these days things on the internet are the most important archive, and so I've tended to focus on trying to make that the main effort. But okay, when you get into the Royal Society, the society does ask you to deposit some biographical stuff in their library – which is still on my list of things to do, I'm ashamed to say [laughs]. But I do... if I don't get Alzheimer's first, I will probably do that at some point. So yeah, it's always worth asking in the library.

And what about letters and, I don't know, drafts and that sort of thing? What will happen with them?

Well, my old correspondence is in a state of chaos. If somebody would like to collect what there is of my old correspondence, it's sitting there in the filing cabinets. I wouldn't mind... I suppose I could just bung it all off to the Royal Society and say, there it is, chuck it out or make what you will of it. So that might survive. I'll have to move out of this office sometime, and I'm sure a lot of the papers in it are just going to be chucked into the recycling bin. But, you know, old letters – who knows, somebody might be interested. I don't know.

I mean, if somebody... you have to, well, probably attain a level of eminence or greatness greater than mine, before somebody's going to spend years looking through old papers. Er, I... it's not for me to say whether anybody might find it interesting, but I'm happy for somebody to look through it, you know, especially after I'm too old to care myself [laughs].

[0:43:24]

And finally, could you say something about how you've found being interviewed for National Life Stories and the experience of it? And you can answer that imagining I'm not here, if necessary. But, you know, how you found the process.

Oh well, it's always... well basically it's been fun. It's fun to have your memories jogged about early parts of your career you haven't thought about for ages. And it's always a valuable exercise to have to try and say how significant your work is [and why], in lay terms. I mean, Lord Rutherford always used to say this to his research students, "If you can't explain your work to your wife it is valueless." Well, with rather technical work like mine, that's a little bit tricky. But it's still useful to try, I think. So whether I've achieved it or not is up to the listener to judge [laughs]. But there you go. No, no, no, I've found it a positive experience, and of course I'm very honoured that the British Library should want to bother to have anything about me at all. So thank you [laughs].

[End of Transcript]